



Archaeology's Diachronic Dimension, Historical Anthropology, and the Hawaiian-Shirt Renaissance: An Interview with Kent Lightfoot, Professor of Anthropology at the University of California at Berkeley, on Friday, November 19, 2004 at Lightfoot's home in El Cerrito, California, and a Follow-up Telephone Interview on February 10, 2005.

Seth Mallios

MALLIOS (M): Before we jump into a discussion of today's archaeology, I'd like to start with some personal background. What were the inspirations for your career? Specifically, why didn't you join your father and brothers in Ophthalmology?

LIGHTFOOT (L) (Figure 1): I can't stand the sight of blood! *[Laughs]* I bet it's like a lot of archaeologists: I got hooked when I was a kid. The key thing for me was when my family packed up our '63 blue Travel-All, and we toured the American Southwest. We visited a number of national parks, including Mesa Verde and the Gila [Cliff Dwelling] National Monument and saw a lot of great archaeology. I knew then that this is what I wanted to do when I grew up. So I had it in my head, beginning when I was about 10 or 12 on that trip, that I wanted to go into archaeology. And I just continued to think that way. When I was in college and continued on to graduate school, my idea was to do a career in National Park Service archaeology.

M: If you hadn't become an archaeologist, what do you think you would have been?

L: Tough question; archaeology was ingrained in me so early. Probably geology, paleontology, or some related field. Not medicine, though *[Laughs]*.

M: You did your undergraduate work at Stanford, right? Didn't you also play football there?

L: Yes, sort of.

M: Any "Big Game" [Cal/Stanford] memories?

L: Well, yeah, I did suit up for the Big Game. It was the 1972 game; both teams emptied out the bench and we had a big brawl out in the middle of the field. I remember a couple of Cal's mammoth offensive tackles came over to me just as I was hopping on top of one of their little safeties. They told me to get my hands off him, and I said "sure" and immediately went back to the bench and sat down. That was it; that was my big role *[Laughs]*.

M: Why did you go to Stanford?

L: My eldest brother went there; he's five years older than me. When I first went down [from Santa Rosa] to visit him I was stunned. It's like a country club. It just seemed like a really nice place to be. I applied to two schools for college: Cal and Stanford. I would have had to have been a walk-on for Cal's football team. However, since my brother played for Stanford they offered me a tuition scholarship. So I was able to go to Stanford tuition free, and I got to play football—that was the clincher.

M: Were you an anthropology major, and who were your most influential teachers?

L: Yes. Ezra Zubrow was the key person I worked with before he went to Buffalo. Also, there was Bert Gerow, a California archaeologist.

M: Did Gerow do fieldwork close to Stanford?

L: Yeah, he did work in the Bay Area. He hated [famed California archaeologist Bob] Heizer! Gerow used to begin his lectures on California archaeology by trashing Heizer.

M: Growing up in Santa, Rosa, did you have a personal connection with Fort Ross and the people who worked there?

L: Yeah, my connection was with the ranger, John McKenzie. In those days, rangers were everything—historians, cops, and they even got in a little archaeology. But for the most part, McKenzie was more of an interpreter, a historian. He was also a patient of my dad's. My dad was an eye doctor, and he used to go out to see McKenzie after doing cataract surgery on him and to do some follow-ups. I went along and got to know him pretty well. He used to take me around the fort when I was a kid. Then later, when I was an undergraduate at Stanford, we'd talk about how it [Fort Ross] would be a great place to work.

M: Did you consider taking time off between college and graduate school?

L: I thought about it, but I figured if I took too much time off I probably wouldn't come back. I applied to several different schools: U of A [Arizona], ASU [Arizona State University], and Cal; but I didn't get into Cal.

M: With whom did you work at ASU?

L: Fred Plog was my adviser. I also worked with Ray Ruppe; he's a cultural historian who specialized in the Southwest.

M: Now, you were at [SUNY] Stony Brook before Cal, right?

L: I was there five years from '82 to '87.

M: When the Cal job came up, did you know immediately you wanted to come back home?

L: Well, at that point in time, there were two issues: First, Roberta [wife and archaeologist Roberta Jewett] decided she'd had enough of Long Island-

M: *[Laughs]*

L: and was ready to move west. And second, the department was going through a tough time. The dean had called me in right as I was going up for tenure, and he said, "There's a chance we might cut the department." It didn't happen; he was just a nice guy who was giving me a shot across the bow. At that point I started looking. The Cal job came up, so I applied.

M: Wait a second; didn't you also have a Northern Illinois University connection?

L: I taught there for a semester. I went from ASU to Northern Illinois to Stony Brook and then back out to Cal.

M: Was it tough to leave the Southwest?

L: Only because they kicked me out! *[Laughs]* After I got my PhD, I taught an intro class at ASU. I was all ready to teach it again, and they had me signed up. But then this one-semester position at NIU opened up. I didn't want it; I didn't want to drive all the way out to Illinois. Ray Ruppe, the chair of the department, pulled me aside and said *[whispering]*: "You better go take it; you're too comfortable here." So, they booted me out!

M: When you left ASU, were you consciously looking for an entirely different kind of project to undertake? As you went to Illinois, New York, and California, were you deliberately trying to find something new?

L: Not really, I just couldn't get a job in the Southwest. I could have done CRM, but it was pretty clear to me that in terms of academic positions I wasn't going to get a job doing Southwestern archaeology at a university. My strategy has been—and continues to be—go where you can, and go with the strength of doing local work. Sell yourself by developing a local archaeology program. As opposed to saying I'm a Southwestern archaeologist, I said that I'm an archaeologist and I can work anywhere.

M: How did it feel ultimately coming back to Northern California?

L: Oh, it was great. Yeah, it was very nice. Cal was and is a great place. Jim Deetz was still here, Meg Conkey was just coming on board, Ruth Tringham was here; it was just a great crew of people. Bill Simmons was the chair then; he's the one that hired me. They hadn't had a California program in years, and the potential to try and set something up was really exciting.

M: Reflecting on your career up to this point, has it been turning out the way you expected? Are there any surprises or cautionary tales that come to mind?

L: When you get into academia, you find out there are all these little hidden land mines that, if you're not careful, explode around you. I think the toughest thing about academia is dealing with colleagues. That's always difficult.

M: And yet you are very diplomatic. I often see you doing your best to keep the peace among academics, archaeologists, etc.

L: Hmm. Like how?

M: Well, you led Cal's archaeology division at a time when Anthropology departments across the U.S., like Stanford, were splitting apart. And your 2000 article with Feinman and Upham (Feinman, Lightfoot, and Upham 2000) is incredibly conciliatory and open in comparison to your work from the '80s, especially the '82 article (Lightfoot and Feinman 1982). I remember [Jeff] Hantman telling me at UVa during the '90s that although most of you ASU grads are no longer in the Southwest, the Southwest is still on your mind, and you occasionally like to throw some intellectual grenades from afar. The 2000 article is no grenade; it's downright inclusive. Since we're on that topic, what were your motivations for the 2000 article?

L: That article was a re-evaluation of some of our earlier models and ideas. It took place through a series of discussions over a couple of years. Originally Jeff [Hantman] and Steve [Plog] were part of it, but over time they dropped out and it just came down to the three of us [Feinman, Upham, and Lightfoot] putting it together.

M: But the tone is strikingly different. Were you calling for everyone to get along?

L: Yeah, I was being diplomatic. There's always room for diplomacy. But, sometimes I do it [diplomacy] a little too much. You'll have to ask Berta about that.

M: The next group of questions concerns historical archaeology. How do you define historical archaeology?

L: Me? As historical? Or do you mean how I see other people?

M: [*Laughs*] I think it's funny that you still hesitate in acknowledging that you do historical archaeology. Let's start from an outsider's perspective. How would you define what others do as historical archaeology? Then, bring yourself into the discussion.

L: I define historical archaeology as an archaeology in which you have some kind of written historical record that exists for a particular area. This, of course, varies from place to place. And, that's also how I do it.

M: So, it's methodological; using both the historical and archaeological record?

L: Yeah, but it's temporal as well. Time is built into it too.

M: Similar to Jim [Deetz]'s post-1492 definition from [*In*] *Small Things [Forgotten]* (1996)?

L: Yeah.

M: Would you ever modify that decision to attach to, say, the spread of European civilization?

L: No, not necessarily. I look at that as kind of a special research topic or problem, not a field in and of itself.

M: What about historical archaeology as the spread of capitalism?

L: I don't see that as the basis of a field called historical archaeology. I see it as a separate field of Colonialism. But, that's more than just archaeology. Colonialism is an interdisciplinary study of a particular set of research questions; it's more than a field with distinct theories and methods. When I look at archaeology, I don't see that much difference between prehistoric and historic[al] archaeology (Lightfoot 1995)—I think you know that.

M: Yeah.

L: I think there are differences in terms of time periods and areas you work in, and in historical archaeology you have to develop particular expertise in researching and understanding historical records. In many ways it's no different than prehistoric archaeology because you also use different kinds of materials to build an understanding of different cultures. The fact is, you need to understand how to use written records no matter what the hell you're doing because so many other lines of evidence, like oral histories and oral traditions, have been written down. We all need to understand how to work with written records. I don't see a firm [prehistoric/historical archaeology] boundary at all. I see it more as a general temporal difference.

M: I talked to my archaeology colleague Joe Ball about this—he's a Mayanist—and tried to bait him into saying that he did historical archaeology because the people he studies had texts. But he said he felt there was a line you could draw on how much texts are used in your analysis; minimal for him, substantial for me. I understand what he is saying, but I'm not entirely comfortable with that, because I know that there are some archaeologies that fulfill many of the other usually cited criteria—time (post-1492), space (the Americas), form (a methodological use of historical records), thematic (European expansion, spread of Capitalism, etc.)—that are neither isolated nor consistent. Like so many issues in archaeological taxonomy, the closer you get to a boundary, the less clear the distinction appears. For example, archaeology at a site from 1491 closely informs findings from a site occupied in 1493 [rendering a 1492 distinction less meaningful]; English colonial sites in North America have important ties to similar sites in South Africa, India, etc. [undermining a spatial emphasis on the Western hemisphere]; and many historic-period peoples produced no written records and were not directly part of the emergence of the modern global system. There are just so many exceptions to strict temporal, spatial, and formal definitions of the field.

L: But that's where I think the strength of anthropology is. It's in historical anthropology and a four-field approach. That's where it really takes place; that's where you've got the ability to use all these different sources and different lines of evidence. Instead of trying to set historical archaeology apart with these criteria, you can see it as part of an important broader dialogue

going on in historical anthropology. And I think historical archaeology can play a much bigger role.

M: That's interesting, especially in regard to your recent work, which draws significantly on the work of Marshal Sahlins and his work in historical anthropology. You don't seem to embrace much post-modern thought in your work. In fact, it's very distinctive that you often cite *Islands of History* (Sahlins 1985) but not Obeyesekere's *Apotheosis of Captain Cook* (1992). Do you feel that the field of anthropology has been derailed by certain post-modern critiques?

L: Yeah, to some degree. I think that some of it's healthy, like evaluating where the discipline's going and scrutinizing alleged objectivity. But if you go too far, it turns it into where you really can't depend on any kind of real empirical analysis.

M: Do you think your emphasis on empiricism is tied to a tangible archaeological reality?

L: Well, yeah. You know that there is an empirical reality. How you generate your interpretations, that's a whole other matter. But, I do believe there's some kind of actual body of evidence that you're working with. Sometimes postmodern deconstructionism gets beyond the ability to see that, and in that way I don't think it's been necessarily healthy for the field.

M: Do you know Marshall Sahlins and have you followed his debate with Obeyesekere regarding Sahlins's Cook-as-Lono theory?

L: I met Sahlins through Pat [Kirch], but I don't know him very well. Yeah, I know their opposing views but I haven't followed the debate that closely.

M: I'll give you a legal analogy (Mallios 2006) and ask you which side you favor. In U.S. civil cases, the verdict is based on the preponderance of evidence. In criminal cases, the defense only needs to raise reasonable doubt. If Sahlins has the preponderance of evidence in his favor through his established knowledge of native Hawaiian culture and history, and Obeyesekere has raised reasonable doubt through his emphasis on the politics of oppression; which side are you on?

L: Definitely the empirical side. We all need to be using real evidence. I like that analogy; it's a good one.

M: Now I have some questions about the comparative nature of historical archaeology. Do you think that historical archaeology is necessarily comparative?

L: I think it should be. As a discipline, I think that's where the strength of anthropology and archaeology are. The ability to do comparative work in terms of looking at research questions is critical. I think that part of the problem with anthropology today—and some of it has to do with post-processual archaeology—is that it's too historically contingent. It's to a point where comparative work has gone out of fashion, and I think there's a problem with that. And a lot of it, of course, has to do with the critique of processual archaeology and its search for broad, cross-cultural generalizations. But I think there are ways of doing comparative work that allows you

still to look for certain kinds of broad processes while understanding that there are a lot of distinctions and differences that take place, especially on the local scale. It seems like a lot of that's been thrown out, which is unfortunate. It restricts your ability to do comparative anthropology, which is a real strength of the discipline. I'm big on cross-cultural comparative work in archaeology, although when you think about it, there's not much of that being done right now.

M: Is that where your research is going? I noticed after the first two Ross volumes (Lightfoot, Wake, and Schiff 1991; Lightfoot, Wake, and Schiff 1997) that you started to do more journal articles that synthesized what was going on in terms of regional trade patterns and even in the emerging world system (Lightfoot and Martinez 1995; Lightfoot and Simmons 1998; Lightfoot 2003). Are you deliberately being more comparative?

L: Yeah. I have a UC Press book (*Missionaries, Merchants, and Indians* (2005)) that's just about to come out that is a comparative analysis of the missions versus the mercantile colonial program in California with an emphasis on our work at Ross. So, yeah... that's definitely where I'm headed.

M: We'll get to your new book in a little bit. First, let's put it in context by looking at some of your earlier work, which seems to place a great deal of emphasis on ethnographic analogy—the '82 article (Lightfoot and Feinman 1982), the '84 book (Lightfoot 1984) and the edited volume that you, Berta, and Upham did (Upham, Lightfoot, and Jewett 1989). These ideas are very different from your recent work that is much more dominated by a direct historical approach (Lightfoot, Wake, and Schiff 1991; Lightfoot and Martinez 1995; Lightfoot, Wake, and Schiff 1997; Lightfoot 2003). It is a careful use of a direct historical approach, but your use of ethnographic analogy seems to have become minimal. Has the nature of the sites that you are studying changed, or has the change been in you?

L: Mmm, probably a change in me, I suppose; a kind of rethinking.

M: Have you become more skeptical of ethnographic analogy?

L: It depends on how it's used. But to answer your question: yeah. You have to be careful. What I like about the old Julian Steward direct historical approach is that you're really looking at change and trying to understand change. The basis of ethnographic analogy is to find some kind of continuity, which is probably true in most cases. However, you can't always assume that continuity is there, and that's where people get into trouble.

M: When I look back through your work, it is mostly cohesive, but 1995 really stands out. Something happened with Lightfoot in '95. Not only do you sound the call to unite prehistoric and historical archaeology (Lightfoot 1995), but you and Nettie (Antoinette Martinez) write an important article critiquing frontier studies that I would almost call postmodern if I didn't know you two so well (Lightfoot and Martinez 1995).

L: Well, I don't know if I'd call it postmodern-

M: C'mon, a critique of a macro-scale approaches that emphasizes hegemony? What I'm wondering is, is this where your prehistoric training ran into a slightly different animal—that of historical archaeology, and did historical archaeology cause you to rethink the assumptions of certain models?

L: Mmm, that might be. Yeah, it might be. I hadn't thought too much about that, myself.

M: This is very different from our conversation up to this point. How do you reconcile a comparative macro-scale approach with this post-colonial critique of world systems, frontier zones, etc.?

L: Hmm. I look at it as macro-scale in the sense of dealing with broader and more comparative research questions. But, you know, the problem with some of the world-systems stuff is that it is a model that was already put into place; it was entirely top-down. It was so rigid that it was unable to evaluate how it would play out in different places and under different conditions. The problem with the world-systems approach is that it tends to be a cookie-cutter format that oftentimes is put into place with certain expectations that do not occur.

M: The cookie-cutter you're describing is like viewing historical archaeology as one of those old-school horizons; a broad trend that has cohesion because of something like the spread of European civilization or colonialism or capitalism, etc.... I also see those as too rigid.

L: Yeah, that's what I mean; these top-down models often have very little fluidity.

M: Your 2005 book is true to your vision of macro-scale comparative archaeology; yet it also exposes the way in which a pre-eminent anthropologist—Alfred Kroeber—inadvertently shaped the future of indigenous culture. When you walk the halls at Berkeley, do you hear Kroeber's ghost? Was it difficult to critique an anthropological legend that is so cherished, especially at your university?

L: It was difficult, especially following the whole recent Ishi debacle. Some of our emeriti, like George Foster, who knew Kroeber quite well, felt that Kroeber had been critiqued unfairly with regard to past decisions about Ishi and his remains. Kroeber was fantastic for what he did for California ethnography. Without him, of course, much of the information we have today would not be around. I feel that you have to look at all his strengths and then recognize that there were some other aspects too.

M: You could have only written about the colonial history and archaeology. You could have left out the ending analysis that identified the highly influential role that Kroeber as an ethnographer had in determining which tribes would ultimately be federally recognized.

L: I wanted to give the book a contemporary focus. I didn't want it to be strictly a historical comparison, but a genuine look at Indian country today in California that tied together the past and present legacy of colonial history and archaeology. If you're going to do that, you need to bring in the anthropology and touch on Kroeber.

M: Did you begin writing the book knowing that you would end with this deconstruction of Kroeber?

L: No, not really; it developed over time. The ideas stemmed from a class I taught with [Bill] Simmons (See Lightfoot and Simmons 1998). Our class compared Spanish, Mexican, and Russian colonialism. It included Anglo-Americans as well, but I cut that from the book because it was just too much. As I was writing the book and trying to figure out patterns in Russian and Spanish colonial practices, I kept thinking about the legacy of native Californians today and the issue of federal recognition. At about the same time, Robert and I went up to Grinding Rock State Park in the Sierras where they had this huge map of current Indian groups. It did a really nice job of showing all of the federally recognized tribes. I looked at the thing and said, “Holy Moly!” The pattern was suddenly clear. So no, I didn’t start out writing the book with the intention of critiquing Kroeber, it came to me as the research came together.

M: How important is the last section on Kroeber to this book?

L: I think it gives the book relevance to a larger group of people beyond archaeologists and historians. It brings many of the book’s issues into the present and addresses the “so what” question. The only way to understand today’s multi-ethnic community is to understand the archaeological and historic past.

M: You analyze seven factors [1) enculturation programs, 2) native relocation programs, 3) social mobility, 4) labor practices, 5) interethnic unions, 6) demographic parameters of colonial and native populations, and 7) chronology of colonial encounters] across three colonial systems [1) northern missions in Alta California, 2) southern missions in Alta California, and 3) Russian colonization at Colony Ross]. It is very methodical; it has a processual rigor—I expected to see a chart. But it takes into account very contemporary and post-colonial factors, like indigenous agency. Was it difficult to bridge these two disparate paradigms?

L: Not really, because historical anthropology is the bridge. It allows you to address broad research questions in contemporary ways. Look at [Marshall] Sahlins or [Pat] Kirch or [Eric] Wolf’s *Europe and the People without History* [1981]—I’ve always thought that was a monumental book. There’s some really good work out there. It’s too often the case today that any publication over ten years old is ignored. And, I didn’t include a chart; that would make me too much like Ezra Zubrow [*Laughs*]. But I did use some things we learned back in the processual days.

M: In terms of the relationship between artifacts and text, historical archaeology has been burdened by a label that Noël Hume gave it in 1964 (Noël Hume 1964), declaring that archaeology is the handmaiden to history. In your own work, you use historical records as a springboard for robust archaeology. For example, while the census for the Alaskan village is important, you emphasize that we need to move far beyond it, in order to appreciate the multicultural components of the village. Describe the relationship between texts and things. Is there a balance between the two? Do the artifacts tell you more?

L: I tend to emphasize the archaeology more than historical text because in the situation I'm looking at, the historical records don't provide that much real meat and potatoes. They tend to be situations where the people really don't have a lot written about them. In addition, the records are typically written by outsiders who may not be too knowledgeable about the people they are describing. Those records can be very helpful, but they can also, as you well know, have a whole series of biases built into them. This makes the archaeology so critical. I don't think you can even do that kind of historical analysis without the archaeology. Look at California mission studies for example. There is a huge literature on missions and Indian neophytes. Most of it's based on historical research, and most of it was written by the padres, as well as some European visitors recording various accounts. And so you have this whole synthesized narrative about the neophytes that is built primarily on the padres' accounts. Good stuff, but it's not balanced very well. Most historians don't incorporate all the mission archaeology that's been done into their own work. I think if they did, there might be a very different perspective about the daily lives and practices of the neophytes. Overall, the field of History has marginalized archaeology and I think it is going to have to change. Archaeology provides a lot of great information. I don't think you can write a history of colonial California without having archaeology. I feel that pretty strongly.

M: I also noticed in the second volume of Fort Ross (Lightfoot, Wake, and Schiff 1997) that you did a lot with the oral history. Do you feel that analyses of oral and written histories are similar? And, do you feel that these studies should be done before you dig?

L: I don't know if it all needs to be beforehand. I know that that's traditionally what tends to be done in historical archaeology—you do all the historical analysis prior to the archaeology. But then again, some of the early historical archaeologists—like the key processual guy back east who did all the pattern analysis—made a point of doing the archaeology first without worrying about the historical studies (South 1977)

M: Stan South?

L: Yeah, Stan South. Of course, lots of people said, "Aw, that's terrible." But I think there's a certain element now where the history is overwhelmingly leading what is done, and I'm not so sure that that's always the way it should be done. I think it should be done jointly. If you finish the historical analysis first, you can get into a situation in which your archaeology is often just backing up what the historical sources are saying. Like Jim Deetz says, you know, that's one way you can really spend a lot of money on things you already know (Deetz 1993).

M: *[Laughs]*

L: With a lot of these populations that you're looking at, they don't have historical records. If you're really going to try to include their voice, then you'll have to move into either oral history or oral traditions. And, you need that just as much as the historical record.

M: I see your work through the years, turning more towards the historical period with extra attention on different indigenous responses to European colonialism. When you were setting up the Ross dig, did you initially have in mind to dig simultaneously in California, Alaska, and

Hawaii? Was your research design to uncover the regional pattern with special attention to local histories and indigenous agency?

L: Not at the time, no. I was just trying to get tenure.

M: *[Laughs]*

L: I needed a field project. And quick, I knew McKenzie!

M: *[Laughs]*

L: No really, that just developed.

M: It's interesting because, not only do you have a field project that has spread out so nicely in its analysis of a global system, but you've also had students that have gone through the program, now in tenure-track jobs-

L: Yeah, top-line people. I've been fortunate. I've worked with some great students who have pushed hard and done a lot of the work.

M: Do you feel that, as a dissertation advisor that more often you've assigned topics, or do people come to you with ideas and you just try and tweak your own research design.

L: The latter. I don't assign much- nobody pays any attention to me.

M: *[Laughs]* Is that one of the biggest perks of teaching at Berkeley?

L: I think so. At Berkeley we get some great students. Many are very self-motivated, which is good because I'm not much of a task-master—ask Roberta. I usually let people roll with what they want to do. I give them different options and pose different questions. So far it's worked out pretty well.

M: Do you believe in a four-field approach to anthropology?

L: Yeah, I definitely do. Some of today's socio-cultural anthropology isn't my cup of tea. But, I think that a four-field approach is really the way to go, and I think historical anthropology, which incorporates such a perspective, is really where the strength of the field is.

M: It's funny, I was always taught that a four-field approach was the way to go. But at Cal if you wanted linguistics, you had to take a course in another department. And at UVa there was no physical anthropology. Both schools were big proponents of the four-field approach, but neither staffed it. Do you think that it stretches departments too thin, or do you feel that the biological and cultural extremes are too far apart to be tenable?

L: It's tough to keep a department together. At Cal, I've seen difficulties in communication between some of our biological anthropologists and some of the more postmodern-leaning socio-culturalists.

M: But, you would have no problem having an engaged anthropological conversation with the most scientific of archaeologists—Steve Shackley—

L: *[Laughs]*

M: And, the most postmodern of archaeologists—Ian Hodder.

L: Oh yeah, that's true. But I'm in the middle; I think it's those that are on the ends that have problems. It can be difficult in a big department. You want diversity, and diversity is good. But, the problem is when you get so diverse that there are often no real research questions or problems that everyone sees as common ground; that's when it gets difficult. And that's part of the problem with anthropology departments right now, no common cohesive research questions that everybody understands and agrees as being critical.

M: Let's talk about some of the problems in archaeology right now. Do you feel that there are any problematic issues with accountability right now, for quality control in archaeology?

L: I think in CRM, boy, there are big issues going on right now, and that's where a lot of archaeology's done. And I think that there are some real issues in terms of compliance archaeology. Too much archaeology is being swept under the rug because of compromises with developers.

M: Is this part of your role as professor? Do you feel that we could do a better job at teaching people about issues in historic preservation?

L: Oh yeah, definitely. If you talk to people in CRM, they make a strong point that the academic side hasn't really picked the ball up in terms of preparing people for doing CRM work.

M: Is there anything that you feel that the field has underachieved on? What should we be doing as a field that isn't being done?

L: I think just better quality work. That's probably the critical thing in terms of all the different archaeology that's going on, and the money that is pumped into it. It just doesn't appear that we're getting all that much out of it.

M: Better quality, how?

L: Better quality in the sense of looking at interesting research kinds of questions, and then getting the results out where people—the public as well as professionals—can read it.

M: Are there any conservation or curation issues that you feel are very problematic?

L: There's a lot more here in the state of California that we could do, in terms of preserving sites in-situ. Big cemeteries are being blown away. There should be some kind of move to try to preserve them in place. Both native monitors and the archaeological consulting firms are willing—for a price—to move these sites out of the way. And I just think that that's wrong. I just can't understand how that's taking place. But it's true with other kinds of sites, as well. I see that as an issue that we haven't dealt with very well. I think we were doing a better job, probably, back in the '70s and '80s than we are now.

M: Why do you think that is?

L: I think in the '60s and '70s there was a general feeling of preservation for the common good, for our common history. Now there's very little sense of common ground and even less common history. A lot of sites are being blown away; I bet you've seen a little of that down in San Diego.

M: Yeah, between SCIC [the South Coastal Information Center] and the San Diego Gravestone Project, I see it all the time. San Diego has an unfortunate history of repeatedly yanking gravestones out of the ground. We have a few restaurants built on top of old cemeteries, and there's even one example where they pulled up a bunch of stones at one cemetery and buried them at another (Mallios and Caterino 2006, Mallios and Caterino 2007).

L: Wow.

M: Yeah, a mass grave for gravestones. What's next at Fort Ross?

L: We're working with the Kashaya and with State Parks to put in an interpretive trail that will take people out of the stockade and to the archaeology. I'm writing a proposal right now to be submitted to the tribe and to State Parks to try to get that rolling.

M: I know you've worked with the descendant communities for a while. Was that a priority for you going in?

L: Not going in, no. It just developed. Coming out of the Southwest in the '70s and '80s, there was not much work with the communities at all. It was really part of the old processual archaeology where descendant communities weren't a big part of the game. We were taught that only research-oriented stuff was really critical. Nobody worried about public education. And, descendant communities were not a big issue, because we were busy looking for cross-cultural generalizations that could be used anywhere. So, it wasn't historically contingent. And nobody wanted to change their research design or make compromises.

M: Do you feel like you've had to make many compromises?

L: Oh, yeah; all the time. But you have to, no matter what you do. We do a lot more up-front surface investigations before we do any kind of real subsurface testing. We want to obtain information about site structure before implementing sub-surface investigations, so we can bring all of the players to the table.

M: That's an ironic union, bringing together the most scientific, cutting-edge, high-tech non-invasive techniques to answer an issue that's very culturally cutting-edge, a sensitivity to indigenous stakeholders.

L: Yeah, that's where the future of archaeology is heading: working with descendent communities. They don't want to see ripping and tearing; that kind of archaeology is over. No more bringing in a backhoe. You might call it a compromise, but in many ways I think it's just a better way of doing the work and also thinking a little bit more about preservation, in-situ preservation.

M: Where do you think the discipline of archaeology—specifically anthropological archaeology—is going?

L: I think you're going to see more collaboration and more public archaeologies. And I think the kinds of methodologies are going to transform to be more non-invasive. But you still need to really understand the overall spatial layout, the association of all these materials.

M: Do you consider yourself a scientist?

L: In some ways. I'm not a hard scientist, but certainly I am empirically grounded. I try to evaluate research questions in a rigorous manner to develop logical arguments. I think in many ways, that's what science is all about.

M: Do you think the pendulum's going to swing back, not only in anthropology, but in archaeology, toward being more empirical?

L: Yeah. I already see it. I think the height of the post-processual archaeology has already gone its course. I see empiricism coming back to some degree. I don't think you're going to get the hardcore law-generating hypothetico-deducto-nomothetic stuff, but you will see more of an emphasis on evaluating empirical data sets. I think it will come back; it always bounces around. And that whole particularism—how everything is so historically contingent—I think all that's going to swing back, too. Some of the issues with global warming and other environmental phenomena are going to generate money, and the money is going to drive research. And, you'll see environmental archaeology come back as well. Focus will be put on looking at certain environmental-human relationships, and if you're going to look at that long-term, then you're going to have to include archaeology.

M: Is that what you feel is archaeology's biggest contribution, its long-term perspective?

L: Yeah, the diachronic dimension.

M: Interesting. I believe that is the theme that unifies all your work: diachronic culture change. Even though your diverse body of work employs theories ranging from Cultural Ecology, Structuralism, Processual Archaeology, Practice Theory, Marxism, Feminism, and Post-Colonial Thought, all of the Lightfoot publications address long-term cultural transformations-

L: Yeah.

M: that's something that even Lightfoot, the New Archaeologist/Science guy-

L: Yeah.

M: and this more, post-colonial and historically-contingent/Humanist guy-

L: Compassionate guy-

M: *[Laughs]*

L: Sensitive but compassionate-

M: *[Laughs]*

L: but rather attractive, too.

M: *[Laughs]* C'mon. How am I going to put this in print? So, are you ever going to give up this Rodney Dangerfield act? Now that you've spoken in the same calendrical year in the SAA and SHA plenary sessions-

L: Oh, please.

M –are you *ever* going to drop the “I get no respect” mantra?

L: No, I don't think so... I *am* the Rodney Dangerfield of archaeology.

M: May he rest in peace.

L: That's right. He's my idol. No respect. I get no respect-

M: *[Laughs]* It must be those damn Hawaiian shirts.

L: Yeah.

M: Speaking of, let's end with resolution on an important issue. I've heard conflicting stories on the Hawaiian shirts you always wear.

L: Yeah?

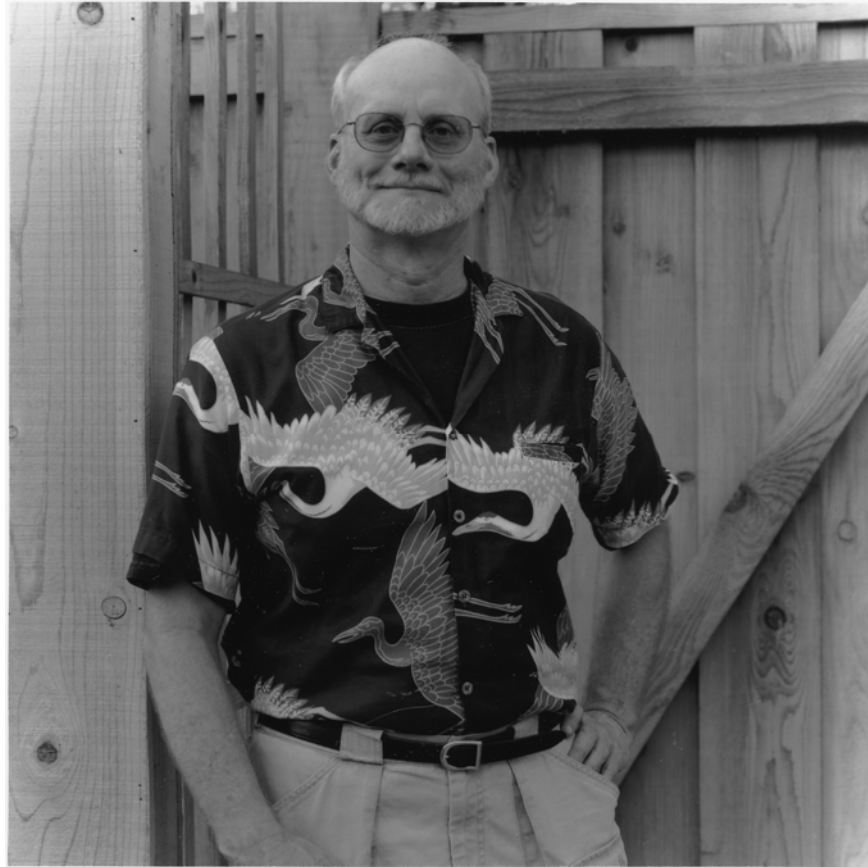


Figure 4.1. Archaeologist Kent Lightfoot. Courtesy of Therese Babineau.

M: Yeah. Version one is that you started working out in high school to become a big, buff football player, and, as a result, had to get a new wardrobe. Evidently, Hawaiian shirts were the cheapest. But, version two of the story is about how you got called in to your junior high school counselor to talk about these Hawaiian shirts you insisted on wearing.

L: Yeah, I've been wearing them since junior high, it goes way back.

M: Ah, the contingency of historical narratives. So, what is it about the Hawaiian shirts?

L: They're just so festive. You can wear them at any occasion.

M: *[Laughs]*

L: There's no occasion with a tie that you cannot go to in a Hawaiian shirt. And, yet they can be worn to the most casual affairs. They're like a reversible shirt; you can join any kind of function—twice. What's interesting about Hawaiian shirts is that the Renaissance keeps taking place, every generation. It's just amazing!

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