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Conversations with Lewis R. Binford on Historical Archaeology

Introduction

Had Lewis R. Binford—renowned as an archaeological theorist since the early 1960s—never been specifically concerned with historical archaeology, his contribution to this field, through the field's association with anthropological and archaeological methods and theory, would, nevertheless, still be immense. But Binford has also excavated historic sites, thought deeply about the interrelationships of historical and archaeological data, and made a number of methodological and theoretical contributions of primary interest to historical archaeologists. Indeed, even when Binford's work specific to the field of historical archaeology is considered by itself, he must be ranked as a major figure in this field (Figure 1).

Binford, then, is in a unique position for assessing the current strengths and weaknesses of historical archaeology as a part of archaeological, historical, and anthropological scholarship. So his views on these matters, expressed here, will certainly be of interest to all the toilers in the vineyard of historical archaeology. And in elucidating the logic of his own intellectual development, in which historical archaeology played a substantial role, Binford's retrospective view of his involvement with historical archaeology must provide food for thought for all those who are concerned with the ultimate aims of archaeology in general.

This interview was conducted 7 March 1997, at the Binford home in Dallas, Texas, where he lives with his wife, Nancy Stone, an academically trained archaeologist and talented chef. The home is about a 10-minute drive from the Southern Methodist University campus, where he teaches. The conversations with Binford stretched out from early morning through late evening. About three hours of talks—those specifically

concerned with Binford's career in historical archaeology, the field's method and theory, and its place in scholarship—were recorded in a morning session and one in the afternoon.

Binford is now at work on his *magnum opus*, which will generalize about hunting and gathering cultures on a worldwide basis. He works on this at his home, with its mixture of comfortable furniture, striking antique pieces, and mementos of an extraordinary scholarly career, which has taken him to the far corners of the world—the Arctic, Tierra del Fuego, Australia, Europe, Africa, Southeast Asia, and beyond.

A glassed-in, tiled, sunken patio, off the Binford kitchen, is dominated by a mural-sized oil of him in an Eskimo village. This painting, by an admiring British artist, was used on the paperback edition of one of his books. The patio looks out over the garden, where Binford likes to putter around with plants.

But the scholarly heart of the house—what had been the master bedroom—is the two-level study where he writes. The main floor is lined with books on one wall. On the other side of the room is a table with a desktop computer. Behind Binford, when sitting at his computer, is a long bank of file cabinets. The files contain, as Binford says in the interview, “nothing but material on hunters and ethnographic material on hunters and gatherers.”

Although most archaeologists are now familiar with the Binford charisma through his public presentations, the massive goodwill he exudes in informal situations can only be suggested by the brackets, such as “[laughs],” in this published interview. He speaks with, what the interviewer's wife used to refer to as, “a really charming accent”—a trace of the South modified by long residence in the Midwest, California, and the Southwest.

In large measure, while Binford's portion of the conversations was spontaneous, the interviewer relied heavily on prepared questions. An edited transcript, with minor changes from the verbatim transcript, was sent to Binford for approval. He asked for only cosmetic modifications,

or as he joked in one telephone conversation, "I'm putting verbs in some sentences that don't have them."

The interviewer was an undergraduate student of Binford's at the University of Chicago, and worked under his direction during the Carlyle Reservoir excavations. Later, he took graduate courses with Binford at UCLA.

The interview was undertaken with one overriding thought in mind: "What would most historical archaeologists ask Binford if they had an opportunity to quiz him about their field?" In fact, the questions of some colleagues, who shall remain anonymous, were solicited before the interview was conducted.

The Interview

[Question:] *Historical archaeology, more than most disciplines, has been very aware of its*

roots. Had Marlon Brando not given the word another meaning, it might be said that some scholars are regarded as the "Godfathers" of the field. Through your career, have you had any contact with, or were you influenced by, the "Godfathers." First, J. C. "Pinky" Harrington?

I first saw Pinky Harrington when I was in junior high school at Norfolk and he was working at Jamestown. I had a field trip arranged through the school I went to. Harrington gave us children a talk on Jamestown and walked us around. This was the first time I had ever seen him. Later I just decided I was interested and went to Jamestown and volunteered at the excavations.

[Q:] *Was that your first excavation?*

Yes.

So I worked as a volunteer. I basically carried things and dug holes where people told me. But

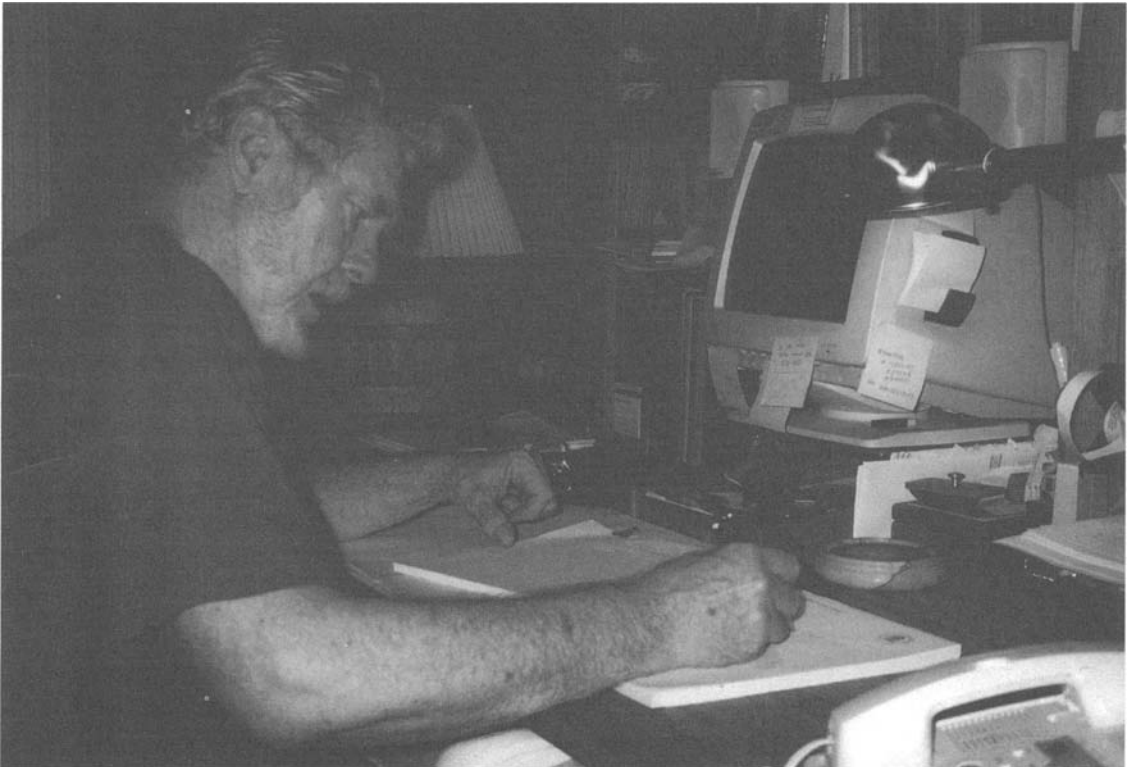


FIGURE 1. Lewis R. Binford at work. (Photo by Nancy M. Stone.)

that was when I met him. I got to know him a bit. And then I didn't see him for years and years.

When I started the pipe stem work, I had, by that time, read a lot of historic sites archaeology because I was working on the ethnohistory of groups in Virginia. And, of course, any time I'd see Harrington's name, I read the paper because I knew him. So I knew his writings and knew his work—early work—on pipe stems. Later, I began to try to apply his early work to material I had collected.

[Q:] *When was that?*

That would have been 1953–1954.

[Q:] *And at what stage were you in your career?*

I had just entered the University of North Carolina. I had just come back from the military. I had been to VPI (Virginia Polytechnic Institute) before. But I had to take all these undergraduate degree courses. I had transferred a bunch of courses. All the science courses were acceptable, but none of the social science courses. I had to take them all again, so I just entered their degree program.

Harrington had given me some—I forget now what it was—piece of information some years before. I couldn't find the note, and he couldn't find it either. We joked that we were losing data [laughs]. So I worked with the pipe stems and a knowledge of Harrington's work. I frequently had questions, and would call him on the phone and we would have long talks. I think I talked about this in one of Stan South's Conference publications. Anyway, I continued to talk to Pinky on the telephone, but when I was at Michigan, I stopped working on pipe stems, and I didn't see him again.

[Q:] *What about Noël Hume?*

I was unaware of Noël Hume until Stanley South started being critical of him. South had made a decision to go into historic sites archaeology, and we talked. He sort of kept me edu-

cated. Up at Michigan, Stanley would keep me informed about what was going on. And we'd write or talk on the phone. Stanley was not happy with the ideas of Noël Hume—particularly about ceramic typology, but also various other things. I think I met him one time when I attended one of Stanley's conferences.

[Q:] *John Cotter?*

That's a different story. You might not have known it, but you were illegally working on John Cotter's money at Carlyle.

[Q:] *Wait a minute, Lew. Are we beyond the statute of limitations?*

Oh, I think so. [MDT laughs.]

At that time there weren't any laws really yet on salvage archaeology. The Corps of Engineers didn't have any legal responsibility to do archaeology at all. But they had decided that they had a kind of moral obligation. So they were looking for people who could finance work in the Carlyle[, Illinois,] Reservoir. But they didn't have any money.

I found some money from Chicago—a little bit. "Mike" [Melvin] Fowler got some from SIU [Southern Illinois University, Carbondale]. But the person who financed that work was Johnny Cotter. I had known Cotter before, and Mike Fowler had also. We called around various government agencies before, looking for somebody who might be sympathetic to what was going on. And John Cotter dumped Park Service money into the Corps of Engineers project at Carlyle and supported us.

He always supported me. Right from the beginning he liked what I was doing. He had been at Jamestown, and he knew about historical work. He always supported me. Right. Every time I needed it, I got John Cotter's support.

[Q:] *Charles Fairbanks?*

Now I knew Chuck, but I didn't ever work with him. I just knew him as an important person and read his work.

[Q:] *"Bunny" Fontana?*

Same thing. Perhaps I interacted with him a little more because of his early ethnohistory on the ceramics of Pima/Papago, which I was interested in getting documented, and he was the only person doing it. But it was more of colleagues talking to one another. And it wasn't . . . "historic" [laughs].

[Q:] *George Quimby was saved for last of the "Godfathers," for Quimby stories have been known to fall from your lips. How did you meet him? Wasn't he then generally known as an eastern U.S. prehistorian?*

Well certainly, when I first met him he was known as an eastern prehistorian. But I actually can't recall when I first met George Quimby. I think, I *think*, that the first time I met him was on an archaeological site up in Wisconsin. I was visiting the site, and Ritzenthaler was excavating.

Subsequently, George used to come to Ann Arbor frequently to look at collections. He was a very diligent comparison person. So, if he had some material, he would travel to wherever there were comparable materials he wanted to look at.

He was a frequent visitor to Ann Arbor when I was a student and would come with some beads or something to compare to the collections. Then there would always be a cocktail party at somebody's house and George and all these people who had gone to Chicago earlier would talk over old times. And this was the context in which I got to know George. Then when I went to Chicago, I got to know him on a much more friendly basis. And I stayed at his house lots of times and we were just good friends.

[Q:] *How did you get together with Quimby for the joint work in the Fieldiana series?*

George would go almost every summer and do some fieldwork. Usually it was mainly making surface collections. And he would come back and try to compare the stuff he found.

One year he had some kind of deal with a wealthy person with a yacht, which took them around the shore of Lake Michigan and part of

Lake Superior. They would put the dinghy over and George would go on shore and make surface collections, then get back on the yacht and go to another place.

He found several sites up in the Great Lakes with what he called "strange lithics," and he brought them to me and said, "What are these things?" I looked at them and told him, "Well, I'm not sure, but let's see more." So he made huge collections.

I analyzed these lithic collections and came to the conclusion, basically, that they were not wedges, but cores. At that time, all we knew about the sites George had visited was that they were relatively recent. He thought this was very interesting, and got all excited. So we published a joint paper in *Fieldiana*.

[Q:] *Were you new at Chicago when you wrote that?*

I think I started writing it at Ann Arbor. Maybe I was at Chicago. Anyway, we didn't publish it right away. It was certainly published while I was at Chicago, but I think I started analyzing stones before I left Ann Arbor.

[Q:] *By the time of your Master's thesis, you were concerned with ethnohistorically known tribes. How did you come to select this topic? Was the topic selected before or after your involvement with Joffre Coe's program of excavations? Was the topic considered to be part of the elucidations of this program?*

Many of the sites that I used in that were sites that I'd discovered before I was ever in the university, when I was in the Boy Scouts and when I was just an interested young person. I would go out and find artifacts, as many people have, but pretty quickly stopped collecting them. I didn't save them very much. I put the information on a map. At that time I didn't even know there was a prehistoric past. I just knew there was a history. So, I immediately tried as a high school kid to relate all these sites to the tribes that were known at the time of colonization. I had these little maps of where the various his-

toric tribes were, and the maps of where I'd found sites.

Some of the sites were Archaic and some of them were Woodland, but they were all organized that way. Then later, even before I went into the university, I began to realize that some sites had early colonial items on them, like pipe stems, and others didn't. So I began, in my own little world, a segregation. Later all that stuff was just stored in Norfolk after going into the service.

When I went back to the university, Joffre Coe was into tracing the Siouan tribes, and his whole thing was identifying the archaeology of the Siouan-speakers. It seemed to me that he was doing what I had tried to do as a high school student. So, he said, "Well, if you want to do a Master's thesis, you'll have to do researching; you'll have to do this, and do that." And I said, "Well, look, I have all this stuff," and I showed him the collections that I had and the maps that I had. This stuff was from an area that he had never worked in. It was just to the north of where he had stopped his kind of research.

[Q:] *So you had never worked for him in the field before that?*

No, I hadn't.

Then Joffre said, "Well, why don't you do the ethnohistory and go back and do a survey on top of it?—because you already know some of the land owners, and you already know your way around. Do a modern survey in this area and use that."

I said, "fine." Then I started driving every weekend from Chapel Hill up to the Virginia–North Carolina border, contacting land owners and surveying for sites. And as I read the ethnohistory I became convinced that Joffre Coe was wrong about the trading path from the Virginia Colony. I said it.

In my opinion, I couldn't defend the argument that all those traders were going out from the Virginia Colony to Okenechee Island, up near Clarksville. What everything said was that the trading path was down the edge of the Piedmont, to what would be Roanoke Rapids, North Caro-

lina, not Clarksville, Virginia. So I began to bring in data talking about this. He was not happy. So our first big argument, our first conflict, was over where the first Indian traders out of Virginia Colony were going. And that was a big part of my thesis. It just seemed that what I'd been doing led directly into difficulties with what Joffre was presenting to me as archaeological method.

[Q:] *Did you ever develop the idea that there was a basic difference between historic and pre-historic archaeological data in terms of how the data should be conceptualized?*

No. Joffre was the big believer in the direct historical method. But my view was, look, if you're going to look at the past, you have to take advantage of your knowledge capital—where you have the most knowledge. If you have the most knowledge about the past for Native Americans during the colonization period, and if you want to learn anything about the archaeological record, then the chances for having some control information are going to be greater for the historic period.

I never thought particularly about this, although I was certainly aware of the argument; but it seemed to me, very quickly, that the direct historic approach argument failed—that is, because so much of the historic period was already modified by Europeans. It was very tenuous to take that back into the Pre-Columbian Era and identify a group of people. I never was very comfortable with that, but I was very comfortable in using the knowledge from history and the historical record to give me some idea of what things might look like in the archaeological record of that period.

[Q:] *Yet through the early part of your career you continued to have a good deal of contact with scholars who were involved one way or another with the direct historical approach. Besides Joffre Coe, who also worked on the Cherokees, there was James B. Griffin, who less directly tried to tie the Shawnees to the Fort An-*

cient Aspect, and Albert C. Spaulding, whose most extensive monograph was on Arzberger, a protohistoric Arikara site. Other than Coe, did any other scholars directly or indirectly influence your views on the value, or lack of it, of the direct historical approach?

It always seemed to me that if we had ethnohistorical material we could say “Ok, the Arzberger site is perhaps a proto-Arikara site. Certainly in the case of the Carolina Hill country, many of the sites were historic Cherokee with historical colonial materials. So we have pretty good reason to believe that this stuff is Cherokee.

But what made the direct historical archeological approach to ancient materials possible were all these assumptions about the nature of culture history—that similar squiggles on the pot meant you’re in the same tribe.

I always thought that the archaeological record was probably more interesting than that; that ethnicity itself was something that was caused. It just didn’t come from God. Ethnicity is something that represents a reorganization of people under new or varying conditions.

Sometimes you have great big ethnic groups and other times you have groups like the Mattole in California, where there are only 200 people in the whole ethnic group and they’re an independent language. What is causing that variability is something archaeologists can address—and the historians and ethnographers can never address.

To take these little assumptions of “similarity equals influence” means there was a mindless passing on of customs. Clearly this was probably not the best way of looking at the archaeological record. The past was probably much more interesting than that.

[Q:] *In one of your books, recalling preparation of a paper for a SHA meeting, you say something about “that word historical” in the term “historical archaeology.” This seems to imply that you see historical archaeology and archaeology which does not use historical documentation as being, somehow, basically different.*

Is this really the case? And if it is the case, about when, do you think, you developed this perception?

I think your allusion is to a conference I attended after I’d finished the first season of fieldwork with the Nunamiut Eskimo. My comments about the word “historical” were all about one issue. When I worked with the informants up there, they’re sitting there trying to remember what they did when they were 20 years old.

Much of what they were doing was the same thing I, as an archaeologist, would do. They were making inferences; they didn’t remember. They were bringing in ancillary information, and saying, “well, it must have been; I must have been doing this because of some other circumstantial evidence.” It wasn’t really historical information. It was just that they had more knowledge with which to make an inference than I did.

Do you see what I’m saying? It was so striking when I worked with the Eskimos—most of what the informants told me was not fact in the sense of an accurate recollection of an event. It was inference, but based upon much more information than I had. It was in this context that I was commenting on history.

But your question goes a little bit beyond that. I never thought there was anything different about historical archaeology. It just, maybe, has a richer body of information to bring to specific archaeological experience. But still the general archaeological experience is no different.

[Q:] *In your publications, you have eloquently developed this position that ethnographic observations are no more “direct” observations than the kind of observations made by archaeologists dealing with the archaeological record. How, then, can one incorporate ethnographic and archaeological observations into a single framework and still be methodologically sound—since there are different sets of presuppositions underlying the two kinds of data?*

In the book I’m writing over here right now [pointing toward his computer], this is the only subject that the book has. That’s because there

have been so many silly arguments in archaeology about the use of ethnographic observations. And my answer is that the only reliable way to use them is to be able to explain them.

If archaeologists are going to use ethnographic material, they have to treat the ethnographic data just the way they treat the archaeological data. They have to analyze the ethnographic data to come up with explanatory arguments as to why the world was patterned the way it seems to have been patterned when documented by ethnographers.

You have to develop a theory—let's say a theory of group size. In ethnography you get people telling you how big the groups are; in archaeology they're not telling you that. We have to infer it. The archaeological record is not giving us estimates here. We have to use all kinds of circumstantial evidence. But in ethnography people give you estimates, "There were 23 people. There were 17 people." I can't use this until I find out what's causing the variability in group size.

Now, if I find that groups are large in the Tropics, but they are smaller at 35° latitude, and that they're big again in the near-Arctic, but they become very small in the Arctic, then I have a pattern—just like an archaeologist might have a pattern in something he studies.

Then I have to explain why the world was the way it was when documented ethnographically. And if I can explain why group size varies with environment, then I have a theory that should apply to the archaeological record up to some point.

[Q:] *Are you saying that it does not make any difference what method you use, because if you use a method and perceive the pattern, the pattern is there regardless of the method you used in the recognition of the pattern?*

No, I'm not exactly saying that. I'm saying that we know that people lived in groups and ethnographers may give me estimates of the numbers of people. In the archaeological record, we have no direct estimates that we can dig up.

What we have is all kinds of other archaeological evidence, perhaps—maybe even evidence that we don't know how to recognize—which can tell us about group size.

But if I can take ethnographic data and study the pattern in the ethnographic terms—what a group is, how big it is—and show that it varies in regular ways with the environment, the demography, whatever—then I can build a theory of group size. Then I can reason from this theory to the archaeological record and see what correlates with these ethnographic estimates, and perhaps then develop a method for monitoring group size with archaeological observations.

That's basically the procedures I think we have to use. Use ethnographic information as prior knowledge for developing a method to use with the archaeological stuff. But the ethnographic information is not used to *interpret* archaeological material. It only becomes really useful when one has a theory—an explanation—for the variability in the ethnographically documented properties.

[Q:] *So an explanation which will explain all diversities of data can be obtained from any particular class of data?*

Exactly, exactly, exactly. It's just a matter of how you use your resources. If I've got better knowledge here, I develop the argument here. Take it over here and see when it fails, because when it begins to fail I've got a chance to learn something.

[Q:] *But can ethnographic observations really be "tested" with archaeological observations, or vice versa?*

I can develop a theory, an explanation, with ethnographic material. Then I can take that explanation to archaeological properties, and I can find where the properties are consistent with what I expect, and where they differ. And when they differ, perhaps I've reached the limits of the range of variability that was documented ethnographically, and I'm now looking at some variability for which there was no ethnographic

documentation. So the first thing the ethnographically derived explanation allows me to do is diagnose that threshold. The second thing it allows me to do is say: “Okay, the most likely situation is that in the ethnographic material I only have this much variability, while in the past I’ve got this much—What, then, is the likelihood that the same variables are causing the variability where only the ranges of their values differ in the past from the ranges known ethnographically?”

So I’ve already got a good way of thinking about what it is I may not know. I’m not lost intellectually if I’ve already done the work on the ethnography.

[Q:] *Your thinking on the interrelationship of ethnographic and archaeological data can be easily analogized to the historical documentation used by historical archaeologists. We have traced the development of this thinking from your first scholarly essays on this at North Carolina. Let’s go back to that time for a while.*

Someone told me that there were five graduate students in anthropology at North Carolina when you were there. These were, it was said, you, Stanley South, Aubrey Williams, Ruben Reina, and someone else. Was this the case?

I was a graduate student with Stanley South, Aubrey Williams, Hester Davis (E. Mott Davis’s sister), and a Belgian woman whose name now escapes me. But I can see her. She was Belgian, and her training was in Acheulian. And there was a Margaret Mead student by the name of John Grant. And that was essentially it. If Ruben Reina was there, it was for a very short period of time. So it was six of us.

Stanley and I, of course, were learning archaeology with Joffre Coe—but it was all North Carolina. Hester was pretty much the same way, only she had a brother who dug lots of things outside. John Grant was trained by Margaret Mead and had decided to go into archaeology. I don’t know why. He knew nothing about archaeology and all he wanted to talk about was the swaddling hypothesis for the Great Russians, or something like that [laughs]. So the Belgian

woman was important to us, because she had archaeological interests outside North Carolina. God, Mel, I wish I could remember the Belgian woman’s name.

[Q:] *If the department had only six students, was your contact nevertheless much closer with some than with others? How did you first meet Stanley South? Did you do any significant amount of fieldwork together?*

There were several reasons that South and I were much closer. We were both older. We’d both been in the military. He’d been in the navy. So we were both veterans. Both of us had had a different career earlier. South had been a school teacher; I had been in wildlife biology. We were married. We had families. We shared all those things that we didn’t share with all the other students. Hester was unmarried, and so was the Belgian woman. John Grant was married, but they were like teenagers—I mean they were very young. The Belgian woman—I never knew what she was doing in North Carolina—was somewhat older.

Stanley and I met there at the University of North Carolina. We became fast friends the first week we met.

I don’t know why, but every time Joffre Coe wanted something done, he would call Stanley and me and say, “I want you to go this weekend and put a test pit on the such-and-such site and bring me the pottery.” He was forever sending us off to do things, and we did lots of those things together.

Then we were looking for thesis material. I already had material for my Master’s thesis; Stanley had to get his. So Joffre got money from the Virginia Electric and Power Company to do some salvage archaeology at the dam that they were building on the Roanoke River. It was just assumed that I was going to help Stanley dig, and the Roanoke Rapids project paid enough money that we could buy peanut butter and bread, I think. We lived in tents and camped way out in the middle of the reservoir and did the archaeology. And that was one of the big

sections of Joffre Coe's doctoral thesis as well.

So Stanley and I did fieldwork together at North Carolina. Hester sometimes came down to see what we were doing, but she didn't do fieldwork with us much. Aubrey didn't. John Grant didn't. I don't know why Joffre didn't get them to do fieldwork.

[Q:] *Can you recollect if South then thought of the possibility of specializing in historical archaeology?*

No, Stanley had no interest in historical archaeology when he started. And part of the reason that Stanley went into historical archaeology in the long run was because it was the only way he could stay in the region and not be told what to do by Joffre Coe. It was the same reason I went to Virginia for my thesis fieldwork, and the same reason I went to Michigan [laughs].

Joffre viewed us as just people in his footsteps. And he also only wanted people who thought just like he did. And if you didn't think like he did, you were a traitor. So Joffre viewed both Stanley and me as traitors, or at least we felt he did. He gave Stan some hard times when he was working in historical research in Carolina. Joffre was never supportive of me after I went to Michigan. Never. No support whatsoever.

[Q:] *You seem to imply in one of your books that your decision to go to the University of Michigan was connected with Joffre Coe having gone there. Was your decision to go a function of Coe's academic lineage, his concern with Leslie White's work, or something else?*

Joffre recommended that I go to Michigan. Although I had problems with him, I thought that was good advice. So I went. And I did want to study with Leslie White.

[Q:] *At Michigan, there were several teachers who seem to have influenced you positively in one way or another. You have several times mentioned the influence of Leslie White's theory, and Albert Spaulding's archaeological outlook. At your famous 1965 American Anthropological Association symposium in Denver, David F. Aberle*

was one of the commentators on the papers. Was the influence of these teachers primarily from the point of theory or method, or both? Do you recall what courses you took at Michigan?

Michigan at that time had a really impressive staff. It was a high-quality department. David Aberle was teaching social organization; Marshall Sahlins was teaching courses in state formation; Elman Service was teaching on hunters and gatherers; Richard Beardsley was teaching Asia and archaeological methods. There was a wealth of really superb teachers.

I had become pretty close to David Aberle because I had taken his courses and found them fascinating. He hired me once to do background research. So I was reading reports of the Indian agencies to the government. These were detailed reports by various Indian agents on what was happening on the reservations in the last part of the last century and the early part of this century. I was just accumulating information, some of which Aberle later used in his peyote book. I got fascinated by social organization really through him. So it was my suggestion that we have Aberle come to the Denver session.

I don't know anywhere today—we never really achieved it at Chicago in the 1960s—where you had really fundamental strong education in all fields of anthropology. At Michigan we had James Sphuler in physical. We had Fred Thieme. We were really strong in physical anthropology. You were really educated in anthropology per se, and that was primary. Then what you did for your thesis was secondary. So it was a superb education in a broad view of anthropology.

This opened up to me areas of relevance that I hadn't even imagined. I learned that it was important to know the ethnography all the way around the world—to know the range of variability.

[Q:] *Do you feel that this is the kind of training that is still absolutely necessary regardless of anthropological specialization, and—specifically for an archaeologist—regardless of the kind of archaeology?*

I do. I think the broader the base of one's knowledge, particularly of the variability in human organized ways of living, the better archaeologist one is going to be.

[Q:] *And should historical archaeologists also have this kind of background?*

Yes. Absolutely—because historical archaeologists usually see their historical materials in a narrow event sequence way, rather than in a systems comparative fashion. I think depth of knowledge is critical to being a good anthropologist, a good archaeologist, and a good historical archaeologist.

[Q:] *I seem to remember that once at Chicago you mentioned that you used to sometimes attend Plains Conferences. I assume that you more often attended Southeast Conferences. In both areas there was then a notable development in the direct historical approach, if more so on the Plains. The Plains, however, in the work of Spaulding, Donald J. Lehmer, and a few others, had a notable quantitative archaeology component. On the other hand, Stanley South's Conference on Historic Sites Archaeology got started in connection with the Southeast Conference.*

Were these factors in your attendance? How would you characterize your involvement with these meetings? At the Southeast Conference, if you gave papers, were they more often directed at prehistorians, those using the direct historical approach, or in South's sessions?

Yes, I certainly attended Plains Conferences, and I read almost all of Waldo Wedel's work and the direct historical approach on the Plains. But my interest in the Plains Conferences was with the early Archaic material that was coming out of the River Basin Surveys on the upper Missouri—not down where the Mandan and Hidatsas were, but upriver.

[Q:] *Let me ask for a clarification here. Was this before or after Willey and Phillips's book came out? One of the problems with their formulation was that they had not been able to iden-*

tify Archaic on the Plains. Was your work connected with that problem?

Yes it was. Willey and Phillips came out when I was a student at North Carolina, but in two installments in *American Anthropologist*. It was not a book, at first. I don't remember when I had the first look at Willey and Phillips, but the issue of the Archaic was a huge one when I became a student. This was because you then didn't have C-14. You had this alleged big temporal gap. And you had the estimates that most people were using. The peopling of the New World was imagined in two waves: the Paleo-Indians, and then another about A.D. 1 That was the magic date. So in the chronology that we were talking about, essentially, all the archaeological record fit between A.D. 1 and Columbus. And then you had Paleo-Indian way down here, and you had this huge gap between.

Once you had the beginnings of C-14, almost everybody was working on this Archaic problem. There was great excitement, and once you began to expand the chronology backward, you began to see an Archaic in places other than New York. Ritchie had defined the Archaic, in 1941, based on New York material. And other people would say, "We don't have any. We just have a lot of projectile points mixed up with pottery."

To get good segregation of the archaeological material, you have to have stratified sites. That's why Joffre Coe's stratigraphic archaeology really impressed Stanley and me. He was way ahead of his time in learning how to find stratified sites. When Stanley and I were students, the only stratified sites in existence in the Southeast were Joffre Coe sites. There was some super-positioning from WPA, but they didn't have any really stratified sites.

[Q:] *Is that primarily in Coe's American Philosophical Society publication?*

That's right. But that was published very late. Joffre had most of those sites before the war [WWII].

[MDT:] *You were talking about the Plains Archaic, and Plains and Southeastern Conferences.*

At that time there were several “capitals” for archaeology. Lincoln, Nebraska, was one of them. Joe Brew was there. All of the offices for the River Basin Surveys people were there. Preston Holder was there, at the university. And Wedel and his people were in and out of there all the time. So it was one of the great field capitals of archaeology.

Atlanta was another one, with A. R. Kelly and all of his people in and out all the time. So you went to Lincoln or Atlanta to find out what was happening in that area of the world. And I started doing that fairly early. I was not in Lincoln paying much attention to the direct historical work. I was there on the problem of the Archaic gap and the excitement of finding and getting chronology on these projectile points—because at Ann Arbor I was working on projectile points.

As far as the Southeast goes, I also attended Southeast Conferences. Stanley South and I participated in the first one that I remember attending. I think we were both at North Carolina then. Both of us were arguing about prehistoric archaeology—with Steve Williams and the Harvard gang—on Duck River materials and the Tennessee Valley stuff. It was not until later that Stanley actually went into historic sites work, when he went to Brunswicktown. About the same time, I had the opportunity to work at Fort Michilimackinac.

This was a time when Stanley and I were both working on historic material. He would send me slides of his ceramics, and I would send him slides of mine, and we would trade a lot of information. That was in 1959 and then again in 1961, because I ran crews those years. Moreau Maxwell ran the crew at Michilimackinac in 1960.

[Q:] *Were you doing pipe stem work then?*

When I went to Fort Michilimackinac, I'd already done a lot of work on the pipe stems from

Virginia, even though Joffre had thought it was silly to try to develop a new dating technique for them. He told me I was wasting my time. When I went to Michilimackinac, I just continued the use of what was already done.

[Q:] *Under what circumstances did you first come across W. W. Taylor's book?*

That was in a course with Joffre Coe. And the course was called “theory.” We had the two articles in the *American Anthropologist* which later became the Willey and Phillips book. We had Taylor, and we had Duncan Strong's article on the direct historical method. And that was all the readings there were in the methods and theory course. [Laughs.] Well, there were a few others, but there was really nothing to read then. So we all read all those articles many times.

[Q:] *What was Spaulding's reading list like when you got to Michigan?*

The first course I had with Spaulding was in African prehistory. It was a very up-to-date reading list on the literature of the Paleolithic of Africa. This was before there were the potassium-argon dates from Olduvai. We read the pre-war stuff of Leakey, and all of Dart's stuff, and Broom, and all those people. But it was before dating. The first dates came in 1959.

[Q:] *Did you have a theory course with Spaulding?*

I did; it was more about method than theory. It was basically on the use of statistics.

[Q:] *What kind of reading list did he have?*

He had a lot of readings on statistical literature.

[Q:] *Do you mean like Brainerd and Robinson and that kind of thing? Or do you mean mathematical statistics?*

Mathematical statistics. We had to learn how to do all these things, and then of course we only had slide rules to work with, or do it all by long hand. Then it was an application. You

know, we had to read Brainerd and Robinson and all the seriation strategies in archaeology that required counting things. So we had to read Kroeber's article on Zuni potsherds and we had to read Ford—all the Ford literature. We had to read examples of quantification which, at that time, were only from the lower Mississippi Valley survey by Griffin, Ford, and Willey. We would take data out of that to use as examples for statistics.

[Q:] *I audited your methods and theory course at Chicago—the graduate course. You had a massive reading list. When did you compile that? What were the circumstances?*

Over time. They're just horrible now. When I would take courses, I would not just do what the course required. I used the reading list to get an *entrée* into the literature. Then I'd go and get what was cited in the readings, and read these. I began to accumulate these files, and I still work that way.

These files—[turning]—they're all full, and there's nothing but material on hunters and gatherers. There's no place on Earth that has this much information [laughs] on hunters and gatherers. And that's the way I've always worked. You may have seen the big bibliography on technology I gave out in California.

“Oh, yeah we done that” [mocking himself, laughing, and rifling through the files]. But these are every case. Every file folder is a different known case of hunters and gatherers. And then you pull them out, and there's 35 articles or something in a file [laughs].

When I was a student there was nothing to read. When we took archaeology of Eastern North America, it was going through Griffin's book, *Archaeology of Eastern United States*, focus by focus. That was what it was.

[Q:] *W. W. Taylor once thought of using the term “functional archaeology” for what he finally called the “conjunctive approach” to archaeology. As I recall the development of your work, the basic character of Processual Archaeology*

had been developed by you before you became intrigued by General Systems Theory. Some have criticized General Systems Theory (GST) as being “functionalist” in nature. Was your turn to GST stimulated by Taylor's approach, or were you trying to move beyond functionalism? Indeed, in your opinion, is it possible for any archaeology to move beyond functionalism?

I don't think my views of functionalism have changed very much since I was a student. If you take Radcliffe-Brown, the father of functionalism, as the example, functionalism was psychological anthropology. He was trying to explain things in terms of sentiments and emotional feelings of people, and so the explanation was not that things go together in a system the way I think about it. He thought that things are related in people's minds in terms of overarching concepts, sentiments, and emotions. These are all reductions to motivational kinds of explanations as far as I can see. Radcliffe-Brown was saying, “This happens because people think this way.” Functionalism—in the anthropological sense—to me is just another idealistic thing.

Yet, particularly in the world of physics, function has another, very important, meaning. And it means the dynamic role elements or material phenomena play in a system. What is the function of a carburetor? Well it does this, *vis à vis* the maintenance and flow of energy through the system. So my notion of function is this second conceptualization of function. I never paid much attention to functionalism as it developed in anthropology.

When it comes to looking at a system—anything for that matter—there are two things you have to do. You have to ask, “What is the world like?” Because, if I assume that the world is flat, I can waste a hell of a lot of time trying to explain why. So [laughing] you want to know what the world is like. Then you have to define how the world is organized into systems, and how energy flows are integrated.

You can't look at the history of World War II and not understand what a system is. It has various kinds of differentiated roles. There was all

kinds of hierarchical state differentiation. There was differential wealth. The United States was essentially financing most of the hardware of the war. And so there were all these integrations of flow of material matter, energy cost, energy sinks. The battlefield is a giant energy sink. So you have to be able to describe the world in ways that are germane. You may not know yet what is really germane. But you have to be able to describe the world in ways that may be germane to the way it works, not what one “feels” about it, or what motivated the participants.

My view is that what people thought about World War II is not of much help in understanding the system involved. (They claim you have to understand how people are used, how people are manipulated, how people are made to be motivated to kill themselves.) You have to be able to look at a system in a more materialist way than this—as a system of energy flows and cycles of nutrients and such things.

So that has been my view of function and structure. Given that view, structure is a statement of the limitations of function; a simple thing. If the piston is going up and down in the engine, then structure is the upper and lower limits of movement for the piston. It—structure—is the constraints on dynamics in a system. So systems have structure, and if dynamics get out of hand, a system blows up, it goes to a new level of organization, or it goes extinct. It just doesn’t stay the way it is. That has been my idea of structure and function. When I talked about function, I was not talking about the kinds of things Radcliffe-Brown was talking about.

There is a third usage of “function.” This is the archaeologist’s view, as when one asks: “What is this used for?” “Is this a skin scraper?” “Is this a lance?” “Is this a projectile point?” I never really thought I could give an answer to such questions until I did analysis and saw what role the artifact played within systems of the sort already mentioned. So I ask about the artifact: “Does it co-vary with bone? Is it more common as an isolated find and less common as a complete item in a village?” Until I can see the patterning, I can’t give you much of an opinion.

This is why I argue with Schiffer and others, who want to interpret everything. They make up little stories of the Boy Scout sitting around the fire. They want to see the event. I never thought the event was that important. I want to know how things were constrained by structure and pushed by dynamics, repetitively over time.

That was my idea of structure and function.

[Q:] *You raise a question in my mind. You gave the example of World War II. If someone said, “Lew Binford, we will do the archaeology of World War II,” how would one go about doing the archaeology in terms of defining the system operating for carrying out World War II?*

You would have to have a lot of money. [Both laugh.] Almost as much money as there was in the war. But the point is well taken.

[MDT:] *With inflation, maybe a lot more.*

That’s right. [Both laugh.] You would have to begin to understand how the events that were taking place in the U.S. were related to the transport of material to the Old World—and related to Nazi submarine warfare. Why did the first big push come in North Africa? Isn’t that interesting?

Well the Germans, obviously, were in North Africa with their crack troops. You basically didn’t have to go through the North Atlantic in order to get logistics into North Africa. You didn’t have to go through England. You didn’t have to go anywhere the Luftwaffe could get at you. And if you could get inside the Mediterranean—the mouth of the Mediterranean—that was the easiest place to defend from the air, or against submarines. So don’t send all the tanks to England, and then on to Germany. Send them right to the battlefield. And there was only one place on Earth where there could be such a battlefield.

So you didn’t want to take the chance of trying to get the material into England, because the submarines had England blocked. Then they were subject to being destroyed by the Luftwaffe. You want to avoid all that. It was a risk reduction strategy that worked like a dream. And basically, once you had North Africa, then you could go

through Italy, which was the softest part of the Axis military support. And the Germans weren't anxious to really put their goodies in there. And also they made a stupid mistake with respect to Russia. So, if we could go in through Italy, knock out the Italian support, let the Russians take the brunt of the Panzer divisions, then we could stage England for Europe. It was a whole series of very, very systemic, energetically based decisions that stood behind these victories, and failures sometimes.

[Q:] *But would we be doing the archaeology in North Africa or would we be doing it in the United States?*

Given what we know about the production of the logistics components, we would have to do that out of the United States. Where is the entropy sink? Where is it you're going to spend that production? You don't want to spend it, you want to use it. So you don't want to lose it; you don't want to lose the tanks; you don't want to lose the airplanes. The relation between production highs and expenditure sinks are clues to systems organization.

You would be sampling, given what we know. You wouldn't be digging the battlefields, you wouldn't be doing those kinds of things.

[Q:] *Now you've been talking about the study of the archaeology of World War II. One might interpret what you're saying by analogy with the situation of historical archaeologists who are concerned with the study of the spread of the modern world. By analogy, it might be argued by some that you are suggesting that to do historical archaeology (because the primary logistics systems centered in Europe during that spread), one should be working in Europe to a larger degree than in the New World. Is that what you're saying?*

To some extent. Let me see if I can make an example. They're digging down here in south Texas right now, the *La Belle*, which was one of LaSalle's last ships for a potential French colony at the mouth of the Mississippi. Now why do we

have a colony which is not at the mouth of the Mississippi? They couldn't find the river. And even the ship captain had been there before. The ship is lost out here in a Texas harbor, and there were only four survivors of this entire enterprise, and they ended up walking from south Texas to the French settlements in the Great Lakes. They eventually got home. Now why is this endeavor such an enormous failure as opposed to some other colonial endeavors?

Now that seems to be a reasonable kind of processual question to ask. Why are the French putting all this in here. They have boats, they're loading them up; they're sending people and it fails miserably, whereas some others don't.

If you would compare this failure to the Roanoke colony in Virginia, it has an awful lot of the same characteristics. That is, there wasn't continued logistics. They got in trouble, so they have to send a boat back to England to tell them, because there wasn't a pre-arrangement for support regardless of news. And on and on. There are a whole series of parallels between the failure of LaSalle's expedition and the failure at the Roanoke colony.

Why did the Virginia colony from the time of the Jamestown settlement succeed? It had the same problems. A small group of people arrived among indigenous people. That wasn't necessarily a bad scene to start with. But the first winter they starved; they lost about half or more of the total population when the streams were full of sturgeon. They didn't know about the environment, the local place. But in this case the Virginia colony made it, because the British sponsors sent support without waiting to receive news. The assumption of logistics was made directly—that you need to supply these people regardless of whether it's a Garden of Eden or not, until they get established. Even if they lost about 152 out of 280 original settlers, they made it.

[A malfunction occurred here during recording. Binford then summarized his points which had been made at greater length during the recording failure.]

My only point was this: What questions are historical archeologists trying to answer? Are they doing comparisons? Or are they interpreting their sites? If they are *interpreting* their sites, they are behaving like historians according to my philosophical outlook. They are using prior knowledge to give meaning to their sites. On the other hand, if they're taking their sites and comparing them one to another, trying to control variables, and asking, "What variables were different in this settlement and at that one," then they're putting themselves in the posture of learning about processual phenomena. But if they're just trying to interpret their little site, then they're using ethnographic analogy—historical analogy—and they're arguing with one another about the adequacy of the historical records. If they are doing that, then they're doing nonproductive things. One wants to use prior knowledge to guide analysis so that "new properties of the world" are exposed—rather than simply accommodating new observations to old knowledge and beliefs.

[Q:] *How do you go from site to systemic view when you're dealing with historical sites?*

The same way you do it in other archaeology. It's no different. It's no different at all.

[Q:] *And what is that? How does that go?*

Some of the model is in Stanley South's work, where he basically said, "Okay, within the little world that I've worked, here is the Carolina Pattern. Where does the Carolina Pattern change?" He was saying to historic sites archaeologists, "Here is something to which you can compare your data." We can begin to learn when the world looks different than we would have expected. Then we have a question, "Why is your pattern different than the Carolina Pattern?" That provides a basis from which to begin generalizing—as when South started saying things like, "Look, on the frontier we're getting much higher use of iron in construction than we are in plantation settings. Why?"

[Q:] *Is there a basic difference between looking at sites on the periphery of the system and looking at the core of the system?*

Well, generally.

[Q:] *I mean, can you define the nature of the system from one class, or do you have to have sites from both core and periphery areas?*

Look at it this way. When I dug Fort Michilimackinac, if I had only known what I saw in the site, I would have inferred that this is where porcelain was invented; or I would have had to infer that I was in China. In the classic model I'd been taught in school, the dominant pottery is made locally [laughing]. There was more Chinese porcelain per square inch at Michilimackinac probably than any other place I've ever been, except Southeast Asia—not China, but Southeast Asia.

Why are there no local ceramics at Fort Michilimackinac? There are some, but not many Native American pieces—but not inside the fort. It's outside the fort. This is a funny kind of settlement, yet the entire French period is represented there and there are local crafts. They made their own pipes, they made lots of their own stuff. Yet at the same time, I'm getting penknives with Arabic script saying, "Praise to Allah." Now where is this all coming from? All this is telling me about a system vastly broader than Fort Michilimackinac.

[Q:] *I can't go and excavate in China, at the porcelain factories, can I, and learn about the structure of this system? But aren't the porcelain factories part of the major system of the logistics of this network?*

That's true—because we don't know. But we may not have to know about the porcelain factories in order to compare Fort Michilimackinac with some other frontier fort.

It's my impression that the sources of the late Ming material that the Spanish were transporting across Panama weren't the same sources that the British were trading to. They were trading in

north China. I wouldn't necessarily have to know these things to treat the fort in systemic terms.

I wouldn't have to have British King's Eighth Regiment buttons in the excavation to know that this assemblage was British. And what we want to understand is the nature of the system.

Look at it this way. Why is there so much Canton ware at Fort Michilimackinac? When I was a teenager, there was a movie called *Northwest Passage*, and it was about Major Robert Rogers, of the Rangers. The movie depicted Fort Michilimackinac, and Rogers's cabin. Things were so bad he almost committed cannibalism—he almost ate the head of a Native American in his Swedish log cabin [laughs].

Now here I am digging with Maxwell, who was excavating Robert Rogers's house, and it's got molded plaster ceilings, ivory billiard balls, more Venetian glass than you want to imagine.

Now why is all this stuff here? The British are paying the cost of getting this stuff from China, taking it somewhere, and sending it out to Fort Michilimackinac. Now what's this all about? It is paying people to go where they would otherwise never go. It is the perks of being a military officer. It's paying; it's the same thing the Romans did. It's much like Rome. The Romans were successful and so were the British. If you look at the *castella* across the Netherlands, there's more Roman glass in the Barbarian settlements than there is in the Roman forts.

[Q:] *But can the effect of logistics networks only be understood in terms of penetrations such as this on the periphery, or is there a better way to understand them than in studying on the periphery—perhaps through excavations throughout western Europe?*

I think you have to do both. It was not until actual excavations were done in Holland, at the pipe manufacturing centers that anyone realized how different the Dutch methods of manufacturing pipes were as opposed to the British. The pipe-stem technique doesn't work for Dutch pipes. It works for British. If you have mixed samples it still works fairly well. But it's going

to give you an earlier date if you have any Dutch pipes in there. Pipe manufacturing sequences were crucial for learning something about pipes.

You have to do both, but basically, what I'm saying about historical sites in Quebec—and I'm only making one point—is that you don't "interpret" your site. Your site is a datum point with certain potential controls and variables. Its value is realized when it is compared with other sites. And the pattering for which an explanation is sought results, not from the identifications ("this is such-and-such pipe; this is such-and-such house; this is the house of Robert Rogers"). As long as you're staying at that level, rather than saying, "Let's compare houses," or "Let's compare the kilns, or whatever," you're doing what historians do. You're going for the unique, you're going for the particular, you're going for isolating more and more detailed characteristics of events rather than seeing the commonalities and the contrast of events as they're played out in different settings and under different conditions.

[Q:] *Would this be a proper interpretation of what you're saying—that archaeologists work with the sites they have excavated and have at hand, and they should define the system on the basis of what they have at hand?*

Mmm-hmm [agreeing].

But then you use that.

[MDT:] . . . *to define the structure?*

That's the first step. That is telling me that the world's not flat—is telling me something about the world relative to my prior ideas.

[Q:] *How did you come to work with Moreau Maxwell at Mackinac?*

Moreau Maxwell had, as I recall, just recently taken the job at Michigan State. Griffin had been contacted by the Mackinac Island State Park commission about finding somebody to excavate Fort Michilimackinac. So Griffin proposed Moreau Maxwell as an archaeologist they might consider. Moreau then asked Griffin if he had

any students who could help, because he hadn't done field archaeology for many years. He had been in the military, and here and there. Griffin said yes—he had me, and he would send me up. So I was sent up there to begin the excavations in 1959 with Moreau Maxwell.

We worked together, but we sort of decided that he would do his thing, and I would do mine. In other words, Moreau Maxwell was hesitant to tell me what to do, and I certainly wasn't going to tell him what to do. He excavated one area in the French quarter, and I excavated the big British barracks that first year. But it was like having two digs.

We were using prisoners out of Marquette Prison for crew. They were all murderers. But we had as many crew as we could handle, so we didn't have a lack of labor.

In '59 we worked together in parallel that way, but decided that afterwards we'd alternate. In '60 Moreau Maxwell excavated; in '61 I excavated. Then I moved to the University of Chicago and said I wasn't going to do it anymore.

So Moreau Maxwell and I had this strange kind of relationship, and I never knew what he was going to say—good, bad, or indifferent.

[Q:] *When I was at Chicago, I remember illustrations, and so on, that were in the laboratory. Were you still working on the report at that time?*

Yes. And that was an unhappy situation, because I did all these drawings and stuff. Maxwell was in charge of the official reports. None of that stuff was ever used. Well, the historians up there used my drawings in popular versions of tour guides. I was really angry because I had put in a huge amount of labor into all that, and written my report, and it was never published or acknowledged.

[Q:] *You mean that report at Chicago was never published? That wasn't the published report of Maxwell and Binford?*

The unpublished report was on the season that I was in charge of all the excavations—1961.

That thing that was published with Maxwell was on the 1959 excavations. He never published the material from 1960. And I wrote all that stuff for 1961. And there was money to make the publications.

I still have the manuscripts and all that artifact stuff—huge amounts of typological work and distribution studies and so on.

[Q:] *How did you become acquainted with James Deetz? Did his approach to historical archaeology influence you in any way? If not, why not?*

I don't remember when I first met Deetz. Maybe I heard a paper at a national meeting, or maybe I simply read one, I don't remember. But I became aware of Deetz. And when we were organizing things that were new, at some point, I judged his work to be different, and asked him to be involved in some of the meetings where we did things. And he did get involved. I guess he gave a paper in the '64 meetings and things like that. Basically, I sought him out because I saw a different way of looking at ceramic variability which I thought was interesting.

I didn't really get to know Deetz. I was responsible for him being invited to the "Man the Hunter" conference at Chicago. He had no credentials to be there, but I told Sol Tax they ought to invite him because I wanted to know something more about what he was doing. So they invited him. I guess I spent more time with him at that conference than I'd ever spent before.

Then when I was fired at Chicago, Jim Deetz suggested to [Charles] Erasmus [, chairman of the University of California at Santa Barbara anthropology department,] that I be offered the job they had. I was very grateful for this, because there weren't many jobs then.

When I first got to Santa Barbara, Jim Deetz was on leave, and I never saw him. Then he came back from being on sabbatical and the three of us—Jim Deetz, me, and Loring Brace—taught courses that started at 9 o'clock in the morning and went till noon, back-to-back. Loring

Brace was teaching Paleolithic, so he taught from 9 to 10. I was teaching Middle Paleolithic and methods, and I taught from 10 to 11. And Jim Deetz was teaching historical archaeology and he taught from 11 until 12. [Laughs]. We had these huge classes—I mean they were giant classes of 900 to 1,300 undergraduate students.

In that period I became very disillusioned with Jim Deetz, because of his treatment of students. Then I had a difficult time with him in the firing situation at Santa Barbara in 1966.

[MDT:] *You turned to Paleolithic archaeology while still at Chicago, before going to Santa Barbara.*

The reason I got interested in the Paleolithic was not so much that it was fascinating. There were two reasons. One was a practical reason. I realized early that all students of anthropology, at that time anyway, had to take a basic course in human origins. So the Paleolithic was the only place that an archaeologist had a chance to say something to the entire field of anthropology, in an educational sense. If you're doing something with Paleolithic, you get to talk to all the students. So that was a pragmatic reason.

But the more interesting reason was that I thought it was a field where an argument against idealism could be developed. This was because you couldn't make the assumption that *Australopithecus* was like us. You had to make the assumption that they were probably different. So you couldn't do these little thought games—"It would be rational for the *Australopithecine* to do this"—because you had no idea what was rational for an *Australopithecine*.

This meant you had to develop arguments about circumstantial evidence. You had to demonstrate necessary relationships between material things—not arguments about motive. That was why I was so successful in knocking down positions and in arguments within Paleolithic archaeology. They were all doing the reverse. I mean Glynn Isaac was making *Australopithecines* just like us, only they needed a "Head Start" program [laughs]. They were just interpreting

what they saw in terms of what they knew about us.

Those are the reasons I went into Paleolithic studies. The reason I have gone into ethnography so much is because of its wealth of information that archaeologists need to know in order to reasonably think about variability in archaeological material.

[Q:] *Since you began in the archaeological fields, do you feel there has been really significant advance?*

It's hard to say, because I certainly know I've learned a lot. I have learned a huge amount by virtue of making decisions that forced me to learn more and more. I have certainly published a lot. I've tried to share that with archaeology. People have responded, people have argued about it. People have misused it.

If somebody else tells me they've got a hunting camp, I'm going to vomit—or that they have discovered a residential base camp. That's not the point. Identification is not the point. The point is what is conditioning the variability.

But, yes, archaeology is a much different field today than it was when I was a beginning student.

[Q:] *Was there ever a point in your career where you thought of doing much more with historical materials in archaeology? I remember, in California, you once said how intriguing it would be if an archaeologist controlled the languages and archaeologically investigated "the fall of Rome." It was never clear to me if this was one of your numerous good ideas which you never had any intention of pursuing. Or if it was something to which you had given a great deal of thought.*

I thought about it, but it was never something I thought of pursuing. You know, that's a classic example of what all archaeologists have been talking about—collapse, allegedly. I don't know that it really happened, but [laughs], as far as I know, nobody ever looked at it, to see what it

looks like. Is there an archaeological record of the “collapse” of Rome? I doubt it. I don’t think you’re going to find a layer with pillaging and raping and barbarian pottery. I don’t believe it. I’d be very curious, but I don’t know. [Both laugh]

[Q:] *Eventually, as you went on to more inclusive development of your theoretical work, you lost contact with some of your early associates. When you went on to Michigan, Chicago, California, and New Mexico, did you maintain contact with Stanley South?*

Oh yes, still do. Of course I maintained contact with various people I got to know at Michigan and that I got to know in California. But Stanley South . . . Stanley South and I have been very, very close all of our careers. Now this doesn’t mean that we write every day; we don’t. But, any time that something comes up, when I want to know something about historic stuff, I call Stanley and ask him. If he wants to know something about opportunities for students, he calls me. When things happen in our personal lives, we immediately tell each other and try to help. We’ve just been very close for a long time.

As far as people from Ann Arbor, Charles McNutt has been an old friend, and we are still close friends. He’s down at Memphis State. He’s had a stroke now. Newman, who’s down in Louisiana, I met on the upper Missouri. We’ve maintained a relationship, not as close, but we always could count on one another. When he wanted to go to China, I wrote recommendations. So it’s been a lot of people like that, all over the place.

[Q:] *Since the 1980s, historical archaeologists have been influenced by “meta-historians,” such as Braudel, and by similar work from the anthropologist Eric Wolf. Wolf left Chicago just before you joined the anthropology department. Did you ever have any contact with Wolf or meta-historians, and did they influence you in any way?*

I knew Wolf because, when he left Chicago, he went to Ann Arbor and was on the staff there. He was hired at Ann Arbor to teach peas-

ant communities—that general area. On several occasions I went to cocktail parties at Elman Service’s home—and subsequent to my leaving Ann Arbor—where I met Wolf and talked to him at some length. He had not become a meta-historian then. He was still into peasant studies and comparative village studies and things like that.

My only contact with Eric as a meta-historian was to write him a note when he was the distinguished lecturer at the American Anthropological Association, and he gave a fairly old-timey, Marxist presentation. I liked it, and told him so.

Braudel I’ve never read, so I don’t know anything about his work. I don’t necessarily condemn him because of what people have done with this. Richard Gould is forever citing him and various people, and I think it’s silly. But I haven’t read his work, so I don’t know what it’s like.

[Q:] *Most historical archaeologists now see their field as being concerned with the archaeological explanation of the modern world. Do you believe that the basic difficulty for such an archaeology lies, as it does in prehistoric archaeology, in the development of middle range theory?*

The idea that historic archaeology is the explanation of the modern world insures that historic archaeology will never be a science. Science is dedicated to saying, “I’m going to explain the variability in my subject matter.” That variability is what I look at in the empirical world. So, physics says, “I’m going to explain variability in physical phenomena, measured in terms of mass, velocity—these kinds of properties.” And that’s the subject matter of physics. So its problem is generated from the study of its subject matter. That problem comes from patterning in the subject matter, and it is this patterning which is in need of explanation.

My feeling has always been that archaeology as a science is the science of the archaeological record, and that problems arise from comparative study of the archaeological record, and in the recognition of the patterning there.

Any time you run around and say, “I want to use”—let’s say abuse—“the archaeological record to answer some problem that I’ve decided ought to be solved,” like gender issues (“what is the role of gender in the past?”), then you’re not studying the archaeological record and its patterning; you’re using the archaeological record as a vehicle for knowledge of claims about something else.

I’m dedicated to the study of the archaeological record. I think it’s the study of the archaeological record—if we’re going to make any claims at all—that we have to be concerned with. And that it’s historical, or it’s Paleo-Indian, or it’s Paleolithic makes no difference. We should be dedicated to the study of our subject matter. We should not be sitting around saying, “I’m just studying the archaeological record because I might be able to mine some information for totally different purposes—I’m not interested in the archaeological record, as such.” That ensures that we’re not going to be a science—we’re going to be rapists of the archaeological record.

[Q:] *As evolution occurs, systems become more complex. What if we defined historical archaeology in terms of being concerned with a single world system, which is a last systemic development of things in the earlier archaeological record? Would this be acceptable, or would you say it would still be divorcing the subject matter from archaeology in general?*

I think you would still be divorcing it. You have got to demonstrate to me from patterning in the archaeological record that you have a world system. You can’t make the assumption and then accommodate the archaeological record to it. You have got to demonstrate to me that, really, what is going on in Paris is of great importance to the mortuary practices of the Celebes Islands. And if you can’t, then Paris is irrelevant to that patterning on the Celebes Islands, and all those claims for world systems are bulls**t. Now if you can demonstrate patterning that links all this stuff, great. But that’s coming out of the study of the archaeological record, not the romance of somebody’s head. [There was here a slight difficulty with the recorder.]

[MDT:] *You were discussing your objections to taking the definition of historical archaeology as the archaeology of the modern world.*

I take a rather strict view of science, and in science one chooses an empirical domain as that which one studies and then the problems which one seeks to solve arise as a result of the study of that domain. So I’ve always thought that archaeology was the study of the archaeological record and that problems arose from the analytical research that we do, comparatively and otherwise, on the archaeological record.

My experience has been that if that’s not the case, then the archaeological record gets abused in the following sense: when archaeologists thought that the major problem was chronology, they dug sites with deep stratigraphic cuts and little narrow trenches to look at things vertically. The result was that they didn’t record the data that might be useful to someone who might want to look at the horizontal distributions.

Any time one has a narrow problem, even if a big problem, like the world, the modern world, they have a bias as to what is germane to that. And properties of the archaeological record that aren’t in that “search image” get lost and destroyed.

I think, as in physics, any science has to say, “What I do is study a domain”—whether it is a natural one or an artificial one is irrelevant—“Here is the domain of the empirical world that I study. My study leads to the recognition of properties in this domain which we don’t understand—and these are our problems.” Otherwise you are always accommodating what the archaeological record has to say to some other interest. And the archaeological record never gets really studied.

Any time you decide beforehand what your problem is and then say “all right, I’m going to exploit this for solving my problem,” you’re in the position of generating accommodating arguments. You are accommodating what you see in the archaeological record—to what you believe about your problem.

[Q:] *Are there people actually doing that?*

All the time.

Let me put it this way. You mentioned earlier the notion of core/periphery. Well, that's been a big idea. And so, students are told, "well, you are working in the periphery," or "you're working in the core—and somehow or another you have to do this in order to do that."

I had a student like this last week, who had been told she was working in the periphery. She was working in central Asia, and she was working on sites that are pretty much 25 miles apart, or a little more. They're tiny little settlements and they're clearly related to the early use of Bactrian camels in transport. They were like Pony Express stations.

Carleton Coon wrote a book which I think is fascinating. I read it maybe once every three years. It's called *Caravan*. He argued that the development of complex systems in the Near East could only be understood in terms of what he called "the Land of Insolence." Now "the Land of Insolence" for Carleton Coon was what constrained state expansion, what caused states to fail, and what basically supported states in the Near East.

The whole notion of process is based on certain assumptions. If at that time a state had to mount an army, and if the army had to live off the land, and if the land is not productive, then you can't have an army there. You can't have an army in the Sahara. You can't have an army in the Libyan Desert. You can't have an army in the Sinai. You can't have an army in Saudi Arabia. You can't have an army in Jordan. [Both laugh.] You can't have an army in the Iranian Plateau. There's nothing for them to eat.

So these are areas where specialists develop. The whole trading characteristics of the Tuareg are related to being able to operate where they are beyond the control of states. But the states couldn't live without them, and they have enormous power relative to their size. If they decide to change their policies, they impact the states in ways that hundred, thousands, of other people making a change would not.

Coon's ideas about the organization of the system in "the Land of Insolence" were very different than this notion that "here's the core, where everything happens," and the parallel in an old diffusionist idea, "Here's where everything was invented," and then just dribbles out to the periphery—making the periphery somehow or other dependent upon the core. I'm not denying synergy, just skeptical of researchers' bias in how it might work.

I don't know that that is the way the world works. That is certainly not the way my parents felt during the depression in Virginia [laughs]. They didn't feel that they were somehow or another dependent upon what happened in Atlanta or Washington. They were dependent upon what they did and how they did it.

I have never been happy with these global notions of power, and centers of diffusion, and great invention, and all that nonsense. Complexity is the differentiation of things. What are the processes that bring into being differentiation ethnic, rural, economic, and all these kinds of groups, as well as states? You can't handle these with what is basically a diffusionist model.

There are so many arguments in archaeology right now. You have George Cowgill sitting in Mesoamerica saying, "I don't think demography has anything to do with complex society." And then you have somebody like Nathan Cohen saying, "I think demography is the cause." None of these things are accurate; none of them. Demography is not the cause; demography happens. On the other hand, changes in the number of people changes the demand for food. It changes the demand for goods. It changes the demand for labor. So there's no way that demography is not important in changing the system. So Cowgill can't be right either.

Most of these kinds of simple-minded positions on what's important cannot be correct, when you think about it. Yet, if you go and design your research in those terms ("I'm going to prove demography doesn't have anything to do with the origins of Tenochtitlán," or something like that), you're going to look at things in a biased way.

But if I say, “the only way I’m going to learn something new is if I can see something I haven’t seen before in the relationship between things in the archaeological record,” I’m continuously looking for new patterns, new relationships between things. And if I ask, “How do things interact in the statistics, and in the spatial distribution of things in the archaeological record?” Then there’s no way I’m not going to learn something. But I’m not going to learn it by claiming that I know the answer before I look.

So I don’t think most of those great overarching ideas do anything but make people feel warm and gooey. They certainly don’t inspire you to do good archaeology.

[Q:] *Will the documentary record, to your mind, provide the major source for the development of middle range theory in historical archaeology? Or will middle range theory in the field develop primarily from more strictly archaeological work—or from a combination of the two?*

I think documentary knowledge is an absolutely crucial and essential body of prior knowledge for an archaeologist to have if he’s going to work in a historic period. I don’t think there is any question about that. Here’s the information he can think with, and he’d better use it. But whether or not documentary knowledge is going to provide the answers to the archaeological record is another issue. I think you have to study the archaeological record and the problems are going to arise from that subject, and that subject matter used archaeologically.

I may bring my knowledge to bear from documents, but I’m never going to be able to understand the archaeological record by direct analogy to historical documents. The knowledge of history is useful—to help you think with when you are trying to solve archeological problems. But archaeological problems have to be solved within their own domain. I don’t think they can be solved in someone else’s patch.

The middle range work that is needed in historical archaeology is basically implicated in the question, “How do I diagnose the past from ob-

servations on the archaeological record in any accurate sense?” The middle range work has to be done in terms of a problem (or frustration) in trying to deal with the archaeological record. Then documentary work as well as experimental work in comparative study, ethnography, or whatever, may be a major aid in coming up with ways of learning from the archaeological record.

So my view is, the more prior knowledge I have, the better I’m able to think. That’s because I then have more intellectual options. That is fundamental. But the problem, once again, comes down to the archaeological record.

Let me just take Fort Michilimackinac. Historians didn’t want archaeologists to do anything at Fort Michilimackinac but to prove that this map that they had was accurate. They had a map made in 1742, I think, the so-called Magra map (now dated by historians to 1766). They wanted to reconstruct the fort on the basis of that map, and wanted to make sure we were in the right place. That was what historians wanted done.

Maxwell started digging in one place, and I started digging in another. My initial thoughts were: “Well, if we are in the right place, and if the Magra map is correct—as I had been told—then I want to dig a hole in the middle of the parade ground where I know there have been no structures. There I can see what the natural soil is like inside the fort. Then I will have a basis for understanding what it looks like when it’s all disturbed and houses have been built.”

I put my first hole down in the middle of the parade field, where there was not supposed to be anything, and came down on the biggest pair of back-to-back dolomite foundations for fireplaces you ever saw. And not only that, as we followed the foundations, we found this was a big structure. I got paired fireplaces.

Well, there’s not supposed to be anything here, and the historians are all upset. This is in the middle of the parade ground, and here’s this enormous structure. It’s obviously of the British period, and there are King’s Eighth Regiment buttons everywhere—just incredible stuff.

What the historians didn't know was that the British, during the Revolution, had gotten upset about the Americans, and brought in the King's Eighth Regiment, and secretly built a giant barracks in the only place in the fort that there was any place to build such a building—which was in the middle of the parade ground. The British had all these nice little guys waiting for the Americans when they were going to come north. But the historians didn't know this.

Now what is interesting is that once we found the barracks, they found it in the literature. But they didn't know it before. The literature hadn't changed. So why didn't they know from their giant, wonderful research that there was going to be a barracks? The answer was that they didn't know whether it was propaganda or not. This barracks was referred to in all the documents, but had been dismissed by historians as simply propaganda mounted to scare the Americans. It turned out to be real [laughs].

The historians were totally upset, because, God, here's this magnificent structure—a wonderful story—and we can't move it. It's too big. I mean these are dolomitic foundations for huge fireplaces; can't move it. This is in the middle of the parade ground. What are we going to do?

Their response was to bury it all back up and construct the fort as of 1742, or whatever their target date was. But that was not quite possible, because since 1742 there had been at least four building facies in the fort, and there was very little left, archaeologically, of 1742. We had the French period at the time of Pontiac's rebellion pretty well, but we didn't have much before that. [Laughs.]

Working with the historians was a totally frustrating event. And we were finding stuff that, as archaeologists, we had to deal with. We, of course, got French material in certain sections of the site. If we're doing the archaeology of the modern world, do I throw all of that away? What do I throw away in order to argue about the archaeology of the modern world, rather than to deal with variability in the archaeological record?

[MDT Note: *If Binford's use of the "archaeology of the modern world" seems, on this specific point, to be incongruous with common usage of historical archaeologists, they should remember that earlier Binford explicitly denied the value of the simple diffusionist model—which he equated with historical archaeologists' usage of a modern world-system type model. As Binford sees all cultural contact as involving complex interchanges, his main criticism at this point is that the modern world-system type model requires, essentially, a simplistic "acculturative" interpretation, which is not even adequate to explain the found archaeological variability of the locally made French items at the fort.*]

[Q:] *Once you said that historical archaeology might come to hold the foremost position in archaeological theory building. Did you have middle range theory specifically in mind then?*

Yes, I did. Because in a historic context you have lots of different variables that we know—from documents and so on—lots of conditions that you can design experiments with. If I know these conditions are all constant over here and are different over there, then I can compare similar things in different contexts and begin to see the consequences of contextual differences, in organization, and so forth. So the more knowledge I have, the more ability I have to design comparative experiments to learn things.

[Q:] *Could you exemplify this?*

Okay, let me take a very broad example. This is not necessarily all archaeology—some of it is history—but it is an example I know well. If you look at Europe, after the appearance of domesticated plants and animals, you had societies that had both. They had a heavy investment in domesticated animals and agriculture. This is true in Africa. It is true all the way across Europe. It is true in Asia. It's true of South America. But it is not true of America north of Panama. Here there was a major investment in domesticated plants, and there were no effective domesticated animals in pre-Columbian times.

So now—if I want to determine the answer to this question, “What is the effect of having domesticated animals and plants, versus only having plants?”—I can design experiments. I try to hold the demographic scales constant in North America, where they didn’t have domesticated animals, to compare it to places in Peru and Europe where they had both domesticated plants and animals.

When you first start playing this game, the first thing you see is what we get from history—and no North Americanist has ever talked about this, for they don’t do comparisons. What we get from history is that warfare in North America was never for inhabited land; it was always for uninhabited land. It was warfare over hunting territory. In Europe, warfare was over agricultural lands, it was over inhabited villages. It was over infrastructure. It was never over uninhabited land. Now isn’t that interesting?

The entire organization of warfare among Native Americans in North America was in terms of males going off into the hunting territories, taking captives, getting ransoms, doing all these various things. It was a phenomenon of the fall season. It was all about theft. But it was rarely, if ever, about stealing people’s agricultural land or their infrastructure.

North of Panama there were effectively no domesticated animals, only domesticated plants. Knowing this, we can ask, “How does such a system compete with other such systems? As soon as you ask about competition, you realize that competition was for totally different things here than it was in Europe. Now that has to have an effect on the way cultural systems develop and change. So you’ve got an experiment you can do. But you’ve got to be able to get beyond your little history, to see the difference between archaeology in North America versus archaeology elsewhere. Similarly, trade played different roles. It’s really interesting, but we are only providing an example of experimentation.

Another kind of experiment: I suspect that everybody is correct, that Native Americans came into the New World across the Bering Strait.

Well that is a disease filter. Most tropical organisms would not make it across. The people, then, are coming into the New World with a very minimal disease load. They’re radiating, for the first time in human history, from north to south, from polar to equatorial settings. All other previous radiations had been from equatorial to polar settings. So we have a reverse process.

Now is a radiation in reverse direction, from the standpoint of ecosystem, different from one that’s going the other way? Point number one. Point number two: What is the effect of bringing groups of people into highly productive equatorial settings in which there had been no co-evolution of disease species in the past? The answer is that you’re watching the archaeology of a Charlie Chaplin movie. It is speeded up. I’ve already had students do this in two different theses. The average length of time from the appearance of the first domesticates to the appearance of the first public structures, that aren’t palaces or such, in Europe is 6,000 years; in North America it’s 1,300 years. Interesting. [Laughs].

We are now getting close to variables. And that is what we want to know about. A difference between having domesticated animals, versus not having them, makes you have differential competition. Having a disease load or not makes for differential rates of cultural evolution. There are fascinating things like these which we want to learn about. And it’s a matter of trying to find out where you can hold variables constant, and where you can let them vary, to see the different effects of different variables. Historical approaches generally make it possible to do this, but their focus is wrong.

What is the difference between a colony that is essentially an aristocracy and has an aristocratic core, versus a colony that doesn’t? What is the relationship to the initial governing bodies? I think it has to be totally different. We know that in Australia. [Laughs]. And you can compare the settlements that were founded by, in a sense, aristocratic Europeans who were exiled versus prisoners. Oh my God, the contrast in the society in Australia in the early days was staggering.

These are the kind of things we want to get the handle on to begin to see how the world really works and what the important variables are. History gives you a good knowledge background for historical archaeology, and gives you some control over which variables you can say, with confidence, were or were not relevant in a particular place. That is crucial to doing good science. Why don't historians do it?

[Q:] *Since the 1980s, "Post-Processual Archaeology" has appeared, and Mark Leone has been its major figure in historical archaeology. What do you believe are the major contributions, or difficulties, or both, of this approach?*

Post-processual archaeology, in my opinion, is not archaeology. It is anti-science. You can't get much more negative than that. [Laughs.]

Post-processual arguments basically say that we cannot know, we cannot evaluate our own ideas, we cannot learn, we can only reflect the biases of our received knowledge. It says we're all biased, we're all cultural beings, and when we say we have a method by which we may learn something, we are, then, essentially intellectual imperialists. That's their position. The bottom line is that everybody's ideas are equally good, no matter how stupid they are. That's the message of post-processualism.

The position of post-processualism has this nihilistic argument that basically we can't learn because we're prisoners of our own culture, and we don't even know our own biases. It concludes that there is no way of judging who is right or even if there was an actual past that one can correctly know.

Science, of course, makes all the opposite assumptions. It basically makes the assumption that we can learn and that there are methods whereby we can use our prior knowledge to allow us to recognize our ignorance, and in turn that there are ways in which we can manipulate our prior knowledge so that experience can be a teacher of new things. Post-processualists deny this.

Now that Mark Leone is a "good" post-processualist, I would not argue with him when he says some aspects of science are useful. But

then he adopts a posture which is associated with post-processualism, that the explanation for everything in the archaeological record is a reflection of mind. In that sense he is in the post-processualist philosophical domain, but, ironically, he sort of denies the other side of their argument—that you can't learn. He accepts the scientific position that you can learn.

[Nancy Stone enters.]

[Nancy Stone:] *Can I interrupt? But leave it [the recorder] on. Remember—Lew will remember this. Remember when we were in Australia, at Armidale, and people asked, generally, how you know the past? Mark Leone stood up and he said, "Well, in the 19th century we received, or it was presented to us, a theory about why things are the way they are: this was Marxism." And he said, "This explains it." Remember?*

[LRB:] Yes; yes.

[NS:] *Talk about that. He got right up and he said, "We already know why things are the way they are!"*

[LRB:] Yes, I agree. But that's not post-processualism.

[NS:] *No, it's not, and he straddles both domains.*

[LRB:] That's right. [NS exits.]

I agree with what she is saying. But that has been his way. He's not really a post-processualist. He accepts some of the propositions, the mentalism, that the explanation is in the minds of people. But he accepts some of the propositions of science. Then on top of all this, he is a committed Marxist, in a funny sense. But he is a post-World War II Marxist, and they're all idealists. They're not materialists. They have accepted the mind business as well. Mark is not a very pure post-processualist, so he's not a very good one. He's an anachronistic scientist and he's a born-again Marxist. So it's really difficult to talk about him [laughing].

[Q:] *Do you have any other comments on post-processualism?*

I think if you adopt post-processualism there is nothing for you to do but to argue with other people's opinions, because the archaeological

record is dismissed as having no inherent structure or patterning independent of our ideas about it. So all you do is argue with other people over their interpretation of it. This is just a non-event. You can't learn anything this way.

[Q:] *Besides your general theoretical work which has had an impact on historical archaeology, you have made contributions specifically to the field, as in your pipe-stem dating formula. Over, say, the last 10 or 15 years, have you tried to keep abreast, at least in a general way, with developments in historical archaeology? If so, how would you evaluate the general direction the field has taken? If not, why not? Is this from dissatisfaction with the course of the field, or in the press of work have you limited yourself to things more directly concerned with your overall theoretical ambitions?*

I really haven't made any effort for a long time to keep up with the literature of historic archaeology. The exception has been that I keep up with the work of Stanley South because we know each other and we talk about what he's doing. So I track what he's doing. And of course he expresses opinions. [Laughs.] So I know something about his opinions regarding the field. But it's just through him; it's not through any independent work of my own. That's the only way I've kept up with it—through Stanley South.

It's not because I wasn't interested; it's just that you can't do everything. [Laughs.] And I have sort of committed myself to demonstrating to archaeology how you actually use ethnographic or historical knowledge. I chose in this case to use ethnographic and historical records of hunting and gathering people.

Periodically, I get students that are interested in historic sites archaeology. I probably have a biased sample, but what most students here at SMU who have come in to me want to know is how to identify things.

[Q:] *You mean objects?*

Objects. So they're really into objectology. I've tried to make them read other stuff, and then they come back and say, "Well, I don't have the

time. There's not enough time in life to learn all the kinds of nails and all the kinds of bullets and all the kinds of this-and-that. I need to really invest my time in being able to identify these things that we find in Civil War sites (or whatever their site happens to be)."

That has been a common kind of experience with students recently. They seem to be overwhelmed with the notion of what they have to learn at the practical level to actually do historic sites archaeology. And I can understand that. That is daunting.

I think there is this notion that you have to know everything—what everything that you find is—or you're not qualified. When we started—I'm thinking of Stanley and I—we didn't know what anything was. You know, we were in the process of learning. For instance, none of the ceramic chronology that you have today—not even the work of the British ceramicists, was known in a chronological sense then. The late British sequence from the 1700s was not well documented.

We just had all the stuff, and we basically dealt with it as classes of things that we could see. We could see the difference between, you know, blue-glazed tinwares versus something else, and we dealt with things in those terms. And we began to learn the difference between British and French gun flints, and this kind of stuff, because we could see differences.

I almost feel that being able to look at things and see differences is more important than knowing what the things are; that just developing skills of recognizing things might be the important thing to learn.

[Q:] *You're saying, essentially, that you were operating at the level of a pre-historical archaeologist*

Right.

. . . going into an area which was not known

Right.

. . . and making the visual differentiation

Sure.

. . . of artifacts which you could recognize?
Right.

[Q:] *That's right?*
Yes.

[Q:] *I'm not misinterpreting you?*
That's right. That's right.

At least, for instance, on historic sites in Texas, most of which are Civil War, the students come in and say, "We are CRM people and we have got the contract to do this or that site. How do I learn what it is I'm looking at?" And I say, "I don't know." [Both laugh.] I don't know any books on the Civil War or whatever.

That's really their concern. They're not concerned with, "How do I dig?" or "How do I document an archaeological site?" They seem to be more concerned with passing their little test of competency by being able to identify things. But I don't know that that's really true of all historic sites archaeology. But it seems to be true of some of the CRM stuff.

[Q:] *How long, would you say, has it been since you really made an effort apart from Stanley South, to keep abreast of what has been going on in historical archaeology?*

Oh, I think I tried to keep up with things until maybe the middle 1980s. About 1985—something like that—because I had, by that time, large numbers of students in Albuquerque. I had field projects going on everywhere. We had a larger number of faculty, so I didn't have to teach as great a breadth of subject matter as I used to do. I began to cut back at that point. Mainly because I had so many students and I just couldn't keep up with everything.

[Q:] *Mercifully, we are at the last question, Lew. [He laughs.] Are there any points which you feel are relevant which have not been covered, or are there elaborations you would like to make concerning anything already touched on?*

[Very long pause.]

All I can say is that a lot of the thoughts that I had while we were talking came back to one sort of fundamental thing. In terms of goals, in terms of approaches, I don't think there is any difference between historical archaeology and any other kind of archaeology. The subject matter for study is the archaeological record and the patterning in the archaeological record should define our problems.

If we impose on the archaeological record what comes from somewhere else, what comes from history, what comes from the political interests of the time—feminist movements, whatever—I don't think that serves archaeology well. I'm not saying such things aren't important. But unless I can justify from the archaeological record some patterning that implicates those things, then I am trying to find it because I think I'd like to know it. That is the wrong way to go. That generally results in forced accommodations between observations and ideas.

The archaeological record is hard enough to understand if we take it at face value: "Here's what is there; how do we understand it?" Let's work out the methods for being able to relate what we see to past conditions in an accurate way, because we don't do this very well now. Yet half the time archaeologists are walking around talking about "the limitations of the archaeological record."

The archaeological record is only limited because the archaeologist doesn't see any self-evident way of talking about what he wanted to talk about for the past. That is not a measure of limitation, because there are no self-evident facts. The only way we are going to get to the past from the archaeological record is by hard work in the sense of figuring out what are the conditions which unambiguously implicate certain kinds of patterning in the archaeological record. And I think that is true in the historic period as well in the other periods.

I'm perfectly aware that the historic period is better informed—there's more knowledge to bring to bear in doing its archaeology—but I

don't think that makes the doing any different. It's like when I was with the Eskimos.

I would go out on sites that these Eskimos had lived on, where they had in fact created the archaeological record I was looking at. We'd clean the site up and look at it. Here's a house, and the guy that lived in the house is standing right inside it.

And I'd ask him, "Why are all the metapodials over here?"

And he'd say, "I don't know why they're over there."

So I'd say, "Well, you did it. You lived here."

"Yeah," he'd say, "but I didn't pay any attention to metapodials; they're just old bones to me."

Yet then we'd walk around the site, and every one of the houses had metapodials in the same place. So I'd say, "look at this."

He'd say: [scoffing] "Crazy Eskimos. I don't know why."

So then we'd leave the site. Maybe three weeks later I'd see the man again. He comes running across the field "I know why. I know. I know why."

I'd say to him, "Hey, what are you talking about?"

"The metapodials," he'd say, "the metapodials! I figured it out."

So half the stuff that we were seeing as archaeologists, the people who produced it didn't understand. That's because they don't live their life in terms of where they throw things away [laughing] or how they push things out of the way, or what their wife does when they're not looking. They don't live their life in those terms. So there is tons of stuff in the archaeological record that informs us about things that even the participants didn't know about.

This is a tremendously interesting asset, with a wonderful potential. We have to basically look at what the patterning is in the archaeological record and use that to define our problems. Because if we want the archaeological record to be what it isn't, we're never going to learn and we're never going to be satisfied. We are always going to say, "It's limited, it doesn't tell me what I want to know." Because we want it to be something it isn't.

[Nancy Stone reenters:] *Now I'm about to serve. Is that a good stopping point?*

[LRB:] Yes, it is.

[NS:] *Do you want the archaeological record to be something it isn't?* [NS begins to leave.]

[LRB:] A lot of people do, and they don't even understand what it is.

You want to have some supper?

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The greatest debt is to Lewis R. Binford, for agreeing to this interview. But the debt is compounded, for Lew and Nancy Stone also provided bed and board in Dallas. In addition, Nancy also made great—and successful—efforts to get an extra, last-minute, ticket, so I might accompany them to the ballet. And this treat, to one who is culturally deprived, will long be remembered. Attorney, classics scholar, and nephew, Martin T. Sigillito, and my daughter, Tanya E. Thurman, French graduate student, verified my transcriptions of some words. My son, John A. P. Thurman, Kenyon College '98, made the superlatively accurate verbatim transcript. My heartfelt thanks to all.

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