TRACKING ANCIENT FOOTSTEPS

—William D. Lipe's Contributions to Southwestern Prehistory and Public Archaeology

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A Conversation with Bill Lipe

On April 7, 2006, at Washington State University, Tim Kohler and R.G. Matson interviewed Bill Lipe about this book. A few days before the interview, we gave Bill a list of a half-dozen possible topics. About 90 minutes of open-ended conversation then began, later transcribed by Diane Curewitz. What follows is a lightly edited version of that conversation, with inserts noted by square brackets.

R.G. Matson (RGM): One of the questions that Tim and I have had while the book has been developing, is who were the major influences in your developing years in the 1960s?

W.D. Lipe (WDL): Probably we should start earlier than that. My parents were both college graduates, which was not common in those days, so even though they weren't prosperous—my mother and father both had been teachers—they really communicated a love for books and a respect for learning and knowledge. And because I was sick a lot in the 1940s when I was a kid, from grade school through early middle school, I spent a lot of time at home. After my father died when I was eleven and my mother went back to teaching, I was home listening to the radio and reading. I read a lot and got into the mode of looking in my reading for the answers to questions. Trying to find out about sex from a 1941 edition of the Encyclopedia Britannica really hones your research skills at an early age.

We had a Carnegie library in town and I spent a lot of time there. It was very small, probably a thousand square feet, but because it was small, it made it seem like you could get your arms around all knowledge. You could imagine yourself reading all those books. I did a lot of reading, went to the library a lot, always had books checked out. So that was important.

I had some good teachers, in high school particularly, again not for content, but for their attitude that learning about literature and science was exciting, and these were really interesting things to pursue. On starting college in
1953, the thing that struck me was that here I had faculty who were actually engaged in intellectual processes, not just communicating from textbooks but having independent opinions and doing research. That I found very exciting. People who were specifically influential in college included Bob Bell, the archaeologist at the University of Oklahoma. His classes were oriented around facts, and I still remember some of those things, but he and his wife would also take students who were interested in archaeology on field trips over spring break. So twice when I was in college, we crawled into Bob and Virginia Bell’s station wagon and a couple of other cars and went out to the Southwest, once to Bandelier and once to Chaco. I remember we camped out at Bandelier and it was really cold and there were skunks all around. One guy who nobody particularly liked woke up with a skunk on his chest. It really frightened him, so he spent the rest of the night in the restroom at Bandelier. But we also went to Chaco and hiked around. In those days, it was pretty open and we looked at a lot of sites including Shabik’eschee, pretty much on our own. Those experiences were really exciting; that got me interested in the Southwest as a possibility.

On Bell’s recommendation and because I had read an article about it in *National Geographic*, I applied to the University of Arizona archaeological field school at Point of Pines in 1956. Emil Haury, the dean of Southwestern archaeology, directed it and Ray Thompson, who had just started as a faculty member, was there, along with several good graduate students, and some student colleagues, including Liz Morris, Pat Culbert, Frank Hole, Patrick Morris, Jim Griffin, Jim Hester, and Bill Robinson. So it was a really good group, and I learned an enormous amount. I had a chance to work on several different kinds of sites, which was a great experience. This led me to get a summer assistantship at the Museum of Northern Arizona, where the next year I worked with Dave Breternitz. I learned a lot from Dave just in that short time.

I found the linguist and cultural anthropologist, Bob Bittle [at the University of Oklahoma], to have a really good mind for theory, and he was able to show how you could take a whole position and boil it down to a model. We had a class in social anthropology where we went through a number of prominent thinkers in anthropology, and he condensed them to their basic models of how culture and society worked, and what was important to study. I found that really interesting and kind of a guide for my own attempts later on.

In graduate school at Yale [starting in 1957], Ben Rouse was very supportive. He put up with the difficulties of my transition from the “ace” taker of multiple-choice exams, to the person who found it very difficult to get research papers done on time—and everything at Yale depended on writing research papers. The courses were all seminars. Rouse tolerated a lot of stuff that I wouldn’t put up with from my graduate students, but he stuck with me and helped me get through it and was a really good advisor—a very hard-working, supportive, smart advisor. I also ran into a couple of people there who again were intellectually stimulating because of the way they thought and the way they could systematically and logically present complex issues. These were Floyd Lounsbury from whom I took linguistics, and Al Spaulding, who came up on the train on Saturdays [from Washington D.C. where he was the NSF program officer for anthropology] to give a seminar in quantitative methods. None of us in that class had much ability in quantitative methods, which he recognized, so he concentrated on the logic of inference—the logic of using statistics and quantification to make inferences in archaeology.

Tim Kohler (TK): Both R.G. and I have noticed that although you’re not a practitioner of statistics, you seem to have a good intuitive understanding of the process of statistical application and inference. Do you think you owe that to Al Spaulding or did you pick that up earlier?

WDL: I think I got it mostly from R.G., from working with him [R.G. looking shocked]. No really, I sat in on your class at Northern Arizona University [ca. Spring 1971]. We worked together for so many years and R.G. did the analyses, but I would have to understand the logic of it, and he was always good at leading me through it, and so I could see what you could do with it. I always was terrible at the mechanics of it, but I believe I did learn, largely through the work we did on the Cedar Mesa Project (Fig. 9.1), how to use some of those techniques, at least the ones we used. I think we worked well together. I was interested in the logic of the methods and the reasons you would apply them and what the strengths and weaknesses were, and R.G. did a good job leading me through the details of it and how they actually could be applied in practice.

RGM: I would disagree a little bit, though I think we did work very well together. I would credit Bill with a lot more ability, and I noted it when we were talking to a geneticist today—when you were talking about sampling the genetic material for taxonomy both between samples, and within samples. It always seemed to me that you had a great deal of ability to understand these situations.
WDL: Yes, but I never thought about it early on, I never thought about sampling. That's not something that had ever been covered in my background. I started thinking about it when we started putting together the research design for the Cedar Mesa Project. We discussed a lot of those issues and I was able to make some contributions maybe, but that was my introduction to sampling, and it's a great way to think about things. And then I think on the Dolores Project, you (Tim) came in with your approach to it, but I was pre-adapted to thinking that was the way you ought to do archaeology. You ought to really think about quantification and sampling if you wanted to make your case. It was a way of getting a research question on the ground.

TK: In retrospect it was a lucky combination. I'm sure that if I had come in to the DAP and argued for the probabilistic sampling program that we put in place, all on my own without support from you, it never would have happened. So having your support there was instrumental in making it work.

WDL: It's interesting to see that some of the biggest complainers about it from those days are now totally committed to using sampling. When it actually came down to putting together some synthetic statements about the project, everybody ended up going back to the probability samples, because that's what you could actually use to make comparisons. So a lot of people on the DAP were converted.

TK: So your first idea for your dissertation, as I understand it, was to work on problems of archaeological taxonomy and classification, but eventually you gave that up and reoriented it. Do you think that your interest in classification systems and systematics in archaeology is due to Ben Rouse's or Al Spaulding's influence?

WDL: I think partially Rouse. Bob Bell gave me an undergraduate independent study project to classify some projectile points, and I recorded the hell out of everything and got nowhere—absolutely nowhere—with it. I ended up just feebly pushing them around on the table. I believe I didn't even write a paper, but maybe tabulated some stuff. But again, he put up with that as a learning experience. Rouse's approach to classification was not what I ever thought could be readily applied... He was really a philosophical idealist and it always was difficult to understand how he actually got from looking at artifacts to things like modes and types and so forth. He was more interested in the logical consistency among levels of ideas that people had in their heads, which was how he viewed culture. But we did a lot of talking about systematics and he had written papers on phases in archaeology and on types and modes. We read all that and tried to think about it.

My first efforts at the dissertation were definitely an attempt to do something in that direction—the idea I had was to try to take a number of different lines of evidence and see if they lined up [i.e., resulted in the same groupings of sites]. Are phases more that just pottery types? Now I think the best way to make a phase, which is just a culture-historical convenience, would be to use style. But at that time the question was whether these culture-historical units were real—the idea that culture ought to fall into clumps. And Spaulding, to some extent, promoted that idea as well. He had a different approach from Rouse. I didn't really understand Spaulding's approach to that kind of question until I started teaching, using some of his early papers. But yes, I was trying to follow up on some of Rouse's ideas and to adapt the idea of using multiple lines of evidence and seeing if they lined up the same way. I needed to do a numerical taxonomy and I had no idea how to. The computers weren't there, the methods weren't really available, and even if they had been I probably wouldn't have been able to understand them, but it pre-adapted me for later on. R.G., who knew that stuff, once said: "You know, this is the way to do it; that's what we need to do!" So I was pre-adapted because I had struggled with this stuff.

Then I finally started writing my dissertation. "If you don't get this [Ph.D.] done," my dean [at SUNY Binghamton] told me, "you're not going to have a job." Rouse tells me I'm going to be dropped from the program. These were probably first-stage threats, and I probably could have strung it out a little longer, but I took it seriously and I started writing. I did the usual graduate student thing, you know, you do the ethnographic survey, or you do the environmental summary because you can't figure out what to do. So I wrote a long environmental analysis, emphasizing what parts of the environment people were using, the source of their lithic materials, where they were farming. Rouse said that this is a pretty good start on the settlement patterns, why don't you do settlement patterns? Settlement patterns had been around for a while, but there hadn't been a lot of such studies in the Southwest in the mid-1960s.

But once I got into the dissertation-writing mode, and read a couple of things about settlement, I was off and running. I don't think I even read all of the basics, but the idea was just so intuitive and the data really fit that kind of analysis, so I was able to move right on through it and get it done relatively quickly after that. So Rouse's idea really was a good suggestion. I have stayed...
with that interest ever since. I was able to bring in the idea of community, the idea of households and community, and the social aspect of settlement patterning as well in my dissertation, and that's been a theme, too.

TK: I wonder if you would trace your interest in trying to do modeling in Dolores and forcing the DAP into a modeling mode as an important part of our synthesis, to the influence of Bittle?

WDL: No I don't think so. And I don't think that was my idea. You know, you were involved in that and several of us were involved in kicking around how do we approach this synthesis. We had started with a research design that was kind of a Southwestern version of Kent Flannery's *Early Mesoamerican Village* [1976]. We used some of the same levels of analysis, same categories, and so forth. We realized early on that we had to treat these as research domains—bundles of questions—and that it was going to be hard to be really linear about it, in the sense of a very specific question and a very specific data set. So we came up with the idea of "generally useful data sets" that could be applied to a number of different questions in these research domains. The idea of modeling, I think was kind of a committee thing. I don't recall that I suggested that. And I don't know whether it was really modeling or just trying to synthesize. Toward the end I was able to come up with a couple of different scenarios that had somewhat different data implications. All this did was to serve as a very general, loosely defined, vehicle for doing synthesis. It worked because we had the submodels, which is where most of the action was.

RGM: But I believe you already had started to go that way, because I remember when we were analyzing the Cedar Mesa stuff I was always saying that it was only the relative numbers that counted—the number of Basketmaker II versus Basketmaker III and Pueblo—and you were always arguing for absolute numbers. And we did produce them. But in other cases, the absolute numbers weren't really important for the original questions [Lipe and Matson 1971, 1972] we had, but if you're going to do the kind of modeling that you did in Dolores, then you need those absolute numbers. So it seemed to me that you were pre-adapted to go that way from the kind of data you were requesting. You had some questions like that already developed.

WDL: Going back to the dissertation, I was trying to come up with the variables that made a difference in how people moved around and established different kinds of settlements. I tried to recognize the environmental variables,
and the equipment, and left the rest of us to get the reports done. And he saw that we had the people and the organization that could support the reports and support the lab. In other parts of his career, Jennings made great substantive contributions, and so did Breternitz, but both of them in this part of their careers were good models for how to organize and run a big project, and make it effective. Those were really good lessons.

Project directors had to rely on other smart people. In Glen Canyon there was Don Fowler and DeAnn Suhm (later, DeAnn Story), as well as Jennings. I also met Dave Pendergast in those days. There were a lot of other people around who also were important, such as Florence and Bob Lister. Glen Canyon put me in touch with a lot of productive people—Mel Aikens, Joe Jorgensen, Dick Gould, and Dave Dibble were others. I became associated with people who either already were prominent professionals or who were clearly on the track to that, so it was a good group to run with.

TK: What was Jennings like personally?

WDL: His style of management depended to some extent on intimidation. He was on top of things. He read our field notes. We sent them back to his office every two weeks and he would read them and write comments. We had to send the original back and we kept a carbon copy. He would send back notes, saying "what do you mean by this," or "you didn't really do a good job of summarizing this stuff." He required us to write every week what he called the Feature 1, which was a synthesis of what we knew about the site we were working on. We were supposed to mention every active feature, every active excavation unit and structure—whatever there was—and show how they were related in the archaeological context. This was an intuitive approach to what later became called site formation processes—and how our excavation strategy would enable us to understand how that site was put together, and ultimately how it was used at different time periods. So he was good on that, but he would not hesitate to embarrass you publicly, or to really dress you down. He also could be extremely positive and supportive, and, overall, his stance was supportive. He was there to provide what we needed of equipment, vehicles, logistical support, and so forth, but you knew he was looking over your shoulder, and if you weren't doing it the way he thought you should be doing it, he would really let you know.

After I earned my Ph.D. and he knew I was helping to run a big project [the Dolores Archaeological Project], he took me aside one time and said: "Bill, let me tell you; you know, you have a lot of people working for you, you ought to get mad at them at least once a week whether you want to or not" [laughter from TK and RGM].

I may be embroidering a bit here, but that's not inconsistent. He was a very interesting character and also a good editor and writer. He really communicated the importance of good writing, although the person on that project who did the most for me in this regard was Carol Condie, who at that time was Carol Stout. She was an English major, who had done archaeology at Danger Cave and other sites, and went on to earn a Ph.D. in anthropology at New Mexico. But she served as an editor for the Glen Canyon Project. She would work over my manuscripts and write in the margins, and taught me how to edit—how to give someone a critique that was helpful.

That project was a great opportunity, and since my first year in graduate school had been so difficult, I welcomed the excuse to take some time off. I was able to write a couple of monographs, do some serious research, and experience some leadership responsibility in a difficult situation. It was definitely at least the equivalent of getting an M.A. if not more. So when I went back to graduate school—and Yale didn't require an M.A. at that time—I would have had a much more difficult time if I hadn't been at Glen Canyon. Writing those two monographs [Lipe 1960; Lipe et al. 1960] gave me some confidence and experience that really helped a lot in getting through the rest of the graduate program.

RGM: With Glen Canyon being one of the largest contract programs of the time, I think it's natural that it leads to the conservation model [Lipe 1974] and the question is, how did you ever think that up?

WDL: Well, it was kind of in the air. One of the things that contributed to it was the loss of sites because of the Glen Canyon flooding. In the 1950s, when the project started, reservoir building was just kind of inevitable, and people didn't really question it. There weren't any legislative tools to question it, but by the mid-to-late 1960s and the early 1970s you had a whole change of consciousness. You had the fight over the damming of Marble Canyon—the dams that would back up into the Grand Canyon. That was in the early to middle 1960s, and that really raised the profile. You had Rachel Carson's Silent Spring [1962], and Stewart Udall also wrote a book called The Quiet Crisis [1963]. There was really a sea change in public attitudes about the environment—you could, in fact, do something about it if important things were going to be lost. So that's part of the context. And then, in the Glen Canyon project, we dug a lot of sites outside of the reservoir area because it was treated
as a regional project—a kind of settlement system study as you would call it a little later. But we also dug a bunch of sites that weren’t going to be flooded, and so that example was on my mind. Also in the 1960s, when I arrived at SUNY Binghamton, I did the Engelbert project. For several years, I was involved with an amateur archaeological society chapter in Binghamton, and we became the sponsor of this group at the department, so we got to know a lot of the amateurs. Then the Engelbert project came along, which was a big gravel knoll that was being taken out for an interstate highway. Although the legislation was in place, the regulations weren’t, and so basically nothing was going to be done. So I and a couple of dedicated graduate students, Dolores Elliott and Marilyn Crannell Stewart, took it on and worked with the amateur society to do something about it [Elliott and Lipe 1970; Lipe 1976a], and it turned out to be a huge site. We worked there for two long field seasons, and we brought in literally hundreds of volunteers, probably 300 in total. Some of these would work for two hours and some worked almost every day for two summers. The graduate students really kept it going, but I was out there a lot of the time. So that really tuned me into public participation in archaeology and the importance of public education, because the community really became involved. I probably gave 15 or 20 talks to service clubs all around the area, and it was a consciousness-raising experience for the whole community.

Then Bob McGimsey and Hester Davis started publishing—Bob’s Public Archaeology [1972] and then Hester’s Crisis in American Archaeology [1972]. I’d already written a draft of the conservation model paper at that time. When the Section 106 regulations began to be implemented, then things began to change in how salvage was being done, and it began to become more based in project planning. We also were affected by the possibilities of moving archaeology into a planning mode under the National Environmental Policy Act [Grady and Lipe 1976, 1977]. At the Museum of Northern Arizona [MNA], we were heavily involved in highway projects, but we hadn’t really seen the implication of the Section 106 regulations coming out of the National Historic Preservation Act that eventually took over. Lex Lindsay at MNA had been sponsoring and organizing meetings where archaeologists discussed this sort of thing. Then, at the 1974 Denver conference [Lipe and Lindsay 1974], which was around the same time that the conservation model paper came out, there was the emergence of a new way of doing archaeology in the Southwest under these planning-oriented processes of the National Historic Preservation Act and the National Environmental Policy Act [NEPA]. And the agencies [Forest Service, etc.] started staffing up with their own archaeological expertise. So things really were changing in a variety of ways at that time, both in public consciousness, in the academic thinking about it, and in the actual practice of doing archaeology on the ground, particularly with federal agencies and some state agencies.

I also was affected by the work I did in 1969 and 1970 at Cedar Mesa, because that put me in a direct relationship with the Bureau of Land Management [BLM]. I became very familiar with how they did the multiple-use approach by having specialists who looked at different resources. And I got the idea that archaeology is a resource that ought to be managed in the same way that they were managing watershed, wildlife, and minerals, and all those other things. That was the direction the agencies were moving, too, under NEPA, and as the agencies attempted to develop their multiple-use philosophies. The BLM didn’t really formalize that until 1976, but they were definitely organizing themselves in that way earlier. So I wasn’t the only one thinking about these things. There were lots of other people thinking about them as well, including McGimsey and Davis.

RGM: Bill, one of the streams you didn’t mention when you were talking about it, was recreation land managers. As I remember, fairly early on you knew some people in that area, and obviously had talked to them. Wasn’t there somebody who had their students do some plans for Cedar Mesa, for recreation?

WDL: There was a guy from the University of Michigan, who was doing some studies on outdoor recreation, and I ran into some folks from Utah State University who were trying to assess the effects of chaining on watershed values—on infiltration, vegetation, erosion, and so forth. So that was all part of the mix. I mostly remember working with some of the BLM people who definitely took a resource management perspective and that did have an effect. I was doing that in 1969, 1970, and particularly in 1971, when we did the Cedar Mesa project pilot study, but I also spent some time looking at vandalized sites with a BLM range technician named Carl Mahon, and talking to other people in BLM, when I started writing that paper in 1971.

I think Ray Matheny organized an SAA session that I commented on [Lipe 1971], and then Tom King organized another SAA session called Salvaging Salvage Archaeology the next year. I wrote a paper [Lipe 1972] for that, too, which I sent to Jennings, who was going to publish it in that short-lived series of Addison-Wesley separates in archaeology. The idea was that these would be useful in classes, and they were. Some were reprints, but there was
a lot of original stuff too. So I submitted the conservation model manuscript to Jennings, who was an editor in that series, and he held on to it for a while. Then the whole publication thing went belly-up, so he sent it back to me. I then submitted it to The Kiva in 1973 or early 1974, and they published it right away [Lipe 1974]. So it had kicked around for at least three, four years, in various drafts.

TK: I always thought that The Kiva was a strange place for that paper because of its national importance. It would seem as if it should have come out in American Antiquity, or something like that.

WDL: Well, I just wanted to get it out because I'd been sitting on it for a long time. I needed to get some publications out, and I thought that this was timely. I wanted to be heard, at least in the Southwest. Bill Robinson, the editor of The Kiva said they would publish it. If they had said "We can't do this for another two years," I might have sent it somewhere else. As it turned out, I sent reprints around to a lot of people and it caught on. Then it was reprinted in the George Gumerman and Michael Schiffer [Lipe 1978b] volume, and it gained circulation pretty rapidly, which I think is because it was timely. I believe the reason that it was successful was not that it's such a great paper—I have some criticisms of it now, and developed some fairly shortly afterward—but it was timely, and a lot of people were thinking along the same lines.

RGM: In our list of questions, we asked about your current evaluation of the original Kiva paper. Here, we don't want you to repeat the 15 or so publications that you've done in the last 30 years, where you have made some modifications.

WDL: Well, to summarize, one of the things that I didn't put in there, and should have, was the value of archaeologists as political activists. In fact, that already was well under way, with a lot of people effectively lobbying congress and state legislatures. McGimsey and Davis had lobbied in Arkansas in the 1960s to set up their terrific program there, a statewide archaeological survey. There was a lot of effort to start revising the Antiquities Act and things like that, so that was something that should have been covered and wasn't. And the other two things are already touched on in some of the other papers in Tracking Ancient Footsteps. One was the subordination of archaeological research to society's development needs—I really think that that gives the message that finding out new information about the past from archaeology is less important than building another access road to an oil well. That's not the right message. There ought to be room for archaeologists to say that an archaeological question is important enough to intrude on preserved archaeological resources. In fact, when I started writing that paper, the mode for salvage archaeology, and really research archaeology in general, was to dig up whole sites, or significant portions of them. But, with the advent of sampling, remote sensing, and more intensive analysis of smaller quantities of materials, archaeology really doesn't have to have much of an impact. We're way down the list in terms of what activities negatively impact the archaeological record. I believe a lot of people have adopted the sort of feel-good approach that if we quit doing our job and quit trying to find our new things by excavation, somehow we're better people. I think we have to keep our eye on the ball; our main value to society is studying the archaeological record and making sense of it. Conservation is only one means to that end.

And the other point I'd take back is the argument about preserves, which sounded good at the time. That was affected by some of the things that people in ecology were doing, trying to establish ecological reserves. But I believe that in practice, if you do that in archaeology you create black holes, in a knowledge context. You have to keep learning from the archaeological record right along, and not sort of quit for a decade or a century. Research ultimately is what drives the field and justifies employing archaeologists to do what they do.

TK: Much later, when you became President of the Society for American Archaeology [SAA], did you feel at that point that you still were trying to advance the goals that you put forward in the conservation archeology article? Or, did you feel that things were so different by that point, that you were trying to advance a different program?

WDL: The thing I focused on was trying to get CRM archeology—Cultural Resource Management archeology—better recognition in the archaeological community as an important and legitimate part of the profession. To that end, I worked to try to bring more people from the CRM field into committee and leadership positions in the SAA. I also spent a lot of time lobbying, as that was the Newt Gingrich Congress, elected in 1994. They came in, and one of the things that they tried to do was to get rid of the Advisory Council on Historic Preservation, and to cut back on some other components of the system that made CRM work. I spent a lot of time trying to work with other people to whip up the troops to combat that. So I think that I was more in the
mode then of trying to preserve what we had going. I was trying to keep that from being decimated, and also to get more recognition for CRM archaeology as a part of the professional field. I also spent an enormous amount of time on the transition from the Society of Professional Archaeologists [SOPA], to the Register of Professional Archaeologists [RPA], initially at the urging of Fred Wendorf. This was to try to make the signing on to the standards, ethics, and grievance procedures that SOPA had developed more widespread in the field of archaeology. Wendorf concluded, and I concurred, that the best way to do it was to integrate it more closely with the SAA, and ultimately the Society for Historical Archaeology [SHA], the Archaeological Institute of America, and other organizations. It was a lot of work to get the major societies to sign on, and to help sponsor it by putting some money into it. I believe it has been reasonably successful in that the number of Registered Professional Archaeologists now is probably twice or three times what the SOPA membership was, and they're fairly broadly distributed throughout the SAA and the SHA. I think that helps to some extent, even though most of the registered archaeologists are working in the CRM field. There is a substantial number of academics, however, who've signed up too, and it has helped to bridge the gap between those two parts of the field.

One of the things I'm concerned about in CRM today is the lack of academic people who have much experience in CRM archaeology. We often don't provide the kind of training that students want. In some sectors—I don't believe this is true of our department [WSU]—there's still a tendency to look down on CRM employment as somehow lower in status. I think that on the CRM side, there's not a sufficient effort to bring people from academia into the design of projects and so forth. So that's still a problem. And on public lands, there's a real problem now in investigator-initiated research; that is, in getting permission to do research that is designed based on the research needs of the field. Federal agencies, such as the National Park Service, BLM, Forest Service, etc., often have sort of top-down management plans, and have such a strong preservation focus that they often don't treat archaeological research as either a legitimate use or as something of value. They take into account the interests of livestock raisers, miners, recreational users and so forth, but somehow archaeology and the societal value of learning new things about the past doesn't get treated as a value they need to manage. This seems to vary tremendously from one agency and district to another. In some cases, they're very open and in some instances they're almost completely closed to it. It is a continuing problem.

TK: Do you think that federal agencies have the same bias against investigator-initiated research in other disciplines, such as wildlife biology or geology, or is archaeology singled out for some special treatment?

WDL: I don't know, that's a good question. I think there are more people in, say, wildlife ecology, who have a long history of working with the agencies on applied research that also has academic implications. Think about departments of wildlife—they're often oriented toward producing people who go on to work in management situations...I do have a sense that it's not as difficult to do investigator-initiated research in these fields. In the earth sciences and the ecological sciences there often are stronger traditions of academics being involved in applied research in conjunction with agencies.

TK: I wonder if we could turn to talking about what's going on in Southwestern archaeology. Here's a discipline, Southwestern archaeology, that you've been working in for thirty-plus years.

WDL: I started in 1956.

TK: So, it is fifty years, and you've seen enormous changes in the practice of archaeology in that period in every sector. I'm wondering if you'd like to share some of your thoughts about where we are now, and if you have any ideas about where we should be instead of where we are.

WDL: There are some questions I continue to be interested in that I believe are of fairly wide interest. One is the sort of macro topic concerning the large-scale movement of people. I really don't think we understand that very well. We've made a lot of progress, particularly in looking at environmental relationships, and maybe that's going to explain it all, but I think there also have to be some important cultural, social, and other economic factors as well. I don't think the answer is Stephen Lekson's model [1999] of a kind of central power base at Chaco, Aztec, and Paquimé that somehow caused people to sweep back and forth across the region. But I believe Lekson has the right scale of thinking about it, and he, more than anybody else in the Southwest, has opened people up to thinking about these issues. Personally, I've mostly been involved in the Four Corners issue—you know, why did the Four Corners area become depopulated when it did? But if you look at this question more broadly, you see lots of big changes. What happened to the Fremont? The Virgin branch? You have this pull-back from the far northwest [of the
Southwest initially, and then from the north, and at the same time population was growing in some other areas. And then you have a big depopulation in the Mogollon Rim, and Upper Little Colorado in the 1300s and 1400s, and in the area of the so-called Salado phenomenon. Dave Wilcox [1999, 2005] is another person who's been trying to grapple with this, and I believe that we need to think harder about those issues and try to come up with ways to get a handle on them. We're making some progress—at least people are beginning to think about this, and trying to deal with it. Good starts were made in *The Prehistoric Pueblo World* that Mike Adler edited [1996], which came out of the Crow Canyon symposium, and *The Protohistoric Pueblo World* that my colleague Andrew Duff [Adams and Duff 2004] was involved in. But there's a lot more to be done in that area, and I think it can drive research for a long time. I believe that's one of the things that I can see continuing to happen.

RGM: I think it's something that only became possible recently—much additional information, or big chunks of it, are now available to attempt these sweeping evaluations. I believe Mark Varien's dissertation [1997, 1999c] is a good model that shows what can be done now. Steven LeBlanc's [1998] fine new treatment of Pueblo IV times and conflict wouldn't have been possible to write very many years ago.

WDL: I definitely think there's an information component, but if you go back and look at, say, Alfred Kidder in 1924, he didn't have a lot of information, but he had a big perspective. Of course, it's easier to have a big-picture view if, one, you don't have much information, or, two, if you ignore most of what's out there. Look at Erik Reed. He was talking about some of these big pattern shifts from his wide reading in the Southwest in the 1940s and 1950s, and some of his work [e.g., Reed 1946, 1956] is still very interesting. So it's partly a data thing and partly a mindset thing, and I do think it is a good direction for some people to go.

The Southwest has a long-term record of being the trial place for new methods and theories. That's one reason why we now have agent-based modeling [ABM] programs in several places in the Southwest. It's a good place because of the richness of the environmental and archaeological evidence, and the good chronologies, so you can try things out. ABM is very promising and will drive one aspect of scientific archaeology. The one thing that we haven't seen as much of in the Southwest as in some other parts of the world, and even other parts of North America, has been the use of Southwestern data to support, or as a platform for, post-modern interpretations. I believe that some effects of post-modern thinking have been in many ways positive, such as the kind of thing that Michelle Hegmon [2003] was writing about—processual-plus archaeology. It's what Gordon Willey and Jeremy Sabloff [1993] earlier called expanded processualism. I picked up on that in my 1999 book [Lipe et al. 1999]. This has helped us shed some of the shackles of top-down integrated-system thinking that came out of the early days of the New Archaeology. We've gone more to the notion of agency, as you [Tim Kohler] have shown in a scientific mode [ABM]. This is where you can look at systems emerging from actual practice and actions of the real entities on the ground, which are people, households, small groups, and communities. That's going to continue to be a very successful approach. Southwestern archaeology has continued to be a test bed for other scientific approaches, such as the good work being done in chemical archaeology and sourcing. There's so much to work with in the Southwest. I believe this is healthy.

RGM: But in some ways, is Southwestern archaeology a victim of its own success? There's so much going on there, and so many publications, that you tend not to have general Southwestern archaeologists; instead there are Mogollon or Mesa Verde specialists. In some ways, they aren't up to date on other developments in the broader region because it's a full-time job just to keep up with their own little area—but also, they aren't involved in other events in archaeology.

WDL: That's a good point...

RGM: You mentioned Jesse Jennings and Emil Haury. The whole Southwest was their expertise and they could see all those patterns, but most people now aren't doing that.

WDL: I think that they're not doing it because they haven't tried. This is what Lekson has been arguing about, saying that we need to look beyond our own mesas and valleys, but it really is hard. You either must have someone who reads prolifically and remembers everything, or you need a systematic approach. A couple of volumes have brought a bunch of people together and said let's just look at these variables across the Southwest. If you approach it systematically, you can pick things to look for. The old Southwestern Anthropological Research Group was an attempt to do that early on, by focusing on some questions and some variables, and then getting people to work collecting
the data. It's not easy, but it's certainly easier than trying to do everything all at the same time.

I think that the Southwest could be a test bed for the development of real theories of cultural evolution. A lot of what we've done over the past 35 or 40 years has focused on society, environment, and demography—that is, social and economic organization, population, and so forth, and that's really productive. I think a kind of etic, comparative approach, focusing on areas that are likely to have regularities of process and outcome is good, and that's going to continue to be important. But I think that we need to turn back to culture, and look at how culture gets transmitted and how culture changes, and whether there's anything like a selective process going on. Robert Dunnell [1978, 1980], Michael O'Brien and Lee Lyman [e.g., 2000], have floated some ideas and stirred up some interest in that, but I don't believe they have the solution. We probably need work in psychology to look at what are the units. Are they schema, are they memes, are they traits? What are the units? Evolutionary biology made a lot of progress after people discovered genes. I don't think we have anything corresponding to genes, or a unit that is self-replicating in any way. I think that we have a kind of reproduction process that involves human agency. Material culture, though, is a heck of a good way to study it, because people have stuff that they can copy. Well, how do they do that? I don't believe that kind of research has been done, and I think if it is done, maybe it has to be done by archaeologists with living people. But then you could use the results to test those ideas against the wonderful long-term record of material culture variation and change that the archaeological record provides. And I can see the Southwest, because of its great chronology and highly variable material culture, as a test bed for that. I don't know where that's going to come from. I think some of Scott Ortman's work [e.g., 2000], attempting to apply the notion of metaphor and schema from cognitive psychology, is a starting point, and he's a systematic researcher who has begun to tabulate attributes for a pretty substantial body of sites across a region. There may be some germ of an approach there that could be built on. And other people are doing some of this work. I'd like to see us do more in that direction.

Fraser Neiman [e.g., 1995] and others have made progress thinking about the kind of variation you can expect under drift. There's some of this work coming out and you [Tim] have been a contributor [Kohler et al. 2004]. I think there's some potential there for getting the units right and then beginning to focus on the processes by which some things survive and some things don't. I really think we're kind of the inheritors of Sapir's notion of drift. People are actually using that. It's kind of gone away in linguistics, even though I think they can use it. But there is a lot of random variation that's not that different from what you see in genetic drift. I think we could specify this better, and then look at how it differs from things that have more selective value, whatever we mean by that.

I also think there is more work to be done in this classic processual stuff we've been doing—the nexus of environment, economy, social organization, population, and technology—where you find a lot of that work has focused on regularities of outcome. Bruce Trigger's book [2003] essentially redoes what Adams and Steward and others were doing in the 1940s and 1950s, only at a new level. He finds significant regularities of outcome when he looks across early civilizations, but we also need to focus on the regularities of process. This is what the biologists have done; they can explain all kinds of regularities and differences in outcomes with a relatively small numbers of processes—selection, drift, mutation, gene flow, and so forth. And I think we have the handle on some of that, with things like intensification, and there are a lot of regularities in how hierarchy is achieved and expressed. We have a lot of this kicking around, but somebody needs to synthesize it in the same way that Trigger synthesized the regularities of outcome. We have had attempts at it, and the Southwest may not be the best place, because it may be better to try to do it with bigger differences between simple and complex societies. In the Southwest, we're stuck with these in-between societies in the late period that are harder to make sense of, but I think it may be more of an intellectual problem than it is a data problem. So I'd like to see Southwesternists, or somebody, take that on in a more straightforward explicit way.

TK: Well, I hope that it is the Southwesterners that take it on. That's an exciting area, one that Southwesterners are well equipped to work in because of the nature of the data base. We're beginning to educate a cadre of people who are interested in those problems, so maybe it'll happen.

WDL: But I think that the shift needs to be more explicit and to recognize what we want to do. We talk about processual archaeology, but it's really sloppy. Process sort of comes in ad hoc and people don't really try to talk explicitly about this process or that process and what it can and can't do.

RGM: Bill, are there things that haven't been covered, either in this interview or in the book, that you've thought about and would like to put on the record?
WDL: Touching again on the broader topic of public archaeology, I do think that it's essential for archaeologists to keep working on trying to find common ground with Native American groups. I think it's important morally and ethically because of the history that we all understand—both the larger history of oppression of Native Americans, and the marginalization of living peoples in the way archaeology was conducted for so many years. But I think it's also politically and intellectually important. It's politically important because the tribes, in fact, can have a large effect on what does or doesn't get done in archaeology, and, of course, the tribes employ lots of archaeologists and are part of the system in that sense. Intellectually, I think it's been of great value to archaeologists to have to examine implicit premises about what's important, and how to present the history of traditions and history of a people who are not in the same historical tradition that we come out of. It has been intellectually challenging, but also intellectually productive.

I think it's a mistake to handle the differences in views of history held by archaeologists and Native Americans by saying: "Oh, well, they're different realities, and they're different pasts." There weren't different pasts, there just was one. What we have is different records of it, and different understandings of it, and that always will be there. In a broader sense, the archaeological record is good at revealing certain kinds of insights and understandings, and archaeologists need to understand that and play to their strengths and recognize it. Oral history and oral traditions have their own version. You're going to get certain kinds of insights about the past from oral traditions that you don't get from other records. And the same for written and documentary records—they give you a different view. And, environmental history provides a different view, and linguistic history gives you another somewhat different view. There's some overlap among all of these, but you just have to recognize what it is that our particular kind of data and our particular methods can give us, and play to those strengths and be willing to have critiques of our own approach but also a critique of other approaches, and the same from linguists and the same from people who are interested in oral history. I hope that's how it plays out, and thus not have some kind of mushy idea that all of these different records and methodologies are going to give us exactly the same story, because they're not. That's just how it is, but we may be able to play these different approaches off against each other and achieve deeper understandings of what actually happened, although they'll never be perfectly in tune. I don't think we need to start arguing that one kind of approach is necessarily better than another. They're different and they have different strengths and weaknesses. I hope that's the direction that this goes—the recognition of those strengths and weaknesses, and a willingness to be critical of the results of these different ways of understanding the past, without saying that this is bad, that's good. And I think that way of looking at the past is essential if we're going to live in a multicultural world, with multiple traditions of doing intellectual work and perceiving history. That may be Pollyanna-ish, but it's something that we all need to think about and work at—to try to find ways of working with other scientists and with people who come from other traditions of thinking about history and the past.

TK: What makes a good archaeologist?

WDL: Hmm—I think there are different ways of doing it. I believe that some people are better at some aspects than others. What I see, with students in particular, is that one of the most important factors is the ability to frame a researchable problem—a question that you can actually get data for, that can be linked to your question in a reasonable way. Vision is important, and the ability to think up realistic questions that you could ask of the record and not just sort of repeat what everybody else has done. An understanding and respect for the character of the archaeological record, a willingness to think about what kind of record it is and how it got to be that way, also are important.

TK: Where does that creativity or vision come from that you mention?

WDL: I do think that the people who do best in a field like this have questions on their minds all the time, that are always ticking away in the background. They really care about trying to figure things out, and they keep turning these questions over, even when they're not actively working on them.

RGM: I'd like your ideas on something I've observed, and it's a willingness to be wrong, to actually test your ideas instead of defending them, and I've argued to undergraduate classes that the field moves ahead on the basis of people being wrong, and willing to be wrong.

WDL: Something that I always tell students is that if you wait until your thesis is unassailable, you'll never finish it. Everything is always provisional. There's an evidential component and there's an idea component, and if either of those things change—you get a better idea or you gain better evidence—you're going to have to come up with a new interpretation of what this all means. If a field, in fact, is empirically driven and we actually find out new empirical
things, it follows that the work you did 10, or 20, or 30 years ago is not going
to be very good by today's standards. You have to understand that and be will­
ing to live with it. On the other hand, the field does progress by critique and argument, so you can't just toss away your hard-earned understandings easily.
You do have to be willing to defend them, but you also have to understand
that they are provisional and they're only as good as the data and the ideas that contributed to them. That's definitely something I've tried to communicate to
students. I don't know whether I've practiced it in my own work.

TK: Because he's never been wrong...

WDL: It's because I've moved into writing pompous essays instead of saying
what I think the archaeological record means.

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Abbreviations—
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DA – Department of Anthropology
GCS – Glen Canyon Series (Flagstaff, Arizona)
MNAB – Museum of Northern Arizona Bulletin (Flagstaff, Arizona)
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