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RICHARD KLEIN

Richard Klein: F. Clark Howell and the Modernization of Paleoanthropology in the United States  
at Michigan, Chicago, Berkeley, and Stanford

Interviews conducted by  
Samuel J. Redman  
in 2012

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Born in Chicago 1941, currently Professor of Biology and Anthropology at Stanford University and co-chair of the Scientific Grants Committee for the Leakey Foundation, recollections of childhood visits to the Field Museum of Natural History (FMNH) in Chicago, impact and memories of museum – including especially the *Hall of Human Prehistory*, dioramas, reactions and limited memories of actual human skeletons displayed at museum, comparison to AMNH and Smithsonian, other childhood memories at the museum, attended University of Michigan for undergraduate, started training in physics and Russian, context of the Space Race and Cold War, introduced to F. Clark Howell via Frank Livingstone, description of his introduction to Howell and background information on him as future mentor, small world of physical anthropology in that era Desmond Clark at Berkeley, William (Bill) Howells at Harvard, choosing a research topic with Howell’s influence and input, Howell as teacher, Howell as a field-school and archaeological research project supervisor, fieldwork in Spain, experiences as a graduate student studying with Howell at the University of Chicago, Howell invited to Berkeley for academic year and Klein joins him as student, description of life as a student in mid-1960s at UC Berkeley, Free Speech Movement, San Francisco, meeting and befriending François Bordes in Berkeley, memories of other scholars such as Sherwood Washburn, Theodore McCown, graduate students and the personalities of faculty members, thoughts on attitudes toward Howell and other physical anthropologists (especially at the University of Chicago), issues and controversy involving the study of race, Howell’s major contributions to anthropological fieldwork methodology in Africa and Europe – including thoughts on the interdisciplinary nature of Howell’s approach, publications stemming from Howell’s work and particular approach, Garniss Curtis and Jack Everden – geologists who collaborate with Howell on the dating of prehistoric sites, the start of his work in South Africa

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Attempting to sort out pressing questions about Neanderthals and Cro-Magnons, the place of South Africa in the history of studying human evolution (since Raymond Dart’s discovery of Taung Child), his expanding work in South Africa by the mid-1980s, reactions to the political situation (namely apartheid) in South Africa, the 1950s as a major turning point for biological anthropology and human origins research, the work of the Leakey Family, the issue of casting of fossil remains for study and research, the Native American Graves Protection and Repatriation Act (NAGPRA) of 1990 and its special resonance in California, comparing Stanford and Berkeley’s different approaches to the issue of repatriation of human remains in 1980s and 1990s, viewpoints on repatriation (especially of ancient or prehistoric remains), Kennewick Man and other controversies surrounding prehistoric skeletons, considerations on F. Clark Howell’s

place in the history of paleoanthropology, and consideration of Howell's importance for his own career considered in retrospect

[End of Interview]

## Interview History

During my time as a graduate student in the Department of History at the University of California, Berkeley, I was approached by my advisor, Richard Candida Smith, to conduct an oral history interview with the noted paleoanthropologist F. Clark Howell. The interview would be my first for ROHO and while I was keenly interested in the subject matter, Howell's rapidly declining health posed a major obstacle in completing the final portion of the interview.

Howell's legacy is monumental for the fields of physical anthropology and human origins research. Serving as a faculty member first at the University of Chicago before moving to the University of California, Berkeley – Howell introduced interdisciplinary methodological approaches to archaeological fieldwork in Europe and Africa. He also worked closely with some of the most significant scientists and scholars of the era – becoming one of the first Americans to see the Piltdown Man forgery at the Natural History Museum in London and becoming close friends with the Leakey family in Africa. Howell passed away before the entirety of the oral history interview could be completed, and yet the nearly ten hours of interview reveal important themes in the recent history of anthropology. The major sessions that were unable to be completed included a session on teaching and continued conversation on the legacy of physical anthropology for modern debates surrounding human remains collections in the United States and abroad.

Ultimately, the oral history interview with F. Clark Howell, *Modernizing Physical Anthropology through Fieldwork, Science, and Collaboration*, was published in 2012 and contains a little over 100 pages of oral history transcript. It reveals valuable insights into Howell's recollection of the development of the field of biological anthropology over the course of the second half of the twentieth century.

In hopes of building upon this important interview, I worked with the Leakey Foundation to approach several of Howell's former students and colleagues. Due to constraints in funding and time – a desired series of interviews with former students and colleagues of Howell remains in the works. In September of 2012, one of Howell's most accomplished former students, Richard Klein, hosted me in his office on the campus of Stanford University for a two hour oral history interview. Richard Klein is a Professor of Anthropology and Co-Chair of the Scientific Grants Committee for the Leakey Foundation. The focus of the interview was primarily intended to be on the influence of Howell, in particular, as a mentor, teacher, and supervisor of field sites overseas. Although much of the interview remained focused on Howell and his influence, my own research interests in the history of museums, physical anthropology, and human origins research are likely clear to the reader throughout this interview. Klein, to provide an important example, spent his childhood in Chicago, where major new museum exhibitions on human prehistory and the races of mankind had opened to much fanfare only a few years before his birth. While updated several times over the ensuing years, these displays would remain on view at the Field Museum of Natural History (FMNH) for decades. My own personal interest includes the visual aesthetic of these displays and I was interested to learn how one of the world's leading experts in *Neanderthal* might recall early memories of visiting dioramas intended to visually represent moments in human prehistory.

Following these conversations about his early influences, I began asking Klein questions more firmly geared around his academic trajectory from the University of Michigan to the University of Chicago, where he began working with Howell and a group of other important mentors. Klein contextualizes some of his early decisions regarding his studies into the context of the Cold War. His choice to study Russian and physics early on, he explains, was clearly influenced by the context of the era.

In the mid-1960s, when F. Clark Howell was offered an opportunity to teach at UC Berkeley for an academic year, he agreed to sponsor Klein as a visiting graduate student at Berkeley. Klein remembers this as one of the more formative years of his intellectual life, with vivid memories of ongoing debates, scientific breakthroughs, and the personalities of the various characters involved. Indeed, I would argue that scholars who have written about the histories of these fields have to some degree overemphasized the pure intellectual standpoints of individual agents, without considering the inter-personal relationships that actually work to shape the nature of the field. These can include high-profile rivalries and differences of opinion, but also scientific collaborations and even sincere friendships.

In my view, this brief oral history substantially improves the value of our anthropology oral history series in a number of ways. Most critically, it provides additional background, context, and thoughtful, independent (and sometimes competing) viewpoints to the existing interviews in our collection. Although Klein clearly holds his older mentors, including Howell and Desmond J. Clark, in very high regard, he also maintained differing viewpoints on certain issues, understandably. While providing valuable material on F. Clark Howell as a mentor, teacher, and scientist (the primary goal of the interview) – this transcript should provide additional material of great value to scholars. Future students and scholars interested in the history of biological anthropology will encounter a rich discursive community, marked by rivalry and collaboration, and one shaped by both external and internal forces – making oral history a valuable addition to the historian's toolkit in undertaking this study.

Samuel J. Redman, Ph.D.  
Berkeley, California  
April, 2013

**Interview 1: September 25, 2012**

[Audio File 1]

Redman: All right, my name is Sam Redman, and today is September 25 and I'm on the campus of Stanford University with Richard Klein. I wonder, Professor Klein, just for the benefit of the record and the transcriber, if you wouldn't first state and spell your name for me.

01-00:00:20

Klein: Richard Klein. Last name K-L-E-I-N.

Redman: And when were you born?

01-00:00:31

Klein: April 11, 1941.

Redman: And whereabouts where you born?

01-00:00:35

Klein: Chicago.

Redman: All right. Just for the record as well, would you mind just telling me your current affiliation with both Stanford and then the Leakey Foundation.

01-00:00:45

Klein: I'm professor of biology and anthropology at Stanford, and I'm the co-chair of the scientific grants committee at the Leakey Foundation.

Redman: That's terrific. Actually, I'd like to begin our conversation in Chicago, if that's all right. I've read some previous interviews that you've done, where you talk a little bit about as a young boy, taking trips to the Field Museum of Natural History. That would've no doubt, at that time, held some really fascinating sights. I'm wondering if you could tell me a little bit more about two major exhibitions that still would've been on display when you were a young kid, namely the Hall of Prehistoric Man and the Hall of Races of Mankind. Those were particularly significant exhibitions. Do you have any memories of these exhibits?

01-00:01:34

Klein: Of course. I've never thought about the one that was supposed to display races in that kind of context; I was interested in the fossil people, the reconstructions of fossil people, Neanderthals and others. The person who was primarily responsible for making these exhibits was Henry Field, who was obviously part of the Field family. I don't remember now who actually established the Field Museum; it wasn't Henry. I think it could've been Marshall Field. Marshall Field?

Redman: Marshall, that's right. Yes, he was the department store owner.

01-00:02:01

Klein:

Oh, yes. That's still a very prominent department store in Chicago, when I was an adult, although I think it had long since been bought by someone else. Anyway, so yeah, these were typical 1950s exhibits. Glass fronts and figures inside doing things. Except, of course, they weren't mobile. Most of the other exhibits, which I also thought were interesting, were Victorian sorts of things, where they would have cabinets full of stone tools that you could look at. It's not the sort of thing that would attract people today. I don't know how attractive it was then, but everybody expects something much more animated, much more exciting today. Museums are having difficulty competing, as a result.

Redman:

You mentioned the dioramas, which were particularly important.

01-00:02:52

Klein:

Yes.

Redman:

And in particular, the dioramas of Neanderthal.

01-00:02:56

Klein:

I thought they were fantastic.

Redman:

So for a young kid, that stuck in your mind, that made a mark on you.

01-00:03:03

Klein:

Yes, I suspect it did on most of the kids that I would go to these exhibits with. Maybe I was a little bit more interested than they were, but you could tell; the kids hung around these things, they thought this was pretty fascinating stuff. And when I talk to kids that age today, they still do.

Redman:

What was it about it? Do you think it was the way you could visualize these past humans in a new way? Or what made that series of dioramas particularly memorable or successful?

01-00:03:36

Klein:

Well, I already knew a little bit about human evolution; they talked a little bit about it in my school. Just a little bit, because even today, grade schools, high schools, don't deal with this very much. But I was interested. Kids are just fascinated by stuff like this. Think of the dinosaurs, particularly. So you go and you see it represented three-dimensionally and it's done well, it just piques your interest that much more. It's hard to say exactly why. Do you have children?

Redman:

Not yet, no.

01-00:04:04

Klein:

Well, because my experience with kids is they hit five, six, seven, sometimes even into their early teens, fossils are really interesting to them. Now, maybe more dinosaurs than Neanderthals, but when they become aware that there

were these weird kind of people around before, they get interested in that, too. And then it passes. I don't know whether it's more a male thing than a female thing; probably is. But then you move on to other things. I forgot about it for many, many years.

Redman: So we've talked a little bit about the dioramas. Let me ask about actual human skeletons or casts of fossils that would've been on display in those early exhibitions in the 1950s. I believe up until maybe the middle of the 1960s, the Piltdown Man cast would've been on display as a genuine fossil, really until F. Clark Howell goes over to England and is able to see the originals, and some others then start to dismiss the supposed find. But at that time, it still would've been on display for many years. But perhaps even more significant was the display of Magdalenian Girl, the oldest set of human skeletal remains in North America. Did the skeletons grab you in the same way, I wonder, as the dioramas?

01-00:05:32

Klein: I don't remember them making the same impression. As a child, you're more interested in something that's more real. Bones are interesting, and I certainly was interested in bones and I can remember looking at some; and I teach about bones extensively now, so it's very much part of the business. But I don't remember anything clearly except those dioramas.

Redman: That's great. That's really interesting.

01-00:05:54

Klein: And I really looked forward to going to the museum. Have you been to the Field Museum?

Redman: Yes, yes.

01-00:05:58

Klein: Yes. Because they had lots of other stuff. In fact, I think the dioramas are gone. But they had lots of other stuff, which was very interesting, a big Egyptian thing and that sort of stuff.

Redman: I'm asking in particular about the dioramas, but what you're saying is for a kid, the whole spectrum of the natural history museum might've held similar awe and wonders.

01-00:06:21

Klein: Oh, yes. I can remember first going to the American Museum in New York. I was older, but it impressed me enormously. And then later, I think probably when I was already a graduate student, going to the US National Museum [Smithsonian Institution] and how impressed I was by that. It's just some people are into that.

Redman: What about the Hall of Races of Mankind? You'd mentioned that maybe this one didn't stand out as much in your mind. The Malvina Hoffman sculptures, of course, they're still at the museum, though displayed now under very different pretenses. They're explained in a very different way than they were between the 1930s and 1960s. Do you recall that exhibition at all?

01-00:06:58

Klein: Hardly. I remember they were bronzes. They weren't dioramas, they were statues in bronze. I have a vague memory of them, but it didn't make much of an impression on me.

Redman: So anything else to add about those early Field Museum exhibits? In particular, the Hall of Prehistoric Man, which I'm interested in, and thinking about to what extent that may have set you on a course, whether subconscious or—

01-00:07:25

Klein: It didn't really because— I used to get excited, the way kids do, every time we'd go, maybe once a year. They'd pack the kids on a bus and off you'd go. The trip itself, of course, was pretty exciting for the kids. Then the cafeteria and all that. So there're a lot of different aspects to it. If you did this sort of thing when you were a kid, you perhaps remember. Then by the time I went to high school, it was pretty much a thing of the past. When I went to college, I barely remembered it, if I ever thought about it at all. It was only when I took a course from a person who's still around and also a close friend of Clark's— Art Jelinek, now at the University of Arizona—this sort of brought the memories back. It was kind of a dull course, actually. It was a big university, University of Michigan. I may have been one of the few students in the class who really thought it was interesting. Just personal inclination. What is it? Why do some people end up doing one thing and some people end up doing another? It's kind of hard to know sometimes. I spoke to him and another person there—who I knew something about; not as a fossil person, but someone who was interested in fossils, another faculty member—about whether this was something than an adult could study, or whether it was just a thing for kids, because that's how I remembered it. Well, they were both studying it as adults, particularly Jelinek. The other guy wasn't so much. And they said yes, and I said, "Well, what is the job situation?" They got a little bit cloudy; I could see the clouds coming down. They said, "Well, jobs haven't been very good, but if you really want to do it, [it's] worth a try. Who knows? Maybe when you finish, there'll be jobs." Well, maybe turned out to be more than maybe.

Redman: This is a good opportunity then to move ahead to college. To contextualize this a little bit, I understand you started at the University of Michigan in 1958. Historians have talked a lot about the launch of Sputnik at that time. But I understand this was already an important time of transition for American science and science pedagogy, how science was taught in the United States.

This is in the context, of course, of the Cold War. I wonder how you found the experience of being a young kid studying both Russian and physics in Ann Arbor.

01-00:09:46

Klein: Well, it was all Sputnik. I don't know what I knew or what I didn't know. My parents were eager that I did something that would be meaningful. And it looked like the Russians were going to outpace us in physics, at least in the really visible part of physics that involved spaceships and exploration. So I thought I should do that, and that's what I did. Then I ran into this childhood interest again and was told that yes, you can pursue that. I remember talking to the man who was my advisor in physics and telling him I was going to go to the University of Chicago and do this other thing, and he was appalled. That's not science; that's a waste of time.

Redman: Oh, that's very interesting.

01-00:10:34

Klein: I think that if you were to go around here— Now, this is very much the Caltech of the north here. According to *The Times of London*, this is the finest university in the world. And it's not the finest university in the world because of anthropology. It's all this other stuff that of course, is why Silicon Valley is here. You'd get that same thing today.

Redman: That'll be a theme that I'll return to again. You meet, I understand, a biological anthropologist named Frank Livingstone. He's the one, I understand, who ultimately recommends that you—

01-00:11:04

Klein: Well, I asked him who I should study with if I did this, and he said, "Well, I think there's really only one good choice, and that's Clark Howell, at the University of Chicago. He mentioned a couple other people, but he thought Clark Howell was the leading figure, and he was. I didn't know that, but I took Livingstone's word for it and it turned out to be correct.

Redman: So let's talk a little bit about who Howell was at this point, at this stage in his career. He had had a pretty tremendous career trajectory. He almost had routine job offers from outside places like the Smithsonian, Yale, Michigan, that resulted in promotions at the University of Chicago, from assistant professor in 1955 to associate professor in 1959, and then to full professor, by 1962. He is publishing an enormous amount of amazing scholarship; he reads multiple languages; he's on this upward trajectory. But by the time you arrive, he has just been named a full professor. Can you tell me a little bit more about who he was when you arrived and your first impressions of him?

01-00:12:19

Klein: Well, I don't know. I went to speak to him after Livingstone and Art Jelinek, too had recommended that I go [to] Chicago. I went to speak to Clark—that

was before I left Michigan; I was from Chicago, so it was on one of the vacations, like spring vacation or something—about the possibility of coming, and he was encouraging. But when I came back the following fall, he didn't remember me. Maybe there were a lot of people who came to speak to him like that. But you have to understand what a different time it was.

Anthropology was a tiny field. Really, really tiny. Now, it's not very big today, but it was tiny then. If I'd wanted to go elsewhere, maybe there were a couple possibilities: Berkeley, although Berkeley later became much more important, after Clark and Desmond Clark moved there; maybe Harvard, a guy by the name of Bill Howells. But that was pretty much it. These people in Michigan who had recommended Clark, they didn't do this kind of thing, really. They didn't do what I wanted to do. So you go. They've told me Clark is the most prominent person. I'd read some of his things in this course that I took, and it very quickly became apparent to me, as I read more and more, that Clark was it. There weren't a whole lot of alternatives. It wasn't as if there were thirty people and Clark was the best; there was just Clark, basically.

Redman: Now, tell me a little bit more about that moment in your intellectual life, in terms of discovering your own interest in Neanderthal and modern humans, and how some of that story developed.

01-00:13:51

Klein:

Well, that was my central interest, just coming out of undergrad school, but I didn't know very much about it. In my first year at Chicago, there was some coursework, all from Clark. I thought that was interesting, but I hadn't yet perceived a problem that I could pursue for a PhD. Then Clark was going off to excavate in Spain, at a hand axe site, and he asked me whether I would like to come. He had a grant and he could pay for me to come. I was delighted and I went there for four months. There was another person. There were some other Americans, but there was one in particular, who was going to do his dissertation on this site, his PhD. I was a ways off from having to worry about dissertations, but I was looking at this and [thought], oh, that's interesting; that looks like a really good project to me; this site looks very interesting. And this other guy then decided that he didn't want to do this kind of archeology anymore. He wanted to do something totally different. Peruvian Andes, Inca or whatever. So the topic looked to be open to me. I went to Clark and I said, "What if I do it?" He obviously didn't think of me in that kind of context. He said, "No, I think you should stick with other stuff. You came here interested in Neanderthals and stuff like that; you should stick with that. What I think you should do is go off to Russia. We don't know anything about the Neanderthals in Russia, anything about the artifacts they made, because it's all in a language that few of us read." He didn't read Russian. I did. I kept pushing to do this other thing, at Ambrona, in Spain, and he kept pushing for me to work in Russia, and it was pretty clear that he was going to win. And that's what I did. He had an impression of me, now looking back on it, as not somebody he really— He thought I would be better off working with books and stuff like that. I don't know where that came from; I must've given it to

him. It wasn't mean. That's what he actually believed about me. Subsequently, I've done more fieldwork than Clark did. A lot more.

Redman: Tell me more about who Clark was as seminar leader. I'll ask again as a field site manager or field site mentor; but first, would you tell me, do you have any memories that stand out [in class]?

01-00:16:01

Klein: I didn't take any seminars from him. The only course I took from him— They had a year-long course at the University of Chicago, when you first came. All the anthropology students had to take it. It was called The Human Career. I wrote a book by the same title; it's sitting behind me here. I used that course for the title. Clark lectured in the first quarter. That was the only coursework I ever had with Clark.

Redman: What do you recall about that?

01-00:16:29

Klein: Clark was clearly not all that interested in teaching. Lectures were semi-organized. He liked to list numbers of artifacts and things like that. He wasn't into synthesizing it in a way that would excite you. So if you weren't already interested, that wasn't the course to get you interested. And there were very few students who were interested. Principally, at that time, it was me and Freeman. There'd been a couple before us, Maxine Kleindienst and Glen Cole.

Redman: Freeman was—? Sorry, what was his first name?

01-00:17:03

Klein: Leslie.

Redman: Leslie [Les] Freeman. Thank you.

01-00:17:04

Klein: He was going to work in Spain. I didn't meet him when I arrived at the University of Chicago. He was already in Spain when I went with Clark to go excavate this Ambrona site. He was going to come down there and direct the excavation of a sister site called Torralba. What I didn't know at the time is that he was also romantically involved with the daughter of another University of Chicago professor, a very prominent one by the name of Sol Tax, who was doing her PhD on the village of Torralba, which was right next door. She was a social anthropologist. We became friends. We did fieldwork together, projects together. When he finally did come back from Spain, he came and stayed with me in my apartment in Chicago for a month. It was quite an experience. And he eventually married this woman, Susan Tax. They're now living in Portland. But he and Clark were very close. First of all, they were closer in age. Les must now be seventy-eight, seventy-nine. I'm not young, but he was closer to Clark's age than to mine. Les had been in the National

Guard. He had also been a surveyor in private industry. Very smart guy. Maybe the smartest archeologist I've ever met. And he and Clark got on very well. There was a restaurant, a bar—well, you can read about it on the web, because it got famous after Obama got elected—near the University of Chicago, called Jimmy's. And Clark used to really like this place. They sold shoe-leather hamburgers. But you could get a couple beers at lunch and he thought that was good. He and Les went there a lot and sometimes I went, too.

Redman: Okay. Let me ask, how about then in fieldwork in Spain? What was that experience like? Can you tell me a little bit more about how he approached that site and maybe the activity of mentorship, if at all?

01-00:19:00

Klein: Well, he was up several levels. There were two sites. Freeman was directing the excavation at Torralba. He's a first-rate field archeologist, as good as any I've ever seen. Then there was this fellow I didn't mention; I didn't mention his name before, Tom Lynch, who was supposed to be directing the excavation in Ambrona. And then there were three American students and a bunch of Spanish students and other people. But the three American students were me; Mike Moseley, who subsequently went to Harvard; and Craig Morris, who has died now. He was from Chicago, too, but eventually went to the American Museum. And Clark for a while, we barely saw him because Lynch was in charge. Clark would show up, go away. Then Lynch decided that he was going to do something else and Clark had to take over more day-to-day direction; but he wasn't really into that. So we had a procedure that we followed at both sites, and we just did it. Clark didn't direct very much. I had a camera—he'd given me a camera, loaned me a camera—I was taking pictures. He said, "Just take as many pictures as you want. I want all the slides, and then any that I decide that I don't need, I'll give to you." I had piles of slides; I enjoyed doing it.

Redman: Yes, I'm keeping these. That's funny. So eventually, both Desmond Clark and F. Clark Howell come to Berkeley. Ultimately, you come to campus, I understand, and serve as a TA for Clark? Is that right? Can you tell me that story? Can you explain that for me?

01-00:20:48

Klein: Well, I'd been at Chicago for two years. I'd already been off working with Clark in the field and he didn't have a lot of students. Freeman was working on his PhD and was mostly in Spain. I was it. And we were very close personally. I did a lot of babysitting for them. They had a little boy, whom I've seen since. He's in his forties. They lived about, oh, I don't know, thirty miles south of the university, on the Illinois Central train line and I'd been born out there, so it was a familiar area to me. I'd take the train out and have dinner with them. If they were going out, I'd sit with the kid. So he and his wife and I became friends, although they were a little older than I am, about fifteen years older than I am. That was our relationship. And it was important

to me; I don't know how important it was to them. But then he told me, he said, "I've been offered the opportunity to visit Berkeley next year, to take the place of Ted McCown, a very well-known physical anthropologist, who's going on leave." I said, "Oh." I was thinking to myself, selfishly, that's not so good for me because you're my only person here. I have nobody else. So I thought, what the hell? So I said, "Can I come with you?" He said, well, he was thinking the same thing, and he was thinking since he was being asked to do more teaching out there than he did in Chicago, that maybe I could pick up some of the slack. Worked out wonderfully. So they came, I came. Again, piles and piles of babysitting. And I helped in his courses. I helped in a big way; I did a lot of the lecturing and all the grading.

Redman: This was around—

01-00:22:26

Klein: This is the year of the Free Speech Movement.

Redman: So 1964.

01-00:22:28

Klein: 1964, '65.

Redman: Then tell me what it was like to me, as a young kid from Chicago, in many respects, who's been in Michigan and the Midwest and Chicago, to come out to California in the midst of this dramatic moment in campus history.

01-00:22:50

Klein: Well, I was interested in that. But when I arrived at Berkeley, that was just starting. It was like Stanford. Saks Fifth Avenue was right across the street from the campus. That stuff all got trashed during the Free Speech Movement, the big glass windows and so forth. And women could walk around the campus at night, something they can't do there now. Women can still do that pretty much here. But it was an amazing place, to me. And there were a lot of students. We didn't have students with interests like mine in Chicago, but they were at Berkeley. I can name some for you that would remember Clark. But I immediately became part of that. The other thing that was different about Berkeley from Chicago, in Chicago, the students dispersed and I don't know where they went. But Chicago's a place, it's dark, it's constrained. Berkeley wasn't like that. There was the lounge in Kroeber Hall, where you could find students day or night, and you could always find somebody to talk to or somebody to go out and have a beer with, or somebody—I had a car—to go over to North Beach with. I had never experienced anything like that, so it was certainly the best of the four years I spent in graduate school. I thought it was fantastic. I was very reluctant to leave.

Redman: Oh, is that right?

01-00:23:56

Klein:

Certainly. Oh, yes. Well, I had no choice; I had to go off and do my dissertation, finish my dissertation. I had a fellowship and Berkeley was also paying me to teach. It made it possible for me to have this car and to have a really fantastic time. I lived on— You know, Euclid Avenue?

Redman:

Yes.

01-00:24:24

Klein:

Yes. About three blocks north of the campus.

Redman:

It's a great area.

01-00:24:27

Klein:

The only problem was parking. I had a hell of a time finding a place for my car, but I always managed. And subsequently, during the year, Clark had contacts everywhere and he and Desmond and invited François Bordes, very famous French archeologist, to come and make a documentary on flint working, [which] he's very good at. So Bordes arrived with his wife, also a very good archeologist, was arriving anyway, and they didn't have a place for them to stay. And Clark and Desmond weren't too worried about it. I thought they should've been a little more worried, this poor French guy and his wife, he's going to— Clark said, "Well, maybe you can find them a place." Oh, sure. So as it turned out, I was living in this small apartment building and I went and saw the manager and he said, "Well, just by chance, I do have something upstairs." Sort of like mine, not very nice. But I figured François and his wife wouldn't mind it, and he moved in there. I got to know him extremely well, because— It was a big favor Clark did for me, because this guy was the leading paleolithic archeologist of the day, French. He would come down, three o'clock in the morning and knock on my door because he'd discovered there was a grocery store, a twenty-four-hour grocery store in Berkeley. Now, there are lots probably now, but this was the only one then, and he claimed he needed matches for his pipe. He was an insomniac; he just wanted to get out. I got to know him very well; I got to know Desmond Clark very well. I'd met him before, but I really got to know him well. He was an amazingly social guy. And Sherwood Washburn was still there. He was Clark's professor. I had never experienced anything like that.

Redman:

Tell me a little bit about Sherwood Washburn's personality, if you would. Did you have any interactions with him? Or was he a little old?

01-00:26:02

Klein:

Oh, yes. Oh, yes. He was a tough one. They had a seminar, Clark and Washburn and Desmond Clark, once a week. It moved around among their houses. It was very nice, in the evenings. Again, all these students, I was very pleased to find. I don't know if you've encountered the name Vince Sarich. These really fantastic people. Adrienne Zihlman, who's now teaching or just about to retire from Santa Cruz. Really, really, really good people. So we had

the seminar. I was a little more advanced than they were, maybe by a year. They're all about my age. So they decided I should give one of the first talks in the seminar, maybe the first talk. I don't even remember what I said, something about Neanderthals, but it irritated Washburn. He said, "Who taught you physical anthropology?" Clark's sitting right there. Come on, tell him. What I said was not dumb, actually. And Clark just sat there. But I got to know Washburn fairly well. I thought he was a pretty special guy. Why ever he said that, I don't know. He was kind of acerbic and sort of like that. But it didn't have any deep meaning, and I got on with him okay. Not like I did with Desmond Clark, who became a very, very close personal friend. He was still alive when we moved to Stanford, and his wife. In the years that we all overlapped— Because Clark died five years ago, so fifteen years we overlapped in the Bay Area. We were never in their house. They were in ours. We invited them to ours. But I often would go over and see Desmond and Betty Clark. They also had a place in Marin, where we would go to see them. I just regarded them as about my closest friends.

Redman: It seems like the physical anthropology community, it's not divorced from personality. That's certainly true with other sciences. But what you're describing overall seems like a very collegial atmosphere, but there might be these moments or personalities that might, every once in a while— or ideas that might come in conflict or interpretations.

01-00:28:20

Klein: I suppose. And graduate students have pretty good antennas for this kind of thing, I think. My sense of it was everybody got on pretty well. There was a lot of socializing, mostly among the students, but also with the faculty. I wasn't taking courses, I was TA-ing. Some of the people that I TA-ed for are now pretty prominent and probably a couple of them have retired. They remember this as a very kind of golden time, too. They were undergraduates at Berkeley. It was spectacular. I don't think that'll ever return, getting that kind of critical mass of faculty together and these really top-flight students, most of whom were enormously successful through their careers. We didn't have that in Chicago. It was just Clark and then a couple students. At Berkeley, you had three or four faculty and twenty students. Clark, he wasn't really a faculty member there at that point, he was just visiting, but he made good contacts with these students. And I suppose—I never spoke with Desmond Clark or Sherry Washburn about it—but it was probably always their hope that they would bring Clark there permanently. It wasn't very long after. I think he went in 1969, that he moved to Berkeley. The connection there is that I got his job in Chicago.

Redman: Wow. Now how about Teddy McCown? You'd mentioned that he was the one that was replaced for the year that Howell visited. Did you ever have any interactions with him?

01-00:29:50

Klein:

No. He wasn't there that year. I'm not even sure I ever met him. He's famous for his study of— It turns out they're not— Well, they're partly Neanderthals, partly early modern humans, in the Mount Carmel area of Israel. But no, I didn't have anything to do with him.

Redman:

Just as an opportunity to ask an expert, those fossils took years for him to, I understand, separate from the rock that they were found in and describe, but—

01-00:30:20

Klein:

It was described, certainly.

Redman:

Okay. Those continue to be of great interest, I'd imagine, to—

01-00:30:29

Klein:

Oh, sure. Particularly since in more recent times, the last fifteen years or so, they've been well dated. McCown would've been very interested to know those dates.

Redman:

Oh, that's interesting. Okay, thank you for that. All right.

01-00:30:42

Klein:

No, it was very special. But it's been changed. It's hard to know exactly why. Tim White came quite a lot later. Before him, it was Glynn Isaac, and that was a strength. Glynn was a strong person. He moved to Harvard. Then Desmond retired, and that was dreadful. They treated him, after he retired, like he was some kind of hobo who just happened to float around the buildings. They gave him a closet to sit in, as an office. When they went to replace him, they replaced him with somebody who didn't *remotely* compare to him, either in interest or in quality. And it killed— That was it. That did it. Clark was still there, but— He must've cared at some level, but he allowed this replacement of Desmond to go ahead.

Redman:

Okay. I'm going to ask a couple more questions about the transitions of anthropology at this time; but first, let me just ask one wrap-up question. You'd talked about it really well, in terms of what the experience was like for you at Berkeley in that year. Do you care to add anything else about what that memory is like for you, to think about now, in retrospect?

01-00:32:20

Klein:

It's a shining year for me. Lots of aspects of it, because I had a car, I had friends, and I was all over San Francisco. I was in San Francisco many, many more times that year than I have been in the twenty years since I've moved here. Now, partly it was easier. There was no BART, of course, but it was still easier to get to San Francisco from Berkeley than it is from here. It took less time. I got to know San Francisco pretty well, and North Beach and all these friends that I had. I wish that I could see something like that recreated. I don't know if that'll ever happen again. This is all part, in a sense, of Lyndon Johnson's Great Society idea. Multiple centers of intellectual excellence. So

suddenly, beginning about 1964, there was vast amounts of money available for universities in general. It didn't seem to matter what the field was. I benefitted from that when I went to look for a job. University of California was, of course, growing, and Berkeley benefitted from that. That's not going to happen again, unfortunately. Berkeley now, of course, as you're probably aware, is in the doldrums. And Stanford is not. Berkeley and the rest of the University of California system is tied to the tax base; Stanford is tied to the stock market. Stanford is doing just fine.

Redman: Let me ask, then, when you arrive at the University of Chicago, anthropology at this time is, as we talked about, still essentially one field, in that it's linguistics, it's cultural anthropology, biological anthropology, and archeology. But the sort of four-field approach is breaking down, is being questioned. I wonder if you could talk a little bit about that, in the context of your first arriving at the University of Chicago.

01-00:34:17  
Klein:

Well, I wasn't aware of it right away, but there were social anthropologists at the University of Chicago—particularly a guy by the name of David Schneider—who clearly was antagonistic to what Clark represented. He and Clark were kind of friendly enemies. There were others who were on Schneider's side. It intensified enormously in the decades that followed, including after the time I became a faculty member there. I was only sort of vaguely aware of it. It was still a situation where the antagonisms were pretty superficial; it didn't impede anybody's ability to get a fellowship or anything of that sort. This guy Schneider—he was chair—[was] nice enough to me. Would make rude comments about what I was doing, to me; but they were supposed to be rude-friendly, rude-funny. I guess that's when you might've began to sense that there were ill tidings in the winds, by the seventies, that would've been superficial before then, was deep-seated. It was Chicago, it was Berkeley. If you go back to the mid-sixties, there were two places to study paleoanthropology, really—you could say Harvard, so maybe three—but Berkeley and Chicago. Neither of them have it now. At Berkeley, you have Tim White, in integrated biology, but there's nothing in anthropology. Forget about anthropology. Chicago doesn't have it in anthropology; they've got one guy, Russell Tuttle, who's probably near eighty, who hasn't retired yet. But that's it. Columbia, which was never a strong place for it, has no one anymore; they used to. Harvard's the only place that's maintained any strength in it. And this is all, in large part, a result of what happened within anthropology. Because wherever there were these antagonisms, the social anthropologists were always more numerous, and they could vote the other out. It may take a little time, but they did it. Clark encountered a whole lot of this kind of stuff at Berkeley.

Redman: Let's see. You talked about a lot of the transitions in anthropology. There's obviously another transition in physical anthropology, in terms of its emphasis in an earlier era, on racial classification, towards an increased emphasis on

prehistory and human origins. I wonder if maybe you think that maybe there were some readings of physical anthropology that were misguided, by the sociocultural anthropologists of that era, or if that's how you view some of these developments. Or do you view them as more political in nature, or if there's an intellectual origin for that?

01-00:37:55

Klein:

Well, if you want, sure, there was what would now be regarded as a racist aspect to biological anthropology, where different groups of people were kind of arranged on a ladder of less and more primitive kind of thing. That was more of a Victorian idea than anything else, and it lived on into the twentieth century; but it was pretty much gone by the time that I came to anthropology. Social anthropology, even today, however, will refer to biological anthropology as biological determinism, kind of intellectual Nazism. At Harvard, where they— I was the chair for five years, of their visiting committee, so I used to have to go there all the time and watch these disputes. The social anthropologists all but said that they thought the biological anthropologists were intellectual Nazis; and the biological anthropologists regarded the social anthropologists as creationists. The way they've resolved that now is to move biological anthropology into a totally separate department called the department of human evolutionary biology. The antagonism obviously persists, but they no longer express it at the departmental level. That's a good solution, except that they happen to have enough people to make a department. Most places, including Berkeley, which is a much bigger university even, wouldn't be able to do that. There just wouldn't be the man- and womanpower.

Redman:

So it's not a sustainable model on a national or international level, for anthropology?

01-00:39:17

Klein:

No. They say this, and they say it here—I got it when I first arrived here, from a couple social anthropologists here—about this biological determinism sort of thing. You think people are like they are for strictly biological reasons; it has nothing to do with culture. Which of course, wasn't true. I didn't think that; and they, I think, must've known I didn't think that. But it becomes an excuse for their political actions.

Redman:

I'll ask again about the development of some of your own ideas, but I want to ask about the increasing interdisciplinarity in fieldwork methods and lab science innovations in dating, and work in both Europe and Spain— Sorry, I'm thinking of Clark's work in Europe and Spain and Africa, in both Isimila and in Omo, as being in many ways, representative of those changing fieldwork methods and new dating techniques and things of that nature. Can you talk about where you think Howell fits in the transition and incorporation of—

01-00:40:22

Klein:

He *was* the transition. Clark was the person who recognized the importance of incorporating earth sciences, biological sciences, in a different sense than they had been before, into paleoanthropology. The person who also knew Clark very well, who you might speak to, is Frank Brown, who's the dean of geology. What do they call it? School of Mines, at Utah. A very close friend of mine now, too. Clark recognized that if he was going to go work in the Omo, he needed people like Frank, who was a graduate student at Berkeley at the time, to go out there and do the basic geologic work. No point in collecting these fossils unless you knew the order in which they were dated and how old they were and so forth. Everybody sort of understood that, but nobody pursued it with the eagerness that Clark did, and actually got people in the field. Butzer was another example, bringing Butzer to the field, another geologically-oriented person. But a lot of paleontologists who specialized in things like horses, carnivores. Clark got into carnivores a little bit himself. But Clark was the person who understood the need to have all these different specialists, many of them with no connection to anthropology at all, in the field with you, if you were going to make progress on these important problems—we think of as important problems—of human origins. That was him. He saw that early on. It's become a kind of model now. Other people, when they go to the field, try to do the same thing at a level; but usually they're not as committed as Clark was and they don't seem to be as capable of attracting the kind of talent that Clark attracted to his projects. Clark is the instigator of all that.

Redman:

So those ideas were really reflected in the teams that he picked to work on the sites. Do you think they were also reflected in the publications, then, that followed?

01-00:42:10

Klein:

Oh, sure. Oh, yes. They did a book with University of Chicago Press, on the Omo work. I think it was published about 1976. There's a bunch of chapters and you can see it right there. Clark would do the synthesis; he'd pull it all together. He was very good at that. He wasn't the field geologist himself; he wasn't a particularly dedicated bone morphologist, so he couldn't do— He did a little bit of [everything], but he wasn't particularly specialized. But he understood how to synthesize all this stuff and he understood why it was all important.

Redman:

So that was, in many ways, his great intellectual contribution to the field.

01-00:42:43

Klein:

In my opinion, it was the realization that you had to have all these natural science contributions to your fieldwork, if you were going— In the dating, of course. Garniss Curtis and others who worked— Do you know Garniss?

Redman:

Sure.

01-00:43:02

Klein:

He's a long retired professor at Berkeley, geological sciences. A pioneer in potassium-argon dating. I met a lot of these people because of Clark, and it was a great pleasure. When I was at Berkeley that year, there were two guys. There was Curtis and a colleague of his, a professor in geology, by the name of Jack Everden. They were fantastic people, I thought. Everden had a boat down in the Berkeley Marina, and I used to go out there with him on weekends and sail around. And I also learned about this dating. The connection was through Clark. I was just a graduate student. But Clark had made it his business to get to know these people, get to know them well, and to work with them.

Redman:

So these are the types of connections that could be developed and cultivated on a campus that could extend to a fieldwork site in Africa, and in many ways, could tie together different campuses in the future.

01-00:43:56

Klein:

Ultimately, it did tie together. When Clark was in Berkeley, in I think that year that he was just a visitor and he was thinking about going to the Omo, he went over to talk to Curtis and Everden. He knew he needed somebody to come to the field to do the basic geology, and he asked if they had a student. There wouldn't have been anybody at the University of Chicago; there just wasn't that kind of interest. But these people had it. They said, "Oh, yeah, there's this guy, Frank Brown." He's a remarkable guy. He was sort of dropped in on a parachute and walked around there for six months and did the geology, in a very remote, inhospitable place. And has continued to do that for forty, forty-five years. Fantastic guy. But Clark recognized his talents early on and involved him quickly. When Clark saw someone like that, he would provide all the resources and all the encouragement he possibly could. He was really good at that. He once told me— This was the second time when we were in Spain, in the early 1980s. By this time, I was doing something; now he'd accepted me as somebody who could do something besides deal with books, and I was studying bones at this site. There was a little problem in the museum where the bones were being kept, with getting in there on the weekends, staying through noontime and stuff like this, and I only had a couple months. So I was telling him. He said, "You going to finish?" I said, "I hope so, but it's going to be tough." He came back a day later and said, "It's all taken care of." "That's what I am now." He said, "I'm a facilitator." The work had to get done; he figured out a way to get it done.

Redman:

By that point, you were already well established as a teacher and a mentor. But I wonder if seeing those sorts of things had any influence on you, in terms of— On the other hand, Howell, based on what you've described, I don't know to what extent he offers a model for a mentor that you seem to aspire to emulate in every degree. But on the other hand, it seems like there were many aspects of which he was very successful at it—making introductions and teaching you.

01-00:46:24

Klein:

Intellectually, he was a great mentor because he didn't preach any, he just did what I thought was the right way to do projects, put projects together in what I thought was the right way, and I've try to do that myself. Didn't tell me I had to do it that way, but it was a model to follow. He was a great synthesizer of information from all these different fields, even things— He didn't do geology, he didn't do paleontology, but he would get the results and put them together in a really meaningful, thoughtful way, in these wonderful synthetic papers. I've tried to do the same. I don't think I've done that as well as he did. So he was somebody to follow, in that sense. He wasn't somebody who could teach you how to do anything—how to study bones or how to study deposits or how to study artifacts; he just wasn't into that. But he put you in contact with people who were. This guy François Bordes; I went and studied with him in Bordeaux, and learned all about stone artifacts from that, in a way that Clark never could've taught me. But because he had such a close relationship with Bordes, Bordes was happy to take me in his lab. A lot of people like that, that I probably— Desmond Clark, whom I met mainly through Clark Howell, [who] was responsible for the next forty years of my career.

Redman:

Can you tell me a little bit more about that connection, in terms of Desmond's influence on you?

01-00:47:47

Klein:

Well, Clark wanted me to work in Russia, on these Neanderthals and their artifacts and so forth. It did actually mean looking at objects, but you couldn't go look at sites. The mid-sixties, you weren't allowed out of Moscow and Leningrad and whatever. I tried to work in Yugoslavia, which was a little more open. The former Yugoslavia, the former Soviet Union. But still, it didn't work out very well. And by 1968, I didn't know what to do. It didn't look like I was going to have fieldwork opportunities, and I was sick of going to museums and universities. I remember sitting at dinner at a national meeting with Desmond Clark and telling him about this. He said, "Well, would you consider South Africa?" Now, nobody considered South Africa at the time, because of the political situation. I said, "Yeah, I guess. The politics aren't great, but if there are opportunities to research there, I probably should try it." Desmond knew everybody there, put me in contact with people and I never turned back. I've even been president of the South Africa Archeological Society. That was 2002-2004. Long time ago. He just opened doors like that. Wasn't hard for him; he was regarded even then as the grand old man of African archeology. It wasn't hard in that sense. Everybody knew him. He did have to write a letter or two and tell people that I wasn't horrible.

Redman:

With that, I'm going to change tapes.

[Audio File 2]

Redman: All right, my name is Sam Redman, and today is [September] 25<sup>th</sup> and this is my second tape with Richard Klein at Stanford University. What I wanted to ask you about a little bit is how some ideas evolved, related to Neanderthal, and the reaction and response to some of the more controversial ideas that were cropping up in this era, of the other anthropologists that we talked about, and sort of situating your ideas, as they evolved in this time. I understand that anthropologists were essentially split into two different camps on the issues of Neanderthal: those that said that Neanderthals had evolved into Cro-Magnons, and the others that said that Neanderthals were replaced by Cro-Magnon Man. Tell me about those in Chicago and at Berkeley, people like Howell and Washburn and yourself and others, how they were trying to sort this out; and in particular, with limited evidence, how one would go about trying to answer this question.

02-00:01:16  
Klein:

It wasn't really answerable until probably 1989, 1990. Again, the answers came out of Berkeley. Allan Wilson and his students, who did this mitochondrial DNA work, were the first to really convince the majority of paleoanthropologists that the Neanderthals were not ancestral to modern humans; that our ancestors lived in Africa and the Neanderthals had become extinct. I became convinced when I was in Russia, looking at these stone tools, that the Neanderthals had a different mindset that I did—I could go into that in more detail—and that the Cro-Magnons, as they were called, must have come from somewhere else. But I didn't know Africa. It could've been Africa, but we had no evidence for it at the time. Clark perfectly accepted that. Clark was probably the most forceful person, early on, to argue that the Neanderthals were physiologically adapted to the cold climate in which they lived, and that accounts for many of their peculiar features. Of course, Africans didn't live in that kind of climate. I'd argue that accounts for some of the peculiar features of modern people. Curiously, when I told him about this, when I really got into this in a big way— And it's mainly what I write about now. I write about all of human evolution, but that's my particular research. Just a few years ago, maybe seven, eight years ago, when I mentioned to him, "You were right—" He wrote these wonderful papers in the 1950s, on the Neanderthals. "You were right. It makes a big difference now." He really didn't seem to remember. He'd started out with a focus on the Neanderthals, and I think that was Washburn's influence, because Washburn thought that was important. Then he went backwards in time; first to the stage in human evolution just anterior to that, *Homo erectus*, and it all made sense. When he went to Torralba and Ambrona, he was hoping for human fossils. An elephant might've stepped on somebody; it would've been a big thing. Then he moved back further, when that didn't work out. He moved back further, into the Australopiths and really early human evolution, and sort of forgot about the Neanderthals. I think he was right to do that, in the sense that until the genetics came about and new dating methods and so forth, it wasn't a

resolvable question. I thought I knew the answer. I was right, too. But just thinking about it from an artifactual perspective, that the Neanderthals didn't seem to make a whole variety of stone tools. They made sort of three stone tools. We thought there were lots of different kinds, but there're basically only about three or four. Then you go look at the Cro-Magnon tools and there're dozens of different types and it looks like the Cro-Magnons were going to a hardware store. They weren't, of course. And the Neanderthals were sort of trying to figure out how to do things in a crude way. I thought that that meant that the Cro-Magnons had an intellectual mindset like mine and the Neanderthals didn't. It was inconceivable to me that the Cro-Magnon mindset could've evolved directly out of the Neanderthal. But that, again, was a kind of a jump, an inference. You need harder data. So much in paleoanthropology is just abstraction, it's inference, it's hypothesis—sometimes treated as fact, but isn't. Then there are things like dates and genes—and bones, when they're— of course, because they have to be dated, but those are facts. And when they're hard, then problems get resolved.

Redman: This is something, of course, that has a much longer history; and in particular, related to South Africa. I'm thinking of the Taung Child, when it's first discovered, and then how long it takes for that discovery to become known in England, how long it takes for that discovery to become known and really understood in the United States, and sort of the place of that fossil, the place of South Africa. Obviously, now we think of South Africa as an important point of human evolution; but that's because of a lot of work that's happened there since the 1920s and thirties. Can you tell me a little bit more about the history there and what you witnessed when you arrived, in terms of how that's changed?

02-00:05:17

Klein: Well, if you go back to the discovery of Taung, it's 1924, published 1925. Here you have this fantastic fossil. Raymond Dart had it in his laboratory, but it wasn't the sort of—Clark was a great one for traveling and if he'd been around, he would've tried to go see it. But it wasn't such an easy thing to do. Not like it became, even in the fifties. Even now, going to South Africa is a bear, I can tell you. It's a forty-hour trip and I find it harder and harder to deal with. But if there's a new fossil, you can go and take a look at it. It's even possible now to send three-dimensional faxes, so you can get models or casts of these things very quickly. Everybody's pretty much looked at everything. Or you can have three-dimensional representations and virtual ones on your computer. But early on, until 1950, information flow was very different than it is now. Obviously, it wasn't what it is now, but by 1950, it had changed. One of Clark's earliest papers was called "The Villafranchian and Human Origins," which was— Because he decided early on in his career he had to go off to Africa and look at fossils and talk to people and whatever, and he wrote a paper on this, which is, in my mind, still a classic. I think published in 1959, something like that. When I arrived in South Africa, it was 1969, and that was no longer much of a problem. Although still, if you wanted to make a

telephone call to the United States, you had to book it three days in advance. We didn't have a telephone in our apartment. It was still kind of primitive in many ways. But the paleoanthropology thing, other than because of Apartheid, was fairly well integrated with the rest of paleoanthropology, and most people were interested in these Australopiths—and I can name people who are still active, like Milford Wolpoff—had been to South Africa to see them. What changes Apartheid made— I was the only person—and it was nothing I'd brag about—probably the only American, the only outsider to go there consistently, until the end of Apartheid. People always comment on that. You're the only one who comes here. Other people came occasionally, but they were reluctant to come frequently; and when they did come, they tried to keep it a secret. Then of course, everything changed, 1991. Now they're piles of foreigners, and particularly Americans looking for things to do in South Africa.

Redman: What was the politics of that decision to do fieldwork there, for you in the United States, if at all?

02-00:07:53

Klein: I often wondered. I remember going once to the University of Wisconsin to lecture in Madison, while we were still living in Chicago. It was about '84, '85. There was actually a demonstration in the capitol building there. State of Wisconsin pension funds or whatever should de-invest or dis-invest, whatever that word is, in companies, other funds that invest in South Africa. Big signs. Free Nelson Mandela, all that kind of— Here I was, there to give a lecture on prehistory in South Africa and I wondered when I was going to run into trouble. Never did. Only time I ever saw anything like that or any problem was a few years later, about the time, actually, that South Africa changing. But there was a meeting in Cambridge, England and they actually forbid South Africans to attend. They would not let them attend the meeting. I could attend because I was an American. But I thought, when I found out about this afterwards, this is ridiculous. I go there voluntarily. The people you're keeping out were born there, for the most part, and don't have much choice about whether they do their work there or not. You don't just say, well, I don't like system here so I'm going to go somewhere else. You've got to make a living, right? So no, I never suffered for that. In South Africa I was, if not rewarded for it, at least congratulated for it. I never saw a problem. I guess I was lucky. There are people even now who say, "You're the only one. We really appreciate it." This is twenty years after the change.

Redman: One of the things that I want to ask you, just to clarify or just add one or two more sentences for me, you'd mentioned that international communication, by 1950s, the post-1950 world for a paleoanthropologist working abroad, versus the pre-1950 world of the way paleoanthropologists would study, it seems like that's a pretty big moment, in terms of what realistically, a scholar or a scientist working from the United States is able to do and study. Is that an accurate assumption?

02-00:10:01

Klein:

The fifties were definitely a turning point. Clark recognized that if he was going to write about these things intelligently, he had to go looking at them and talk to the people. He might've been the first American. It was more Brits than Americans who were willing to go there and look at stuff. He might've been the first American to really make an extensive tour of Africa and look at these early fossils. He always was like that. He understood the necessity of having an empirical base for whatever it was you were going to write about, and not just sitting in a chair in Chicago—or before that, he was in St. Louis—and writing things. He was great that way. The travel didn't bother him. He'd just get on an airplane and go, and sometimes for weeks or months at a time.

Redman:

How about popularly or amongst the public? Do you think that moment, and then a little later on, as the Leakey family starts to publish more and things like a cover issue in the *National Geographic* being devoted to discoveries in paleoanthropology— It seems to me also, too that that was a big turning point. I wonder if the scholars felt like that publicly, that was an important thing for the field.

02-00:11:15

Klein:

Yes, that's 1959, actually, the discovery of the first Australopith called Nutcracker Man, a skull at Olduvai Gorge. Mary Leakey found it. Clark, of course, had already been there, already knew the Leakeys. He had sort of an inside track for more information. But yes, that was an enormous turning point, for all kinds of reasons. Up to that point, there'd been very little money to do paleoanthropological research in Africa. Once the *National Geographic* got involved, they poured money into the Leakey's operation at Olduvai and elsewhere, but mostly Olduvai and Mary Leakey's operation. So that was an enormous turning point. And Mary also brought in— Independently, but she was kind of like Clark. She recognized that she had to have experts in other things. Dick Hay—who was a professor now, first a Berkeley and then Illinois, and died a couple years ago—but to do the geology at Olduvai Gorge. They became very close and his work there was invaluable, in conjunction with hers. She recognized that she had to have people like that. She may well have gotten to Dick Hay through Clark, I don't know. By that point, Clark was closely connected to Berkeley; Dick was a very prominent geologist at Berkeley. The other I was talking about were actually his students. So Berkeley was such a tremendous center for this kind of stuff. But yes, 1959 was a real, real, a big, big turning point.

Redman:

I'm almost done with my questions. Let me ask about casts and casting technology and making copies of fossils. That, to me, too it seems like prior to World War II, that there's a lot of conversation in museums and universities in the United States about the quality of casts that are available to them. Presumably with technology advancing, this would actually make a pretty big difference for scholars working out of the United States, to be able to see

fossils that they might not be able to get to [in person]. Can you tell me a little bit about that or what you know of that?

02-00:13:31

Klein:

Well, obviously, casts today are made in epoxy. The procedure's actually fairly sophisticated, fairly technical. The person I know who does it best is Janet Monge, at University of Pennsylvania Museum; but there are others who do it—Yoel Rak in Tel Aviv, and there used to be somebody in Paris who made very nice casts. And they're very high quality today. Back before 1950, they were these plaster things. And the molds that you made made a big difference. Of course, as you make molds, even today, you have to make sure not to damage the originals. But if they're plaster— We've got some. The Beijing Man cast. The Beijing Man fossils were lost in 1941. December 7<sup>th</sup>, 1941, to be exact. And fortunately, the person who'd been studying them made a series of plaster casts, and we were fortunate to have some of those here. But you look at those and the quality is terrible. They were very good for the day. His name was Franz Weidereich, also university of Chicago professor. But casting today— And it's changing. We're not going to have casts, at least not like those. You won't need them. Well, this is what I hope, because it's now possible to digitize something three-dimensionally, and there's no reason to have a cast. I can show you things on my [computer] screen. You can twist them, you can turn them, you can enlarge them in size, you can zoom in—you can do all kinds of things that you can't do with a cast. You can measure them right on the screen. If people in my field were a little bit more generous with the materials that they have, everybody could have a complete collection in their computer, of all the fossils that there were, and could do anything in the way of analysis they might want to do.

Redman:

I'd like to ask you a final wrap-up question about Clark in a moment. But before I do that, let me ask about museum collections in the United States, of modern humans, as it's an interest of mine. I'm wondering if maybe you could just tell me a word on what your sense is of the transitions for physical anthropology, not just in the university setting, but also at places like the American Museum of Natural History, the Smithsonian, the Field Museum and the Hearst Museum here in California. I've just named several prominent collections, but you could add to that, University of Pennsylvania, Kentucky—numerous other collections—Howard University. But I'd like to hear your thoughts on those types of materials in the United States and what the story is there.

02-00:16:24

Klein:

Well, there's an obvious tension between the people who want to study them and the people who consider them their ancestors or consider studying them by itself kind of sacrilegious. Stanford gave back all its Native American material, at least all that could be identified positively as Native American, back in the 1980s. Even before that—

Redman: Now, this is prior to—

02-00:16:41

Klein: This is prior to NAGPRA.

Redman: Prior to the Native American Graves Protection and Repatriation, actually.

02-00:16:43

Klein: Yes. But we still have some skeletal material here, some of which is probably of Native Americans from Northern California. And more—not a lot, but more—that's from outside the country, and that becomes a problem. NAGPRA does not cover Peruvian skeletons. As you may know, there's a big collection of Peruvian skeletons in Berkeley. Tim White has been fighting and fighting and fighting to make sure it stays there. I think he'll probably win, unless there's some kind of international law that I can't imagine, that requires that things like this be returned. I don't study stuff like that, so— Actually, what I study is animal bones out of ancient sites, and there's not much controversy about that. But if you're a scientist involved in studying human remains, you obviously don't want to see these things sequestered somewhere where you can't get at them. Or worse yet, reburied. It's a much bigger problem, from a scientific point of view, in Australia than it is here, because there are aboriginal groups that are absolutely adamant about reburial. Here, it varies. The politics get complicated because the people around here— The Yakima, for example. If you get some human remains from a place they think that they can claim they once inhabited, and they can claim those human remains are Yakima Indians, it's a land claim, basically. I've been involved in it only a little bit since I've come here. I think there are two sides, as always.

Redman: Now, this is an unconventional interpretation, but I want to test it out on you a little bit, if that's all right. The conventional wisdom is that NAGPRA resulted in sort of the streamlining of a system for repatriation of culturally identifiable human remains in museums in the United States. But I think the story from Stanford and Berkeley show[s] somewhat of a different story, in that actually, NAGPRA, having the federal legislation in place, may have actually impeded the return of human remains of Native Americans, when you look at a place like Stanford, that was— Prior to the law, they were able to legally just outright return human remains. That's not a conventional interpretation, but I wonder if you think that that holds any water.

02-00:19:16

Klein: There's California NAGPRA, too. So when sites are dug around here, and Stanford campus is huge and there're sites dug all the time, somebody—and I've often been the point person—has to go through the skeletal material—it's mostly animal bones—but to pull out the occasional human ones. That's the law. Then they have to be put somewhere separately. I don't know what to say about things like this. If it were my ancestors that somebody were digging up and putting in a museum somewhere, maybe I'd be upset. The thing is that for the most part, we're dealing with people who, when we do the fossils, or the

sub-fossils, their connections to any living people are pretty tenuous. And I have often seen it as more a matter of the politics than of any deep-seated feelings about treating human remains one way or another. I do things here. When I teach human bones— And the bones that I teach with are mainly from indigent people who died in Stanford Hospital, when it was still in San Francisco, 1959. Some of them have names attached. I don't quite understand how the anatomy department got those bones, but it must've been with permission. But no drinking, no eating in the rooms where they're kept. The blinds are kept closed and we try and give them some degree of respect. So I'm for that. If somebody then came to me and said, no, sorry, you can't teach this anymore; we're taking the bones away, you're going to have to teach human osteology from a virtual perspective, I wouldn't be too happy about that. I don't think my students would be either, because they all think of it as one of the things that sort of prepares them to become a medical examiner, and they want to see bones. I don't know. This is not an easy thing to work out.

Redman: Do you differentiate at all, or have maybe even stronger feelings about some of the earliest sets of human remains discovered in the Americas? I know Kennewick Man was a major flashpoint for this. I wonder if, given the context of the types of skeletal remains or fossils that you study chronologically, if that makes a difference at all, in terms of— But you also teach human osteology, so I wonder if that's not a differentiation you make.

02-00:22:15

Klein: I don't know. I was digging a 300,000-year-old hand axe site in South Africa about ten years ago, and there was a young woman working with me who in South Africa, was called colored, being a person of mixed descent. Mixed descent can mean a lot of different things. A lot of the genes there came from Indonesia, not from South Africa. Anyway, I was talking about how much I wanted to find human remains at this site, because I thought this would be a real brass ring kind of thing. We never did; we dug there for years and we never got any. But one day she said, "If you find human remains, you're going to have to rebury them. Those are my ancestors." I said, "Cecilene, they're also my ancestors. 300,000 years, drawing a line of descent is pretty difficult. And if modern humans originated in Africa, as we now believe, and spread from there, that person's as likely to be my ancestor as yours." It actually satisfied her. At least she stopped complaining about me talking about this all the time. But obviously, the further back you go in time, the weaker the connections to anybody alive today. Kennewick is a very special kind of case. When Kennewick was first found, it was thought to be European, maybe. It was only when it was dated that Jim— What's his name?

Redman: [James C. Chatters], is it?

02-00:23:32

Klein:

Ah, gee, I forgot his name. [Interviewer also forgot Chatter's name]. I've got his book over there. But anyway, the guy who first made a scientific thing out of it, realized that it dated to about 9600 years ago, it's unlikely to be closely related to anybody in the area today; and it doesn't look like a Native American. It looks more like an Ainu or a Polynesian or something. Everybody who lived in the Americas before about 8,000 years ago looked like something else. You get people who look like Native Americans only after that. Presumably, there were multiple migrations, something of that sort. But with something like Kennewick, which not only has no biological affiliation with anybody alive today, no cultural affiliation, if there were artifacts, we don't know about them; it wasn't found in that kind of context. I think the courts ultimately made the right decision. It's now, if you've got credentials, you can go to the museum at the University of Washington and study it. But it was sequestered for a while. The Army Corps of Engineers, they wanted to be very politically correct about this.

Redman:

The final question I have today is maybe even the hardest. I wonder if you could summarize for me, by way of thinking about F. Clark Howell's place in the field of paleoanthropology and what he meant for your career.

02-00:24:47

Klein:

Well, yes. To me, he was the great intellectual leader of what I think of as the multidimensional, multidisciplinary field of paleoanthropology. He was the person who defined it that way. I've tried very hard in my career to follow that. I am a vertebrate paleoanthropologist, not an archaeologist. So I took up one of the subfields that he felt was important to all this, and he certainly inspired me to do that. What has disappointed me is how little influence he seems to have had beyond a handful of people, of whom immodestly, I may be the most prominent one. So most of my colleagues, they pay lip service to the idea that all this stuff is important, and if they write a grant proposal, they'll certainly have somebody in there to do the fossils and whatever; but if it doesn't get done, it doesn't get done; it doesn't matter that much.

Redman:

Do you have anything else that you'd like to add? We've talked about a number of great topics today, starting actually in the halls of a museum in your childhood, at the Field Museum, all the way through your intellectual development and meeting Howell. Is there anything else you'd like to add?

02-00:26:06

Klein:

Not really. Clark did so much for me. Not just intellectually, but because Clark was part of a group of people, he made connections. I don't think it was done calculatingly; he thought it was important to know these people if you were going to write productively about human evolution. So there was Desmond early on, the Leakeys, this man François Bordes at the University of Bordeaux, Hal Movius at Harvard. Clark knew all these people intimately, and he didn't know them because he thought they would help him in his career. Sometimes they probably did, but that wasn't the main reason. He thought that

they had information to offer that he should have. So I got access to that information, too; but more than that, I got access to the people, and it made an enormous difference. My PhD committee was Clark Howell, Desmond Clark, François Bordes, and Hallam Movius, Jr., at Harvard. Those four people were the four most prominent people in paleoanthropology at the time. I don't know there's anybody else who could claim a committee like that. It wasn't because they were particularly interested in me, but because Clark was the Nexus there.

Redman: With that, I'd like to thank you so much for sitting down with me. Thank you.

02-00:27:18

Klein: Yes. My pleasure.

[End of Interview]