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F. CLARK HOWELL

Modernizing Physical Anthropology through Fieldwork, Science, and Collaboration

Interviews conducted by
Samuel J. Redman
in 2007

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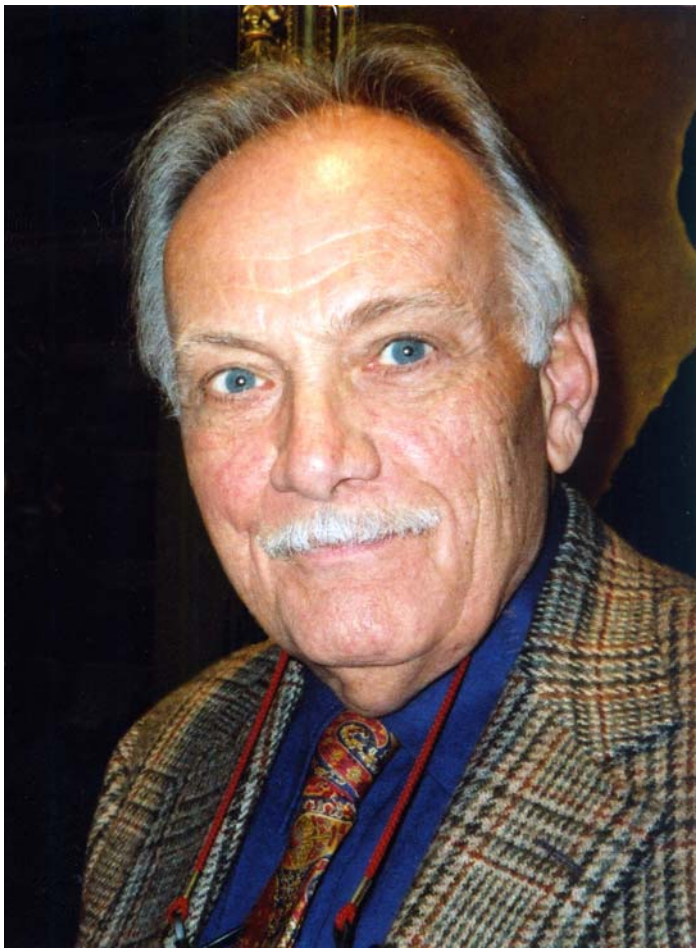

F. Clark Howell

Interview History

I learned about Francis Clark Howell (more commonly F. Clark Howell) at some point during my training as an undergraduate, possibly in my first physical anthropology textbook. Howell's name is so frequently cited that it was easy to become familiar with his career, even as a student at the introductory level. As an undergraduate, I studied both history and anthropology – interests that eventually merged to inform my current interest in the history of collecting and studying human remains at museums in the United States. Before arriving at graduate school at U.C. Berkeley, I learned about the field of physical anthropology in bits and pieces from both my various experiences as an undergraduate as well as my time interning with physical anthropology and archaeology collections at the Field Museum in Chicago and the Colorado Museum of History. One of my first tasks as a wide-eyed museum intern was to assist in cataloguing and re-housing a collection from an important site for prehistoric archaeology in Africa called Isimila – a site that revealed both thousands of stone tools as well as important clues about human prehistory. When working with these types of collections, the ideas and discoveries of individuals like Louis and Mary Leakey, J. Desmond Clark, and Tim White were frequent topics of conversation. As this small, cosmopolitan network of scholars interested in human history expanded into a larger, more interdisciplinary array of individuals, F. Clark Howell somehow managed to engage with nearly all of them. Howell was best known, however, for his extensive experience in the field – viewing and studying a large number of the most important sites for human prehistory firsthand. A few years before our oral history interview, as I worked with museum collections (some of them from the same sites discussed in the interview), it gradually occurred to me that I was more interested in the *history* behind these collections than in the modern science that surrounded them. I became increasingly fascinated with the textured story of human evolution and how that story had been shifted, changed, and complicated with the rise of the "New Physical Anthropology" of the post-war era. Advances in science, improved communications, and expanding professional organizations facilitated the rapid growth of the field – a series of changes detailed in this oral history.

Following my arrival at Berkeley, I began working with the Regional Oral History Office (ROHO). Late in 2006, I was asked to begin preparing for my first interview for the office, a multiple session oral history with Howell. Not only was this my first oral history interview for ROHO, I considered Howell to be something of an intellectual celebrity; the prospect of interviewing someone of his stature was intimidating. Adding to my apprehension was the fact that I had, to that date, only conducted two interviews – a pair of short interviews for an undergraduate class. Further complicating these feelings was the fact that Howell was steadily losing his battle to terminal cancer. Despite these concerns, I recognized the importance of preserving Howell's stories – and he quickly put me at ease with his ability to recall and relate stories from his life – even those about individuals he interacted with over five decades prior.

Although it was not clear to me at the start of the oral history (due to the overshadowing nature of my intimidation) the opportunity to interview Howell proved to be a deeply stimulating experience. Howell began his training in physical anthropology before the integration of genetics, molecular biology, and isometric dating techniques into the field.

Eager to know about important discoveries from Germany and France, Howell mastered numerous languages. He devoured libraries of literature, becoming known for his habit of gently checking off references from extensive bibliographies. Making his stamp as a graduate student at the University of Chicago, he briefly taught at Washington University in St. Louis. He was soon coaxed back to Chicago where he rose to full professor, several times receiving job offers at other museums and universities. Howell eventually moved to the University of California, Berkeley where he played an important role in establishing the university as a major center for studies in human evolution, a legacy that continues to this day. At Chicago and Berkeley, Howell mentored dozens of students, many of them arriving in United States from abroad.

Howell was also a frequent correspondent. He not only notes this fact in his oral history, but I have learned that evidence in support of this can be found in archives around the United States. Historians interested in his life and career will likely continue to find letters in foreign archives for years, and traces of his changing thoughts and ideas can be gleaned from the papers of several of the other major players in the field. Howell details at great length in his oral history how networks of scholars continually influenced his thinking through regular interaction at conferences, in the field, and through steady streams of published papers. Scientists like Howell were eager to learn about new finds – hoping to consider and reconsider how each new discovery might fit on the family tree of humankind.

This oral history lasted just under eight hours and now comprises over one-hundred transcribed pages. Nevertheless, this represents an unfinished product; Howell passed away before we could complete the interview. Missing from the interview is a complete exploration of what is perhaps Howell's most important legacy – his significant influence as a teacher and mentor of future scholars in the field of physical anthropology. Howell explains during the course of the interview that his passion for academic research at times overshadowed his commitments to training students and popularizing ideas about human evolution. Nevertheless, his passion for original research and fieldwork clearly proved infectious. Furthermore, his ideas about the human family tree will continue to have an influence on the field for decades.

Samuel J. Redman
Berkeley, CA
September 2011

Interview #1: February 8, 2007
Begin Audio File 1: Howell_Clark1_02-08-2007.wav

01-00:00:05
Redman: My name is Sam Redman, and I'm here today with Dr. F. Clark Howell. Today is February 8, 2007, and this is the first tape of our first day of interviewing. Dr. Howell, I was happy that you told me that your background is easy for you to talk about. Perhaps a good place to start is at the beginning, with both of your parents and how your family wound up in Kansas by the time you were born, in 1925.

01-00:00:43
Howell: My father came from Kansas. He was of a family that was a Kansas family, not terribly far from the Nebraska border. His parents were immigrants from the Eastern United States. He has family from Wales, and his mother's family from England. My father and mother met when he was a salesman, after World War I, in Northern Indiana, and he met my mother, who was the only child, and they had two brothers of an Irish Catholic immigrant family, who worked for the railroad, at a tiny little junction in Northern Indiana. The closest town is Lafayette, or the closest medium town is Fowler. It's up in the northwestern part of the state. My father met my mother when she was serving as bookkeeper and telephone operator and everything else, in this little town, in a hardware store, probably about 1920, 1921, and ultimately, returned to the Midwest, where he came from, as a salesman. Subsequently, he took over his father's farm and threw his father out, and that brought the family. After they were married in Kansas City, Missouri, I lived there the first few months of my birth and ultimately lived in Topeka, Kansas, and then moved to a farm. We spent my years, four and a half to almost twelve, was on a farm in Eastern Kansas.

01-00:02:53
Redman: So I'd like to talk about that period of time, when you were on the farm, and your interaction with the natural world. Was this, I believe, between 1929 and 1937, is that correct?

01-00:03:06
Howell: That's approximately right.

01-00:03:08
Redman: What was the meaning of this relationship for you, between you and the natural world? How did the physical and environmental world around you impact your thinking at the time, at this very young age?

01-00:03:24
Howell: Well you don't know anything other than what you experience. So for a youngster, you grew up with very big eyes and ears, or whatever of your senses are particularly favored. I was surrounded by country. This was a small farm, 160 or 180 acres, subsequently enlarged to over 500 by leasing and renting, but the setting was in a little valley, between hills, and the house was

on the edge of the hill. The barnyard was below it, and the barn was below that. There was a chicken house not far from the back of the house. The house was subsequently remodeled, because we hired a full-time carpenter, usually with a helper, just to work on the farm and turn it into something or other. I walked to school, of course, eventually, about a mile each way, and unless the weather was completely hopeless with blizzards and things, where you might be kept at home. I was surrounded by nature, and I was surrounded by the farm animals. I can remember the first times, when in fact I was permitted or encouraged to watch animal reproduction in action, so to speak. I was probably about seven maybe, something like that. I learned early on to deal with nature, outdoors. I never felt lonely or ill at ease with being along among nature, among animals and so on. That never bothered me at all, interestingly. A dark alleyway would bother me much for, for other reasons. The walking to school, the doing things that have to do with nature, the animal life. Not very much, if anything, about geology or those things, except by some aspect of form or something like color, something like that, but learning about trees and shrubbery and life.

The spring I remember very well. It was very important. The spring never failed and always flowed heavily, which we later tapped into and used that resource actually. I used to first help, and then ran my own trap line. In the winter months, there weren't that many things to catch, mostly muskrats around the water, sometimes skunks, possums even. Once I even caught a raccoon, probably because he was sub-adult and not very smart. Most raccoons don't easily come by those kind of circumstances. They keep away from those things, they're not tempted. I think all those things were—I mean, nature surrounds you in the country, as opposed to the blackboard jungle in a ghetto, and that surround is all encompassing, so much so that it's not overt, it's sort of just there, and only if some special thing evokes something, some analog or something, some color that strikes you in some way or some odor is common. Of course, you're with the wild and you're outdoors and in a farm and so on. We had a few pigs, enough that we could slaughter eight a year, and share them with the people who would help with the slaughtering, and to put them up also in a cold room that we had in the basement of our house. I participated sometimes in that, because you could stay up all night long at that time of butchering and rendering and all those kinds of things.

There were state and local fairs we went to once or twice in those years. The whole thing is about animal life, domestic animals, farming, crops, a little bit machinery then. Tractors were scarcely around when I was five or six or seven or something like that. If so, they were old or they were steam or they were still primitive tractors. So it was an animal life; horses, mules, and then of course cattle, because we had cattle for milking essentially, although we butchered one steer a year, just as we did the pigs and so on, so you'd be able to eat "fresh" or almost fresh meat. We'd smoke meat also, hams and so on. We had a smoke house that my father made out of a different kind of a little house, which was interesting. So the whole world was just a different world.

When I left the country, the country of course, never left me. When I left the country and entered a town, you have to remember that Topeka was hardly bigger than a town. You'd scarcely call it a city. It was a little city, but it was really a big town. I don't know how many people were in Topeka in those years, thirty five, forty five, forty thousand. Maybe that's too many, I don't know. It was different because the streets were paved, first of all. No more paths through the woods and those kinds of things. There were sidewalks, gutters, curbstones, driveways; all the things that are mostly missing in a normal farm. I'm not talking about some big fancy estate, but those kind of situations. The first thing I learned when I got into town, is that I had a terrible problem in school. I could out-read anybody and so on, but I had no skills whatsoever in drawing or arty kinds of things, and it was a real hit for me. I almost wept on several occasions because I felt so helpless. I always felt very positive, positive thinking and all those kinds of things, not helpless, so helpless really brings you down very quickly, just like a morass. It was a real transformation for me, that brief period that we lived off the farm, in a little rented house in South Topeka, Kansas, not far from a little college called Washburn College. I'll bet it's some kind of Protestant, Baptist or Methodist affiliated, small college, like a junior college essentially, and it was only two blocks from our house or something like that, which was very nice, and I lived in a very different world now.

01-00:11:14
Redman: Let me ask kind of these, the reflections in nature. My follow up question was going to be whether or not this is something that you have reflected on over time; your relationship with the natural world on the farm, but from the sounds of it, your parents wanted you to engage with the natural world. Thinking about things like animal reproduction, your parents encouraged that type of interaction, is that correct?

01-00:11:43
Howell: They didn't discourage it.

01-00:11:45
Redman: OK.

01-00:11:46
Howell: I think it just happened on a certain occasion, and I was present, and instead of saying go off and mind your business while we breed this mare and this stud or this bull and this heifer, those things were—my mother really did like the farm. You can't say she was a city girl or a town girl. She came from someplace that maybe had 75 people in it or less than 100 anyway. I mean, a tiny little place. So she got out of there in one sense, because she ended up, for several years, in Kansas City, Missouri, which was then not a small place. My father, who was there, was of course a participant in these things, and it was natural that I would have these interests. The interests were never theorized, I should say. They were mostly observational and empirical, and had sensualness about them because of odor, vision, all those kinds of things and

so on. My reading in those days, was always in history, escape, boy stories and so on. Some of those things actually involved the outdoors, in the sense that I read, later in this period after other ones, which would be like *Tom Swift and the Rover Boys*, and those kind of things. *Hardy Boys*. Subsequently, I read some interesting stories about frontiersmen like Davy Crockett and other kinds of people. These were schematized and so on, and they had a lot to do with Indians in that particular part of the world, which was essentially the southeast, from Ohio south and east, into the Alleghenies, and which was about those kinds of things. I was very turned on by those things.

01-00:14:13
Redman: Were you reading a lot at this point, when you were on the farm?

01-00:14:20
Howell: I read incessantly. My mother, she lost apparently, the first baby, and then I was the number one child and the only boy. I had two sisters later. She must have, and she has mentioned it to me, so it must be true. She read to me a lot, probably when I was infantile. The only time I might remember, if something was to stick in my mind that she actually did so, as opposed to almost surely did so, would have been when I was really ill. Often, when you have a fever, you don't remember what you're supposed to remember anyway, but she did read to me, and I learned to read very early. I was reading in—whatever that means in modern [perilogue?] terms, you know, better than Tom hit Jane and Jane ran away, those kinds of things. I mean sentences and paragraphs and so on, I was reading by the time I was three, three and a half, and then it was much easier later. As a child that was often ill, at least annually, in what we used to call bronchitis and those kinds of things, a bronchial condition, I'd be laid up, and if I was really in trouble, I'd be laid up for a period of time because sometimes it turned into pneumonia. I had bronchitis probably every year until I got in my teens, like maybe 14 or 15. It's hard to believe, but otherwise, it could turn into pneumonia, and I had pneumonia three times before I was 12. In these instances, mostly my mother was self-taught at these kinds of things, but became a very adept, health insistent mother in this sense, and it was very important to me.

As the only boy, there's both a plus and a minus to that. The plus is that if you're first, you're presumably favored and second, if you have sisters only that goes with it, you have to be careful. You could really get into trouble, and maybe not of your own doing. So there's those kinds of things to think about. My feeling is that this encouragement was left open, it was driftless. They didn't say it was best to read about that. I discovered those things myself. I had always, a very curious mind.

01-00:17:14
Redman: Can you tell me a little bit about your one room schoolhouse. You mentioned there was a mile to and from school. And maybe a little bit about your first teacher. Was it Ruth Kannarr?

01-00:17:28
Howell: Ruth Kannarr.

01-00:17:28
Redman: Kannarr.

01-00:17:28
Howell: K-A-N-N-A-R-R, as I remember.

01-00:17:33
Redman: OK.

01-00:17:36
Howell: Well I had never thought about it. The kind of world in which we lived in those days included one room schoolhouses. Only later did consolidated schools come on line. Very often, for the initial sixth grade, it would be almost typical, at least in the west and middle west, as opposed to maybe Ohio and parts east, Pennsylvania, where probably they'd moved ahead in other ways. Otherwise, these little schoolhouses were the way station for at least the first six grades. Then the question is, would one go to one that had more than six grades, it could go to eight, and then where would you go to high school. In the country, there were no school buses, and once you reached the age where you'd be going to secondary school, you know, people would come to school on a horse, people would be dropped off. Maybe several people would be picked up and dropped off. Many people walked from various places, farther or nearer. We were about as far, I suppose, as anybody. It was a mile over the hills and up through the meadows and down, ultimately, past a place like a creek and crossing over a plank bridge, and there's this little school. It's on a corner of a plot, with some swings outside, and the school is interesting in the sense that it's in vertical rows. The teacher is up front, there's a big pot bellied stove in the back that's going full blast all winter, and you start out at the left of the school, in grade one, and move successively, from left to right, row upon row. Each year you move one time. Some people were kept behind, and you might see somebody that weighs 160 pounds, et cetera, but he was in like the fifth or fourth grade. And that doesn't work very well, so they sort of try to put him in such a position that he's in with his age group, et cetera, but in a position where he's a little more removed, if possible. It's immensely complicated to deal with the difference between people. I recognized that already, and I won several prizes for something. I remember two, so it must have been only two. One of them was a pocket knife, which pleased me immensely. It was quite different from anything I'd had up to that time. I guess I was about seven maybe. Those kinds of challenges and so on were rewarded, and those were valued and so on, but I was like anybody else with play and diversion and what do I do when I'm not working, as a kid. I did those things just like somebody in the city, except in the city, there'd be different options.

01-00:21:15
Redman: I was going to ask if you were generally well behaved, or if you got in any trouble at that age.

01-00:21:21
Howell: I got into a little trouble.

01-00:21:22
Redman: OK.

01-00:21:27
Howell: Shooting off my mouth or saying something smart, or not doing something I was supposed to do.

01-00:21:34
Redman: What interested you the most, and I don't want to say "academically," but what subjects interested you the most during your early school days?

01-00:21:46
Howell: Well I don't remember early school days, but in general, my interests were always historical, and that was partly probably due to encouragement from my father to an extent, maybe telling me stories whenever he was home, which was not so often. I was up and down pretty good at math. When I was down, I needed help, and when I was up, things were doing what they were supposed to do. I did well, in general, in math. Strangely, I never had any biological sciences in high school. I had no choice. I had physics in high school. Later, there were other things. I did a lot of formal training in shop. That started in the seventh, the eighth grade, in one school, which was a teachers training college later, and that was in Lincoln, Nebraska, actually when I was a little older. So my feeling is that these major—one couldn't say that I was more interested in this than that, or that I was focused on this, or I was a big collector of that. I did collect bird identification cards and tried to identify birds as I went through the woods and things like that. I also had a lot of chores to do. The older I got, the more chores I had to do on the farm, and the less time was I "free." I also had more weight put on me, in the sense that, you know, I had my own things to do. I had one, and then later on, I had a second bull that was my responsibility. You didn't want to do something that one of your parents would think was irresponsible. You'd get into really big trouble. This kind of life has a certain expectation and set of values kind of thing. I don't have any idea what it's like. I can't conceive what it's like any more in this kind of world, but that world wasn't too bad for me in one way, and when I subsequently was able to move into a town briefly, then into a small city, Lincoln, Nebraska, where we lived four years, I was really off—my past was fading very fast away. It doesn't mean that's [not] imprinted. I'm sure the imprint was stuck, but other things were leading me on and you're driven by your own curiosity in these new circumstances, including grade after grade in school, more options.

01-00:25:09
Redman: Would you like to talk about that a little bit; why your family left the farm in 1937, and what your family did afterwards?

01-00:25:20
Howell: Well, I just mentioned briefly that my father took over his father's commitments when we moved there in 19, just going on 29, I guess. Early '29 probably, we moved from Topeka, Kansas. He took over his father's debts, I guess. I was too little to really know or remember certain things, although they often filled me in, when I participated in something. Everybody's an adult and I was a kid, but I was there, so I'd remember that way. My father built this into a really interesting place and so on, developed it extensively with building and rebuilding, electrification, terraced plowing, which had never been done before in Nebraska at least, and to avoid erosion and all kinds of things like that, but deeper and deeper and deeper. In 1937, the bottom fell in as far as we're concerned, and the farm was auctioned. It's usually minimally a day long thing. All I remember is it was day long, and people came from all over in bits and pieces, not all at once. The farm was taken over by a bank, so it's a mortgage foreclosure, and this and that was sold. Sometimes traded and then something else sold. All the cattle, chickens. I think we had a couple of turkeys, horses, mules sold. We still didn't have a tractor. All other equipment sold, auctioned by an auctioneer, who sounds just like an auctioneer that's in your own mind. At the end of the day, we drove away and went to a little house that had nothing in it, so I guess we must have stayed in a motel, but we had a little house, that I mentioned to you, in South Topeka, and that's what happened between leaving the farm and so on. That was the end of that. I was shocked by leaving. It really hurt me a lot. I thought, at the time, I left the dog behind and that really got to me. I don't know why that had to be, still to this day, but my father said it had to me and was for the best, and I never had a desire to cross him and so on, in this regard. It was a very big shock when we moved in '37, off the farm. Then, four years later, I went to Lincoln, Nebraska. Four years later, we moved from—and that was just on the brink of World War II—Lincoln, Nebraska to Indianapolis, Indiana, where we were for two years.

01-00:29:09
Redman: So can you tell me a little bit about, in both of those places, going to school. You mentioned a little bit about the differences between say the one room schoolhouse versus going to a different school, where you were asked to study different subjects. At one point you talked about choosing a different school, and how the mechanics of that worked, but at your first school, it was a bit of a shock to you right?

01-00:29:38
Howell: Well, I think when I left the farm and got into town, I went actually into a larger building. Well not very large, but a larger building, an upper primary school, and that's where I said I had trouble with the drawing and maybe my handwriting. I don't remember all that clearly, but it didn't go very well. On

the other hand, if you performed exercises in classes, reading or Johnny, will you read that paragraph, that was easy and that was OK. I was certainly more restrained than I had been in the country. My roots were gone and I became more private, I'm sure. So I was probably shy, extremely shy.

01-00:30:33
Redman: So when you moved to another high school in Indianapolis, the high school there was a little different right?

01-00:30:41
Howell: Oh that was a wonderful high school.

01-00:30:46
Redman: Was this your junior year of high school?

01-00:30:50
Howell: I did the freshman year at one particular high school in Lincoln, and then we moved to Indianapolis. I had my sophomore year and my junior year in high school in Indianapolis. It was in what was then like a suburb, or the edge of the east side of Indianapolis, in a typical treed neighborhoods and brick houses and those kinds of things. We had a rented house there for two years. That high school was wonderful. It was new, two years old, and it was very big. Of course it was new and big, and all those things were positive. I spent two years there, sophomore and junior years. That was different. You were doing the courses that were on the way anyway. This was at a level, I would say, a little higher than it was where I was a freshman in Lincoln. The quality of the school and the neighborhood was different, but there I did some history, and instead of taking some biology or something like that, which I suppose I should regret, I'm not sure if I did, I had two years of Spanish. My mother said you can take Latin or why don't you take Latin. In those days, my father had no thoughts about foreign languages, that's for sure. I could have had—that was the onset of the war, and in that school, it would not have been possible to study German, almost for sure, so it would have been French or Spanish as modern languages. They still taught Latin.

01-00:32:37
Redman: Did you have a reason, in your own mind, why you chose Spanish over French or Latin?

01-00:32:46
Howell: I was probably lazy, if I knew enough, or maybe because of the culture, which one knew a little about. There were probably no Hispanic people in Indianapolis in those days, or if so, the absolute bare minimum. The world was very differently structured. Populations were much, much smaller and it was just a very—that was a very different universe. I enjoyed Indianapolis. I had two years of mechanical drawing there. I've forgotten his name. Christianson, I think, Scandinavian. I had a metal working class one term, and then I had things like English, history. The math would have been solid

geometry and later, advanced algebra. I don't remember what it would have been exactly.

01-00:34:04
Redman: That's fine, that's fine.

01-00:34:05
Howell: I worked when I was in school, in Indianapolis, during the school year, and in the summer. I had one summer at the Indiana Historical Society State Library, a wonderful library. It certainly had an influence in me, and my bookishness.

01-00:34:36
Redman: Can you talk a little bit about how working at the Indianapolis Historical Society really interested you in history? At that age you seem to really start to engage with history.

01-00:34:46
Howell: Well I had the history interest and somehow or other, I don't know why I went there, to find if there was a job, which I doubt, or just asking. I have no idea how that happened, but it happened, and I got the job, and so I was in this environment over a period of how ever long that was, maybe it was six weeks, eight week. I don't know how long. It was a chunk of the summer. Otherwise, I was mowing people's lawns and doing those kinds of things. When I was in high school classes, I think the last one of those years, I worked in the cafeteria. As I remember, I got free lunch and something else in exchange for that.

01-00:35:34
Redman: And now your senior year, you transfer to a new high school, is that correct?

01-00:35:37
Howell: Senior year was my third high school, because this was after the war had started. This was the year '42 or '43. '42 was the first full year for us, of World War II. I lost the thought of where I was going.

01-00:36:09
Redman: Senior year of high school.

01-00:36:11
Howell: Senior year, I was very occupied with the war. I knew that as soon as I graduated, that I was going to go. I would choose to go rather than be drafted. Everybody had to be registered at a certain age, but I decided I was going to go. Any thought of higher education beyond high school was not on the menu, because the omnipresent was the war, and this war was something that because of history and curiosity, I was very much interested in warfare in reading and so on. It probably started freshman year, but really sophomore and junior years of high school, I got very interested in these things. A lot of Civil War focus. My major focus was Civil War and post Civil War, not much pre Civil War history, and middle American history, the history of the Great Plains and those kinds of things. So that was a kind of natural for me actually.

01-00:37:27
Redman: And during your senior year at this new high school, you explained to me earlier that you were introduced to several basic biological concepts by other students in the high school. Is that correct?

01-00:37:42
Howell: That's partly correct. I had an association in Indianapolis with a very smart neighbor, older than I by a half a year, and he had an impact on me in terms of science.

01-00:37:57
Redman: Do you remember his name?

01-00:37:58
Howell: Ralph something.

01-00:37:59
Redman: OK.

01-00:38:01
Howell: That's as much as I remember, Ralph *something*. But again, my whole preoccupation, certainly much of the sophomore year and of course the final year. The final year was in this little town of 11,000 people, Beaver Dam, Wisconsin. It's not far from Madison, and that's where the factory my father worked for and traveled for recalled him, and they made him personnel manager during those years of World War II. He was able to sell, as personnel manager, the people who often spoke only German at work, and others, to buy bonds, that is to make this what we used to call E for efficiency company, and B for bonds, and that they won awards because of this, everybody contributing. Be everybody in an institution that's only contributing to Red Cross or Salvation Army or something like that. That was my last year of high school and I struggled with physics, as I remember. The other things were boring. I had a wonderful ancient history course from a teacher who had been to the University of Chicago for a masters, named Robert. He was a Quaker—I didn't know what a Quaker was—and very attentive. We had a good ancient history text. I discovered suddenly, there was a lot of history before the Civil War and the Revolutionary War and so on, and I was just—I had missed lots of boats, and I still have missed boats way back in the past. I mean, I don't know very much about Greece except superficially, big chunks of the early middle ages were just no, no, no. They were never induced. It doesn't mean you can't learn about all these things without having them force fed down you and so on.

01-00:40:28
Redman: And now you're working with this high school ancient history teacher. That was the first that you'd really thought of the University of Chicago.

01-00:40:36
Howell: I've got his name. His name was Bowers.

01-00:40:39
Redman: Robert Bowers.

01-00:40:40
Howell: Robert Bowers.

01-00:40:42
Redman: And he first got you thinking about it, but eventually, you were offered a scholarship before you left.

01-00:40:52
Howell: Yes. They gave me a scholarship to college.

01-00:40:54
Redman: To the University of Chicago specifically?

01-00:40:56
Howell: And for the University of Chicago. One year tuition, $300, and I'm sure that when they made the decision who will be awarded for this, who for that, that he probably supported me, as maybe did another man, who was both a minister and a teacher, of a different kind of history.

01-00:41:18
Redman: Now with the war kind of looming in your mind, and knowing that you were ready to enlist, how did that make you feel? As soon as you won this scholarship, were you concerned at all that that wouldn't work out, or did you know right away that you could postpone that?

01-00:41:42
Howell: Yes. I found out soon, I guess, and I figured, Well that's down the road, and you're activated by getting in touch with them, and they'll look it up and they say yes, you have a scholarship of this amount. All I thought about was getting out of high school, graduating. I spent two weeks with three people, up on one of those family lake cottage, to one of the many numerous lakes in Northern Wisconsin. Isolated, drive in off the road and so on. So we batched and went out and ate, and fished and told lies and whatever it was when we were out of school. Everyone was going to a different place. They all survived World War II. One became a doctor, an MD, others in business or something like that, but we had a great—I'd never been to anything like that. I learned how to paddle a canoe and things that I hadn't done before in this regard. When I came back, I went to work at a factory, different parts of it as I progressively said I'd rather do this not that, that not this, and so on. My father said you know, hire him until he goes into the military. Nobody's going to turn anybody away, you know, there is a kind of paternalism in the factory. My father worked for them for over 20 years, and so that was going to be OK. After that, as far as I was concerned, my world was consumed by the military, and that was including from the end of '43, when I went in, until I got out in May of '46. To a limited extent, I didn't do anything that had to do deeply with science. I read a lot of paperback books of detectives and things like that in the Navy. I was on a Norwegian ship, along with two other people, another

signalman and a radioman, and we added to the communication staff of this Norwegian ship, which worked with our fifth fleet.

01-00:44:18
Redman: Which was in the Pacific Theater.

01-00:44:20
Howell: Pacific Theater. Central and South Pacific, yes. I was seriously devoted to what I did and learning about it. Historically, I knew about the World War I Navy and so on. I got involved with a game that was depicted, of a well known designer and so on. There was a big article about it in *Life Magazine*. I got into the hobby, this as a naval game, about naval warfare, and it follows rules and so on. This is pre computers and all that, but you'd say you do this and you have two destroyers and three destroyer escorts, and I have one tanker and, you know, these different things. I made ships actually. I'd done shop since I was a little boy and encouraged by my father, so I could do something or other. Eventually, I had a lot of power tools and other things that I was involved working with, so I could do those kinds of things.

First of all, after the war ended, it didn't end for many of us with its end, because I was there. It ended in September, '45, and I was in the Navy until darn near the end of May, '46. I had to put in my time to get the points and so on, but what do you do when there's no war to fight any more? It was a very funny feeling, and I spent that time, after I went home briefly, I guess two weeks in the winter, I guess. I went home by train, from Los Angeles to Chicago, and then to Beaver Dam. When I went back, I had some weeks in San Francisco. I don't know how many, moths, six weeks, or whatever. I could do whatever I wanted. I could be gone overnight if I wished. I was at a receiving station down at Schumacher. First of all, that's San Diego, and then I moved up. I read a little science in the last of the war years, because I found a wonderful bookstore in San Francisco; John Howell Books, just off of Union Square, which was there until not too many years ago. I didn't really know about such things as bookstores and so on, and I learned a lot from that experience of browsing through areas. He had things related to anthropology, some things related to evolution and biology, and things like that.

01-00:47:21
Redman: Were those the types of things you were reading specifically?

01-00:47:23
Howell: Well I actually read then, that year, a copy of Julian Huxley's, *Evolution of Modern Synthesis*. I didn't really know what I was reading. I was kind of untrained. I went right out of high school to the military, but I got plenty out of there, and I still have that copy I believe, in my library at school. I read other things, you know, that I might come across. I had already read, in the end of high school, *Men of the Old Stone Age*, by Henry Fairfield Osborn, which was one thing. Later on I'd find lots of other things, but in many instances, things weren't "available" unless you happen to be in a big city like

New York, possibly Chicago. Boston maybe, but I doubt even Boston. I was shipping always out of San Francisco or out of San Pedro, Long Island, in Los Angeles Basin, and later I decommissioned a ship in Portland, Oregon, a landing ship, and then I was sent home to be discharged. But I was still of the Navy. I didn't have any thought except what will happen with this and so on. Will we have to move from Wisconsin to North Carolina, where my father has decided to put the family, as he moves into this new post world, back on the road again selling thing that he was unable to do during the war years. I had no ides what it was going to be like for me. I figured I'd activate my scholarship.

01-00:49:26
Redman: Could I ask just briefly, before we end the tape here, why you chose the Navy as opposed to the Army or the Army Air Corps.

01-00:49:39
Howell: Well, I was very interested in flying and all those kinds of things in the '30s, so I certainly had an interest in that, more than in boats and things like that, but that came in high school because of reading. I had a couple of major works. I ran across and think that I later got copies of Alfred Mahan about naval warfare and naval history. A famous guy, and I sort of got into that. I just drifted into that, but these interests in what was going to be early man and so on, was, I would say, a pre history of later on, anthropology. These came at the end of high school, these enter at the very end of high school, simultaneous with my graduation from high school, a carry over from my junior year in Indianapolis, added to by my senior year in high school. It was bringing me towards history but also perhaps, into the history that involved science, not the history that is narrative or whatever. That's sort of where I was, when suddenly I come flapping out of the Navy, make the move with my family and help them as a boy back from the war, a guy, and get them settled into a house. Eventually, I get in touch with the University of Chicago, maybe Wayland at the same time, Academy, and we activate my fellowship.

01-00:51:31
Redman: All right, sounds good. We'll stop the first tape there, take a break and switch.

01-00:51:37
Howell: All right.

Begin Audio File 2: Howell_Clark2_02-08-2007.wav

02-00:00:08
Redman: My name is Sam Redman, and I am here with Dr. F. Clark Howell, and this is our second tape on February 8, 2007. Where we left off, you had—you were kind of in limbo with the Navy, but you knew that your term was coming to an end. You're compiling points so that you can finish up and get your discharge papers. You've now called the University of Chicago, to see if your scholarship will carry over, and it turns out it will. So at this point, you're

thinking about attending the University of Chicago, but if I'm not mistaken, you also thought a little bit about Duke and University of North Carolina, or was that your father really pushing you in that direction?

02-00:01:00
Howell: Yes. My father thought well you've been gone and so on, and we moved here. There's universities here. If you want to go to a university, go here. That came up sort of after we got the house settled and everything and I said no, well I had this fellowship, and I knew the University of Chicago had some things that would be positive in terms of my insipient, purported interests, matters of evolution and biology and paleobiology, paleontology and those kinds of things, which is what I was sure I wanted to do. I thought I wanted to do it within the umbrella afforded by the anthropology department rather than the biology department. I mean that's what I thought about, but I didn't know very much. I had been out of high school for some years and away from this sort of thing, and never really immersed in a university setting or circumstance, except to observe. I did have some in my last year of high school, when I went sometimes on a Saturday, to Madison, Wisconsin, twenty miles from this little town of Beaver Dam, where I spent my senior year in high school. I'd wander around town and I'd see bookstores and go through the university, finding buildings that were open that I could see classrooms and office, and all the things I knew nothing about then. I was determined to go Chicago, because I wasn't going to put instead, Harvard or Columbia or Yale or anything, because I figured those kinds of places were not in my world anyway. The main thing I did have one year at Chicago.

Now I was due for the GI Bill, and the GI Bill guaranteed me money, tuition and a check each month for so long. I've forgotten how long, something like 30 months. It must have been longer. Anyway, later on I had help at the university that had to do with money and things like that. I had the greatest opportunity at Chicago, as I talked about in our first informal meeting, of just being like I was turned loose to do almost anything I wanted to do. I had to do certain things within a structure and then otherwise, the structure was quite loose. And it was great because I went over and did many things in the natural sciences that I couldn't do or wouldn't normally do under social sciences, which is what anthropology was, but I was able to do that. People encouraged me, and not always to the extent I wanted, but mostly it was encouragement or just that's a natural course that you should take anyway, and I did that, but I couldn't get back into the next year class, which would have been '46/'47, because it was full by the time I got in touch with Chicago. They fortunately started a mid year class in January, and so I went there in January, a very cold January. I remember my father letting me off. We had driven from North Carolina, and that was my introduction to the University of Chicago. I marched through that university from January, 1947 until June, 1953, when I was ultimately sent a doctorate. I wasn't present for my PhD graduation, but I got a Bachelor of Philosophy and an MA previously from there, and you do the thesis for the MA degree.

02-00:05:36
Redman: Being there in Hyde Park, you're situated between the real south side of the Chicago and the loop to the north, in the real center of the city. What parts of the city engaged you most, if at all, or did you spend most of your time on campus? Talk a little bit about your experience in the city itself.

02-00:06:08
Howell: Right. Well, I'd never been to Chicago before, in a real sense. I was discharged there, through there, up at Great Lakes, but I was never really in Chicago in a proper sense. When I went in the Navy, I went the other way around. For the first year, I think maybe two years, I lived in a dorm, sharing a room with somebody or somebody else, several instances. I went to all my classes, which were across the midway, at the university. I interacted with ex GIs, that is of my age or older, who were on my floor or in my building, or I met in class in some instances. I had an interesting, in one way, roommate, Dwight Parker, for two years. God knows how we stood each other. He did nothing but study incessantly and in depth and so on, normally in his underwear, because he was always hot. Anyway, he was interested in various things that one could share, certain kind of popular—he was going to be a lawyer, which he did become, in Kansas City. He was into certain pop culture. He was a well educated, literate guy, interested in the radio and people who—disc jockeys, who played records and commented, and woke you up in the morning, and those are the kinds of things we shared. He never dated or anything like that. I dated, if I could find anybody, and I repeatedly did, mostly for short intervals. Students, students ahead of me, not—or comparable to me, sometimes older. One particularly woman, I went repeatedly with to music and things like that. Otherwise, I went to movies, told stories with colleagues who had been in the military, although none of them had been in the Navy; they were all Army. I did this for two years, as I picked up the missing parts of what I'd need for a bachelors degree from the credits I got—I used to go for this, I used to go for that, and placements tests. We had three or four days of placement tests to tell where we were going to fit into this system with our education, and everyone would be different because their backgrounds are different. If we had all finished high school, which was the case normally, you know some had a year of college, some now, and they came from very different backgrounds. They came from deprived situations, lower class working people, pastoral farming people, still before there was a mass exodus to cities for jobs. I think that Chicago was then a much smaller town. It developed during the Navy really, but it started previous to that, surely in my younger period. I was interested in music and songs and things, probably country and things like that, when I was a boy on the farm, including some folk music. Later in the city, hearing what was on the radio, I had my own radio through all of high school. In fact I had one, and later I had another one that was better. These were little battery radios, or run them of the mains by plugging them in. My father would get something like this for me. Often I'd pay or he'd pay, so I'd have my own programs that I'd listen to when I'd come home from school. Music became very important. Swing, of course, was

still around, and I learned about the past again; the history side comes up. I ultimately got involved with ragtime and traditional jazz from New Orleans, Chicago jazz from Chicago, all these places, and I followed that throughout my college. I did a minimum out in World War II, because there was Lu Watters, Yerba Buena Band, was in San Francisco and I heard them a couple times, I guess, on some little alley where they had a place. In L.A., I went a little bit, maybe a couple of times, to some place in a bar, where there was a Dixieland band or something like that, and I happened to be there when I knew about this and so on, mostly if I was with a girl or other. There were several girls in L.A., several in San Francisco. Usually, these were people who had just finished high school, or finishing high school, so they—some were first year of college actually, maybe second year. Normally, we would—God, I don't remember what we did. Later on in my life, we ate dinner, and go to a movie or maybe a play or something like that. Of course, when I was a student, I went and listened to a lot of jazz.

I participated in some jazz concerts with somebody who had the money to put them on. One of those was held in Evanston, Illinois, near Northwestern. It was great, typical Chicago, with visitors from New York and L.A. coming for this; local people who had been there all their life. A number of black musicians. I got very much involved with that. It was a diversion for me, away from the central, hardcore science that I was trying to pursue, and so I liked being diverted. I went to symphonies, not so many chamber music things but symphonies, several operas, in high school and in college. One of the people I lived with, after I move out of these dorms, was sharing two rooms with a guy who had just come there from Reed College, Portland, to get a masters degree in non-directive therapy, which was a field developed by a man named Carl Rogers, which I don't know much. But anyway, you know, we just happened to meet and you're going to go see here and you're going to go there, and do you want to share this space or something. We got along for two years. He was busy doing his thing in psychology and so on, kind of applied psychology, and I'm doing my things proudly, as I'm marching towards gross anatomy, neuro anatomy, vertebrate cranial morphology. Whatever I was doing, I'm marching through this and so on. I'm beginning to develop a thesis, which I later wrote for a masters, which was the first beginning thing that took me into a deep involvement with Neanderthals and their world at the time and so on, and only the last several years, did what I eventually did for a doctors thesis. The title and area really sort of crystallized into something or other.

02-00:14:40
Redman: May I ask your—you've eluded to the fact that you were able, at Chicago, kind of you were allowed this intellectual freedom of being able to take courses in the anthropology department, but also taking courses in the natural sciences.

02-00:14:55
Howell: Yes.

02-00:14:56
Redman: Do you think that—and a lot of what we'll be talking about later on is that you've often been credited with having a lot of interdisciplinary work later on. And so do you think this was a unique experience for you, as early as your undergraduate education, to take these different types of courses, whereas maybe other scholars, early on in their intellectual development, were taking courses more strictly than say the anthropology department.

02-00:15:25
Howell: Yes, certainly true. Well by design, the nature of our field is both social, behavioral and biological, so you've got to know both sides of the coin in general, for something like this. I think Chicago was special, in the sense that the university is small, about 10,000 in those days, easy to get around on it seemed like. It was in a neighborhood, even though the neighborhood later became threatened, impinged upon and threatened, so much so that they even thought of once, selling the university and moving, and starting a new one. It didn't seem like that could ever be done, envisioned, but that was seriously discussed as an option. There were small classes. There was a wide diversity of courses. It was easy to go and to meet with professor so and so, and say I want to know about course so and so and such and such, and you're going to do this. Would you permit me in or not? Piece of cake. None of this, no bureaucracy, a very low level of bureaucracy. The greatest thing was of course, with the GI Bill that I had, was that every month the check would come. Every semester year, we were quarters, not year, my tuition would be automatically paid, and I didn't think much about that. I worked also sometimes in college, to make money, essentially two kinds of work; inventorying, as part of an inventory. This was through one of my colleagues in the anthropology department, a student, who was always promoting something. So we'd go and close—the store would be closed for one or two days, and all we do is we go there on a Saturday and Sunday and we inventory everything in the store, down to the shoe laces. I did that on several occasions, one of which was that kind of place. It was actually a clothing store called Robert *something*. They advertised nice clothes on plain pipe racks kind of place. Another time, we inventoried, and we're being paid so much an hour. I was inventorying a foundry, then which art, and dirty the place would be covered with gold dust and other things. And we did one other, and I don't remember, but it was important to me, and I don't know how much I cleared, but you know maybe I got $5 an hour, and I worked five hours and had $25. That seems to be probably it was more money than that, but I can't imagine that would be the case. That supplemented what I did. I later had a fellowship from the [Moynigran?] Foundation. It was one of the first fellowships given to the group of people of 1952/53, the first year of fellowships from the National Science Foundation, and I had one of those, which was very good.

02-00:19:14
Redman: Speaking about your bachelors work, do you remember specifically, what courses that you were taking at all? Do you remember things like that?

02-00:19:29
Howell: I don't know. That was sort of middle time. I had finished the work in the college, and I was in graduate school already. Of course, I marched through the same school, in the same buildings, you know, it wasn't like that.

02-00:19:44
Redman: So does that—do the moments—

02-00:19:47
Howell: I did that paper mostly on my own.

02-00:19:49
Redman: OK.

02-00:19:50
Howell: The first major paper I wrote, it was an outgrowth of other things I was thinking. I wrote a general paper, with some analysis and so on, called *The Place of Neanderthal Man in Human Evolution*, and published in '51. So I wrote it in '50/'51, and this was an outgrowth of a masters thesis, which you have to do some sort of thesis. It started to be both a thesis and a publishable paper, which was encouraged by several of my professors, that if you could write something good enough, was good to publish as early as possible in your career, and not wait until you write a big doctorate or something like that.

02-00:20:43
Redman: Do you remember the individuals that were supervising this, I mean, your early research, that led up to things like your publication of the structure of Neanderthal face and the place of Neanderthal in human evolution—

02-00:21:03
Howell: That was the first one.

02-00:00:00
Redman: —that research? Who was supervising you?

02-00:21:05
Howell: Well, there were different people.

02-00:21:08
Redman: OK.

02-00:21:10
Howell: There were different people. I mostly learned about everything except the very particular, from these people, because none of them had the kind of grasp that I already had, from extensive reading et cetera.

02-00:21:28
Redman: Was a lot of that reading on your own?

02-00:21:30
Howell: The fossil record. Yes, on my own, on my own, and in college, when I—the first thing I went there, I said what are you going to do with a foreign language thing, now that college is actually on your plate? The first year, which was this mid year to September, academic year they specially made, allowed me to fill in courses that I didn't do in high school or didn't do enough, or they wanted me to have more or a different variety, several choices. And I did some of that, having to do with English and composition. Of course, I took Social Studies II that year, and I took some other courses in biology I want to say.

That next year, and I think it's the next year, I started two years of three languages; a year of German, a whole year in one summer of French, and a three quarter year of Italian. I figured this would allow me into the bulk of the literature, excluding of course Russian. Chinese was not yet significant to speak of, or was published often in English or German. But I knew right away, once I got certain things out of the way, and I would use my electives. That's this freedom that I'm talking about, that was very important to me. I mean, I knew it was going to be there, and that's the way I just happened to go, which was not against the current, which that was a good thing. I made a good call without maybe knowing how come.

02-00:23:36
Redman: And so, can you talk a little bit about the mechanics of the guided reading. It seems as though you went into the library, and you felt somewhat like you'd exhausted certain bodies of literature that was available, and that you, at that time, there wasn't enough there for you to keep you going, and so you almost had to explore the set of German literature or the Italian literature or the French literature.

02-00:24:06
Howell: Well you'd be reading them anyway, because if some particular paleological specimen is found in certain parts of Europe, it's going to be published in German.

02-00:24:16
Redman: OK.

02-00:24:17
Howell: And if it's found in France, it's not going to be published in German, it's going to be published in French. Now the whole world is different, with English wise, but I had to have those languages. The major one I never got—later on I tried on more than one occasion to start a program with other professors in Russian. In one instance, I started it and lasted a week. In another, I lasted maybe multiple weeks. I never—I was always so busy doing other things and so on, and this was after I was on the faculty actually. So I never had that particular help, and had to rely on other people. There were

people around me who had Russian, for example, that I knew in college, and so I could always do that.

02-00:25:06
Redman: OK.

02-00:25:07
Howell: All right. But this paper, this thesis, which became my first major published paper, was something that was based on the literature, also a certain number of replicas that existed that we had or somebody else had, here or there, and ideas that I gained from general principles of evolution. Certainly, I was interested in ecology. I didn't probably know what ecology was truly really was, but I was interested in it, and so there were a number of these things that sort of came together, but they came from different sources, I think, of literature.

02-00:25:54
Redman: So your—from the sounds of it, your advisors encouraged you to publish early on, and at this stage, after having your first major paper. Were they specifically encouraging you to publish in the *American Journal of Physical Anthropology*, or is that something that you were familiar with that journal and you felt like that was an appropriate place to start.

02-00:26:21
Howell: Well that became the journal of the society, the American Association of Physical Anthropologists, which was found in 1918 as a journal, but didn't become part of the society until I think 1928, ten years later. So physical anthropology has its roots earlier, but it's formal formulation and things like that is something of the twentieth century, and has its roots in anatomy and medicine and other things earlier on.

02-00:27:08
Redman: So you would emphasize that a big portion of the period between your masters degree and earning the PhD was a lot of independent reading and a lot of independent—

02-00:27:24
Howell: Yes, and the lab work.

02-00:27:26
Redman: Can you speak about the lab work a little bit?

02-00:27:29
Howell: The lab work had to do with microscopic and other work related to bone and things like that.

02-00:27:35
Redman: By this point, it sounds like you maybe had some specific questions in mind that you were looking to address, or were you still—did you still feel like you were filling in general gaps in your knowledge that you wanted to address?

02-00:27:56
Howell: I think the last year, the year of my doctorate OK, that last academic year, I assisted in a course, like whatever that meant, and was paid for it. It was like we didn't have teaching assistants, but it was as if someone might be a helper participant. I finished the writing of my dissertation. I probably went to jazz concerts sometimes. I was between girlfriends, more or less, and eager to graduate, eager to leave, eager to get a job, eager to get on with it and so on. Here I am, you know, walking around the campus and so on, almost 27 years old. I never had a car or anything, which I recognized as something that if you didn't have it, you didn't think about it, but if you had it, you'd go crazy if you didn't have it any more. And it was so limiting, if you didn't have one, getting around the city. You could use the elevated trains in Chicago, and that's the way I commuted back and forth to meet people. All the rest of the time, I lived right around the campus, after the dorms, which were on the campus, and after that, various houses here and there.

02-00:29:37
Redman: So the title of your doctoral dissertation was *The Cranial Based Structure and Man*.

02-00:29:42
Howell: Yeah.

02-00:29:43
Redman: Looking back on this, and this sort of desire to finish and get done, did that—if you were to go back and look at your doctoral dissertation now, do you think you would recognize this kind of a hurried—

02-00:30:01
Howell: Yes. Boy, you're sure right about that. That could be kind of prickly. You're right. It exists in some file in my office and I have seen it, but not recently.

02-00:30:17
Redman: But it wasn't as important?

02-00:30:18
Howell: It's a thin thing. It's not—I don't know, 150 pages, 140 pages.

02-00:30:23
Redman: One hundred and forty.

02-00:30:24
Howell: A lot of references. How many?

02-00:30:27
Redman: A hundred and forty pages.

02-00:30:28
Howell: A lot of references, so a lot of going to the literature, a lot of use of the literature, and across different fields, and this has to do with this kind of approach; what does it have to do with human fossil history. That's really

what it's about. I remember I ran out of money in the spring, although I had this money for that, and that working for that. I just wasn't going to make it. In the spring, I was offered, and accepted a job in teaching an anatomy course at the University of St. Louis the coming autumn. Like a jerk, I did not arrange for my salary, my 12 month salary to be paid over the 12 months starting June 1st. I didn't do that, I didn't know that. I was really stupid. I sold a huge record collection that I had built, after I was at Chicago, from various sources over many years. I sold it to a guy that got me some money, to carry me through this and that. And then I went, of course, overseas that summer and worked in France. I went to a meeting in London, saw the big city, the Natural History Museum. I was overwhelmed by all of this, and then spent the summer at a prehistoric dig.

02-00:32:11
Redman: I'd like to talk about your trip to Europe, but before I get into that, just a quick question about how exactly you found the job in the department of anatomy at Washington University. Was this a connection that someone at the University of Chicago had, or was—

02-00:32:31
Howell: Yeah, both.

02-00:32:32
Redman: —there an application?

02-00:32:33
Howell: It was really between my professor, on the one hand, and some other person, who is in the American Association of Physical Anthropologists; in this case a woman, Mildred Trotter, from Washington University, who was both an anatomist and a physical anthropologist, who did a lot of work on hair and things like that. My professor sold her on me, and she met me and so on, or had met me, and I agreed to go there and so on, after my summer was done. But my summer was guaranteed, in a way, because the Wenner-Gren Foundation would send me to Europe and back, for this meeting in London, and then somebody else would support me. A colleague at Harvard had mentioned me during my time in Chicago was opening a huge dig, and asked me to come, and he'd take care of my—pay my hotel bill, my food, et cetera, and you know, I'd be there.

02-00:33:45
Redman: Can you talk a little bit about that relationship with the faculty member at Harvard, and how you developed that relationship.

02-00:33:51
Howell: Well we overlapped, and he probably heard of me through somebody. The chain of connections were so easy and so on, with a small number of people and so on, like this. He knew of me through one or both—several of my professors, how many students are there that are studying so and so and so and so. One, you know, and that—I was able to travel also, a number of summers

in a row. Not the whole summer, but summer, and often to New York, and sometimes also to Boston, Cambridge, and to either Baltimore, because of relationships there, or down to the Smithsonian in D.C., the Smithsonian Institution. I had these interrelations because of what I was writing, what I was doing, what I was becoming, and one of them was especially with this person at Harvard, who was a prehistoric archaeologist. We had a prehistoric archaeologist on our faculty, whose interest was in the origins of agriculture and settled farming villages. Robert Braidwood was his name, and he was encouraging everything of me always; when I was a student, on my thesis, and as a junior faculty member. That was good, but he wasn't interested and didn't have the knowledge, of things of greater depth, the way Professor Movius at Harvard did. Movius was quite unique in those days, and one of the nicest people, but there were other people who were extremely helpful and encouraging, promoting and so on, of me in geology, physical geography, in anatomy, paleontology, and those kinds of things. A whole series of people, and those were at Chicago, not at Chicago, overseas later. I mean, you would build up your relationship by writing—there was no email of course—letters back and forth. Yours would be handwritten and his would be typed, with errors, and then maybe you'd meet each other later. But you'd start a correspondence; would you please send me a copy of your so and so paper, da da da, that's normally the way it begins. Or ask a set of directed questions relating to some subject matter; I need to know if it's available and so and so. It was a small, open, very open world.

02-00:36:56
Redman: OK. Let me ask about on these trips to—well, being in Chicago and going to Cambridge, Massachusetts and going to—you mentioned going to the Smithsonian. In Chicago, was going to the Field Museum or going to the American Museum of Natural History a part of your stay in those cities?

02-00:37:17
Howell: Absolutely, a major part. Often, I would be in New York for a meeting of some kind, what were called summer conferences, in physical anthropology. I was invited to those repeatedly, from 1949, I think, on. I was invited to things when I first went to Europe, in London, if there was a meeting, a workshop meeting, a gathering at the Natural History Museum. I was invited to that because Wenner-Gren had given me this help post doctorately, and so on, and I was paid for it out of Washburn's money, to help organize this program. This was the start of a program that had to do with the encouragement and enhancement and intensification of research on early, overall what we call paleoanthropology in Africa now, but it meant prehistory, skeletal studies, and this was before the application of genetics and molecular biology, although that was on the horizon.

02-00:38:35
Redman: So at these early conferences, were you presenting papers at all, or were you mostly concerned with meeting people?

02-00:38:43
Howell: I presented papers when I was a student.

02-00:38:45
Redman: OK.

02-00:38:47
Howell: At annual meetings that I went to, of the physical anthropologists, sometimes to the American Anthropologists Association, also where I published later. These were places that if it was convenient and the money was worth it and available, you would go. That's where you met people, met your peers, saw your peers again that you hadn't seen for a while. It was good.

02-00:39:22
Redman: And of course, the University of Chicago encouraged you to do to these meetings.

02-00:39:25
Howell: Absolutely, sure, yeah.

02-00:39:26
Redman: Yeah, yeah. We'll get into this a little more later, but I'm assuming, when you're going to these conferences, from '49 on, they must have a pretty different feel from what a young academic would see at conference today. I mean, going to these same conferences—

02-00:39:46
Howell: No resemblance.

02-00:39:47
Redman: No resemblance.

02-00:39:48
Howell: No.

02-00:39:49
Redman: What were they like then?

02-00:39:50
Howell: Well they were very—the general association meetings were of a kind. Physical anthropologist, always few, always stayed, almost all old times, except the younger ones of us, just over my age or my age, coming along, you know, were part of it. You'd be going away and have the meeting. The meeting would be in New York, that was nice. The meeting would be in Cleveland, that's OK. You know, you spend several days in a hotel and stuff like that. The major thing is you're interacting with the people you choose to interact with, one because you're small and so you interact anyway and you know each other—get to know each other better. Or the other, you meet new people in a context that maybe you hadn't met them before, or they weren't present before, or a different kind of venue. And this would enlarge your mind, you'd say he's a great guy, it's wonderful to meet somebody with that

interest and so on. I'll have to learn to think that way, go and buy him a beer or something like that, whatever it was. I took advantage of those things. I didn't pursue them, but I took advantage of them. I think all these things, there isn't anything really, as I look around, that hasn't added to this enhancement of curiosities or knowledges that I already have. The knowledges that I have get enlarged and changed and transformed, and so on, and the curiosities and so on, suddenly become something that you fixate on and bury yourself in. That has always been the case with me, and I've felt increasingly in life, that it has not been difficult for me to transgress on what was else otherwise obscure, something unknown. I don't know anything about that. Why do I want to plunge in to understand that? I won't understand a word anybody's saying or whatever. You know, it would be like reading calculus when you don't know anything about it, but it never bothered me. I think that's because one thing builds on another thing and builds on another, and so there are some levels of continuity and no major breaks. There may be evident breaks between the end of one and the beginning of the other one, but otherwise, they're not. I think there's a gradualism and a lot of building up of knowledge curiosities. How do you do certain kinds of research, what are the tricks of the trade and so on, and I had those, at least some knowledge of those, that junior year in Indianapolis, at the State Historical Library, that particular piece of summer. Also, my last year of high school, I worked at the little public library, the Carnegie Library in Beaver Dam, for so many hours a week, and was exposed to a library situation there and so on, as part-time work during my senior year of high school.

02-00:43:15
Redman: The networks that you're building up in the United States, and I'm going to bring this into your trip to Europe with the Wenner-Gren Foundation after I ask you this. The networks of people that you were meeting, were they mostly—and you said there was a real divide between an older generation of scholars, and then a younger generation that was coming in. Were most of these people Americans, or were there a number of Europeans that you were meeting already, that were living in the United States?

02-00:43:46
Howell: No, I met people in—well, I met people in the States, who were from overseas, through the latter part, four years latter, four or five years, of my doctorate. So let's say the last four years of my seven years post high school. England, notable England, later on France and later on Germany, Austria. These were people who came to the States, or who were present in South Africa, present at a meeting in New York, at the Wenner-Gren. That was often the way people met. Otherwise, sometimes somebody would come to this country and also come and spend some time with us at Chicago, and that happened early on, to a limited extent, in prehistory or that part of—it's not really prehistory. We never had a prehistory professor visit, but a lot of ethnologists, British ethnologists. Some archaeologists with older interests also would come and visit us but you know, Chicago is not the east coast. In

the east coast, everybody was there, and might never get farther west or somewhere else. But I met a lot of people, and also corresponded with people. So I had relationships with people, according to their writing and so on, although I had never met them, maybe saw pictures of them. Later I would meet them, these people. Some I met before, here in the States when they came. Others I met when I first met them overseas.

02-00:45:53
Redman: How long did you stay in London, this first time, with the Wenner-Gren Foundation?

02-00:45:58
Howell: I don't know. I suppose I was there some two weeks or something like that, where we had meetings, and maybe it was 16 days or 17. Anyway, I worked at the museum also, on collections on fossils, in the library, a wonderful library. I had a close friend there, who had visited the States and also visited Chicago when I was student. His name was Kenneth Oakley, and he was one of the first people to be interested in the application of isotopic things to physical anthropology, and dating, relativity of an absolute method to dating and refinement, carbon-14 was the only dating method of any consequence, relative to pre-human history. It had come online in—the idea came about in '47, it came online in '50, and radiocarbon laboratories began first in Europe and concurrently over here, at different places. There was one at Chicago.

02-00:47:15
Redman: Which was with Libby, correct?

02-00:47:17
Howell: Yeah, Libby was the responsible person for this development, the dating use, the disintegration.

02-00:47:25
Redman: Did you know him at all?

02-00:47:26
Howell: I met him and heard him talk and so on. He moved, still about the time I graduated, or somewhere around that time. He moved to the UCLA, and he was there until his death.

02-00:47:41
Redman: OK. So you saw him speak, but it wasn't—

02-00:47:43
Howell: Yeah, and he interacted with colleagues of mine. He was interested in applying data methods like this, carbon disintegration methods. Two things that we were already pretty clearly or strongly circumferentially, you know strongly, as far as one can figure out, certainly dated or certainly age determined.

02-00:48:14
Redman: The first he—

02-00:48:15
Howell: Like Egyptian boats.

02-00:48:16
Redman: The first thing he dated was an Egyptian boat from the Field Museum, right?

02-00:48:18
Howell: Yeah, or certain kinds of things like that.

02-00:48:21
Redman: Certainly.

02-00:48:21
Howell: So, but Oakley applied these and all kinds of related methods of different sorts. He was a major person in the resolution of the Piltdown controversy, and he was trained as a geologist but did a lot of things having to do with geology plus prehistory, prehistoric archaeological excavations, early excavations. Mary Leaky and the Thames Basin and all kinds of things. He had been doing some trips to Africa, before I went, several of them, and was a big stimulus to this Wenner-Gren program about Africa.

02-00:49:05
Redman: In London, working at the Natural History Museum, did you—and you spoke a little bit about working with the collection there. You had mentioned that most of what you were working with at the University of Chicago was either secondary literature or a cast of Neanderthal.

02-00:49:22
Howell: That's right. You've got that right.

02-00:49:23
Redman: So was this the first time that you had handled actual Neanderthal fossils?

02-00:49:26
Howell: Yes, really, that's true. That's a fact.

02-00:49:28
Redman: OK.

02-00:49:30
Howell: And there were several, plus some other things that are there at the Natural History Museum. I was, over a period of years, once I got to London and so on. I used to more often go to England, but I rarely do any more, because of just connections from here to the continent, that's really it. But he came to this country, and he got involved in what later became a major book of his, it went through several editions, called *Frameworks for Dating Fossil Man*. Very provocative for the day and so on. So he was a major contact for me in this instance, and he was the one I would go to see, or I'm here to see Dr. Oakley

at the door. It was much more informal at the Natural History Museum in those days than later.

02-00:50:31
Redman: Was that particularly exciting, for you to—or did you not necessarily draw those lines between something that was a cast of a Neanderthal specimen versus handling the "authentic" specimens?

02-00:50:49
Howell: Yes, that's true. Well, I'd seen only a few original human fossils in Cambridge, at Harvard, when I did my first week. The rest were casts, pictures and the literature, et cetera. There were some casts that we had both at Chicago and at the Field Museum. My relationships with the Field meant that I could use that, but so yeah, I was overwhelmed to be able to see original things and so on, and that was the summer that really led to the demise of the Piltdown hoax, and that was happening under our eyes, where we were half aware of how extensive it was happening, it was being formulated and so on. All the big players were there, whom I either knew directly or knew of.

02-00:51:49
Redman: You've meditated somewhat, even—correct me if I'm wrong, as late as the 1990s, about looking back and reflecting on the Piltdown hoax. Is that correct?

02-00:51:59
Howell: Well to a limited amount.

02-00:52:00
Redman: OK.

02-00:52:01
Howell: Yes. When I saw, with some others, the actual Piltdown remains, which had been kept from anybody's eyes for many years.

02-00:52:11
Redman: Interesting.

02-00:52:12
Howell: And which were not appropriately treated, except very rarely, by one or two people, in terms of their potential incongruence. The main thing that I thought was that anatomically, and that's what I brought to it. I was seeing the original, but I had already saw a good cast of those at the Field Museum, and we actually had in Chicago. I dismissed—I essentially dismissed the whole Piltdown thing, before it was formally dismissed. In '51 it wasn't yet dismissed. In late '53, and I said we just put it aside because there's too many questions, and of course little did we know, the kinds of questions that were involved, that this had been deliberately fabricated, et cetera, et cetera. I never dreamt of that in 1950/51. I just knew, and that was already some of Oakley's earliest work, that there was something really incongruent about the purported similar ages of things that have in fact, very different ages. So that was

coming out in those days. These were very primitive days of geochronology, I can tell you, very, very. I mean we thought radiocarbon, where it's helpful just oh, it's going to be wonderful. Potassium-Argon Dating was coming on board later, and it had been used for very, very, very, very old rocks. The thing is, could you measure such miniscule amounts as remain from half life, half life, half life. How could measure these infinitesimal amounts? We haven't got such a device for measuring, and that had to wait until somebody invented the mass spectrometer, which was done here in physics by a man named Reynolds.

02-00:54:09
Redman: That was here? By here you mean?

02-00:54:11
Howell: Here in Berkeley.

02-00:54:11
Redman: In Berkeley, at the University of California.

02-00:54:12
Howell: And that was done yeah, at the University of California in Berkeley, and that enabled the application of—and many modifications later, changes enabled things to be measured that we never dreamt we would measure their residual amounts, and hence, how much has gone and hence, how old they would be. And then other things came. Uranium lead was modified for later things. Other special things came online. Fission track dating came online as a way of measuring also, the amount of shocking that some sample has undergone, and you measure how much time has elapsed since the first one of these shocks, these scars. And many other things. Carbon, calcium, uranium, I mean, a whole series of different methods emerged. They weren't emerged yet in the '50s. The '50s were a time, we were just approaching the time in the '50s, where the impact and the seriousness of the impact of plate tectonics was to really hit science and to be put to use by everybody in the geosciences, as opposed to the earth sciences. It was a time, if you knew the moment, and I would already know about some parts of it peripherally because they interested me, but if you knew about them all, this was a tremendous time of ferment of various branches of science, both geophysics, geochemistry, and various parts of geology. Formalized deep sea oceanography was online, and ships were highly successful in their cruises right after they published. So there were a whole series of undertakings that were emerging, research directions, new ways of being able to get at some scientific question. You could ask a question before and you wouldn't know how to get at an answer. Now you'd ask a question, knowing how you might get an answer through methodology and so on. It was a big thing.

02-00:56:49
Redman: So before you go to France, I'm going to change tapes.

02-00:56:52
Howell: OK. What time are we?

02-00:56:56
Redman: Let's see. It looks like we are almost at 4:30.

02-00:57:00
Howell: OK. Let's go a little longer.

Begin Audio File 3 Howell_Clark3_02-08-2007.wav

03-00:00:07
Redman: My name is Sam Redman, and I am interviewing F. Clark Howell. This is the third tape of our interview today, which is February 8, 2007, here in Berkeley, California. When we left off last time, we were speaking about your Wenner-Gren Foundation trip to Europe. This was before taking up the position of instructor of anatomy at Washington University, but we were speaking about your time in London, before going to France, and eventually here, we'll move over with you, we'll cross the channel and go over to France. We were speaking a little bit about Piltdown and eventually, one of the major themes, obviously, in the interview will be the development of new dating techniques, and how these sorts of things impacted paleoanthropology. But I'd like to hear, if you have any more thoughts about—you were describing that the Piltdown hoax was really being discussed when you were there, and you were really in the heart of it, in London, at the Natural History Museum, and so I wondered if you had any more thoughts about the people that you were meeting at the Natural History Museum, and even though you were only there for a short time, how that experience, and working with those museum collections, impacted your development.

03-00:01:38
Howell: I think that I, and maybe others who were present over that interval, probably didn't realize the historical significance to this situation. People had such varied—they had had such polarized opinions about Piltdown, one way or another, and explaining it away, half explaining it, and evaluating it properly, critical evaluation, which it never really had, which is unfortunate. But it did get it later, and there were methods that were developed with fluorine, using fluorine, or a relative dating method using nitrogen, nitrogen something else, as a molecule, percentage wise and things like that. All those things were coming online. They were part of the development of all of the isotope work and knowledge that led up to, of course, the building of the atomic bomb and all of those kinds of things. It went off in other directions and increasingly, chemistry became stronger and stronger and stronger. So one had all these things happening. I was involved with the people who were there. I was there to do certain other things on this occasion, to attend the meetings, and any of the extra days we have, I would be at the museum, doing what I wanted to do, if they would allow me to do it at the museum, which they did. I opened everything, took things out of the safe, you know, that kind of stuff, and

Piltdown was one subject. Later one morning, I think it was morning, it was over several hours, so many 10:00 to 12:30 or something like that, there was people. It was in a certain place. We went to this part of a lab or something and were able to see it, and in those days, you could even handle a part of it. That would be less the case now, you necessarily don't have to. But anyway, I was one of the people who said it was an incongruent thing. I didn't know it was a fake or different kinds of things put together to make one thing. I'm going to avoid it, because all the other evidence—it has no other corroborating evidence whatsoever, of any kind, no second place or whatever. So you know, so I wasn't too surprised, but I was over pleased. Other people who were there, (inaudible), an atomist, one of my professors, Sherwood Washburn, physical anthropologist, and atomist from Oxford, Le Gros Clarke, Joseph Weiner, who was also a physical anthropologist, and this was a provocative moment, but I treated in quite a perfunctory way, because I didn't see anything in what I could see and determine, that was going to change my mind, which was already made up, that this was a waste of time, and forget it and pay no attention to it. And of course, that's what it proved to be ultimately, with the publications on all these analyses, and we found out that the mandible, as had been suggested by more than one person, a modified, real, orang partial mandible, with the teeth deliberately treated in a certain fashion. The skull, a pathological or a quasi pathological one in part that is human, and you have many of the diagnostic parts of it knocked off or seemingly knocked off, and it's all stained, to give it an artificial—a truly honest looking, but artificial surface and appearance and so on, that I was not overwhelmed by. I was overwhelmed more by the other things at the museum, and the comparative specimens that were there that I didn't know about, and to check things in a library, that was very easy to do. We just didn't have ours there. Harvard had them, we didn't, so I didn't see things.

03-00:06:33
Redman: So did that—

03-00:06:35
Howell: And I enjoyed London.

03-00:06:37
Redman: OK. That gave you—the experience of working there really gave you a good impression of what working with museum collections would be like?

03-00:06:46
Howell: Oh yeah, absolutely.

03-00:06:47
Redman: OK.

03-00:06:47
Howell: The Natural History Museum, it's then still called British Museum (Natural History), is of course, a pretty bureaucratic organization. There's guards and you can't just go anywhere, and you can't just say I'm so and so, and they say

go ahead. It doesn't work that way, it's very structured. Once I was there, you know, for a number of occasions and repeatedly, certain guards would know me and you know, doff their hat, and it would be much easier for me to get around. If you go to some places, there's some people that are foreign to you and you're foreign to them. That was the first and biggest museum, other than the American Museum, which is very large and diverse, and which I've known. Even before I went to London, I went to the American Museum in August of '46, after I got out of the military and went to see the museum, see New York, listen to jazz, see fossils at the American Museum. Franz Weidenreich was there, writing about things from Java and so on. I got used to museums, smaller museums, bigger museums, you know college museums, not big museums, early on and moreso all the time, through my student days. Later, they're an integral part of my life. There's two integral parts of the kind of life that I've had. One has to do with overseas in the field, or overseas without being in the field. You can be in a museum, working station labs or whatever. And the other, of course, is laboratory work and all those kinds of things, which is what I do. You can't have one without the other.

03-00:09:02
Redman: So you would say that your work in the museums, your work in the laboratory and your work in the field all held equal importance by this point.

03-00:09:11
Howell: Well I don't know about equal.

03-00:09:13
Redman: OK.

03-00:09:13
Howell: But they are all woven together into the same kind of Scayne like wharf and woof, and if I'd been one of those people, who was strictly a lab person, teaching anatomy, research and anatomy, and never went overseas to do field work, I would not be a hairbreadth of what I am as a person and scientist and so on since then. Instead of getting involved, I didn't, because I had overseas interests. I didn't use any opportunity to do any summer field school during my student days. I didn't even think of it then, because I didn't want to take the time of going to summer field school that had to do with geology. I figured I'd pick up all that on my own or other ways, which of course I've done more or less, but no. So this whole business of these possibilities of turn one way, and turn it the other way, and you see something different each way and in between they're neither. Neither something of the same, but something different and something altogether unusual, and they're vital, all these things are vital. Every one of these things, which come together in ways that are very hard to dissect sometimes, or things that I chose to practice or chose to think about practicing, in a way that maybe later on, I practiced it but not like that, or I used something inappropriate when I could have used something else, that kind of thing. All those things came to me progressively, earlier on. One added on top of the other and the other, over a period of time. Time has

different significance, and sometimes time is elapsing and nothing's happening. Other times, time is the same time as it was before, it seems to go extremely rapidly because so much is happening, and that's true in science. The growth of these things or the elaboration of these things, because things are never elaborated to the same degree at the same time. Some are following, some are leading, some are branching off, and all this stuff is happening. That's extremely important in what will eventually materialize as something newer and derived and different than whatever preceded it, and it's meaningful. Maybe you haven't jumped to paradigm, but you jumped a substantial amount, and yet you don't see the beginning of, you know, the end of the beginning and the beginning of the subsequency. You don't see that because you just do it. You're just living it and you're not paying attention to it.

03-00:12:22
Redman: Right, right. So then you had already been invited to come over to France. So you—it was a given when you started your trip. You knew that you were going to—

03-00:12:33
Howell: Yes. He said they would be excavating all summer.

03-00:12:38
Redman: And this was? Who was this?

03-00:12:38
Howell: And this was Professor Hallam L. Movius, M-O-V-I-U-S, of the Peabody Museum, Anthropology Department, at Harvard University. He was an older scholar. I suppose he was probably middle age really then. He had been an officer in World War II, in intelligence I remember, maybe with the Air Force, but I got to know him through mutual interests and interactions, including my going to Cambridge on several occasions as a student still; maybe once because of a meeting, once because of something else. I got to be close—I contributed certain chapters to certain books, certain things, and he encouraged me. He didn't have anything to do with my job, which was teaching anatomy, and he wouldn't have had anything to do with—he was interested in me as a person. He was interested in prehistoric archaeology, and I was interested in that. I acquired that interest and I got deeper in that interest. I didn't give up others, which sometimes, when I was juggling too many things on my plate, but these became, each in their way, stronger and stronger and stronger.

03-00:14:07
Redman: OK, so you—

03-00:14:08
Howell: So it was a great chance for me to go to France—

03-00:14:10
Redman: Absolutely.

03-00:14:11
Howell: —to do this, and he laid it on, so I came down by train from Paris and was met at the train by a woman, whose name I couldn't understand at all, who later turned out to be Francois Bordes' wife, Denise de Sonneville Bordes, who is still alive, she is. He died in '81. They became friends of mine, and I corresponded extensively with him about things and stuff like that. Later, we were to become friends for years. She too, a terribly nice lady, and a very good archaeologist on her own.

03-00:14:50
Redman: OK, so they brought you to the site and also the place where you—

03-00:14:55
Howell: Well they met me. He wasn't there. She met me at the train, which was like a little Toonerville Trolley, with two cars, that you take it—you get as far as [Paragu?], which is a large city. After that, you get on this little Toonerville Trolley. In those days, 1953, this was two little cars, one behind the other, on of course, the French narrow gauge railroad, and it let you out in [Lazizi?], very close to the center of the Legion [Cromanon?] Hotel. I stayed at the Cromanon Hotel, as did the Movius' and their daughter. Laborers were hired for the excavation, and students. But I met colleagues, who passed through sometimes. I did meet Francoise Bordes, saw his wife several times, went to their house for Sunday dinner. I also spent several days digging with the Bourdon's, at a site called—he was digging a very big site called Combe Grenal but most—the one that I got involved with, with him for two days, and I was staying at his house—he had a little summer house nearby, at Carsac, was Pech de l'Azé, one of the Pech de l'Azé caves, one of which is yielded a Neanderthal like skull, cranium, and others that have yielded various levels of occupations of Neanderthal, presence of debris and stone tools and animal bones, and things like that. So I had my first actual excavation with Bordes in those few instances, because I had never had any before. I had never gone to field school.

03-00:17:00
Redman: Was the learning curve pretty steep there, or?

03-00:17:05
Howell: Oh I had a great summer, it was simple. I had fun with it. I was to do all the sorting, the typology and the categorization is really what it is, of the stone artifacts that came out of this very large test trench. It was both kind of a bore, and then you'd be at different levels or different technologies and things, and it would be interesting and so on. But anyway, we went down through quite a range of time, and I had the help of a young Frenchman, so I learned a little French from him. I could read it and so on, but I couldn't understand this language, and I'd had very little, and also forgot, vocal understanding of pronunciation of all these things. So I could read fine, you know, but there

you are and what do you do, and these words are strung together the way Frenchmen say them, with this so called French accent. This is my job, and it took some years to get over that, my French is not bad. My German used to be very good and now, it needs a lot of effort any more. My Italian, I can sort of read it, but so much is written in English in all these languages now, that you don't have to—you're not forced to have language as you were in the past.

03-00:18:37
Redman: OK.

03-00:18:39
Howell: Certainly in science, you should have always had some kind of foreign language, in my opinion, both the tool and time and needs, as a tool, so it's a working man's knowledge of German and da da da, that you need, or other things just because of reading for fun, which is a very important thing and a way to expand your intellect and challenge it is good.

03-00:19:10
Redman: So the day to day work, it was mostly you organizing these finds, but then also taking short visits to different sites in the area?

03-00:19:22
Howell: Yes, we did that on Sundays and Saturday—Saturdays, and Saturday afternoon, we'd go to somebody's house on Sunday morning. Maybe we'd go to certain sites and then have lunch at somebody else's house if they had invited us. This included the Movius' and her daughter, if she was there, myself, maybe one or other student of Movius was there part of the time that summer, mostly after me. I got to know some professional people, who happened to live there in the summer. I saw a man, a paleontologist, who became a colleague and a friend of mine later, and who worked in the Omo Valley in Ethiopia in the early '30s. He had a summer house there, and we went to see his summer house, that was very nice. I saw many sites in these trips, almost all are extra trips. I went to [Lasco?] of course, and was able to do that with hardly anybody else there, not with a big group. That was one advantage, because people were in charge of things. This person with you, I mean, the doors are open for you, and that was wonderful. I observed about how things were recorded, those kinds of things, in the excavations. Of course later on, I was to do substantial excavations in Africa. After all the summer, they had one of the biggest strikes, general strikes, ever in France, and there was a real question—this is August, and I had to get up to Paris, and I had to get back to the States, go to St. Louis and find a place to live, which I have now forgotten how I did all that. Our thought was, how are we ever going to get—there were no trains, there was no buses, there was no anything, no underground in Paris. Professor Movius had a car, plus another car, and the other car was an old one, a sedan, but it ran like a scared rabbit, and so he decided to send it to Paris, all the way, and the driver and whoever else it was, including me. So maybe there were five people, and we drove from very early in the morning, to all day long. We stopped for lunch and eventually got to

Paris and so on, and I was back out at the field in the country, and I spent more than a week, maybe ten days or something, in Paris, visiting museums, several museums, an institute and so on, that had to do with these kinds of things that I was interested in. And that's not a good time to go and visit because August, nobody's there. But in some cases, things had been arranged ahead of time for me, and I could do that, and that was great. I'd go there, and a lady would come and she'd say fine.

03-00:22:41
Redman: Can I ask how long were you in the field in France? How long were you in the country, the countryside?

03-00:22:49
Howell: Well I suppose it must have been somewhere between six week and eight weeks.

03-00:22:58
Redman: OK, OK. And then—

03-00:23:00
Howell: Something like that, and then I went back to London, spent some days with a colleague at Cambridge, stayed with him.

03-00:23:10
Redman: Do you remember the name of the colleague?

03-00:23:12
Howell: Jack Trevor was the guy, and he's a physical anthropologist too. Mostly, he did human biology, but I had gotten to know him otherwise and he was very friendly, and asked me to stay. I had met him when I was first in London, and when I came back, I said I'll stay with you. So I went up to Cambridge on a train and spent several days with him. I made two trips towards the coast of England; one to visit Down House, Darwin's home, and which was lived in then by a professor, who was—had became like the keeper of Down House or something, so he lived there free, and this was a way of rewarding him for service. He'd been a professor for years in the College of Physicians and Surgeons, and his name was Arthur Keith, and a very big figure in the Piltdown puzzle and in certain claims for Piltdown, the significance of Piltdown, and in many, a number of things in comparative primatology and locomotion, and lots of things about fossils and public books kind of for a very literate public, and it was nice to meet him.

03-00:24:38
Redman: So what had more of an impact? Was it sitting in Charles Darwin's house or was it sitting with the esteemed—

03-00:24:44
Howell: That was interesting, and I sat there with this man, who was over six feet eight or something, all bent like this. He said, "You look so young Dr. Howell." I said well, I was also—and he said, "Well I'm 80" and so and so. We had tea,

high tea, and we talked about all—I have no memory of what, but what do you do, where do you do it, what are your plans. You can't ask somebody 85 or 88 years old what his plans are. He was writing in the last so and so, whatever. In this case, he was writing a book about Darwin, but it was an opportunity to meet him. The main purpose of the trip was to go to a place not far away from Down, a place called Birchington, on Kent, so this was all in Kent. There was a—Birchington on Kent had this incredible collection of African animal skeletons, unbelievable, and it was most particularly known, since the late '40s, when somebody discovered it, because it had so many primates, monkeys, apes and so on, including non-African. These were all brought together by a guy who left him at his estate and set up a museum on the estate, and the availability of things for people to study if they wanted to. He was an African hunter, et cetera, et cetera, very serious, and so we went out to Birchington and met the keeper, who was a woman. I've never been back since, but many of my students have used that facility and all of its collections and so on, for many reasons. Many people all over the world have gone to this place, and I had an afternoon to see what that was like. Plus, an afternoon with Sir Arthur Keith, having tea, which was one of the grand old men. I suddenly, this summer, am meeting all these people I had only dreamed of. I never met them before.

03-00:27:13
Redman: That's right, that's fantastic. So would you like to end with speaking a little bit about coming back to St. Louis, finding a place, and getting set up, if you recall.

03-00:27:27
Howell: Yeah, well I wish I could recall how I did that. I certainly did it on the train or plane. I had shipped books and things like that, to St. Louis, to the department, before I left Chicago, and emptied my apartment. I don't remember how I did that.

03-00:27:45
Redman: And you had never been to St. Louis before?

03-00:27:47
Howell: Yes, I had been there for meetings.

03-00:27:47
Redman: Oh you had been there. Oh, OK.

03-00:27:50
Howell: And so on.

03-00:27:51
Redman: So you had some familiarity.

03-00:27:52
Howell: That's all, yes. That was the only real reason I had been there. In September, after awhile, I don't remember where I went to St. Louis, how I found a place

to live. What I did eventually find as a place to live was a carriage house, behind a house, with a garage below, and up above, two apartments, and I got one of the two. It was a good thing for me. It was about five blocks walking, from the University Medical School and my job. I didn't have a car. A one block walk to—right in front of me, a bus stop, electric bus, back and forth, up and down town or out to the—I never went out to the main university, which is called on the hill, except for near that wonderful zoo, which is close by, and that was subsequent. But I did whatever I did. Eventually I met my—the woman who would end up being my wife, on a blind date. It took a while for that to happen, but that ultimately happened. My time in St. Louis went rapidly, because first, the way it was and second, being gone. I was overseas almost seven months or so. Eight months of that time, I was gone. So I missed part of one teaching term, a half of term teaching.

03-00:29:27
Redman: OK, and that was, you were working in the field.

03-00:29:29
Howell: That's when I was in this long trip in the field, in 1954, right.

03-00:29:33
Redman: OK.

03-00:29:35
Howell: And then in '55, we of course left the current world of medicine at Washington University, moved back to Chicago, and became a part of the Anthropology Department at Chicago.

03-00:29:47
Redman: Do you have any thoughts about starting teaching there, for the—becoming an instructor?

03-00:29:53
Howell: Well they made it easy for me the first term. The second term, I think I taught probably like a little seminar. A few people came. A more general little lecture course. A lot of what we did was very small in those days, because we were still mostly a graduate school, and I was not formally appointed into the college, which had its own appointees, or those jointly appointed. I was still in the department. I was the first such person hired for a faculty that had been there previously, was there previously, and I was the first one added to it. There were five people, six people, and I made it number seven.

03-00:30:40
Redman: All right. Well, do you think that's a good place to end?

03-00:30:43
Howell: It's a good place to stop, I think.

03-00:30:44
Redman: OK, sounds good.

03-00:30:46
Howell: OK. Now you've got your kitten caboodle.

03-00:30:50
Redman: Oh boy, that's right.

03-00:30:52
Howell: It's amazing though, but you went through this one step at a time, like you know exactly where you're going and where you're coming from, and that's good. It's always better if you do it that way.

Interview 2: February 16, 2007
Begin Audio File 4 Howell_Clark4_02-16-2007.wav

04-00:00:06
Redman: OK. My name is Sam Redman. I'm here today with F. Clark Howell, and today is our second meeting. This is, I believe, our fourth tape, but our first tape today, and it is Friday, February 16, 2007. OK. So the last time we spoke, we left off, you had just started teaching—you were teaching anatomy in the Department of Anatomy at Washington University in St. Louis, and you were also holding this Wenner-Gren Foundation grant, that allowed you to do some exploratory field work in both Africa and in Europe. In 1954, you did a paleontological study tour of Africa. So this was your first time in Africa, is that correct?

04-00:01:08
Howell: My first big immersion in Africa.

04-00:01:12
Redman: OK.

04-00:01:13
Howell: Of all kinds. My first and biggest immersion was that particular time of year, and that was at a—I had finished one year of teaching anatomy and the Wenner-Gren had talked to me about its program, which I had been involved with at a meeting in London the previous year, at the Natural History Museum, for encouraging paleoanthropological research in Africa, and the offer came from them, to pay for me for kind of like a post doc trip of extended duration. As it happened, I was gone from May, I believe, until December of '54. So I finished my teaching commitments in the autumn and winter, and then I went to Africa in the spring, and I was there until winter time. I stopped in Rome on my way back and gave a lecture, in December, as I remember, and I got back just after Christmas time in St. Louis, in time to teach the second part of anatomy, the first part of which I had missed, and a substitute talked for me. In essence, the Wenner-Gren paid all my expenses to Africa, that is the air expenses and such. In fact, they did it as first class airfares, because of baggage problems, and that tells you something about the cost of things in those days, versus what happened to things much later, where they became completely outrageous, just kind of unheard of. The idea really was for me to experience what people were doing in Africa, to go to places where things were active and so on, and where there were people, and if possible, to share with some other people who might be there just visiting, to share some part of my time in Africa. Actually that particular part didn't work out, as it happened. I had a terrific thing arranged in the Belgian Congo, in a part called Katanga, the capital of which used to be Elizabethville. There was to be a meeting there, and I was to attend this meeting, which had to do with colonial geological surveys. And so there were people who were both with surveys or people who were outside but part of such a thing, and I was to interact with one particular person, who was from Iowa, and we were to see

things through his eyes. He was a special on pedology, the study of soils and stuff. As it happened, he couldn't make the meeting, and so I was half stranded in Katanga, in terms of our being together. On the other hand, I had arranged things ahead of time, so there were people there that I was with and so on. Everywhere I was with or related to—I was under somebody's umbrella, and whether I went initially to Johannesburg, to the Orange Free State, to Cape Town and the Cape. Then when I came back, I went to Pretoria, that's one of several capitals in South Africa. Then I went to Southern Rhodesia, Northern Rhodesia. Tanganyika was still Tanganyika. It's still colonial Africa, 1954, and I went to Katanga, Belgian Congo. Also from Northern Rhodesia was not Zambia yet. I made a foray up into Tanganyika that led to my subsequent excavation of a big site in southern Tanganyika, that had been found accidentally by some travelers from South Africa, driving from Capt to Cairo, and they had found this place in the southern highlands of Tanganyika, a very interesting place called Isimila, one of a few such places known at the time, one of really only two at the time, and I was very provoked by it, and later I excavated it in 1957 and '58, with two students and my wife. I ended up my trip in Uganda, visiting in Kampala and then [Tebe?], that's a geological survey, and then in the field. I was in the field for about a week, on the border between Tanganyika and Uganda, along a big river called the Kagera River, where excavations were going on and geological work by a one time of the Geological Survey of Uganda, quite an amazing guy. He asked me to stay with him. And then I went to Kenya, and Kenya was in Mau Mau State in those days, high security, everybody carrying weapons, side arms. You couldn't got anywhere without having armed askaris, as they're called. They're armed military or police with you, with machine guns or ouzis or things like this, and we did do some fieldtrips. Louis Leakey took me on fieldtrips. I made one long trip of eight days, down to see Olduvai Gorge, which he laid on with me and sent a former prisoner of war from Italy with me, a driver and a cook, and I camped at the gorge and walked around and so on, and kind of saw it from different perspectives for about six days, along with the Crater Highlands, and I worked at the museum in Nairobi, and that's where I finished my Africa trip. I went off to Rome and then home.

04-00:08:40
Redman: OK.

04-00:08:41
Howell: It was a very big immersion for me in Africa, both in the field, seeing institutions, and what the institutions were like. They were often government institutions, even the universities, or institutions within government. So you'd have a department of archaeology and antiquities and things like that. I met some young people. I met a few young people of my age or a little older, and we were few, and I met the famous names of the time, wherever they were. So I really got a hands on feeling for what Africa was like. Now the year before I graduated, my degree, the Wenner-Gren had employed two of us at the University of Chicago to collect, criticize, critically evaluate various kinds of

information having to do with the prehistory of Africa. I was to do the Pleistocene, ice age, et cetera, the older prehistory, the so called stones and bones and so on. So I had a very good grasp of the literature, primary and secondary, for this kind of thing. So when I went to Africa, I knew a lot about where I was going to, and what I would learn were the things that were unpublished, or the things that were not emphasized or things like that. I was taken in the field by other people. I accompanied other people on certain occasions or special arrangements were made for me to do this in various ways. So all this was kind of optimum. I had a car at my disposal, when I was in both Northern Rhodesia on the one hand, and ventured way up into Tanganyika, and I had it went I went down into Southern Rhodesia from Livingston, which is where I was based, at Victoria Falls. Desmond Clark was on leave in England with his wife and family, but had arranged things for me. I had a pickup truck that was given to me for my use, and one of his young—I've forgotten his name—assistants from England, who had been there a very short time, so he wasn't very knowledgeable, was assigned to me. So between him and myself, we drove wherever we had to go. He didn't accompany me to Southern Rhodesia, but he did the trip up in Northern Rhodesia. Went to a famous site called Kalambo Falls, which had just been tested, discovered and tested the year before, in '53. It's just above a gorgeous fault, at the south end of Lake Tanganyika, where the fall as a 730 foot freefall into the end of the lake, from a very steep gorge cut at the head of the valley, at the end of the lake. A very fascinating place. Kind of terrifying to be around. It was full of the biggest raptors you ever seen, going on the thermals and so on, all around this gorgeous country. And from there we ventured up, and we found Isimila, stayed at a little town called Oringa two days, collected some things at the site, which I had left at the museum in Livingston later. This trip whetted my appetite for Africa. I did not have such a whetting, because of the brief immersion, in England in '53, or in Southern France, in the Dordogne, where I was busy sorting somebody's artifacts from a big test excavation. I didn't have the same thing. Of course it would be true that anything that has to do with human evolution and early ice age, and ice age people's and so on, is going to have to include Europe. By definition, of course, it's really Eurasia. But Africa was more of a still, kind of dark continent and was often regarded as peripheral in a number of ways, and was coming to be known only under certain kinds of circumstances, especially because of the increasing number of remains of Australopithecus or something like Australopithecus, from sites in South Africa. Little would we know, there would be important sites, important finds made and Olduvai a few years after I was there, in '59. Mrs. Leakey found a so called Zinjanthropus one day and so on, and there's more to that, to talk about later on, because that very year, I was up in the Omo for the first time, for one month in '59.

I began what was to be a long-term commitment to Africa, because of this immersion, and this immersion was made possible by the insight and encouragement of the Wenner-Gren Foundation, and its Director, Paul Fejos, who had the confidence in me that something good would come out of this,

and it did. I had a lot to do with Africa over a period of years. I never sort of backed away from it, although later on, I would devote chunks of time to work in Spain, then in Turkey, and of course in Ethiopia, for a long period of time, which was a part of Africa. Before I went to Ethiopia, it was kind of an abortive trip in ’59, I didn’t know anything about Ethiopia. Little was known about Ethiopia. It was a different part of Africa, it was the horn. Although it’s Eastern Africa, you don’t call it part of East Africa, because it would have nothing to do with the British Empire or anything like that. Well I’m—that’s too long a comment in relation to what we’re talking about.

There’s one other thing that I should mention that I didn’t previously. I increasingly became involved, midway through my university days, in activities that were already professional, by going to meetings, annual meetings of the Physical Anthropologists Association, for example. Then to other invited meetings, where I was one of several probably junior people. The rest were all senior people, some younger senior people but nonetheless, and all the senior people. And these were things put on by the Wenner-Gren Foundation, at their headquarters in New York City, on 71st Street. There were summer seminars in physical anthropology of one kind or another, and there would be visitors there from overseas, depending on circumstances and serendipity and whatever it was. I was able then to be in New York, to participate in this give and take, although I was a student and they were all professors and things like that, and I was able to work at the American Museum of Natural History, extend my visits and take advantage of the Department of Mammals and Paleontology and all these kinds of things. I got to know people there on the staff and so on, and I was progressively being professionalized in this, and I eluded to the fact that this was enhanced further at the end of my doctorate, when the Wenner-Gren asked myself and another student of mine with me, to do this kind of background critique work about Africa, which went into their dossiers at their foundation, and they could use to make judgments about people and things and stuff like that.

I went to one very important meeting already, in 1950, and 1950 was really only the fourth year, I guess. We would say fourth academic year of my university. So somewhere, I was getting close to the masters. I was probably already writing some of what would be my masters thesis, which was about Neanderthal peoples. In any case, I went to a very important meeting at Cold Spring Harbor on Long Island, which was, for its day, an important center for research on evolution, genetics and things of that sort. This meeting was arranged jointly by one of my professors at Chicago, with Wenner-Gren Foundation encouragement, and people at this Cold Spring Harbor institution—it was called Spring Harbor Symposia on Quantitative Biology they were called. This one was on the origin and evolution of man, and the idea was to bring together the diversity of people, regardless of where they practiced their discipline, in a series of day meetings, in the morning and afternoon, sometimes a special lecture in the evening. I was among very few students who were there. There were young colleagues of mine who were new

PhDs, but very few. I met many people there that I wouldn't have met otherwise, or did I know previously from physical anthropology meetings, but outreaching of people from genetics, molecular biology, ecology, gypsophila genetics, some people in prehistory that I already knew of, some people in physical anthropology that I rarely met, because they were on the west coast and we never met on the west coast. So if they didn't come to the meeting, I hadn't gotten to know them. Extremely broad treatment of this field. All of these things were published within a year by Cold Spring Harbor, their own publisher, and all of the originals of these things were, for many years, a very distinctive red covered volume, about eight and a half by eleven, and they were done in a kind of quick way of setting up the type and so on, that they had mastered. This had a great impact in this country, and maybe England, because it included some people from England, and to a lesser extent Europe probably, but indirectly, it would affect Europe, because there would be people in Europe who would be affected by it if they knew about it, and there were actually several Italians, for example, at the meeting. This was a much bigger affair, because it was an invitational, and because it was so inclusive. The number of people, I don't remember any more, but the total number of people was well over 100. The number of actual people presenting and so on was well up in the dozens of people. I think that this was a major step towards professionalization again, and I happened to be present on this occasion, rather than being excluded from it. All of this being included, not excluded, certainly had a major impact on me, and people considered me as maybe a favored child or something in this regard—I don't know how you want to put that—just because people were mentoring me, and I was included. That's the way it was.

04-00:22:43
Redman: All right. I actually, I'm going to have several follow up questions.

04-00:22:47
Howell: Yeah, sure.

04-00:22:47
Redman: But I'd like to fix our sound a little bit, so I'm going to shut this off. OK, so we left off you brought up several interesting point, talking first about the Wenner-Gren Foundation, post doc in Africa, but then also, the history of your relationship with Wenner-Gren Foundation at some of the meetings in New York City, and the multidisciplinary nature of those meetings. So a couple of questions come out of that. With the relationships that you've then built with these individuals, studying, I mean things both in your field and things outside of your field, those are people then, that you've kept in touch with throughout graduate school via correspondence often, and just seeing them at—

04-00:23:49
Howell: Yes. Telephone, correspondence. We're talking about pre-electronic everything of course. It was a different world. So you'd interact with people at

meetings. If you had other reasons, you'd interact with them outside of meetings, through telephone or sometimes letters. With overseas, it was always letters, so I have, probably in storage and so on, letters that go back into the '50s, that are packed away in various places. Of course, you can't keep them in my lab and stuff like that, and that's because we had those kinds of records these days. You don't have those kind of records. Everything is email or something, and it has a half-life, and that's the end of that.

04-00:24:41
Redman: OK. So there were two other questions about the Wenner-Gren. You brought up, when you were talking about your trip to Africa, that a lot of the way things were paid for, that if you could compare it to the way someone taking a trip to Africa today, it would be vastly different.

04-00:24:59
Howell: Yes it would.

04-00:24:59
Redman: And then also you mentioned that at these meetings, there was a pretty fast turnaround for publication, for like publishing things within that year.

04-00:25:10
Howell: Well that was very true. The deliberate choice and structure of the Cold Spring Harbor Symposia was such that the symposia is held in '50, and it's published in '51. Papers were expected to be presented at the time of the meeting, and handed over before leaving the meeting, to the editor of the particular volume. They ran a very tight ship, and it's always been that way ever since then. If you went into the library, where you'd find all of these things, that's in the science life library in the building where I live at the university, you would find all the original, red covered volumes up to a certain point in this format. After that, they dropped down to a smaller format size, and their paper and their bound, and there are endless numbers of them. So that means that instead of having one or two such things a year, they may have a half a dozen or more a year. And of course for years, that's been directed, that Cold Spring Harbor, by James Watson of Watson & Crick, who has turned it into an extraordinary place. He's now emeritus and so on, but it is a remarkable scientific institution, much bigger than it ever was, endowed, and so on.

04-00:26:43
Redman: OK. And now—

04-00:26:45
Howell: Yes.

04-00:26:46
Redman: —a couple of other questions about Africa. You mentioned meeting some of the big names, including you mentioned Louis Leakey. Was this your first time interacting with Louis Leakey?

04-00:26:57
Howell: Yes. It's really the first time. He was briefly in London, when we were having our meeting in London, and I saw him briefly. He attended maybe one of the meetings in London in '53, but when I really met him was in Kenya in 1954, and Mary Leakey, and of course, the boys, the family, the three boys.

04-00:27:22
Redman: OK. I mean, was this the start of a lifelong relationship?

04-00:27:26
Howell: Yes, yes, we became—I became friends with Louis and Mary Leakey, and ultimately cooperated and worked with Richard Leakey much later in time and so on, in the '60s and '70s. We were close friends. Louis used to stay with us in Chicago, at our house, when he was in the States and in Chicago. Sometimes I would introduce him at lectures and whatever things, so yeah.

04-00:27:54
Redman: And you found working with the Leakey family in general, you found them—so it seems like you found them both personally engaging and friendly.

04-00:28:06
Howell: Oh yeah.

04-00:28:06
Redman: And intellectually stimulating?

04-00:28:07
Howell: Yeah. They were accepting of me, and I was—it was always shop talk and things like that, of course, which is what interested me; the experience that people had, the life they had, that I could learn about from sharing with them and so on. So it was sort of Africa in depth through them and so on, and that's been more or less true of others, not so much so, that I've known in Africa. They were especially so, because it was for such a long period of time.

04-00:28:47
Redman: OK. Now, and tying that in with one interesting you said about the Africa trip, is that it gave you access to unpublished ideas and materials—

04-00:29:00
Howell: Yeah, sure.

04-00:29:01
Redman: —in talking with people. So was that—eventually, as you get to know people that are doing this work in Africa, and the fact that as you said, Africa is still considered sort of a dark continent at this time, and not a lot is known about the fossils and the finds that are coming out of Africa. So were people starting to use each other as sounding boards for ideas, and were they willing to talk to you about ideas that they weren't ready to publish?

04-00:29:33
Howell: That's a good point. I hadn't thought about that before. Well, the first thing to say is Africa is really a huge place, and just using the term Africa as opposed to Europe or something like that, can give very much of a misleading kind of color to something. I think what you have to think of about Africa is its vastness from north to south. Many degrees of latitude. That it's of different ages in terms of settlement, occupation and so on, with South Africa being older in a number of ways because there's Dutch, early Dutch settlement and so on. Far north, along the coast of the Mediterranean being very different, because it's always been part of the Mediterranean Basin, and so places like Tunisia, Algeria, Morocco, Libya, et cetera, Egypt of course, they're part of a very special part of Africa that has nothing to do with Africa south of the Sahara, truly in a way. So also, you have the difference between the French part, which is in the north and the west, all French, culture, French language, French science, French everything, and you have the part of both South Africa plus Eastern Africa, where it's English, English Colonial, and colonial in a very big, far flung empire, and run in a very different way. The question is, how do these people get together? In 1947, actually the year that I began going to university after military service, actually a meeting of Pan-African Congress on prehistory was organized by Louis Leakey, in Nairobi, and people came from all over for that meeting. It was published about four years later, I guess, a very much edited down, brief. Not long papers in a proceedings volume, but in a barely published form, with very short contributions, but you can get an idea of the diversity of people from south to north and from east to west; different languages and so on. That had a—that set a tenor, thanks to Louis Leakey and subsequently, other people picking it up, for other such meetings of the Pan-African Congress on prehistory. Later it became on prehistory and quaternary studies or if you wish, ice age studies.

The second one was held in Algiers, for example, in '52, and then subsequently, in the Canary Islands, under Spaniards and under—in the Belgian Congo, '59 in Kinshasa, which used to be called Leopoldville, et cetera. It's still going. It skipped some years. It's gotten bigger, it's gotten more diversified. It's gotten more inclusive in the sense that it includes things into the later prehistory, proto history, almost history of Africa. That's inevitable and so on, but Louis Leakey recognized this need, and anyone who knew much about Africa or to have tried to cross it or to cross it from east to west, I mean it is a huge distance. If you tried to fly from Jabudi on the Red Sea, to Lagos or something like that in West Africa, is a huge distance. If you go from north to south, and you want to drive from the Cape to Cairo, on the famous Great North Road, as the British called it, you're talking about a lot of driving and a lot of trouble, and there is no assurance that there's continuity between this strip of track and that strip of track, wherever it may be. I mean, there's nothing in between. There's no blacktop highway. Communications is a very major problem, and that would still manifest to—that was certainly manifest when I first went to Africa, and up until some years. It's less so now, depending on where you are, but of course the infrastructure in Africa has

deteriorated unbelievably outside of colonial powers. I'm sorry to say that's the case, but that's the way it is, it's true. So there's a real problem of communication and so on.

Now people did share many things with me. These things included access to archaeological collections that had been made and never analyzed, or things that were under study. So I did a variety of things. I did quite a bit of work when I was in Pretoria later, on fossil, australopithecine fossils that were being discovered and reported, and taken out of the ground by Robert Broom and later his assistant, John Robinson. I became a friend of John Robinson's later. He was invited to Chicago for a term, to teach in the States and so on, and he was doing his doctors dissertation on the dentition, as I remember. The dentition of Australopithecus species. And so some things I was able to—I was able to study almost everything there, and some things I was able to study less because he was involved with them more. But in general, he was fairly understanding, and I spent three and a half weeks there. So I was at the beginning of—well he'd been there since 1949, when he had come from Cape Town as a zoologist. I was there at the time when C.K. Brain was beginning all his work on the cave fillings, and what it might mean as to how they got filled; the composition of the bone, the composition of the fauna, as indicated by the bone, and the sedimentary conditions leading to the infill of these carstic cavities or how the sediments got in them and how the bones got in the sediments, and all this sort of thing. He wrote a wonderful thesis later, which was published by the University of Chicago Press actually. So I was quite—everywhere I went, something was happening in different ways. Sometimes there were younger people, like these people. In other instances, there was—they were few on the ground, and there was a few other people, functionaries, guys who were doing archeological things under the bureaucracy of the government for permits and protection, protection of sites, so they wouldn't be raided and so on.

04-00:38:33
Redman: It sounds like an extremely exciting period, but was it also a bit overwhelming too, to be introduced to all of these different new concepts and ideas, or do you feel as though you had such a grasp of the literature and background that you—I mean, it was really just a lot often, probably meeting people that you had read about.

04-00:38:53
Howell: Yeah. I think it was very much the latter, and it was, I mean there would be things that are new or things that I knew less about, and I discovered there was more to know. But no, all the preparation and so on, that I had been involved with, with this work the last year of my doctorate, was exactly in preparation for this, but little did I know that at the time.

04-00:39:29
Redman: Now—

04-00:39:29
Howell: So that made it vastly easier.

04-00:39:31
Redman: OK. The last question I'd like to ask about that trip is speaking about museum collections, and you mentioned that working with the different museums was quite different. From my understanding, you worked in museums later on, in later decades in Africa.

04-00:39:52
Howell: Oh always, yeah.

04-00:39:53
Redman: And obviously, you had experience working in places like the Field Museum and the American Museum of Natural History.

04-00:39:59
Howell: Right.

04-00:40:00
Redman: And in museums in Europe. If you could, could you compare what your experience was like with these museums a little bit earlier, versus then going to Africa and working in these new institutions. How they compare, both in terms of their collections, and in terms of the bureaucracy and physically working in those institutions.

04-00:40:22
Howell: Yeah. Well, the knowledge that I had of American institutions was really the Field Museum on the one hand, the American Museum on the other hand. Later on, '53 and later, the Natural History Museum, the so called British Museum of Natural History, London, along Cromwell Road. I spent a lot of time there, repeatedly. In Africa, it was museums in different ways. There were formal museums of one sort of another. That would include, for example, the South African Museum, which was founded in the—earlier than maybe after World War I, in Cape Town, and which is a natural history museum of antiquities and fossils, reptiles, living—representative of living organisms, including things from the sea, because Cape Town is on the sea, Cape Point, and so it was a formal museum with usual cabinets and cases. Not so much exhibition in most of these, but a limited amount of exhibition. That would be true also of the Coryndon Museum, established under Coryndon's name in Nairobi. There was a formal museum, the National Museum at Salisbury in Southern Rhodesia, now Zimbabwe, which was mostly for all kinds of natural history, plus some archeology and prehistory there. At another museum, a local museum in Bulawayo, also in Zimbabwe. Then you'd go to others, they would be makeshift things, different kinds of building, bit kwansit hut like buildings or whatever. That would be true in the Katanga, at Elizabethville, where one particular man, his name was Cabu, C-A-B-U, Cabu, had devoted himself to things archeological and so on, in the Katanga, which is Southeastern Belgian Congo, or Copper Belt, like the

Copper Belt across the border in Northern Rhodesia, and he had built this into a big kind of museum, very simple. Simple cases, simple storage, simple this, hardly any exhibition or explanation of things that are on exhibition. But you know, it was a place to house things, and to keep them from vanishing really, and of course to give access to other people.

An outstanding museum for its time, of course, was the Transvaal Museum in Pretoria, and the Transvaal Museum is an older museum, going back to the earlier part of the twentieth century, maybe before that, and it has exhibits, quite a few exhibits, although the South African government, then being strongly against evolution, was not able to explicitly deal with evolutionary matters, although they were talking about fossils that have to do with human ancestry. So it was very enigmatic. There was really a thing for—like a museum, I would expect, in Europe, in this case Africa. Later on, I would become somewhat familiar, but never in detail, with museums in North Africa, Tunisia mostly. I've never been to Algeria, and when I've been to Morocco, it was not related to my work, it was related to a holiday or something, so I can't say. But there are museums in Rabat and in Casablanca, and those are government.

04-00:45:22
Redman: OK. Were the collections at what you described as the formal museums, or the larger institutions, were they necessarily more exciting than what you were looking at, at the local museums, or kind of the makeshift museums? Was that necessarily the case, or was—

04-00:45:37
Howell: Well that could be.

04-00:45:38
Redman: Were there—sometimes, were there just fantastic things in this little makeshift museums?

04-00:45:42
Howell: No. I think the main thing that might be present, is that there would be something that is of local provenance, and that's, if you want to see it, that's where the collection or the representative collection or the original collection happens to be kept, rather than moved to the capital or something like that. Most of these places are the capital, a small museum, but very well done and so on was the Rhodes Livingston Museum in Livingston, Northern Rhodesia, that Desmond Clark had built over a period of years, since he went there in 1938. Very nicely done, a lot of effort there. Although you're in the middle of the provinces really, you know, few towns, few and far between major settlements. You're in a different kind of universe and so on, so the quality of a museum in let's say Pretoria, Transvaal, couldn't be compared with a museum in Livingston or Lusaka, Northern Rhodesia, et cetera. The museums in Uganda were—a major museum was at the geological survey in Entebbe on Lake Victoria, not in the capital, Kampala, which is where the university was.

Makerere University was there, but the museum was related to the geological survey and so, if you wanted to see artifacts and so on, you would be looking at stones, and stones were in the province of the geological survey.

04-00:47:42
Redman: OK.

04-00:47:42
Howell: That was kind of a make do thing, but it depended on how much effort was being put into taking care of what was there already, or adding to what was being done because of a particular ongoing research project, like the one I visited in the field, on the Kagera River.

04-00:47:58
Redman: OK. And now, the other thing that you spoke a little bit about was seeing Isimila for the first time, and you said it was a very rare site, and you were very provoked. And later, you would return to that site, in ’57 and ’58, and we’ll talk about that more. You said you were struck by it right away.

04-00:48:24
Howell: Well it’s very—the description was published and I had come across it in South Africa. Its attention had been brought to me by a man in Johannesburg, who was a self-made pre-historian. He was an engineer actually, on roads and things like that, and he got sidetracked and became ultimately, a government functionary and dealt with things archeological and so on. A very nice man named Clarence van Riet Lowe. Anyway, very friendly, a very fatherly figure and so on, a good guy. He called to my attention, he said well you’re going up north eventually, here’s an article in the newest *South African Archeological Bulletin*, which I’ve been a subscriber to since it began, by the way. Anyway, here’s an article and some pictures, and some pictures of artifacts and so on. It said pretty clearly where it was, and so when I eventually made this trip into Northern Rhodesia and worked my way up with an assistant of Desmond Clark’s, to the north, and then eventually crossing into Tanganyika, at the border, and then driving up north and so on. We knew that it was just to the south of the town of Aringa, which is several hundred kilometers up from the border, in what’s called the Southern Highlands part of Tanganyika, and you can see it from the road, because it’s a big area of erosion. A stream crosses the road under a culvert, and you look up and there’s hills and so on, and the edges of the hills show erosion. So you can see this erosion very clearly, with beautiful colors of reds and browns and russets and so on, and soil formations and so on. Before we ever drove into Aringa, and tried to find a place to stay, we just walked up the stream, towards these low hills. Then you come to this area of erosion, and over this area of erosion are just tens of thousands of stone artifacts and so on everywhere, everywhere. It’s very easy to be overwhelmed and, you know, and disturb this. Anyway, so we stayed, I think two nights in Aringa, at White something inn hotel, an old, colonial kind of hotel, and spent a whole day and a quarter or something like that, at the site, wandering around. I made photos and some notes and things like that. At that

time, I didn't think I had—I mean, I have no memory of thinking ahead, that this is something that you are going to do in two years.

04-00:52:14
Redman: So you were taking these notes thinking oh, I'm going to apply for a grant and aid.

04-00:52:20
Howell: Well I don't think—I was mostly just kind—of course, I wrote an article about the site.

04-00:52:26
Redman: OK.

04-00:52:28
Howell: After I'd been there, and it's probably—it's also in the *South African Archeological Bulletin*, and had some pictures and artifacts and things like that. But I'm not sure that I really was 100% set on doing an archeological thing. I was really not trained as an archaeologist. I was trained as a naturalist, and more in the area of the bones and so on, and not the stones so much, et cetera. But as time went by, within two years or so, I had decided that Isimila had to be dealt with. I had the first two students that I had, who would be appropriate to do this work, and they had experience, a lot of it. One came from Oregon and the other came from Wyoming, and had part of her training at Tucson in Arizona. It was a natural for us to think of doing this, and we raised money to do this from the Wenner-Gren Foundation and from the National Science Foundation, and took a long time off. I mean, we took at the end of the spring until the end of December, and we didn't come back until January really. The two students stayed in Africa and worked there the following summer actually, to do more extensive, or to finish up excavations that needed "finishing" and so on. Anyway, so I became a kind of archaeologist in a way. I would say what I became is a kind of field person, doing things that were archeological, with the help of archeologically trained people, and I've done more than some of that in the past. In the process, I've learned a lot, so I really know what's going on or what ought to go on, if it's not going on, and what's the best set of choices to make and so on. Although I may not have a lot of firsthand, hands on experience that's truly intensely archeological, but it's been like this, that I mentioned at Isimila. Isimila was a remarkable place, because the only other such situation that was analogous to this, was found during World War II, by Mary Leakey, south of Nairobi, near Lake Magadi, at a place called Olorgesailie. It became, after the war, and in due course, an area that was kind of enclosed off. It was not far off the main road, down towards Magadi, and it's a place where exposures were extensive, widespread, often very open, that is not vegetation covered, and in which artifacts were just unbelievably abundant. The Leakey's put a series of walks over parts of this big set of exposures. They took limited parts, and they raised these up like a catwalk, so you'd walk over them like you'd walk over an elevated sidewalk in Alaska, so you wouldn't be in the mud. In this case, you

wouldn't be walking on the surfaces, with all the stones and bones and stuff coming out. I saw Olorgesailie when I visited, in my 1954 trip to East Africa. I went down there one day, maybe with both Louis and Mary, but certainly with Mary. I don't remember. But anyway, so I knew about that. Not much had been published, brief things, but I knew the kind of circumstance that this was. This was a great concentration of materials from human habitation and so on, and if you could get things still in place, through excavation and so on, not derived from erosion, there was great promise. So when I saw Isimila, first in print, and then I actually see it on the ground, certainly I was encouraged to think that would be a worthwhile thing to do, although it's not exactly what you're trained to do in this world, but it's very likely what you ought to do, everything being equal, since you're in this position. You know, you'd better do it, and so I did.

04-00:57:40
Redman: OK. I'm going to switch tapes.

Begin Audio File 5 Howell_Clark5_02-16-2007.wav

05-00:00:06
Redman: All right. My name is Sam Redman, and I'm here with F. Clark Howell, and this is our second tape for today, February 16, 2007. When we left off, we were speaking about your first experiences seeing the site of Isimila, and realizing that someone needed to excavate the site, and where you didn't feel you maybe had exactly the background to do it, the situation had presented itself, and it was something that needed to be addressed for the general understanding of the field.

05-00:00:43
Howell: That's right.

05-00:00:44
Redman: And I'd like to get back to Isimila a little bit later, but I would like to talk—you returned... After your first trip to Africa, you returned to teach a little in St. Louis, at Washington University. Also, you met your wife, was it before you left for Africa?

05-00:01:06
Howell: Yes, I did.

05-00:01:08
Redman: Can you tell me a little bit about how you guys met?

05-00:01:11
Howell: We met through a blind date, (laughs) through a colleague in anatomy department, who was dating her sister. It came up one time and so, a double date was arranged, including Betty for me. We started dating a number of times before I went to Africa, and then it was interrupted by being gone a long

time to Africa, and then it sort of renewed itself, somewhat painfully, after I returned, and then eventually we were married in June of ’55.

05-00:01:52
Redman: OK. So things progressed pretty quickly then, after you came back.

05-00:01:56
Howell: Yes, it did, and we moved from St. Louis to Chicago, to the Department of Anthropology at Chicago in August of ’55.

05-00:02:09
Redman: OK. Could you speak a little bit about that. Were you seeking to move from St. Louis at that time, or did Chicago come to you to offer you the position?

05-00:02:21
Howell: Yeah, they really came to me. I went there. I think I gave a talk or something, and I think the issue came up that he might be able to add to the faculty. It’s a small faculty, really about six people, and they were thinking of growing, and maybe in physical anthropology and would I be interested and so on. I said, “Well I guess I would, yeah.” Since it meant I’d be, in theory, closer to the center of my—in theory, I could be closer to the center of my discipline, than if I was in an anatomy department. Actually, that doesn’t necessarily follow, as I’ve thought about it over the years, because we have many people who have spent their lives, as it happened, not in an anthropology department, although their degree is in anthropology, but they’re in an anatomy department and their major responsibilities have to do with the teaching of human anatomy and things, and research can be on whatever it’s on. That’s true of some of my own people actually. It was an opportunity to return to something I knew. I suppose I was flattered. I certainly was eager to get out of the university and that university, where once I got a PhD, I wanted to get out, you know. So I mean it was a damned if you do, damned if you don’t. Eventually I ran back. So anyway—

05-00:04:12
Redman: Yeah, so another question about that. I’ve heard, from time to time, just talking to academics about returning to the institutions where they’ve gotten their own degree.

05-00:04:25
Howell: Yes, absolutely.

05-00:04:28
Redman: Sometimes that can be a bit awkward in the transition from being the student to a real colleague.

05-00:04:36
Howell: That’s true.

05-00:04:37
Redman: Did you experience any of that?

05-00:04:38
Howell: I don't think I really experienced that. I was pretty much accepted and of course known by the people in anthropology, as well as other places in the university, because I had been there through my whole, you know, university years after the military, until I got a degree in '53. So, a kind of small department, the department is small. I knew everybody and so on, and after I was there some years, from '55 to '58/'59, the major person behind bringing me there was Sherwood Washburn, who had been one of my major professors, and he had left to go to UC Berkeley. So some of the pressures that I might have felt from him, if anybody was going to give me this or that, he was mostly encouraging and friendly, were removed, and I knew all the other people. So I was kind of on the ground floor, and I was promoted relatively quickly. I'm trying to think of exactly the timing.

05-00:06:15
Redman: It looks like you were an assistant professor from '55 to '59.

05-00:06:20
Howell: That's it, '59, I was promoted.

05-00:06:21
Redman: Then from '59 to '62, an associate, and then promoted to full professor in '62.

05-00:06:27
Howell: Yeah. I had, in each instance, there were inducements to leave Chicago to go somewhere else, et cetera.

05-00:06:37
Redman: Oh OK.

05-00:06:38
Howell: I took advantage of several such instances, and they promoted me at Chicago, fortunately. I was asked to go first to Yale. Subsequently, I was asked on several occasions, to go to Michigan, and finally, I was asked to be the head of the biological anthropology and other programs in the Natural History Museum at the Smithsonian, and that's when professorship came through for me.

05-00:07:19
Redman: So these are of course—those are major institutions that are giving you these offers. Were you looking to apply for any of these positions, or were these people calling you?

05-00:07:30
Howell: They all came to me.

05-00:07:32
Redman: OK. And so each time they came to you them, you would go back to the department and show them the offer.

05-00:07:41
Howell: Or I would get into a discussion with somebody about it, or they'd say, I've heard that, is it true that, you know, that kind of thing. The formality of formal applications for positions, on the advertising of positions, and all those kinds of things, didn't really come until after the '60s. That was formal, and after that of course, it was a different world. I mean, it wasn't a world of somebody calling you up saying, do you want to leave and come there, because there would be a whole line of people and you'd have to be chosen to get to the short list, and all that kind of stuff. Much more politics and so on, a very different kind of thing, yes.

05-00:08:28
Redman: And so managing the politics at Chicago for you, they were manageable, but they were—I mean, did you feel like you were treading lightly in mentioning in conversation that you had another job offer. You weren't necessarily trying to force your way into—

05-00:08:48
Howell: I can't remember now, but I certainly used the opportunity to—I didn't let it go by. I thought it was not a good idea to do that, and if I was thought well enough of and was productive enough, they could consider me for promotion, even though it might be a year or so ahead of time. Due to circumstances, in the end that's what happened.

05-00:09:20
Redman: And obviously—well were any of these—I should go back and ask, were any of these other job offers particularly tempting? For instance, say with the Smithsonian, was that particularly attractive?

05-00:09:34
Howell: It would have been attractive in a way, because it was a museum. It wouldn't involve teaching. It would have been a government, civil service position, about which I knew very little, but you could find out a certain amount, and so you knew what that meant in terms of retirement and promotion and those kinds of things. I would have had a very good chance there for the job, and probably a relatively senior, upper mid level or lower upper level position, because the acting director or the director of the Natural History Museum, who had been a head of this program for many years before, was a good friend and liked me a lot. He had actually visited me once in Spain, when I was working in Spain in the field, and I'd visit him in Washington when I was at the Smithsonian or whatever, and we'd have lunch always and so on. So I had a natural inclination, but in the end, the thought of living in Washington and so on just did not ring a bell for either Betty or I. The one place that Betty was interested in moving, strangely, was Michigan.

05-00:11:03
Redman: To Ann Arbor.

05-00:11:03
Howell: Ann Arbor. I was not that overwhelmed by Ann Arbor, but she thought it was favorable, compared to what we were doing in Chicago. As a consequence of that, we actually moved from town, Hyde Park, et cetera, to suburbs, in ’59. Our first house was in a little suburb called Homewood. It was on the Illinois Central, and we had that for some time, and that changed that sort of business of being around Hyde Park, wondering how safe it is and all those kinds of things. That began to be put aside, until a chance came much later for me to—well, the Smithsonian, and then much later for me to go somewhere else, and then CAL at the end of ‘60s. I didn’t think about such things. I just took things the way they were and the way they were going on and so on. You know, I didn’t have any complaints.

05-00:12:24
Redman: And actually, I’m looking at 1956 here. So you were actually back at Chicago, and you would be teaching a little bit. You mentioned that you’d seen job offers from the Smithsonian, maybe as attractive for at least the fact that the teaching responsibilities wouldn’t have been there.

05-00:12:46
Howell: Right.

05-00:12:46
Redman: So it seems clear that you were very much more interested in the research side of your position at Chicago than the teaching side.

05-00:12:56
Howell: I always was.

05-00:12:56
Redman: OK. What were your teaching responsibilities?

05-00:13:00
Howell: Well normally it was minimally, two courses a term, and we were on quarters, but you could do as much as two one term and two another term, and maybe only a seminar the third term, and joint teaching counted as one. So if you’re joint teaching with three other people, each of you gets count for that. There were major things that we did like that. Most of my teaching was at the mid level or the seminar kind of level, and then I participated in large—we can’t call them survey courses, but it was like a survey course. You have to remember that Chicago had small enrollments and so on, and we had only 10,000 students, counting the law school, the business school and the hospital, or the medical school.

05-00:13:53
Redman: So were those more like—were they introduction to anthropology courses or introduction to physical anthropology courses?

05-00:14:01
Howell: They were topical courses mostly. The bigger courses would have been the ones that were jointly taught with maybe anywhere from three to five people teaching them, and rotating through them as you got into certain areas. Some people would drop out, another person would come in to teach and so on, not only from anthropology, but from the Oriental Institute, from Oriental languages and literature, from linguistics, different kinds of things, and this would be one major introductory course having to do with prehistory called The Human Career. That's the same name that Richard Klein adopted for his book, which is based on that kind of course, which he taught at Chicago after I left and so on.

05-00:14:49
Redman: So the topical courses were those courses that you were choosing based on your interest at the time—

05-00:14:56
Howell: Yes.

05-00:14:56
Redman: —or were you teaching courses about say Neanderthals or?

05-00:14:59
Howell: Yes. It would be things about various aspects of human evolution. I would pick this subject, that subject, this seminar, that seminar, and some things were done with other people. In the seminars for example, with people in dental anthropology and other fields in paleontology. People would cross over from other fields to come; from zoology, anatomy and so on, would come over for these courses and so on.

05-00:15:29
Redman: Did you feel like you were—and we spoke earlier today about your interaction with people in other disciplines at Cold Spring Harbor, and with the Wenner-Gren Foundation, and then also we talked about your interactions at conferences during your graduate career. Did you feel more comfortable, do you think, than some of your other colleagues, going over to say the zoology department and knocking on someone's door, calling somebody up and saying, you know, I'd like a colleague of mine to teach this part of the course.

05-00:16:00
Howell: Gotcha.

05-00:16:02
Redman: Was that something that was—

05-00:16:03
Howell: No, I never backed away from that. No, no. I was always comfortable to do that. I've done that all of my career actually, whether here or Chicago or whatever.

05-00:16:12
Redman: OK. Do you think that that was maybe slightly different than some of your colleagues?

05-00:16:16
Howell: Yes, I think so.

05-00:16:18
Redman: OK.

05-00:16:18
Howell: Some people really jealously guarded their particular space, and they didn't necessarily want anybody else around that was utilizing their space and their time and so on, and I was just the reverse of that. I thought the interesting thing was the different perspectives that people have, different backgrounds, and that's very healthy.

05-00:16:44
Redman: That must have given you, in some ways, an interesting reputation on the campus, as somebody who was happy to cross over.

05-00:16:53
Howell: Maybe so, yeah.

05-00:16:53
Redman: Yeah, and happy to talk to other people in other disciplines.

05-00:16:56
Howell: Yes maybe so, sure.

05-00:16:58
Redman: Were you getting a pretty warm reception from some of the colleagues that were willing to cross over?

05-00:17:04
Howell: Yeah I think so. Sometimes, sure. The thing is, everybody's busy and everybody has their own idea about what they're willing to spend time at. So if this is something that's rewarding for people and so on, then they'll participate. If it isn't rewarding, if it's a one way street, then they're not so keen to participate. But it's a good thing for all the students involved, there's no question about that.

05-00:17:28
Redman: Do you think, and maybe you could speak a little bit about your experience, specifically at the same institution, and whether or not there were some shortcomings in your entire span of your education, that then you were maybe working to counter, that you know, maybe—were there moments where you were saying you know, I really wish that I would have been able to get a background in X, Y or Z?

05-00:17:54
Howell: Oh yeah.

05-00:17:56
Redman: You'd be able to do that.

05-00:17:57
Howell: Yeah. I've always been aware of the things I don't know or the things that I know enough to wish I knew more about, and their relevance and so on, and also the way science has changed and changing. It's a process. It's not a bunch of absolutes and so on. So I was always aware of that, keenly aware and actively engaged in bringing reality to this awareness, that is dealing with it through associations with other people, through reading, tracking things down, learning about things that I might have missed. All those things, I've fed on, absolutely.

05-00:19:00
Redman: Then in 1956, you participated in the International Neanderthal [Centenary Congress?] in Düsseldorf in August of 1956. You were invited to that, I'm assuming based on your work and your dissertation and some of your early publications. People knew that you were interested.

05-00:19:23
Howell: Some people did, yeah.

05-00:19:25
Redman: OK. Was that a very large—

05-00:19:29
Howell: Oh it wasn't so big. It was published in a book in '58, and I knew the organizer, Ralph von Koenigswald, who did a lot of work in java, and although he's a German originally. So this was a meeting just for that, the 100th anniversary of the discovery in Neanderthal Valley in '56. And by the way, just last summer, I attended the sesquicentennial, and that's the 150th year. That was held in Bonn, Germany, and I was a plenary speaker the evening before. The actual meeting began the next day. The evening before was an introductory evening at Landesmuseum Bonn, of an exhibit having to do with lots of things about Neanderthals and fossils and sites and artifacts, and all kinds of things. I gave a talk. I gave a plenary talk. I had about 250 people, I guess, listening and so on. It will be published in the transactions of the Neanderthal Centenary, I think the end of this year; '07 probably it will be published. There was a big meeting, quite different from the one of 1956.

05-00:21:02
Redman: So significantly larger?

05-00:21:03
Howell: Yeah. The big thing about '56 was that I used that summer... My wife and I had been married for a year. We were gone overseas from May until maybe June I guess, June until September, and we were in England first, then we

were in France, with my wife's sister and her husband part of the time. Other time, I was working on fossils in Paris and other places, and we were also in Switzerland, Italy, Austria, Germany. We used this to go and study collections and museums and so on. All were relating to things Neanderthal, broadly speaking, and I did more of that later on, although progressively, my interests have moved away from Neanderthals and into other areas. I never abandoned them utterly, but I never pursued certain things or places ardently. When opportunity offered, I would work on things. Otherwise, I wouldn't do that. So that summer ended up this very big—for its time, very big, not subsequently.

05-00:22:37
Redman: OK.

05-00:22:41
Howell: I met the founder of the Wenner-Gren Foundation there actually, Axel Wenner-Gren, who was a Swede, and then who had become indebted to Paul Fejos, and at a certain point afforded funds to establish this foundation, which has become a very important foundation for anthropological research, a very important funding agency.

05-00:23:08
Redman: Were you ever—you spoke about meeting Wenner-Gren. Were you ever intimidated working with donors? I've spoken to some academics, who have told me that they have no problems conversing with other academics, and speaking in front of popular audiences isn't a problem, but sometimes working with donors can cause people anxiety. Was that ever the case with you?

05-00:23:33
Howell: I suppose it was, but then became a non-problem, because in 1968/69, I became involved with what would become the LSB Leakey Foundation, and that was strictly people who were lay people, and who had more or less money. So by that time, I would have been over any other kind of obstacle that might make it difficult for me to interact with them. Also, when I was at Chicago, I got to know people who were trustees of the university, as it happened, and that made it easier for me to talk to somebody who's in investment banking or somebody who is in publishing or whatever the fields are. But initially, it would have been foreign to me, absolutely.

05-00:24:44
Redman: It looks like in 1956, that was also the year that you became one of the contributing editors for *Encyclopedia Britannica*.

05-00:24:51
Howell: Yeah, I did.

05-00:24:53
Redman: Could you speak about that experience a little bit, because that's—from the looks of it, it was a bit different for you, but something that you've kind of continued with.

05-00:25:03
Howell: Well they set up a board of—they expanded their Board of Editors, I think, as I remember, and they changed some things and so on. I was added to the board. I think we were paid—I want to say we were paid $300 a year, but I also want to say that it was more than that for a while, and it might have been. Anyway, and I did certain kinds of things that had to do with the structure and the coverage and the representation of paleoanthropology, prehistory, early man studies, human evolution and all that stuff, with Britannica. Britannica was then owned by a trustee of the university. It had been bought from Sears, Roebuck & Company, which owned it. I did that for some years, even after we moved here. Later on, the amount you made was zero ultimately. You got a set of the Britannicas. But then I did certain things subsequently with Britannica, having to do with the treatment of certain fields. So you know, I left my imprint at different times through this. I also had something to do with the Institute for Philosophical Research in Downtown Chicago, which was something run by Mortimer Adler, when I was there in the late '60s, and had meetings with them on different ways of going and so on. This is a little difficult. Adler was Jewish, who had become Roman Catholic atomist, and it was almost essentially impossible to talk to him about evolution and materialism and those kinds of things, it was amazing. (chuckles)

05-00:27:18
Redman: Let's—actually this—

05-00:27:20
Howell: That's just an aside.

05-00:27:21
Redman: No but that's, I mean, maybe a good way to—because you mentioned, in South Africa, talking about evolution in museums was difficult, if not frustratingly impossible. Then you were talking about writing about evolution and topics surrounding human evolution for *Encyclopedia Britannica*, and then working with people in Chicago that their religious beliefs impeded these ideas. Can you talk about in the 1950s, speaking about evolution meant something to you then and then now, I think that's incredibly pertinent to some of the debates we're having today. If you could speak a little bit about how things compare and how things contrast about the study of—being someone who studies evolution, and then presenting those ideas to a broader audience.

05-00:28:18
Howell: Yeah sure. Well, when we did the Evolution After Darwin meetings, which were organized by Sol Tax and a consortium of people at the University of Chicago, for 1959. Evolution came right to the fore. Religious people, people

that we would call Evangelical Christians, they were all Christian, and they were various shades of Evangelicalism in those days. I gave almost no attention to that because of the meetings, and they were so interesting and we had visitors coming and staying at the university. I shared an office with Julian Huxley, for example, for three months, like this room and that room; the doors in between open, and it was Clark and Julian and so on. It was important in my life and so on, and the meetings were important, and they were published promptly, in three volumes, including discussions in them and so on. But we had people who came as a consequence of those meetings, and they attended some of them. In the years that followed, in the '60s, I became part of a speaker group from the American Anthropological Association, to go to various places to lecture to small colleges, junior colleges, parochial schools, in especially Iowa, Illinois, Wisconsin. Anyway, I'd be gone for three to five days, and sometimes there would be somebody there who was teaching something anthropological. Sometimes I would give a general lecture in a gym, where everybody would be there and so on, and I would be able to talk about evolution. Sometimes I was told not to talk about evolution and so on. Oh yeah. It was an interesting experience, in the sense that it really took me into a kind of grassroots situation that was really different from anything done in an institution of higher education that I would know something about, where there was "no holds barred" as to what you could think or what you could say. And that was certainly true at some places in these particular places that I went. I'm thinking especially both Southern Illinois and several in Eastern Iowa.

05-00:31:58
Redman: Were you basically—did you have lecture A and lecture B in your back pocket, in case something—

05-00:32:03
Howell: No, no. Mostly I kind of free wielded, but I had slides and things like that, and I tried to communicate basic things. I tried to use the status of evolution as a major concept, a unifying biology in the natural sciences and so on, which had to include humanity. That's the way I went about it, and then I would talk about the specifics of what you find at a prehistoric site like Isimila. What were Neanderthals really like? Do we know anything about Neanderthal infancy and how they grew? All those kinds of things I would talk about, and that would depend on—sometimes I would have what would be like a seminar, and students would come from maybe two classes. They'd come together around a table and I'd say we'll talk about whatever you'd like, have questions and so on, and we'll take off from the questions, and some of those were very interesting and fun.

05-00:33:19
Redman: OK. And so sometimes you received a warm reception, and sometimes maybe was it a cooler reception that you were getting?

05-00:33:26
Howell: Oh yeah, I think so.

05-00:33:29
Redman: Was that exciting? Was that exhilarating?

05-00:33:33
Howell: I took it the way it was, so I just dealt with it as it was. If it's completely over the wall and just hopelessly ill informed and so on, I would normally try to dismiss that and go on to somebody else, where you could actually discuss the matter. I mean, if you can't even discuss the matter, what are you doing there? There's nothing, no point, and if you don't want to learn about that, you know, then that's too bad for you. I was paid to do this. I've forgotten how that worked, but anyway, aside from expenses. Some expenses would be taken care of by the college when I got there, motels and things, meals. Other times I had a car rental and sometimes I didn't, buses. Sometimes I used our car from home, which left Betty without a car and that wasn't good. But anyway, my point was to try to carry the torch a little bit under this time. It was an extension of what impact there had been as a consequence of the Darwin Centennial, and it seemed a very worthy thing to do aside from any emolument that I might get. Plus I'd learn how to do things that I hadn't done before. I learned how to deal with different audiences and so on, and it's amazing how much difference there is between people who are only 15 or 20 years apart in age. That was phenomenal to me.

05-00:35:30
Redman: Do you feel like—so that's a responsibility today for modern or contemporary scientists, and it seems as though you've continued that type of work in various way, in doing things like working with people in exhibits for a museum exhibits or working with popular publications. Do you feel like that's an important aspect of being a scientist, is sort of work with all these different types of audiences?

05-00:36:00
Howell: I think it's definitely a worthy thing to do. The question is when you're going to do it, the extent of what your involvement is, how much control you have. I have felt that—so I invested in, for example, the Man Hunters series, which was a first major TV thing having to do with human evolution.

05-00:36:34
Redman: And that was nominated for Oscars—

05-00:36:36
Howell: Yes it was.

05-00:36:38
Redman: —and Emmys, is that correct?

05-00:36:39
Howell: Something like that.

05-00:36:40
Redman: OK yeah, so it did quite well.

05-00:36:42
Howell: Yeah. And then I worked with Time Life for the Early Man, for a volume in the Life's *Nature Library* for the Early Man volume, including the young readers edition and a revised edition of the regular publication, which I thought was an important thing, both in my career and at the time, and the way we were beginning to think about things and so on. We don't think about things exactly that way now, but you wouldn't expect them to. But this is part of an experiment in thinking and the changing of fields, and the emergence of fields and the emergence of disciplines and cross disciplines and all kinds of things like that, and I thought it was very worthy for me to do. I have not been one of the people at all involved in the struggle between science and religion. I'm aware of it, I read about it. Not everything, but I read about it. It's something that is—I'm pretty much of an agnostic myself. I became so, I guess, probably at some point in my mid teens, when I gave it some serious thought and said where do you stand or something like that, and maybe before going into the military maybe. But I am aware. I'm part of the National Center for Science Education; I'm on their board, Eugenie Scott's organization, which is in Oakland. She does incredibly wonderful things. She and her associates are incredibly strong about the rules about teaching evolution. There is you know, intelligent design is valid and all these kinds of things and so on. I mean, they don't need my help, but colleagues have done wonderful things in this regard. Steve Gould; a man who is no busier than him ever existed and so on, took plenty of time to testify at trials in Arkansas.

05-00:39:15
Redman: And now a question about you bringing up Steve Gould. Some people have suggested kind of this mental exercise of you know, if he had spent his time working as an academic, as opposed to trying to reach these broader audiences, how would he have made his biggest impact. Did that ever concern you, as far as how you—I mean as you said, you were already a very busy individual and you know, kind of adding on these, it almost seems like extra responsibilities.

05-00:39:48
Howell: Right.

05-00:39:48
Redman: But at the same time, you mentioned, with the Time Life publication, it's very big in your career and significant in your career, because it seems to me that that also represents a moment where you're chosen to represent the field—

05-00:40:00
Howell: Yeah.

05-00:40:01
Redman: —to this broader audience.

05-00:40:02
Howell: And it made a big splash for its time, and you have to judge things for their time and what they epitomize. It was a big splash and a meaningful splash. How long the splash lasts is not really the issue, it's the impact that it has that's important. Gould actually was able, who died at the age of 60 of all things, and of a different kind of cancer that he had had 20 years before. Unbelievable. This man had such an incredible set of intellectual gifts and work ethics and things like that, that he was able to do endless numbers of things that had to do with hardcore science, creative science, innovative science, popular science. His columns in *Natural History* every month. He never missed one of them when he had his first cancer, not once. Those columns were bundled together into a series of books, one after the other after the other after the other. Several of them published even after his death, so they were underway, and his enormous tome on evolution. The scientific structure of evolution or something like that, is a huge volume published by Harvard, almost 1,500 pages, was published only a month or two before he died. So he worked on this for years and years. He has a huge publication record that is unbelievable. He's one of the few people, and I have to say there are few, who could do that. There's well only one other kind of person like it; Niles Eldredge at the American Museum of Natural History, who does quite a bit of that, and especially with strong stances about evolution. The reality of evolution, the basis of evolution, its significance, and the relationships of all life and all of these kinds of things. Many people, they respond occasionally. They are not immersed in it and so on. I have felt that I'm not the best person to be immersed in this, even if I felt I wanted to immerse myself in it, because I'm not the best public speaker. I'm not the most comfortable public speaker. Often, I'd rather do other things, and so the reward is not great enough for the abandonment of other things, and I really, I'm interested in pretty much the kind of basic things in science that I can respond best to, and often they have to do with not realizing myself, but in helping other people, I do a lot of things that take away from myself, I want to say what you're doing counts against you, right, you know, you're doing it for somebody else. I had done a lot of that kind of thing, which has to do with meetings and symposia, various kinds of such things, editing, and a lot of work with mentoring foreign colleagues, especially foreign colleagues, and making sure that the world treats them as well as possible, and that they're not lost in the shuffle. Those kinds of things. Plus, I've voided, on the other hand, certain kinds of pedestrian bureaucratic positions in the university, in a department or else-wise. I've never sought any such thing. I was chairman at Chicago for three and a half years, of anthropology, before I moved here. I never wanted—in that instance, I sought the chairmanship because I thought I deserved it and I didn't want somebody else to have it, and I did the best I could. But in other instances, I'm not the right politic person to be in those positions. I know how to make judgments, I know what I know that's valid and what's not valid, and what is political and what is not welcome. I don't like to play that game. I like the intellectual game. I like the game of science. I know how to do that, and I've learned how to do that over the years.

05-00:45:41
Redman: So you found editing a journal far more stimulating as a leadership position than say being the chair of the department?

05-00:45:51
Howell: Yeah. Well, I've helped edit things and so on, but I haven't really edited a whole journal for a period of time. I avoided that also, but some people have done so. Some of the best editors have been those that have been chosen and volunteered, after they're chosen, to be editors. These are naturally gifted writers and experienced writers, sometimes coming in from English, sometimes just the way they are, and that's wonderful. Those are great. You don't find those kind of people growing on trees though.

05-00:46:28
Redman: Right, OK. But that's specifically your—you felt your strong suit was contributing to the academic side.

05-00:46:38
Howell: Yes, yes.

05-00:46:39
Redman: And that's where you—that's the venue that you felt most comfortable.

05-00:46:42
Howell: And increasingly, it became inextricably linked with field work, and that really began, probably with the first immersion in 1954, this long period in Africa, then the work at Isimila three years later, a long period in Africa. After that, it was just repeatedly, repeatedly field work, overseas this and so on.

05-00:47:14
Redman: And one—OK, I'd like to ask a question about a publication that you had from 1957, before I get into how your excavation results from Isimila were published subsequently. But in '57, you wrote an article on the evolutionary significance and a variation and varieties of Neanderthal Man.

05-00:47:40
Howell: Yes I did. *Correlative View of Biology*.

05-00:47:42
Redman: Yeah. And it was reprinted in *Readings in Anthropology*. I noticed, you know, on a couple of occasions, the things that you've written have subsequently been reused in publications, maybe presented to broader anthropological—

05-00:47:58
Howell: Yes.

05-00:48:00
Redman: Were you enthusiastic when you were making—can you talk about the way that impact was felt, as far as teaching younger generations of students, as far as being included in things like textbooks, versus making a big impact with your really close colleagues.

05-00:48:18
Howell: Right. Well that was intended. That was part of a little mini symposium, of which a few papers were published in the *Quarterly Review of Biology*, and later on it was picked up, and it's probably reprinted in more than one place, which I don't always check and have a completely up to date CV. I think when it's done, they have to ask your permission to do that and so on, and sometimes you may say well, I don't think well enough of that for this particular purpose, that I want to have it reprinted. You say that and I probably have. In other instance, I'd say well it's nice of you to do that and so on, and reprint something else. But that was a general one that had to do with Neanderthals and their variation or not, and what that variation might or might not mean in terms of modern evolutionary concepts. So that had some kind of special meaning, that paper, and I felt it was a good one for that kind of a readings volume. I have not contributed, in general, to textbooks. I haven't written any major textbooks as such. I've been offered certain opportunities to do certain things, and usually backed away or didn't fulfill them. I didn't fulfill them once for Random House, and I think had to return an advance probably of a couple hundred dollars. I liked the idea at the moment, and then by the time I got around to thinking about it, I didn't want to do it. I just did not want to do that, and I think in fact, I was correct about that.

05-00:50:12
Redman: So you viewed that work as being maybe a bit tedious?

05-00:50:15
Howell: Yeah, or just pedestrian. It didn't have enough excitement to carry me along to do it, and so that was the case. But doing something actually, that has to do with certain kinds of basic research or certain kind of things that are pulling a group of people along in a conference devoted to this or that kind of subject matter. All of that is very interesting, and that is very provocative. I mean, that's science in action and I like science in action. I like science in action in lots of different ways. I like it in the laboratory. I like it in the classroom. I like it in the field. I like it multidisciplinary, where the disciplines are right there together, often in the field. I was an active person in prosecuting field work situations involving multiple scientists in the field at the same time, and so on, especially in Africa, especially in Ethiopia, but beginning at Isimila actually, and then in Spain. That's the only practical way to practice modern science. Of course we know that so.

05-00:51:37
Redman: Perhaps the last question I'd like to ask on this tape is that in 1959, you published an article in *Science*.

05-00:51:43
Howell: Yeah.

05-00:51:46
Redman: *The Villafranchian and Human Origins*.

05-00:51:48
Howell: Yeah.

05-00:51:50
Redman: Now—

05-00:51:50
Howell: It was a big article for its time.

05-00:51:52
Redman: Obviously, publishing something in *Nature* or *Science* is a very big deal for a career. So I'd like for you to maybe speak a little bit about that, and what that meant to you and your career and the field.

05-00:52:06
Howell: I was working on those kinds of things, so I had this paper, I guess. Anyway, they must have approved of it before I got it all ready. I mean improved of the idea. And they were asking for contributions from senior scientists and so on. It's one of those moments in the history of the magazine *Science*, where the new editor is thinking of this or that, or what are we going to do to get out of this, or whatever. Anyway, so it was accepted. It was interesting in the sense that it was really something that had to do strongly with geology, paleontology, a certain range of time, which was only slowly beginning to be actually datable, and what this might man in terms of other places, where there was perhaps things of comparable age, but there are actually humans involved. In Europe, where so much was known about the so called Villafranchian faunas, that's a locality in northwestern—a place called Villafranca d'Asti. It's in the northwest of Italy. A certain kind of grouping of animals found in a certain sedimentary body of rocks, et cetera. What I wrote about in that was an overview of things that would be called, from this rough span of time, because there is an earlier, a middle and an upper, Villafranchian, or later Villafranchian. In some cases, there is sufficiently one above the other that you can say, that's definitely older than this, and this is definitely younger than that. Other than saying that way over there is different, and so it has to be later and not—but you can't prove it. And I talked about java, I talk about Asia, and the basic Villafranchian, of course, is Western Eurasia, the Mediterranean Basin and so on, and I controlled all this literature very well, partly through a colleague of mine at Harvard, who mentored me as a student. He did a major article that was kind of a counterpart to this, in 1940-something, '48 I think. That sounds right, '48, which he published in the *Journal of Geology*. Mine was an extension and an elaboration, and an intensification of what he had written in part, and I thought I'd try it out, and it worked with science. I was pleased because they wanted it and they took it, and it was a lead article in science. It was reprinted, I don't remember where, several places maybe, usually without all the citations at the end. It had a huge number of citations, in tiny print, at the end, in all kinds of foreign languages that were relevant and so on. And of course, there were many things that I didn't know enough about, that I could have included in it if I'd known more.

For example, Russian literature and so on, which I was very much kind of not even quite at the beginning. But anyway, so that was a major publication for me in ’59.

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06-00:00:07
Redman: Very good. My name is Sam Redman, and I’m speaking today with F. Clark Howell, and today is Friday, February 16, 2007, and this is our third cassette today. When we left off, we were speaking about Dr. Clark’s 1959 article in *Science* and the impact that that made. You spoke a little bit about how that again, was quite an interdisciplinary effort, and that your existing familiarity with those bodies of literature really helped in crafting that article.

06-00:00:48
Howell: Yes.

06-00:00:50
Redman: Was that again—that was something that was unique.

06-00:00:52
Howell: Well yeah, I can add to that. That article was, that paper was a different kind of thing than I had been mostly associated with up to that point, because it dealt mostly with a geological, paleontological set of topics, and geochronology and what we call biostratigraphy, the stratigraphy of groups of animals through time, and those kinds of things. But it really reflected on and mirrored an emerging interest, which had budded since 1954, when I went to Africa, in terms of the African counterpart to the European Villafranchian, and that is where australopithecines and early hominines were—hominids were being found in Africa and later on of course, by after ’59, they would be found in East Africa, first at Olduvai, then at other places, and then in the Turkana Basin, and then where we worked in the Omo, in the ‘60s and ‘70s. So this was a natural antecedent to those kinds of interests that I had. Sometimes these interests were more latent, but present. In other instances, they were kind of just a kind of weaker link to something that preceded them, but where they were overwhelmed by my other concerns with other kinds of things, principle concerns with other kinds of things. So that particular contributed related to a few other things that I’d written earlier, which was about Africa, in *The Age of Australopithecines*, for example, was a long article I wrote after being in Africa, that specifically dealt with those kinds of problems, but this was in a bigger, older context and would lead in other directions that I would follow much later in my career and so on. That happens from time to time. You have something popping up that you hadn’t seen before in a certain context, and then you find later on that it repeats and repeats and repeats, and so it becomes a kind of major or repetitive concern. In other instances, it appears and just vanishes and so on, it’s not worth following up. Nothing’s new, nothing’s added, nothing’s gained, whatever it is, and so on.

06-00:03:56
Redman: So by this time, you're very clearly adding or contributing to the discourse surrounding—

06-00:04:02
Howell: Absolutely. Yes, I was.

06-00:04:05
Redman: —prehistory. In some senses, that leads itself to—it can lead to having people who are detractors or people who disagree with your ideas. Were there people that were standing up around this point? We've gone through several publications now, from 1951 to this article in *Science* in 1959, and then you would subsequently have several other articles, in '59 and '60. Were there people that were talking to you about these articles, either in print or in person, and saying that they didn't agree with you?

06-00:04:46
Howell: The answer is I guess yes. I guess there were such people, a few. I was chastised by one particular person at my own university actually, for using a term that was essentially European or Mediterranean European, for something that is in a different continent, Africa. That's the term Villafranchian, why I use it, and I said clearly in the article why I used it that way. So that's niggling criticism. But others, I think it was, like so many things, just silently received and acknowledged and so on. That would be in a major journal, where people would see it in other fields; paleontology, geology, volcanology, those kinds of fields, because they see nature in science. And so that was an excellent place to put if, if they'll take it, which they did. So it wouldn't be buried in some anthropological journal that nobody else would see outside the field of anthropology.

06-00:05:56
Redman: OK. So that's—you enjoyed that engaging in this discourse.

06-00:06:01
Howell: Oh absolutely, yeah, yeah, yeah.

06-00:06:06
Redman: Then, it looks like in 1960 and '61, you return to write several entries about Isimila, and this is after writing several comments and other articles in current anthropology. Actually, one thing that I'd like to ask you about is in 1960, you wrote, *The Newest Link in Human Evolution: The Discovery of L.S. B. Leakey's Zinjanthropus Boisei*. That was a comment about—for current anthropology. What exactly did you write about in that article?

06-00:06:44
Howell: Well, I really wrote that comment to set the stage for the significance of this—the broader significance of this discovery. There was a feeling among some people in the anthropological community. I can't say how many, but I can say some of whom were involved, who were just completely misunderstanding the various things having to do basically with geology, stratigraphy, dating, all

kinds of things. And so, I wrote this as a kind of clarification, very clear cut, very meaningful, read it and you'll understand kind of treatment of what this all meant, and what the dating results meant so far, which was very important. *Current Anthropology* was new then, and I published a long article in an earlier number of current anthropology on Middle Pleistocene hominids, which again, stretched me some down below Neanderthals and above australopithecines, but that's why... That piece of done specifically for that purpose, and that it caused a complete cessation of any kind of talk, that things were not the way they ought to be.

06-00:08:26
Redman: OK.

06-00:08:27
Howell: There was no comeback saying—nor did anybody say, Now I understand why it was wrong, you know. They never say that.

06-00:08:40
Redman: So that was an important article, really for clarifying what was going on in the field.

06-00:08:46
Howell: Absolutely. Yes it was.

06-00:08:50
Redman: Did you particularly—and I don't want to say whether or not you enjoyed writing some articles more than another, but do you feel... We've now talked about your participating in the broader discourse in science, whereas you're clarifying some of the major things that are being said in anthropology. So do you—did you have particular kinds of enjoyment in publishing those articles, and having both of those articles making a big splash? Were those experiences different at all?

06-00:09:24
Howell: Yeah. I think the experiences are different, no matter how much you write, and if it's incessant or sporadic, that you get more self-satisfaction, you gain more. You certainly gain more experience, whether do you gain insight. I think that what you gain, is that you gain by treating different... You gain by treating similar subject matters for different purposes. I'm absolutely sure of that. Then, to give the deserved treatment of certain kinds of subject matter, which is not or has not received such deserved treatment, is a worthy thing to do. That's like writing an essay, and essays are real magic to do that. Steve Gould was one of the great essayists of the world, and followed in the footsteps of great essayists in France and England. So I enjoy those kinds of things, and there is a number of instances where I've done those kinds of things in the course of my career. Very often, some of those things don't get published, because they come out in reviews of manuscripts and things like that, and they may be passed on to, you know, the editors say would you please review this manuscript author anonymous, so you don't prejudge him,

and then you write this evaluation of the manuscript which, if you're going to publish an evaluation, would include most of what you're including in this critique. If it's valuable and has some validity and things like that, then it may be incorporated into the—some parts of it or some elusion to it, into this accepted manuscript by the author himself. In other instances, it might go by the board just be dealt with or acknowledged that oh, I did that actually in another way, and then you might have to write something very special, because you wanted that to appear. So it actually can stand alone and is not lost. It's very important that certain kinds of things not be lost. I mean, certain things are not worthy of being kept or remembered. Other things, it's very important to remember that they're worthy of being acknowledged and kept, and especially an emerging science, and the direction that science does or doesn't go when it comes to a bend in the road. Does it go more or less to the left or down to the right, or does it sort of stay in the middle and plod along? In some instances, some important judgment calls can be made at those intervals, and it's very important that those calls be made, and are recognized and acknowledged. So that's my response to that kind of a question, that this is a necessary and worthy thing to do, and it has impact.

06-00:13:25
Redman: Now that's interesting, that you bring up the idea of reviewing manuscripts at this time, because you've eluded to the community being much smaller.

06-00:13:35
Howell: Yes.

06-00:13:36
Redman: And so, say for example, a journal like *Current Anthropology*, or even something significantly larger, like *Science*, if it's a manuscript, they're—realistically even moreso than today perhaps, there were only a few people in the world that could read that manuscript and offer an opinion on it.

06-00:13:54
Howell: A limited number.

06-00:13:55
Redman: OK, so—

06-00:13:56
Howell: Now there's a much broader number.

06-00:14:00
Redman: So can you talk about that experience, as far as reviewing manuscripts, and maybe how peer review has changed since that time.

06-00:14:06
Howell: Oh.

06-00:14:08
Redman: I know that's a rather broad—

06-00:14:09
Howell: That's a big question. In the past and unacknowledged in general on most people's resumes, curriculum vitae and so on, is an indication, an evaluation that has to do with all of such activities. Reviewing grant proposals, reviewing articles submitted for publication, reviewing articles that might be being prepared for publication, and they want your what do you think of this, what do you think of that, whatever, book manuscripts. There are all those kinds of things. Those kinds of things can take a lot of time. When you're at a certain point in your career, which is a point that's neither too early nor too late, whatever that means, you can be busy doing a lot of that stuff. Of course that doesn't even say anything about what about all the stuff for students that you do, undergraduates going to the graduate school, undergraduates doing bachelors theses, all these kinds of things. Competing for graduate fellowship scholarships, all this stuff. I've spent a lot of time in such things, none of which is ever entered in my resume, even by saying 1972, reviewed for 37 foundations, 87 international journals, 204 da da da. None of that. I've never kept track. It's overwhelming.

06-00:16:29
Redman: So that you feel like—

06-00:16:32
Howell: But this is a very necessary job. It's not always rewarding. It's always necessary, and what is in between, when is the necessary essential, when is it really ultra-meaningful? And that's when you play a deciding role, assuming your opinion bears fruit. I mean you can say, I was not in favor of this at all, as you can see from so and so lines and paragraph so and so of my review, and so on, and yet you went ahead and awarded the full amount of money, da da da. Generally speaking, you think that you're performing this task as a worthy thing to do and that in fact, your voice will be heard, and not only will it be heard, but it will be listened to. So therefore, it's meaningful that you do this. It's not just meaningful that you'll do it, whether they acknowledge it or not. You have to recognize the fact that that's in their court, and you can't do anything about that. You have to do something because it's called for and it's this what's called for that is very important. I think so.

06-00:18:09
Redman: You eluded to the fact that a lot of these responsibilities, though necessary are unacknowledged, and you're, at the same time, attempting to do your own research while at the same time—

06-00:18:21
Howell: That's right, that's true.

06-00:18:23
Redman: —working with students. Finding that balance, as well as having a personal life, must have been challenging for someone who—it seems as though you took on a lot of responsibility at this stage of your career. Can you talk about trying to find a bit of a balance there?

06-00:18:39
Howell: Well I've always been a workaholic, I mean in general. Not so much any more, but always traditionally was so, and that's what was demanded. That's what was required, and that's what I demanded, if it was required. It wasn't always easy with a family, a growing family with youngsters and so on. And if you're married to someone who is not a professional spouse, in a true sense, then they don't know what you're doing exactly and why you're always doing it. Because it's a singular thing that you do. You're not doing it with somebody else, so that can be a problem. What I felt was that it is very important that certain kinds of things be done by me, in relation to other things in the field of science, that people are asking my judgment about. I respect that and I think that if they ask this, that they're willing to listen to what I say, and that has been the case. I can't tell you how many things I reviewed for National Geographic Society or whatever it is, it doesn't matter. And for years there were many other things I did. I was on the editorial board of *Science Magazine* for six or so years, under Dan Koshland, the editor here. I was on for five or so years of the *Science Year Encyclopedia*; that's the yearly that they had of that encyclopedia. I can't think of the name of the exact encyclopedia. I want to say it's *Grolier*, but it's not. Anyway, it's sitting on a shelf in the front living room. It will show you the science year and the year that I was on the editorial board. Famous people on the board. There were Nobel laureates and people from many different things, and they're writing about all kinds of things in science and technology and so on. We would meet in Washington and we'd meet in Chicago every year regularly, downtown, and deal with this kind of problem. You'd be surrounded by these people, it was very interesting. That was a worthy thing to do because that was *Science Year*, and that had to do with those going into high schools and colleges, and people checking what happened in 1960 in terms of science, all kinds of science, the whole gambit of science of all kinds.

06-00:21:52
Redman: And again, that must have appealed to your—

06-00:21:54
Howell: Yeah absolutely, absolutely. Oh yeah. Yes very much so, I did, yes. But it does take time, all this takes time.

06-00:22:05
Redman: And then if you can talk maybe a little bit about your son and daughter in this time period, that is we're getting into the 1960s, and like you said a growing family. So that must have been at once very exciting and very compelling.

06-00:22:22
Howell: Well we managed. The hardest thing, I think was that during these years, if you look at the—if you tried to diagram the tracking of our, you know, of activities and so on, you would have the academic activities which has to do with teaching, department affairs, classes, student evaluations. Then you have the professional affairs, what has to do with professional science, all these

kinds of things. Then you have the family affairs, then you have field work. Unlike a lot of people, for many years, my summers were generally not free, and so I'm busy all year, academic year, beginning in September, 1st of October. It goes into June and the end of May, I'm teaching more or less. Summer comes and I'm off to go and do field work somewhere for three months or two and a half months, and my family is left in the lurch. There's no vacation or anything. It might be when I come back, there's time for a little vacation or something like that. But in fact, it was like that, and you can figure out readily, because I've clocked most years in terms of field work and so on. In the CV, you can see that in certain years, it was—Clark Howell was gone, you know, and sometimes the gone was for long periods, and family was not with me. We can mark the times when family was with me, which was rare, because of where I went, but in Africa and so on, very rare and very briefly. I might be gone three months, four months, four and a half months, one time five months and so on. By the time I got home, everybody is very unhappy about me, and that's the inevitability. I don't think that that's much different from—it's different in one way but it's not in another way—somebody who is in the laboratory, you know, many hours a day, seven days a week, 365 a year, and when they're out of the laboratory, they're not with it anyway. The only thing is that field work involves, in this instance, distance, and that is what is really different. I mean, you're really gone, you're not right there. Now it's possible to reach people at great distances, with satellite phones. There's lots of ways of doing things in distant countries, that were not possible when we were doing major field work in different places in the Europe or Africa. I can tell you, it was a hardship, repeated hardship on this family, and my children suffered from that, I'm absolutely positive. My wife certainly suffered and was angry about it. Many times, it was a critical juncture. It was the first decade after our marriage, married in '55. '65 was the year Jennifer was born. '61 is the year Brian was born, and those were years, every year, when I was busy or gone or something, every single one. Later it was the same and got moreso when I went into a long-term, ten year project in Ethiopia. The Omo, Ethiopia project, Jesus I was a bearcat in this regard, but this was my commitment of my skills, and my trade required that, just like somebody else's would require so many days a month looking at a telescope all night. Astrophysics or whatever it is, and that's just the way it is. Some people do not get involved in the field work this way. Other people get involved even deeper than this. Often they don't have a family. They may have a spouse and not a family. They may have a succession of spouses in fact, and no children. Sometimes they have spouses that are active in the same kind of endeavor, so they do things together, share things together, but this is a major—has been and remains a major issue for some people. The major thing now, much easier. It's not comfortable, but it's much easier to get around the world than it was. It's everything being equal faster, it's more facilitated and so on, and it's less primitive in many ways, although depending on where you go, it's as primitive as you might want in some places, so that doesn't mean anything. But there's a price you pay. No question, there's a price you pay.

06-00:28:20
Redman: So you've described those motivations as being your personal scholarly drive, but then also, just an understanding of the way your trade works and what was required of you, to maintain.

06-00:28:35
Howell: I definitely say that, yeah. It would be the same thing as saying Picasso needs a vacation and he can't paint for three weeks. The answer is don't be absurd, you know.

06-00:28:48
Redman: And now, if—

06-00:28:49
Howell: And it does take you away. The main thing is all the field work is overseas from the standpoint of our country. It's not living in Chicago and doing field work in Wyoming. It's not the same thing at all, no, which would be true if people at the Field Museum, who may do summer field work in the early tertiary of paleontology of Wyoming or Colorado or something like that. It's not the same thing.

06-00:29:25
Redman: You mentioned that a number of—and this was probably true with a number of your colleagues, that they had rocky marriages and often went through a number of marriages.

06-00:29:38
Howell: Yeah, I said that.

06-00:29:39
Redman: Do you consider yourself lucky that you and Betty ended up having a strong relationship?

06-00:29:48
Howell: We survived anyway. Fifty-one years we've survived so (laughs), which is not bad, whether it's rocky or not and so on. There's a high mortality rate on marriages and things like that among—in academia. It's higher in some parts than others, especially in social sciences it's high traditionally, and maybe some parts of humanities perhaps. I'm not sure about that, and I'm not sure about the hard sciences, I really don't know. I think it's very much an individualistic thing and so on, but that's—and now of course, you know, more than 50% of people live together without being married. I mean, it's another universe that we're in, at least in terms of North America. A very different kind of thing, yeah.

06-00:30:45
Redman: OK, so.

06-00:30:48
Howell: Where have we gotten to? We're almost 4:00. If you have a final question or something.

06-00:30:55:
Redman: OK, so let's see where we're at. So in the 1960s actually, it looked like you returned to Europe, specifically in 1964, you did another study tour of collections, but this was after actually working in Spain.

06-00:31:19
Howell: Yes.

06-00:31:19
Redman: And doing excavations in Spain.

06-00:31:21
Howell: Spain was another set of big excavations. It started out small and got bigger. I got involved in a project in Spain, in 1960, and after being in Europe for some other reason, I then went to Spain and found this place, which I had been encouraged to visit by a Spanish colleague from Barcelona. It's a place that had been worked early in the 1900s, by a local nobleman, and was a special kind of place obviously. I worked there in '61 as an initial season, testing it and finding another site not far away that in fat somebody had mentioned, but nothing had ever been written about it. And then I had two big field seasons in '62 and '63, at these two sites; Torralba was the first one, Ambrona was the second one. They've been very interesting. I worked there late in the early '80s, for three seasons, very big seasons, and we still have lots of things to write about some of that work. People have done other work at the one the—and biggest of the sites since then. Spaniards have done it. But that brought me back to doing things like Isimila, in a different context and in a different latitude, different setting and all kinds of things, compared to Southern Tanganyika. Actually, if I had—if things hadn't have happened the way they might have happened, I probably might not have actually gone to Spain, because in 1959, I went to Africa. That's when we presented talks about—at the Pan-African Congress in Leopoldville about our work at Isimila. We've had a series of papers presented there. But before that, I went to Israel, I went to a Wenner-Gren Foundation meeting at their Burg Wartenstein, a castle in Austria, and then I went to Israel, where a French colleague, who had spent a lot of time at Chicago, said you must come, I want to take you somewhere that might be interesting for you to work. It's up on the edge of the Golden Heights actually, a big Acheulian site, very rich in hand axes and so on. So I saw sites in Israel and so on, and then I went to Kenya, and things had been arranged, and I had a Land Rover arranged for me, and somebody went with me to the field to Ethiopia. I got inside Ethiopia and they wouldn't let me go anywhere. They let me come out and promised I could come back in, but they wouldn't let me go anywhere. They had had no clearance about my coming, my anything. If that would have not had happened, if it had been cleared all the way though—this is the middle of nowhere in Southern Ethiopia, north of

Northern Kenya, and it might have been possible to have been there successfully, and to have put together something in the next two years, I would not have gone to Spain. Spain got on my plate and it getting on my plate in this falling through. Only at the end of the trip in Northern Kenya, Southern Ethiopia, in this Omo Basin effort, was I able to get somebody knowledgeable from the police post, to come with me in the field, and to show them why I was there. I wasn't there to find gold or anything like that and so on, and, "Why would you be interested in these strange bones?" Well this is a so and so of an extinct monkey. "Oh really, how do you know that?" You know. Well, that just all—the greatest part of that trip afterwards was to go to Nairobi to have dinner at the Leakey's house, and following dinner, to have Louis say, "I guess Mary, we should share some of those Danish cookies that you brought home with Clark, since he's here." And he brought it out and he opened it up, and there is a skull of Zinj, which was found while I was in Ethiopia.

06-00:36:29
Redman: Wow!

06-00:36:30
Howell: And I was the first other scientist to see the specimen. It was overwhelming, because it was a whole cranium, with all the teeth, everything.

06-00:36:44
Redman: And that experience is pretty unique, because that would have been one of the first times that you had seen something like that, and been one of the first scientists, really, to look at it.

06-00:36:54
Howell: Yes, right.

06-00:36:56
Redman: That would be unlike looking at Piltdown, where a number of—you were contributing with another.

06-00:37:01
Howell: Yes, yes.

06-00:37:03
Redman: So you were one of the firsts.

06-00:37:04
Howell: Yes, that's right.

06-00:37:08
Redman: So that was a—

06-00:37:08
Howell: It was a quite an experience, and very meaningful. It was a time that of course, set off a kind of quantum effort towards elaborating paleoanthropology and

paleoanthropological researches, and maybe even the idea of paleoanthropology as a concept within an African context. Many things happened as a consequence of Zinj's discovery, or attendant upon Zinj's discovery, and then the intensification of researchers in the field, intensification of long-term, living in researches at Olduvai Gorge; not just going there for a month or something like that.

06-00:37:55
Redman: So why do you think Zinj, Zinjanthropus—was that much of a watershed moment, as compared to—do you think it was a bigger moment to discover that species, as opposed to other species?

06-00:38:14
Howell: Well it was the first thing that was like itself. Everything else had been found in limestone cave fillings in South Africa, of which there were mainly only three such caves then. Here this was, in an open place, in this huge gorge, coming out of a certain place. Their bones were coming out and they happened to catch it just as the teeth were exposed and so on, and that's what caught her eye. She found it, Mary Leakey.

06-00:38:58
Redman: So that, that really—

06-00:38:59
Howell: Brought him to see it and so on, a famous story.

06-00:39:02
Redman: It not only changed the way that people thought about human evolution, it changed the way people were looking for these types of fossils.

06-00:39:10
Howell: Yes, and it changed the way of funding. This went to *National Geographic*, and their committee on research and exploration, and I went to D.C. actually with Louis Leakey, to meet people at Geographic, and also for him to talk to people at the National Institutes of Health, about the primate research center that he had assembled in Kenya, outside Nairobi—it was called Tigoni—raising and breeding and feeding monkeys, and studying them for medical and other purposes and all that. But this meant major commitment of foundation for research, major commitment of Geographic, to support individuals, the Leakey Foundation and so on. The Leakey family, and down the road there would be other such commitments. Eventually our own work in the North Turkana Basin in the Omo, Richard Leakey's spin-off work in Turkana Basin in Kenya, other British people working elsewhere in Kenya, da da da, all these kinds of things. This all was '60 and thereafter, but the roots go really back to the year of the Darwin Centennial, '59, and the summer efforts at Olduvai Gorge by Louis and Mary Leakey, and unexpectedly, not bits and pieces, but a whole cranium came out. It's still one of the most completely known specimens of any of the Australopithecine cranium, because it's very well preserved, and even though some parts of the upper part of the brain case are

missing, you can mirror image a great bulk of it and so on. It's not elaborately distorted or anything like that, so it's great.

06-00:41:19
Redman: All presented to you in a Danish cookie case.

06-00:41:20
Howell: Yeah, it was a very clever thing. I've never forgotten that, and Louis was a great jokester, and so it came off extremely well. (laughs) Does that finish this up?

06-00:41:34
Redman: Yeah, that would be a good time to break. By the next time we come back, we're almost ready to bring you out to Berkeley. OK, very good.

Interview 3: February 22, 2007
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07-00:00:08
Redman: My name is Sam Redman, and I'm here for my third session with F. Clark Howell. Today is February 22, 2007 and when we left off, we had touched on your excavations in Spain in the early '60s. I'd like to know a little bit more about what you learned about—from both a prehistory standpoint, as well as a more practical, methodological perspective, from those excavations in Spain.

07-00:00:39
Howell: OK. Well one could wonder what one is doing in Spain, when I had had this immersion in Africa, including a very long field season in 1957, as I told you about, at the big site of Isimila in Southern Tanzania, Tanganyika then. It was a strange opportunity. We had a visitor to Chicago, whom I got to know, who was an archaeologist in Barcelona, named Luis Pericot and he wanted to talk to me and did so, and said I understand you've done this and that and so on, and why don't you come to Spain. Spain was still under Franco, and it was, I can tell you, a very primitive part of Europe, really. Anyway, there's a very important site named Torralba, excavated in the early 1900s, by Marquis de Cerralbo, et cetera, et cetera. And I said well, I've probably read about it in a book about prehistory in Spain. I said probably Waldemar's book, and he said yes. All right, so in '60, I went to Europe and was at a meeting in Austria, a couple of other places, and then at the end of the visit there, I went down to Spain, went to find Professor Pericot. I couldn't find him anywhere in Barcelona. I was at completely loose ends, my Spanish was almost nil, although I had had some in high school, and I just said, What am I going to do? But I knew that this place was located on a rail line that connected one particular place called Soria with Madrid, and if I could find this particular—there's a junction there, and if I could get to the junction, I could perhaps get out and ask and find where this was, because it was right at the railroad. It was found while the railroad was being built by French people, in the late 1800s. I did that, and I got somebody to take me up to the village and over to where some fields were and he said here, aqui. I got a look at I, I walked around and found bits of bone, on the surface turned up, because it had been cultivated land, and so on, and I decided, Well, I might do something about it. I really would like that. The reason I was encouraged to do that, was I hadn't had any luck in 1959, when I was in various parts of Europe, Israel and Africa, and went up to the Omo for a month, and I literally lost a month because they wouldn't let me go into the field, and my clearance didn't come through and so on. I just figured it was impossible to do anything in Ethiopia, which I would have probably preferred. And so Pericot offered me an opportunity, and I could go under his umbrella or somebody else's, because some Spanish person has to be essentially responsible for you, although you might have directed the rules for working in Spain. It worked a little different then. Then they became much more bureaucratic later. Anyway, so I had a first season in '61. It was fairly brief, and I had a couple of people help me, who came from

England, and visitors. I had a big season in ’62, at which time we worked a second site called Ambrona, it’s about a mile north, near a tiny village of the same name. In ’63 I had a still bigger season at both sites. A former student, now a retired professor named Leslie Freeman, Les Freeman, who later I brought to Chicago as a prehistorian. He did his thesis on Neanderthal tool making and so on, in Spain. Anyway, he excavated Torralba after my initial efforts in ’61, and in ’62 and ’63 he did that, and I worked mostly with people at Ambrona, which turned out to be extremely rich. These are sites with large mammals, especially a deer horse, straight tusk elephants, so called, that never have any straight tusks, I have to tell you. Things of that sort.

07-00:06:16
Redman: So with each—as each field season was getting bigger and bigger, were you bringing more students from Chicago, and then also people from England and Spain?

07-00:06:26
Howell: Well what I did, at the very beginning, I came under the umbrella of a professor in Saragossa, who was a specialist, as a number of Spaniards were, on prehistoric art. There is plenty of prehistoric art in Spain, Pyrenees, and in France, so it’s a natural... I met this man as we came down into going to these sites, and he told me who was going to work with me; two, and then sometimes a third woman, all came, and two of them worked with me all those seasons, in a supervisory position. One of them worked at Torralba with Freeman, another one, who is a Basque lady, named Dolores [Achide?] worked with me and others at Ambrona, and that still later on, to make this story shorter, I did bring a number of students in ’63 especially, and they came from several universities, including Chicago, Berkeley, Harvard and so on.

07-00:08:00
Redman: How did you find students?

07-00:08:02
Howell: Through colleagues. I’d ask, and I said it would be good if, you know. Sometimes I did well and sometimes I didn’t do so well. I had difficulties with one particular student, and his wife, that I brought from Chicago for the ’62 season. We got through that and then in early ’63, he went there to start earlier, and Freeman was helping him. By the time I got there, it was a catastrophe in the field.

07-00:08:33
Redman: You had what type of difficulties?

07-00:08:34
Howell: Well the student said, “I don’t want to do this, and that’s not what I decided to do.” His wife was a complete pain, and both of them smoked marijuana beyond any belief that you could imagine, not that that bothered me that much, but he was just completely out of his real capabilities, and he’s

overseas. Subsequently, he became an established scholar of Peruvian archaeology, and taught at Cornell and elsewhere, and divorced his wife.

07-00:09:19
Redman: Did those types of relationships, with students who were becoming young scholars, who maybe didn't perform so well in the field, did that then color your perception of them basically throughout the rest of their career, or did your perception of that individual change after he became?

07-00:09:39
Howell: Right. Well usually, if something like that transpires—that also happened in the Omo, first field season, where I took somebody to please a friend in the States. I think it was a distant relative, and he initially seemed to be appropriate, and he proved not to be appropriate. At the end of the season, I flew him out of the field and that was that. I never had anything further to do with him, but before he died, I remember he sent me a whole mess of slides that he'd taken years before, when he'd done some survey work with a geologist that we had. No. Normally, if you're around people who don't work out for some reason or other, that doesn't mean that you would necessarily cut any ties to them, or you would sever any relationships that would help them, if they're going to be a positive person but in a different field. So the answer is yes and no, I suppose, to that question, but in any instance, these are problems of working overseas and working under difficult conditions. In Spain, the conditions were not that difficult. Although the country was still very primitive, the country people, farmers and so on, were really peasants, as they had long been, and the country was almost a century or more behind the rest of Europe. God, now it's one of the leading countries in all of Europe in every way. It's a wonderful country and wonderful people and so on. So, but I needed a certain number of people as things got bigger. Excavations, because it has to do with recording, mapping and all those kinds of things. There are newer methods now to do things and so on. Many things are electrified, completely electronic. Measurements are all done through an automatic instrument that goes directly into a computer, and a computer is used in the field, not just back at home base. It's called a total station approach. It's another universe compared to what it used to be. It's very, very different. But I had very good people actually. One of the best people, and I used him as a supervisor in the '63 season at Ambrona, came from Berkeley. He was a very eager field person with lots of experience in California archaeology. Later he went to Harvard and got a PhD, and did a lot of work in Peru, especially in Peru, on proto Indian kind of things, proto American Indian kind of things. Some of these people went on and became well established individuals in their own right, in their own field. That particular season, if we added them all up, I mean, I had four or five people, all of whom subsequently became full professors, well known people and so on. I count them among—I've had nearly 50 PhDs that I've had either quite a bit or a primary responsibility for. There are others that I've had lesser and I was sort of like an adjunct on a

committee. So when I say that, I mean somebody who not only signed a thesis, but had something to do with directing it, and those kinds of things.

That was actually a good experience to work in, because it's in Mediterranean Europe, not in France. It was inexpensive by comparison with elsewhere. It got more expensive later. It wasn't until the late '60s or so, when Franco died and so on, and the king and queen were brought back and all that kind of thing. But anyway, I had a real touch of Europe, and then if you think that I did three more seasons in the early '80s there, to go back and do more, strictly at Ambrona, which is a big place, and we still have plans to do a major publication on Ambrona some day. We've done some interim and important publications, but a bigger, major kind of one will now be done. And in the next two years, the monograph is already finished, will be submitted for publication on Torralba, by my colleague, Les Freeman, and myself, and a host of other people that do special kinds of things on the monograph on geology, paleontology, stone tool making and all kinds of stuff like that. Again, I was getting field experience. I was doing controlled excavations in a modern way. It interested many people in Europe, including England. It was done at a time when this was only ten or a little more years after the end of World War II, and it was symbolically very important. We had many visitors in those '60s years.

Subsequently, of course, I was drawn back to Africa in the '60s, and so my last field season was '63 at Ambrona and Torralba, and I didn't go back until, as I said, 1980, when other things were behind me. The chance to work in Ethiopia came up again, this time, right directly to me, and I jumped at it more or less, and how that happened is worth exploring, because you could say, How does this come about? I had never been to Ethiopia before, except to be in this little corner that I got to from Kenya. By the way, when we worked in Southwest Ethiopia, in what's called the Lower Omo Basin, it's the major water source for Lake Turkana, formerly called Lake Rudolf, we got there from Nairobi. We mounted expeditions from Nairobi, vehicles came from Nairobi. All the vittling was done from Nairobi, tents were bought in Nairobi, clothing was bought in Nairobi. We had weekly aircraft, chartered aircraft that came on one or other day, sometimes towards the end, or whatever, of the week. It would bring up mail, it would bring up professional people to join the expedition when we worked there, because they came from overseas. We had all the vehicles in the field, so the answer is to fly them up. It was about a three hour flight from Nairobi, a two and a half to three hour flight from Nairobi, 780 miles, and we also had—we had—it was a very interesting, and got to be a big operation early on, and so I suddenly became a contributor toward or an instigator of what's come to be interdisciplinary paleoanthropology. I sort of grew into it, because I used my imagination. I recognized needs. I figured a lot of things, if they were unfamiliar to me or just downright strange and so on, it would really be nice to have somebody around me who knew a lot about them. And I've always felt that way. That means you have to be very wise and judicious about picking the people that

may be associated with you in filling this kind of niche. In general, I don't know, I've batted over 90% I suppose. I've done very well in choosing. That means I've not done too badly, except rarely, I suppose.

07-00:19:33
Redman: The question about that, in finding people to fill those specific gaps, did you look for, in trying to find somebody who would successfully contribute, did you look for people who were—say for an example, you needed to find somebody to fill in a certain gap in your knowledge of geology, would you find a geologist who is an expert in the area that you needed expertise in first, but then also interested in prehistory, or was that not as important? Was it more important to find a specific expert in the field that you were working?

07-00:20:07
Howell: It was really the—in almost all instances, it was through prior knowledge of some kind, having met somebody, known of somebody, looked for somebody and found somebody that maybe you didn't know about. So it was a multi sort of source thing. The people were all brought—for example, I had as many as four or five. Five, I guess was the maximum—at one time six geologists doing different kinds of things in the basin, and this was only at certain times. So some, who were there at the beginning, were there for two years and then they went elsewhere. Their job was done. One person who came from California was there one field season only, in 1968 I think. It doesn't matter. So there were sort of some semi-permanent people, some occasional people, and then there were always the sort of nucleus that was there, whether it was some young people that were pre-PhD, but working for PhDs, and these were very important people to rely on for many things having to do with logistics work in the field, back and forth to Nairobi and all kinds of things. In the field, for example, we ran as many as six or so Land Rovers in those days, and if you had all six running at the same time, you were very lucky. I even took an American who lived not far from me in Chicago, for one summer, and he's strictly as a repair person on Land Rovers. We got to be friends and I said, "Would you like to do this John?" And he said yes. Little did he know what he was getting into. But anyway, this was before the days of Toyota Land Cruisers and things like that, which are just unbelievably efficient and effectual, and break down rarely. Land Rovers have a reputed history, from everybody, that certain things are a major problem with, so called half shafts and parts of the front wheel drive and oh God, it's one thing and other. It's dreadful. That's an improvement, to move from Land Rovers to Toyotas, I'll tell you. It was a fantastic jump technologically and very important for field work.

07-00:23:22
Redman: I would say that is something that people don't think about.

07-00:23:24
Howell: Yeah, people don't appreciate the things that go into field work. Of course field work, at one time, let's see five to ten years apart, can be very different

five years later or ten years later, my God. And then if you put the cumulative years in, the differences are just extraordinary, as I mentioned, like doing mapping and surveying with total computer stations and all kinds of stuff. Just unbelievable. I'll give you some examples. First of all, to get into the field, we had to drive almost 800 miles from Nairobi. You go up from Nairobi, down along the Rift Valley, into the Rift Valley, out of the Rift Valley, up into a corner of Uganda, where there are two very big volcanoes, out of Uganda, down into the Rift Valley again, where it's very desertic and dry, and up the north side of Lake Turkana, still Rudolf then, and past some villages, and then you'd try to cross the border. The border is interesting because, of course this big river is coming down there and we're on the west side of the river, that is the right bank, and this river is a huge delta at the head of Lake Turkana. Then you have—if you're lucky, the river is low enough, which is still plenty high, that you can get through a kind of little insular area in between other side streams and so on, that are part of this kind of birds foot delta. Sometimes it was a real test, and in some seasons, we had terrible times getting into the field. You'd have to go way around and so on, and just places through the corner of the Sudan and down. It was a very complicated thing. We have some wonderful pictures of trying to get trucks and Land Rovers unstalled and unstuck and so on. Amazing. We owned a large Lorry several seasons, and then I sold it for money, and then bought a used Army Lorry to take its place. That Lorry went back and forth, back and forth, back and forth incessantly, with a driver and a helper, to Nairobi, because we had to bring in all the petrol for all the vehicles, plus petrol to service airplanes that came up. We built a landing strip every summer, and made it so there was even a Winsock, and used lime to mark the boundaries and so on. It was never as long as we wanted, and that of course made a problem with the airplane. It has to leave early, before too much heat generates, and it can't be overloaded. So you have to make sure that you have some feeling for the weight that's going in it, either human weight—I sent fossils back to Nairobi on every plane. We had a way of talking to Nairobi. We set up a radio, which we had arranged for in Nairobi, but it had been bought in the States and it was sold to us in Nairobi. We had an engineer come in and set up a field radio, with a big antenna, and we had a tent that held the radio, and made sure that when winds came and so on, we didn't get the antenna knocked down. Of course, you can have these storms that come up that are just unbelievable in terms of tearing down tents, antennas, anything and so on. The radio was used, and we'd call Nairobi, and from Nairobi, they'd say who do you want to be connected to and so on. So you'd say, I want to be connected to Mrs. so and so, who is our lady agent in there. In fact, she was a pilot's wife. Or if I wanted to, I could call Mary Leakey at home, if she was home, or I could call her actually at Olduvai, which I never did, because she had such a thing also. In most of my Land Rovers, I put in Motorola radios similar to taxi radios that would be used in Chicago, say, voice operated radios, so people in the field—and they had antennas sticking up, and it was on the dashboard, and you pushed a button; turned it on, turned it off. If you wanted somebody, you picked it up and

called them; this is so and so calling so and so. If somebody got into trouble, had an accident, turned over, somebody had got bitten by a snake, had needed medical help and so on, I had some measure of control over this. My real concern in the field, and that was less so elsewhere, but especially in Ethiopia, my real concern was the status and welfare of the people in the research expedition. It was absolutely vital. It wouldn't be a problem if people would do what they're told, but it's the people who have this singularity of going their own way and doing their own thing, that sooner or later, if the chances of this happening are enhanced by having one or two people like this, you could be in real trouble. We were in general, able mostly to minimize this, but I was always concerned about it and I had difficulties from time to time, because there was somebody who was always smarter than you or smarter than themselves, and that can be a real problem.

07-00:30:07
Redman: Right.

07-00:30:08
Howell: The Omo was remarkable. Omo Research Expedition, ORE, was a remarkable thing for its time. This was begun in '66, with a geological survey that I instigated with a young geologist from Berkeley, at the suggestion of colleagues at Berkeley, and he's been working in Africa, including around Lake Turkana, for over 30 years, as a consequence of my bringing him, or sending him there in '66.

07-00:30:45
Redman: What was his name?

07-00:30:46
Howell: Francis Brown, Frank Brown. F.H. Brown.

07-00:30:52
Redman: Just before you go on, I'm going to fix the sound a little bit.

07-00:30:54
Howell: OK.

07-00:31:05
Redman: OK, I'm sorry. We were talking about—

07-00:31:06
Howell: That's all right.

07-00:31:07
Redman: —Turkana and Frank Brown was it?

07-00:31:10
Howell: Yes. Francis Brown, right.

07-00:31:12
Redman: Francis Brown, OK.

07-00:31:14
Howell: Let me add one or two things, and then maybe you'll have some questions or something. A large operation like this hadn't been really done in Africa before. There were kind of small potatoes. Of course, we started out small potatoes and grew as we could get money and funding, which we did. The thing is, this was in 1966, only seven years after Mary Leakey discovered the skull of so called Zinjanthropus at Olduvai Gorge. So you know, we're less than a decade, and here we were starting something in '67, but with after Brown spending about a month up there, or five weeks, something like that, in the late summer of 1966. Our first big field season was 1967, and it was a limited scope. It involved a consortium of French scientists—they were all French—and a mix of people that I had, a small number. Later it got bigger and bigger.

07-00:32:44
Redman: Again, these people were brought together by colleagues that you knew and contacts?

07-00:32:47
Howell: Well the first person who had—a French person had worked there, had actually walked across this area, an explorer, in early years of the twentieth century. And then, in 1930/31, a French paleontologist from Paris—originally he worked a lot in Algeria—went there and did some field work within this sequence of beds. He did very basic kind of work, which was published in—the last part was published 1943 to '47, and I knew about these monographs. I knew him. I had actually been to his house for lunch in the Périgord, when I dug with Hallam Movius. In 1953 I went to Professor Aramburg's house. We knew each other in several ways and so on. So there had to be a French contingent for an expedition. I'll tell you how it came about though, because Haile Selassie was interested, was still in power. He was interested in the consequences of Louis Leakey's work at Olduvai Gorge, which meant of course, Mary Leakey's work at Olduvai Gorge. There was a congress—first of all, he was invited to come up to Addis Abba, and he met with an emperor, and they were talking about this to Louis Leakey, he said, Why aren't things like that being done in my country or could they be done in my country Dr. Leakey?" Dr. Leakey said they certainly could, and there's this particular area north of Lake Rudolf and so on. So the Emperor himself was interested, and the Emperor cleared the way. On another occasion, Louis Leakey, later in the year, went to a congress that was held in Addis Abba, and this was renewed, a relationship was renewed, a paper was signed, and I flew there and met Louis Leakey, met the Emperor briefly, and an agreement to have somebody from France, somebody under my jurisdiction and somebody out of Louis Leakey's museum in Nairobi to come. That was his son, Richard, who was still very young then, maybe 20, 21. A French, Aramburg himself did—was an older man. He lived until his early eighties, and he went to the field. A strangely wonderful man, but a young mentoree, I guess of his, Eve [Copass?], who had just finished his military service in Chad and so on, was kind of the leader of the French group of people.

The Kenya expedition, which was a little group of people, found some important things the first season, including on the side where we were, we could have worked if we'd wanted to, that bear on the origins of modern humans. We didn't appreciate at the time, the real significance of these, because it wasn't possible to date them accurately, and that was done later. That summer, Richard Leakey was going back and forth as a new director, I mean not quite yet, but he was going to be a new director of the Kenya National Museum in Nairobi. He flew over a portion of the east side of Lake Rudolf. Often the planes went up on the west side, towards our camp. He went down on the other side, wisely, and flew very low. He co-owned the airplane that was flying, with the pilot that normally flew it. In fact, three people co-owned a plane; a photographer, Richard Leakey, and Keith [Bously?], who was a South African by birth. In so doing, he found areas of outcrops of rocks and so on, that looked very promising. And so, from the next year onward, huge amounts of work went into the Turkana Basin in Kenya, under Richard Leakey or his subsequent wife's, Meave Leakey's direction. World famous.

07-00:37:54
Redman: So he knew of sites and discovered sites through both exploring literature, predating some political developments, but also through flyovers?

07-00:38:08
Howell: He really found East Turkana by flying over it. Actually, a part of it had been—there had been a report on the general area, mostly from old aerial photos, World War II aerial photos, by people under the British Colonial Geological Surveys. He certainly would have known about that, but it's not until you're either flying over or on the ground, that you can really determine what it is you're seeing, and what it might mean and how this relates to that, or that relates to that. We used a helicopter for several years and in '67, when he came back, he said is there a chance that we could borrow a helicopter, to have a look over some of these areas in Kenya, east of Lake Turkana. I said sure, of course, and I said, "You can even have it for free, what do you think of that?" And he said, "I don't believe it." But anyway, we made our own aerial photographs of the Omo—there were no aerial photos and so on—and they were done from this very airplane, this very pilot, and the photographer, who was one of the co-owners, sitting on his knees, with a cutout square area, above which he had a Rolleiflex Camera, with multiple backs, and they would fly a direct compass heading and at a certain point they'd say, you know, click on or something, and they would follow this until the end, finish the heading, make a turn, maybe climb and then come back down, and then say heading to, that would be the reverse trip, and we'd try to get the things all to match and overlap, so we could have stereo photos. We succeeded remarkably well, considering. This was done often by kind of brute strength and so on. In '67, I said if we're going to do this thing, we just can't do this otherwise. There are no topographic maps. There were things in Kenya, pretty crude then, but there was something, but where we were, there was nothing, and to do what we wanted to do, we had to have these photos. We made a very big mosaic, which

turns out to be about 15 to 18 feet long, and about six feet high. If you saw it, you know, a certain scale, a scale of, I think, one to 10,000 or something like that. We had cutouts of it that we used in the field for mapping, that you'd use on a clipboard. Things were marked usually by a pin prick and a number, that is finding a place. This is locality so and so and so and so, such and such a day. Everything from that locality would be collected on the day it was found. If you went back there after, it got another date added to it being have attention paid to it and so on. So we instigated a pretty big operation for its time, and that's why the Omo work, which had I think, over 55,000 fossil specimens, something like that, identifiable specimens, was so important. It wasn't so important in terms of getting beautiful, complete hominid, proto-human type remains and so on, often because of sedimentary conditions, where things had come out and broken up, and you couldn't get them before they were broken up, or eroded, or they're in sedimentary deposits that were of moderately course sediments and so on. So it wasn't anything like the wonderful collection of things that had come from some of the Northern Rudolf sites in Kenya, and are of course world famous. What we did have, that no one else had, and it was a kind of baseline, was kind of like a geological meter stick, if you wish a yard stick, that was based on volcanic ashes throughout this sequence. We started a program at the very beginning, when Brown went there, and when he brought back samples for me of pumices and so on, that these were from volcanic sources. They had minerals in them, and was possible using a refinement of the potassium argon radiometric dating method, which was coming to be applied in the end of the '50s, by people here at Berkeley. Brown was a student, of course, of one of these people, so that was ideal. There was a succession of these ashes. Actually, it turned out that that's many more than the basic number that we used, and they were all given letter names, like ash—we called them tufts—Tuft A, B, et cetera, et cetera, and there were over 102 of these.

07-00:44:47
Redman: Wow!

07-00:44:51
Howell: Usually, they could be distinguished, or groups could be distinguished from groups in them. We used this extensively and refined it and refined it. Subsequently, Brown refined it vastly in East Turkana, showed that there were many mistakes in East Turkana, because they misidentified ashes. They may look the same, but they're quite different in age, et cetera. All of that is now added to an incredible radiometric chronology that goes from let's say nearly five million years ago, up to half a million or so. All of that came out of the Omo, and of course with it, the Turkana Basin and so on. And so I spent over ten years of my life involved with that. I had been there a total of eight or nine field seasons, I guess, and other people were there when I wasn't there, on one occasion, and it lasted from '66—if you want to count it that way—until '76, when the French made a last foray in, and that's the time when Haile Selassie died, '76. And then there was a new government imposed in Ethiopia, a

communist military government, and everything came to a halt, and real problems in Ethiopia.

07-00:46:30
Redman: I'm going to return to—

07-00:46:31
Howell: Sure.

07-00:46:32
Redman: Omo in a little bit, because I'd like to talk about specifically, the discovery of Omo one, and then also talk about the—it seems as though many of the things that you worked on, on that Omo research expedition, were published later on in the '70s.

07-00:46:52
Howell: Right.

07-00:46:52
Redman: So I'd like to talk about that a little bit too. As far as your life in the United States, and often kind of the field work and the life in the U.S. kind of were separate but connected. But in 1964 you were a visiting professor at Berkeley, and I'd like to ask you a little bit about that experience as it compared to your prior experiences at Chicago and Washington University, and if I'm not mistaken, by this time, you had been a visiting lecturer at places like Northwestern and Rockefeller.

07-00:47:28
Howell: Yeah.

07-00:47:28
Redman: So you were starting to build some different experiences. How was Berkeley different and similar as far as that experience?

07-00:47:36
Howell: Well, we were invited to Berkeley, for the '64/'65 academic year, and that's because a colleague here, and that was by—my invitation was really extended by a former professor of mine at Chicago, Sherwood Washburn, who was out here, moved here in '58, a few years after he brought me back from St. Louis to Chicago. A colleague of his named Ted McCown, who had been responsible for some of the field work at Mount Carmel, very famous field work, in the very early '30s, end of the '20s, having to do with Neanderthal peoples in Israel, then Palestine. He had gone to India a year, on a sabbatical, and was going to do some teaching and some field survey and things like that. And of course, I mentioned it to my wife and she immediately said of course, tell him right now we're going, we'll come. Chicago did not fit her appropriately. Of course, it's a different universe there, different seasons, and it was—she knew enough of California. She had been out here after she finished her nursing degree, her RN. She and her sister came out here and visited up and down the west coast, essentially California, you know, in

Berkeley, somebody they knew. San Francisco, Santa Barbara, L.A., et cetera. My wife had run out of money somewhere, which she did, and she'd go in and walk in, and do private duty nursing right off the street. So we knew something about Berkeley. I didn't really know otherwise, that much about Berkeley as such, but I knew of people here. That's how I discovered Frank Brown. When the time came, I discovered him through Garniss Curtis, who is one of the major figures in potassium argon dating, along with Jack Everndon, who was here then, and there were many other people in other parts of the university that had plenty of interaction with that year. That year is interesting for several reasons. If anybody knows anything about universities and so on, they would say, isn't that possibly the year of free speech and those kinds of things, and Mario Savio, and all those riots at Berkeley and so on. And the answer is yes it was, and I didn't even know it was going on, I have to tell you, much of the time. The reason is that that year, I was contracted at the end of the year, to write a book for Life's Nature Library, called *Early Man*. So I had that under my belt. I would teach and then at least once a month, sometimes twice a month, I would hop on a plane and fly to New York, and spend two days at their office, Time Life, in Rockefeller Center, working on this thing. Some of this work was duplicitous, or duplicative I suppose, because I had the original writer failed me, and failed to produce a manuscript, and we had to start it all over again with the editor, actually the senior editor of the whole series, a wonderful man named Maitland Edey. The Nature Library is still a remarkable achievement, and there are many other things. It was a hallmark of their series. Maybe you or others would know about other things that Time Life had done, endless things about war and battles, and figures and subjects, wonderful cookbooks, all kinds of things.

07-00:52:32
Redman: Had they approached you about writing this?

07-00:52:35
Howell: I was approached, yes, by them.

07-00:52:38
Redman: And you were obviously quite excited to participate in this.

07-00:52:43
Howell: I thought it was a good idea, because first, I'd be paid some money. Professors always need money. They need two things; time and money. I was of course, paid whatever I was paid when I taught here, just as if I'd been at Chicago. When I went back to Chicago, when we went back—we were here for the year and lived, I think, for $100 a month in a beautiful modern home, by one of the faculty members, who was in Mexico for a year. So that was a terrific thing. We had one child, who was four years old, a boy, Brian, and so the three of us moved out here for the year. When we went back the next year, I went I don't remember where, somewhere or other, but I had no intention of leaving Chicago or anything like that, but I had had a taste of California, and we both had a taste of California. My wife's taste was a very good taste and of

course, my point was well yes, but what about the university, the department, the colleagues and so on. There were tremendous attractions here in terms of colleagues, facilities, and several museums on campus, et cetera, et cetera, and the question is, there's not a chance that you're going to be asked to go anywhere, unless some space appears, some appointment appears, where they can say, we want to hire a so and so or something like that. That's what actually happened, because the man who was on leave in the year I visited here, in India, who subsequently died about 1968, early '69, and it was a coincidence really. There was a meeting in New Orleans at that time, and various people were there, and this former professor of mine, again Professor Washburn, didn't speak to me, but spoke to my wife, and said, "Do you think there's any chance of Clark considering moving from Chicago if something appropriate was found for him?" She said, "What does that mean?" He said, "Well for example, Berkeley." She said well, "I think the chances would be better than zero, but I can't tell you exactly, but I can raise it with him and feel him out." He said, "All right do that." And the rest is history of course. I did come. Chicago did a lot to try to keep me there, but I had been there really too long, considering I'd been there for seven years as a student. So you know, I had been there over 20 years. That's how it came about, that I came here. They had to do some things for me here. They paid my moving costs. I moved a lot of things from the laboratory and so on, that I had built in Chicago, and all that came. We took about a year to find a house, and this is what we found, at the end of the year, just as we were about ready to be thrown out in the street. We were in a rented house of a colleague who was in Cambridge, England for a year, and he was going to return within ten days or something, and we found this house. Betty found it of course.

07-00:56:50
Redman: I'm going to stop and switch tapes.

Begin Audio File 8 Howell_Clark8_02-22-2007.wav

08-00:00:08
Redman: All right. I'm Sam Redman, and I'm here with F. Clark Howell, and this is our second tape today, on February 22, 2007. When we left off, we'd been talking about a number of things. We spoke briefly about Clark's time as a visiting professor at Berkeley, between 1964 and 1965, and I wanted to ask about your experience with—and you eluded to the facilities as being comparable, and the fact that there were museums right on campus, whereas at Chicago, they have the Oriental Institute and the Field Museum is nearby, but the facilities were slightly different but maybe comparable. But then also the people, and I'm curious if they were maybe perhaps, a bit more formal or a bit less formal, or how the professional kind of atmosphere on the campus was at that time.

08-00:01:07
Howell: I think a major attraction would have been first, a number of people in the natural sciences that didn't exist, or the kinds of people that existed in their place were different at Chicago. I didn't have the close linkages at Chicago

with geology. I did some with paleontology. But here, there was a strong representation in the geology and geophysics, also in forestry and in paleontology especially, all of which was an inducement for me. There were separate museums on campus; the Museum of Vertebrate Zoology, which is what my lab is within now, in the life sciences, and there was the Paleontology Museum was in geology. It's now in life sciences, but is its own museum in paleontology. So those were attractive things; the people, those facilities and so on. I thought less, in a way, of what it was like in anthropology. It was a bigger anthropology department than Chicago, although Chicago grew tremendously in the '60s. In fact, the first growth we did was in 1960/61, and we actually hired four people from Berkeley's faculty.

08-00:03:06
Redman: Oh wow!

08-00:03:05
Howell: And almost decimated part of their department, whatever.

08-00:03:13
Redman: This is in what year?

08-00:03:15
Howell: '60/'61. So the main thing, I think, was of course the quality of living out here, the more equanimity of the weather, all the things that made the middle west so typical for us, where I'd been brought up and so on. My wife had been raised strictly in St. Louis, which has a very different climate than a lot of other places farther north, but in the summer is brutal, as a riverine town like Louisville or Evansville, Indiana. These are places that are just, you know, high humidity and very sticky. I think the overall thing, of course, was almost surely the right time in your life. There has to be a right time in your life, and I'd passed by some other times. I didn't think they were right or didn't think they were right for me or for us, sometimes against my wife's wishes, because I think she was seriously considering what it would be like in a smaller university town, away from a big city like Chicago, when people talked to us about coming to Ann Arbor at the University of Michigan, but that has more foul weather in the winter than almost anywhere. So anyway, those were the attractions and eventually, matters worked out and an offer came to me after some consulting back and forth and so on. Matters worked out and we moved here in the summer of '70.

08-00:05:31
Redman: I'd like to ask one more question about Time Life, *Early Man*. It looks as though they also published a young readers edition.

08-00:05:40
Howell: They did that's right, later on.

08-00:05:44
Redman: And obviously, there were several subsequent editions. I was curious, if maybe you hadn't thought about it this way before and maybe you did at the time, but thinking about some of the reading that you did as a child, and some of the reading that you did in the Navy about, you know, kind of your introductions into prehistory. Did you have this in mind at all, when you were doing a project like this, that there were people, the next generation of scholars like you would be reading things like the *Early Man* book?

08-00:06:17
Howell: Yeah, I'm sure I did. I didn't know about the young readers edition. I don't think that it was broached to us until subsequently. It was their idea, once the thing got out, that they would have a young readers edition and they'd capitalize on it and so on, and then utilize what we wrote and maybe reimburse us, that is remunerate us in some token fashion for contributing to something that was used twice, you know. I never pursued things that had a strong biological or evolutionary or naturalistic basis when I was young. I experienced nature by living in it, on a farm, as you know. My reading often had to do with American history or boys novels that had to do with Indians and frontiersmen and things like that, I remember, or other boys series I think I mentioned before, like the Rover Boys or the Hardy Boys or things like that. It wasn't really until I was in the third year of high school, that I stumbled across, at that particular age, something that pulled me back into a history that I had been unaware of. That doesn't mean that I was stupid, it meant I was ill informed or uninformed, and it was a gap in my learning I suppose. That was sheer accident, in my junior year of high school, when I stumbled across—and largely encouraged by a neighbor in Indianapolis, where we lived then, who had scientific interests. I don't remember now exactly what kind of science, but it included things that included evolution and things like that, although I have a feeling that he was more interested in things that would be more fundamental, like some parts of biochemistry or something, but I don't remember. Anyway, we got to be friends. We'd go to the movies together on Saturday afternoons sometimes and so on, or listen to a band, an orchestra, and watch two movies, and would come home. I discovered H.G. Wells' *Outline of History*, and when I read that, because it was in a big, thick paperback, maybe issued by Random House, I can't remember. It began way down. It began with not quite the origins of life, but it began way down there, and it just put a whole new spin on anything I might have—I wouldn't have thought about anything before, because I hadn't been, so to speak, in that potato patch, you know, I didn't. I was excluded. I had been excluded, not deliberately, because there was nothing like this in my family that said you know, we're God fearing people, we don't do this and so on. That was not true in my family. Serendipity often plays a very major role, and the point is to recognize that serendipity is playing and you'd better take advantage of it, that's what's fairly important, and sometimes you really miss those opportunities. You don't recognize them.

08-00:10:54
Redman: Right. So in ’65, with the publication of *Early Man*, and then in ’66, there was an article in *American Anthropology*, with J.D. Clark, on the recent studies in paleoanthropology, and then in ’67, you had an article, *Recent Advances in Human Evolutionary Studies*, in the *Quarterly Review of Biology*. And now these, all three of these, in very different ways, seemed like broader comments on the field.

08-00:11:20
Howell: They were.

08-00:11:21
Redman: So at this moment, you’re very much becoming one of the leading voices in prehistory. So at that moment, did you reflect on that at all? You had also become, in ’66, when you returned to Chicago, you had become the Chairman of the Anthropology Department.

08-00:11:42
Howell: I did.

08-00:11:43
Redman: So those two things brought together, and the fact that you’re becoming one of the leading voices in prehistory, and then also becoming the chair of this significant anthropology department, that had to be a signal that you were reaching a new stage in your career.

08-00:11:59
Howell: Yes.

08-00:12:00
Redman: And so, I’d like to know how that felt, and I’d like you to talk about that a little bit.

08-00:12:03
Howell: Well, I think I really thought it was just the way life turned out. Those papers were done variously, urged by this or that or something. One had to do with writing up something from a major presidential—it wasn’t presidential, but section addressed to the American Association for the Advancement of Science, which I came out here to give after having been here the previous ‘64/’65 year. I came out here, I think the winter, like January probably, and did a long presentation, which later became that paper, one of those papers, in maybe QRB, I think so.

The chairmanship at Chicago was frankly, a kind of—I don’t know how to describe it. The department was powerfully absorbed with social and cultural anthropology, with a minimal amount of linguistics, comparative linguistics in it, a teeny bit of archaeology, hardly any new world archaeology. I had tried to maintain and to build physical anthropology and related areas, once I came there, and after Washburn left, for Berkeley, and I did this at a time when I’m trying to raise a family, be a parent, doing field work overseas, going to

meeting and all these different things, and also trying to get along in the department with colleagues. The structure of the department changed radically when four major people, all from Berkeley—the original intention was to try to hire one person who had originally been a PhD from Chicago, to bring him back. He had worked a lot in Africa and we wanted him as an Africanist. The next thing we knew, there were three other people out here, who wanted to leave Berkeley too, if we could come about doing this. I didn't fight against it because I thought it was, in the end, good for the department. In the end, we were able to do this, largely on soft money from various foundations, especially the Ford Foundation, and some others, by hiring these people into interdisciplinary or aerial studies, what are called aerial studies. We continued to do that in Chicago. Somebody would be hired jointly between history and anthropology, somebody between economics and anthropology. There was just different ways of slicing up the loaf of bread and so on, but the question is, what was going to happen, and I could see the chairmanship and so on. I knew exactly what was going to happen, that they were going to propose a certain individual for chairman, to replace the previous chairman, that is he'd done his term.

08-00:16:07
Redman: OK. Would you like to name any of these people?

08-00:16:09
Howell: Well I could. The outgoing chairman was David Snyder, who was one of these come alongs from Berkeley, and he's a Harvard PhD in social relations.

08-00:16:22
Redman: Was George Stocking in that group?

08-00:16:24
Howell: George Stocking ultimate came in that group later on. Yes, he did. Excellent historian. They were determined to have one of their own, rather than somebody in the natural sciences, like myself, to be chairman. I kind of felt that, without really knowing it. I remember clearly one Sunday—we lived out in the suburbs—they called former professor, one of the senior people in the department, plus the person that they wanted to have as chairman, came out to see me on Sunday afternoon, and talk to me. They must have had a terrible time. None of them knew anything about the suburbs or anything like that. Anyway, they did, they sat down and so on. The well established, long time professor, who had been a professor of mine of course, ethnology and things like that, was Fred Eggan, and the man that they brought along was a man who had been in the social sciences division, that is not in a department as such, social science division, and later became involved with Robert Redfield and other people in India. His name was Milton Singer.

08-00:17:59
Redman: And this was the person that—

08-00:18:01
Howell: That's the one they had decided would probably be the next chairman. I just listened to them and I said, I think it would be inappropriate, and that I had been there a long time, including before Milton Singer had anything to do with the department. He was really more of an adjunct, had always been. I wasn't putting him down, I was just saying, and I felt it was appropriate that I be chosen as chairman. I didn't really have anything more to say about it, but if you want to say more to me you can do so, but my mind is made up and my argument is good enough. If you all decide not to, you know, to vote differently and so on, it will be another matter, or if the Dean opposes it and so on, he has to accept it, then it's another matter, and it might or might not have something to do with my staying here in the future. That's not meant as a threat or anything, it's a statement of fact. It seems to me, I'm talking about equity. Eventually, I was chosen as chairman, in '66, and I overlapped with Snyder for a year. In '67 of course, is when we began the Omo work.

08-00:19:30
Redman: Now, that brings me into an interesting question. So of course, you're chairman from '66 to '69, and then finally, in 1970, you go back to Berkeley for good. Meanwhile, you're still the Director of the USA contingent of the Omo research expedition.

08-00:19:58
Howell: Yes.

08-00:19:58
Redman: So that project then, went with you.

08-00:20:00
Howell: Moved here. The money moved here from the comptroller.

08-00:20:07
Redman: How about your PhD students?

08-00:20:09
Howell: I brought with me, one student, who had been an undergraduate at Chicago, and who had followed and archaeologist and his wife to UCLA. I had taken him to the Omo the second field season. He was under threat for Vietnam and avoided it in '66. He stayed registered, I guess, for something in school, so I couldn't take him, but I did later, and he played a major role in the logistics and surveying and all kinds of things with the Omo expedition. His name is Gerald Eck, E-C-K, and he got a PhD eventually here. He moved out here with me when we move in '70. In fact, he even lived right next door, in Charlestown's basement apartment for several years, which was very convenient.

08-00:21:37
Redman: Actually you brought up a few interesting points about—and we've now gone through the 1960s. It seems as though, for lack of a better term, the times are changing. You talked about the free speech movement happening on campus,

and even though you didn't really notice it at the time because you were really so busy, but you also talked about drug use on the field sites, and then finally, the issue of the draft and students. So you've touched on some major themes for historians in the studying of the 1960s. Did you see, during that time, a change in the nature of the students, or did Vietnam affect the nature of people continuing into graduate education? Were there any themes like that, that you noticed in the 1960s?

08-00:22:40
Howell: Yeah I did, to an extent. I've forgotten much or many of the impressions, but at Chicago, we had minimum problems in the late '60s. I can recall some problems at the university. Remember, it's a small university, it's private not public, but there was some problems, there were concerns and so on. I can remember several instances of going to night meetings, which were held in the auditorium of the law school, and people were gathering there, the Academic Senate of Senior Professors, which included me then, talking about this and that and so on, and being worried about you know, would they do damage to buildings, automobiles, that is if it were to get out of hand, things like that. They did, of course, get out of hand severely in some—

08-00:23:50
Redman: This is rioters.

08-00:23:51
Howell: Rioters or students, or dropout students. They really did in some places, like Columbia University. There's a whole book about these kinds of things. Berkeley had plenty of problems because of People's Park, an area in the south part, off of Telegraph Avenue. So did some other places, to an extent, some less so, some more so. UCLA had difficulties. Wisconsin had some pretty serious difficulties during those days. Students in the '70s were—and it took a while in the '70s, at least the middle of the '70s, until Vietnam began to slowly evaporate, after Gerry Ford came in as President. It took a while, until the '80s, I would say, until a kind of normalcy set in, or late '70s, but there was tremendous feelings left, scars and so on, left from the '60s and Vietnam, that were very hard to overlook.

08-00:25:35
Redman: So kind of the political climate of student culture that time did, in some ways, affect the academic community in your mind.

08-00:25:45
Howell: I think it did. I think less so at Chicago than it did here. The extent that I had contact with other places of any magnitude, it was limited. I mean, you know, I didn't know, except indirectly, what was going on at Columbia. I was very much concerned with my own role in science and with science, and associates in science and colleagues in science, with the overseas circumstances and all those kinds of things. So frankly, I wasn't as publicly aware, deliberately

maybe, as I became much later, in my older age, certainly by comparison with now.

08-00:26:45
Redman: So for instance, you felt, on a personal level, your paying attention to the political developments in Spain and perhaps the political developments in Ethiopia.

08-00:26:59
Howell: Ethiopia, absolutely.

08-00:27:00
Redman: And as well as the political developments of the United States perhaps.

08-00:27:02
Howell: Yes.

08-00:27:02
Redman: But it was difficult to keep a tab on everything.

08-00:27:05
Howell: That's right. I was very much concerned with what was happening that was pertinent and so on overseas, absolutely.

08-00:27:17
Redman: So I'd like to return to Omo, since it looks like a watershed moment in your career, and some really exciting things happened there. I'd like to talk about two things specifically. We talked a lot about the mechanics of how the field site worked, and the challenges and difficulties there. But I'd also like to talk about first, what types of fossils specifically, you were finding in Omo, specifically Omo one, and how early on, you realized that that was significant. You talked about the dating techniques, and then also the ripple effect then, in that a number of articles throughout the '70s were coming out, after your work really ended there, about it seemed like it really influenced your work and your scholarship, the things that you were finding at Omo. Obviously, it influenced other scholars, but especially your work. So talking about the specific fossils there, and then how that really influenced.

08-00:28:22
Howell: Our real focus in the several geological formations, as their called, in the Omo Basin, what's called the Omo Group now formally, were a series of formations in the northern reaches of the lower Omo Basin. The biggest, most extensive outcrops are the Shungura Formation, and then there are smaller formations like the Usno and the Mursi, and so on. We determined early on, the ages of these formations through radiometric dating, through comparisons of the fossil content of the beds of the formations, and through similarities or analogies in sedimentary context. These were all related to ancient proto-Omo river or lake incursions of a proto-Rudolph, proto-Turkana incursion. A real effort was of course too, to seek to determine the nature of these deposits, the extent of the deposits, the composition of them, their age if possible, to be

ascertained by radiometric dating, which was then pertinent, as I explained, especially with potassium argon, and of course, to document as fully as possible, the fossil content of different formations and their members. Those were the smaller units within formations. These are the different kinds of things we pursued in the field over a total of nine field seasons. It was pretty much the same kind of thing most of the time, but in different places. Very often, returning to previously visited places that were inadequately worked, working them again. It also had to do with perhaps the application of new ideas. So towards the end of our work in the Omo, we made a real effort with Frank Brown and a colleague of his, who was then at Utah, to sample through representative parts of these members, of these formations, to try to get a complete documented record, if possible, of the magnetic orientation of the earth's paleomagnetic orientation through time. This had scarcely been tried before, except in this country or in deep sea cores, but it could be practiced today, done now. One of the major leaders in that—several of them were graduates from Berkeley, and they were either at Palo Alto, at the Geological Survey, or at Stanford, in the Geology Department. So there was a kind of regional interest in these kinds of things here and so on. We succeeded at that. We—I should say Brown and his colleague succeeded at that admirably. It was a killer job and took two field seasons of intense work. And then trying to sort out if there are any uncertainties, any ambiguities that need to be addressed, while you're in the field. It's a little hard to address them when you've left the field and you're back home and you say oops, I should have spent that afternoon doing so and so. So those are the kinds of things.

The fossils depend very much on where you are in the sequence and what's preserved, and the answer is everything from the size of shrews and rats and mice to things the size of hippopotami, mastodon, at the very bottom of the sequence, is still present at about five million years ago. Endless amounts of antelopes of all kinds, horses, and by that I mean zebrine kinds of horses. Three toed horses are still around at the bottom, rhinos, like the living ones, and different from the living ones. Extinct animals like kalaikathirs, that had sort of a horse like head, but with big, claw-like feet. They're an odd toed ungulate, long extinct, very well known in this continent, in the tertiary, also in parts of Europe, et cetera. I had special effort paid to finding places with micro vertebrates, that is rodents, insectivores, little things like that, and the last several field seasons, I took one particular person that I had actually met when I gave a lecture at the University of Colorado in Boulder, in the year actually I moved out here, I think, '70. I came out here and I found him here, and he came to study primate ecology, and he was very unhappy. I said, "Well you'll have to become involved with us." And so I took him to the Omo. He did fantastic work in the field, with finding the right places, working hand in hand with the geologists, working also with a French woman, who is from near Marsey, whose field is fossil pollen. She's a palynologist, it's called, and they did a lot of work together, a lot of surveying together and so on, because they each demanded certain kinds of conditions, in which either pollen would be preserved or microvertebrates would be preserved. So you had to have

special kind of sedimentary environments, and preservation of environments. They found a number of these through the sequence. Not as many as we had hoped for, but at least some, because other people elsewhere had had a lot of trouble finding these kind of things.

08-00:36:07
Redman: What was particularly interesting about the microvertebrates?

08-00:36:10
Howell: Well we're really interested because these are the animals that really tell you about what it's like in a small, local community, as to ecology, as opposed to making big estimates and extrapolations from things that may have very wide ranges, that is they have different kinds of—you can distinguish them at the sub-specific level.

08-00:36:37
Redman: OK.

08-00:36:39
Howell: And so that's the real reason, and we're trying to do things like that, not only through time, to talk about change through time, and composition through time, that is species appear, species live, species become extinct, but also species are replaced by other species that take their place. We don't always know why but they do. The pollen was done by this woman, Raymonde Bonnefille, with the French group, although she worked extensively with us, and our geologist, whose name was Henry Wesselman, Hank Wesselman, and he did a PhD ultimately on Omo microvertebrates. It was very interesting work. We also found birds in places sometimes. They were studied by somebody here in this country to an extent, so there were a lot of different things. And of course primates naturally, little things like galagos and lorises and things like that, monkeys, baboons, arboreal monkeys, leaf eating monkeys, colobine monkeys and humans, pre-humans and humans, and we have a long sequence. Over 340 something specimens of humans came from the Omo major, older deposits, and now their relationships can often be determined because we know more about them from more complete finds subsequently, at other places, and that's how we find them out. But when we initially found things, we reported them as best we could, figured them and so on. One could see that there were changes through time of the hominids, including Australopithecus, of several species. Homo like things appeared at a certain time and maybe a more advanced homo at a subsequent time, towards the end of the older part of the sequence, around a million or a million and two, something like that.

One of the final things that was done in the first half, in the very first year of the homo expedition in '67, was the concentration, not really by us, but we got involved with something else that proved to be about 3.4 million years old, and that was in the northern part of the basin, where Richard Leakey and his people were working, at a place called Yellow Sands. That was on the north

side of the river, and this was on the south side of the river, where we were, although quite a few miles south. They did some surveying and then asked if they could do some more surveying, were we going to do that on our side. I said no, go for it. If that's what you'd like to do fine. And they came up with two subsequent finds, one called Omo one, and then Omo two subsequently, and these were—I sent a geologist working with me, Karl Butzer, up to help them with working out the basic geology of these find spots and so on. They were outside the—they were much younger than any other part of the sequence, in what is called the Kibish Formation, not one of the others. They did a basic description of all this. Butzer did the very best he could for what time there was, and tried to use some isotopic methods to determine relative ages on shells and such things. We didn't do very well with that, or at least it was uncertain how accurate it was. In recent years, there's been a great interest in returning to this, because these are primitive modern humans, each of these skulls, one less primitive than the other, in my and some other people's view. And in recent years, there's been a return to that by people, including Frank Brown, as a geologist and a colleague of mine at Stony Brook University, John Fleigel, and other people. John Shea, archaeologist, also at Stony Brook. They have narrowed down, much more accurately, where they were found, precisely what the sediments are, the age of the sediments, the dating of volcanic ashes, which had never been dated before in that sequence, and they found out that the things were of an age somewhere between 160,000 and about 185 or 190,000 years ago, that is less than 200,000 years ago, which is in complete agreement with data from beautiful specimens recovered from Eastern Ethiopia in the last five or eight years, by Tim White and his people, working in the Middle Awash regions of the Afar Depression, and which really throws wholly new light on the way we look at modern human origins, first of all in a temporal sense. Second in the sense that it's Africa, and there's some confirmatory evidence or related kinds of evidence from the Sudan that relates to this, also from Morocco that relates to this. So these particular finds in Ethiopia, both in the east, in the Afar, and in the Omo, have to do not with ancient hominids only, but with this appearance of early modern humans, or the roots of modern humanity, which we certainly didn't understand at all. We didn't have any basis for judging before the work in the Omo.

08-00:43:49

Redman: So with each of these finds, you said about 340 or so, the hominid fossils. Did it ever become—and I don't want to use the word routine, but it seems as though with most of these, you knew that they were significant, but you didn't really realize how significant or in what way until subsequent research could be done or dating could be completed, and other scholars could kind of look at it and contribute to your ideas. So was there ever a point where, or was there always the excitement of finding another hominid fossil?

08-00:44:32

Howell: Well I think yeah, the excitement is adding to the understanding, and having more or better or different or whatever parts is what is important. We would

kind of eyeball things and say it's mostly likely like. We had a good grasp because we studied other things in Nairobi at the museum, whether in South Africa at the Transvaal Museum or in the anatomy department at Witz University in Joburg. We knew about these things, either from the literature or in almost all cases, firsthand, where I worked, and I took Don Johanson one year, his first year to the field, to go with me to South Africa and do a ton of work.

08-00:45:21
Redman: And his first year was?

08-00:45:23
Howell: '71.

08-00:45:24
Redman: OK.

08-00:45:25
Howell: I think it was '71. Yes, that sounds right, but I'm not quite sure now. Anyway, something like that. It was probably about '70, but anyway, I'm not sure. Much has been made of some of these things only subsequently, not by us, after we do the initial announcements and some things of that initial sort. The detail and more profound things have been done elsewhere. Some of the most detailed, insightful studies have been done by a co-student of Tim White's and mine at Berkeley, his name is Gen Suwa. He's Japanese and he has a position at the National Museum in Tokyo. He's a remarkable man, one of the best students I've ever seen, and did a remarkable dissertation and has published some fundamental things, or co-authored things with us as well.

08-00:46:44
Redman: And so it was really—the interesting thing is that the impact of that is felt throughout... I mean, it's not as though—and I know that in my notes here, that in 1976 it was perhaps your most prolific year. You published 15 articles, not including reviews, articles and chapters, and you wrote about things like China, Israel, Ethiopia, but much of it was about findings in Omo. The work doesn't stop there of course. There are constant reinterpretations of the significance of Omo, and people were returning to it, and so it's very interesting.

08-00:47:23
Howell: That often will be the case.

08-00:47:24
Redman: OK.

08-00:47:26
Howell: It's not that it sort of stops, but it sort of pauses, and there may not be further field work, there may be. There's a lot of further field work that was done in the Kibish Formation, to get this new data and so on, and to find some new

fossils actually. But not that, no. You come back to it because you're brought back to it because of developments elsewhere. It could be there also.

08-00:47:57
Redman: And you noted that a lot of what you were finally starting to conclude about Omo and the fossils you were finding was confirming some of the other ideas about Africa and about evolution's place in Africa. So, was that fulfilling to you, to add evidence as opposed to say, finding something that would completely throw off the paradigm. But you find adding evidence was very fulfilling to you from the sounds of it.

08-00:48:29
Howell: Absolutely.

08-00:48:30
Redman: OK.

08-00:48:31
Howell: Oh absolutely. No, there's nothing like solidification. (chuckles)

08-00:48:34
Redman: OK.

08-00:48:36
Howell: Yeah.

[End of Interview]