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Henry Schwarcz oral history interview with Dr. Bogdan Onac, January 22, 2008

Henry Schwarcz (Interviewee)

Bogdan Onac (Interviewer)
University of South Florida, bonac@usf.edu

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Spencer Fleury: Okay, you guys are set.

Bogdan Onac: Okay, thank you.

Henry Schwarcz: My name is Henry Schwarcz. I am a professor of geology at McMaster University in Hamilton, Ontario, Canada.

BO: And I'm interviewing Professor Schwarcz. [I am] Bogdan Onac. [We are at] USF. Today is January 22, 2008, and it's my second interview with a monster of the karst science—there was one guy that we just lost a few years ago, Professor [Marian] Pulina from Poland. He used to say, "We are mammoths, you are just small popples. You have to grow up to get mammoth like we are." So, Henry is one of these mammoths that have done a lot of things for karst. So, we will have a kind of small discussion and try to figure out how he got involved in caves and—well, some of the achievements, because he carries with him lots of very important achievements.

HS: Okay, thank you. So, I'm going to start out by talking about my early history as a student, because everything leads up to the point where I became interested in karst. I was originally born and raised in the city of Chicago in Illinois, and I went to the University of Chicago in the late 1940s and early fifties [1950s], which if anyone knows anything about isotopes will note that this was the time when Harold Urey, a Nobel Prize winning chemist, was developing the theory of stable isotope geochemistry, though he couldn't possibly have known how far it was going to go. And Sam Epstein was a

postdoctoral fellow in his lab, and a few other people whose names are probably less familiar in the karst community. Harmon Craig was also a graduate student at that time. And Cesare Emiliani, a very important figure, was a student of Urey's; and I knew all these people more or less, though I didn't, by any means, meet them all while I was at the University of Chicago in the first instance.

Now, I did my undergraduate degree at Chicago, which in those days was a liberal arts degree, and so really didn't have the opportunity to learn anything about anything isotopic for sure. In the last year I was there I became a student in the department of geology, and then I met Sam Epstein just before he was on his way to Caltech; and as it happened when I graduated from Chicago and then after I finished my one year of graduate work in geology, I also went to Caltech as a graduate student. And there I was sitting at the feet of Sam Epstein, and not very far away down the coast in La Jolla was Harold Urey and Harmon Craig. And I was sort of surrounded by stable isotopes, but not actually personally involved with them in a research sense because I chose instead to learn regular geology, mineralogy, petrology and metamorphic petrology so that my graduate degree—my Ph.D.—was on metamorphic petrology of silicate rocks. Nothing whatsoever to do with isotopes and certainly nothing to do with karst: but, on the other hand, the seeds were being planted in my head for something along these lines because I was, as I say, surrounded by these great fathers of the discipline.

Eventually, I graduated from Caltech and went to—back to the University of Chicago as a postdoctoral fellow. Now, one of the people who had been a graduate student at Caltech with me was Bob Clayton, and he became a professor, first at Penn State and then at Chicago, and I went to be his post-doc. In fact, I was his first post-doc, and I then really discovered stable isotopes for myself because of my Ph.D.—my postdoctoral fellowship was on the study of stable isotopes applied to, still, metamorphic rocks, not having to do with karst; but as it turned out, I was working on metamorphic carbonates and so I began to move sort of in that direction. And all the time that I was at Chicago I was also absorbing a lot of other isotopic information because I was in the Enrico Fermi Institute for Nuclear Studies, which is a seething hotbed of isotopic research of all different kinds. So, inevitably it was likely that I was going to come back. You know, if you think about it, the ideas that I've been pursuing for the rest of my life were stimulated by being in that kind of a very rich environment, first Caltech and then Chicago.

So, in 1962 I left Chicago and took up a position at McMaster University. Now, why did I go to McMaster University? It seems like an odd place to go—a university in a small town in southern Ontario not far from Toronto. Maybe one of the reasons—you could imagine one of the reasons was because Bob Clayton, the guy who I had just been working with, was born and raised in Hamilton, but that's actually completely irrelevant. In fact, he didn't even realize I was applying for a job there until after I got it. And the reason I went there [was] because of a man named Harry Thode. Thode is certainly not a very well known—not so well known a name in isotope geochemistry as he should be.

He was a giant in the field of sulfur isotope geochemistry. He really—in the North America, at least—he was the guy who really created the field of the study of stable isotopes of sulfur in biological processes in ore deposits and every other kind of environment. And so being that there was a stable isotope research program there, it seemed like a good place to go since I had just come from a stable isotope post-doc, and also, I was going to be a professor in the department of geology.

Well, at the same time when I came to McMaster, I also met the professors in the geography department—some of them, anyway, and one of these, I think—I'm not sure if he was—I think he wasn't there yet, but he arrived a couple of years after I got there—was Derek Ford. And Derek and I became casual acquaintances, shall I say—we became good friends in a way having nothing to do with research.

Ah! (catching a breath) So, now the story gets a lot more complicated because—

BO: Yeah. Moving to the karst now.

HS: Now we have to get into karst. Okay.

BO: Yeah. Derek was coming from Cambridge, or from—?

HS: Well, Derek was coming from Oxford.

BO: Oxford. Yeah.

HS: And he had done his degree with Margaret Sweeting on studying the karst of his native habitat; and you'll be hearing from Derek on this series, I gather, because he is going to be recording not long after me, and he'll talk about the Mendips and his Ph.D. research and so on.

But at any rate, I knew nothing what so ever about karst and about stalagmites and about anything of this sort. However, I did know about stable isotopes. After arriving at McMaster, I was—I should have continued studying the isotopes in silicate rocks, which is what I was had been working on at Chicago, but it turns out that's a little practically more difficult than working on carbonates, since to react a carbonate you just need to drop a crystal material in phosphoric acid and analyze the gas that comes off, whereas for silicates you have to have this fiendishly dangerous material—bromine pentafluoride or

fluorine gas or something like that, which scared me a lot. I had done it a little bit at Chicago, but I would prefer not to have to do it. So, I realized there were a lot of interesting problems still in carbonates, so I stayed with the carbonate problem. And then Derek pointed out to me that he had been working for a couple of years at that point about on these interesting looking objects made of calcium carbonate, which were called stalagmites.

Well, I told him that I didn't know anything about the subject, but I'd look it up. And so I went off to the library to learn something about stalagmites and the first thing I found was an article in the then quite young publication *Earth and Planetary Science Letters*—I think it was in there, anyway—by a couple of Germans. I can't actually remember their names now, but they had done a study of the oxygen isotopes in stalagmites, and they had shown very clearly that stalagmites were totally useless objects to measure isotopes on.

BO: I think it was [Johannes] Geiss?

HS: No, it wasn't Geiss. It was—oh, God, what was it?

BO: It doesn't matter, really.

HS: Uh—

BO: I just try to figure out—

HS: I can't remember, anyway, but the point is that they had done this analysis on stalagmites, which came from the mouths of caves—some which were very easy for them to collect. And they showed that they were highly fractionated, that the isotopic composition varied all over the place and didn't make any sense at all, and so they were useless material to work on. And so I told Derek this and he said, "Well, too bad. I guess we can't do anything with the isotopes." But at the same time, I remembered from my days working in—well, I better go back and explain something else about my history.

I was also involved with people who were doing radioactive counting experiments while I was at the University of Chicago. I wasn't actually doing the work myself, but I became very familiar with the kinds of equipment which was needed. Back in those days, the subject of low level radioactivity was very hot—actually that's kind of a pun, actually, because people were very concerned about environmental contamination coming from nuclear weapons testing. And so, techniques for measuring very low concentrations

of radio isotopes by various kinds of counting devices were being rapidly developed in the U.S. laboratories. In fact, the center of this work was in Oak Ridge, Tennessee, which was where the original atomic weapons production had gone on, and there was an Oak Ridge—Oak Ridge—I forget what it was called—Tennelec. I remember it was one of the companies—it was Tennessee Electric Company. It was one of the people [entities] who used to make—

BO: The first alpha spectrometer.

HS: Yeah, alpha spectrometers. Okay. So the idea was, you could count radioactivity using a solid state detector, and in particular, it seems that there was a way of determining the level of radioactivity in speleothems—or, let's say, samples which contain low concentrations of uranium—by placing a purified sample in front of an alpha detector. And I told Derek—or mentioned to Derek this possibility—that even though we might not be able to get useful stable isotope information from speleothems, at least we could determine how old they were, and that certainly interested him.

Now, the crazy thing at that point was that I was thinking about a method which I realized afterwards was totally nuts, and that was to look for the ratio of the two isotopes of uranium, uranium-234 to uranium-238. And I hope that this recording device that we're talking into doesn't mind a few technicalities or two, but at any rate I should just say that the half-life of uranium-234 is 250,000 years, approximately—249,000 years and change. So, it seemed like a very useful isotope for measuring the ages of processes going on in a cave and, particularly, determining the age of karst formations—of speleothems.

So, with that in mind we thought, well, we'll build a system for analyzing this. And we got a graduate student to work on this project. He was a student who had come to work with Derek, but he was a chemist by training. He had done an undergraduate degree in chemistry at [the] University of Hull, if I remember correctly. His name is Peter Thompson, and he later went on to found a magazine about caves. It was published in the western U.S. And Derek tells me just the other day at lunch—he tells me that Pete sold his magazine for a million dollars or something like that, and has kind of gone into retirement.

BO: Amazing.

HS: But it's possible to do such a thing. So, Pete Thompson and Derek and I then began to try to date stalagmites. Well, we needed a piece of equipment to do this. A uh, a uh (sigh)—what do you call it?

BO: Alpha spectrometer?

HS: No—uh. A spectrometer, yes. An energy spectrometer. What—I can't—the—one of my problems now is I can't remember the names of things.

BO: It's the—

HS: Uh—

BO: Let me see. The—

HS: Multi—uh—

BO: Multi-analyzer?

HS: Multi—! Yeah, analyzer. Yeah.

BO: Yeah.

HS: Multi-energy analyzer. What do you call the—

BO: Yeah, that's—well, that's the—that's the—

HS: Yeah. That's not exactly the name for it, at any rate.

BO: Multichannel analyzer?

HS: Multichannel analyzer! That's right, a multichannel analyzer. Thank you. Okay. And it happened that a professor in our physics department, who was working in the nuclear research building, had a multichannel analyzer that he didn't need anymore and so he gave it to us. Except he didn't move it. We left it there because it was too big. In those days, a multichannel analyzer was an object that was about five feet tall and three

feet across and weighed about three hundred pounds or five hundred pounds or something like that.

BO: Having huge energy sources.

HS: Yeah. It was amazing!

BO: Yeah.

HS: The thing—I mean, if you think of it today. And so, Pete started using this. Now, about this time—and you know, my exact chronology of all these things is a little obscure. Like, I came to McMaster in 1962, and I think the events which we are describing are happening around 1968 or something like that, sixty-seven [1967] or sixty-eight [1968]. And about this time, a number of very important things were happening. Not at McMaster, but elsewhere in the world.

First of all, a guy named Chris Hendy was located in New Zealand, and was getting his Ph.D. at the University of, uh, Waikato, I think—in Hamilton, New Zealand—was discovering that the original studies of speleothems and their isotope characteristics were wrong. The stable isotope characteristics were wrong.

He was showing that if you went deep enough inside of a cave—something he was quite capable of doing, since he was a good amateur caver—that he would find that stalagmites, which were growing deep inside the cave, were growing in oxygen isotope equilibrium. And, in fact, he was able to develop—he developed a test for whether a stalagmite was growing in equilibrium, a very clever idea that—that—well, he showed that since in a situation where the water on a stalagmite surface is not evaporating, its oxygen composition should remain constant all over the surface. And therefore, if you analyze one growth layer, which was formed from that layer of water—if it didn't show any variation in oxygen isotope ratios, that meant that the—that particular layer, at least, was growing isotope equilibrium. And if you measured several layers in the same stalagmite, you concluded that the entire stalagmite was growing in oxygen isotope equilibrium with its drip waters.

Now, going back in my story to the very beginning while I was at Chicago in 1950, at that moment, unbeknownst to me, Harold Urey and Sam Epstein and others were showing that if you had a calcite that had grown in oxygen isotope equilibrium with water, you could measure the temperature at which it had grown. And that seemed like a good idea. So, now Chris Hendy suggested that we could use this method to determine

paleotemperatures inside caves. Of course the problem with a stalagmite is that if you know what its age is you can obtain a record of its oxygen isotope ratio back through time, but it's only the record of the calcite part of the story. You need to have a record of the water as well. And, at least at that point, it didn't seem as if there was any water you could analyze.

So, what Chris and his supervisor—and again my memory fails me. [Alec Wilson] But, uh, Hendy and—oh, I just don't remember the guy's name. He was his professor at the University of—I think it may have been—yeah, Waikato. They concluded that what you would be able to learn was some notion about the gradient in temperature between the equator and the place where the—

BO: Cave is located.

HS: Cave is located. That would be about—because they argued that the water would evaporating from the equator at a certain condition and then the vapor was moving to the cave and falling out of the sky and entering the cave, et cetera. So, using this method they published the very first series of speleothem temperature records—or, not temperature records; we'll say speleothem isotope records with some kind of paleoclimate suggestions attached to them.

So, we realized that this opened—we; that is, Derek and Pete Thompson and I realized that this now opened up a whole new avenue of study, which is to measure the oxygen isotopic composition of the same speleothems that we were dating, and so Pete set to work doing this. And this wasn't a very difficult thing for us to do, because we had a stable isotope system. We had the mass spectrometer and a, uh, vacuum line, in which we could react calcium carbonate with phosphoric acid. In those days, by the way, the, uh, the mass spectrometer that I was using was a, uh—

BO: One room installation.

HS: Was a homemade machine that had been built by a former graduate student of Harold—of Harry Thode's, and it had been just sitting sort of idly. And I acquired it because on my wedding day, uh, I was—I had a wedding dinner and I had invited Harry Thode and his wife to come to the wedding ceremony. And at the end of the dinner, he said to me sort of casually, he said, "Would you like to have a mass spectrometer?" And so, in a sense, he gave me this mass spectrometer as a wedding present.

BO: Ah, good.

HS: So, that was a good way to get a mass spectrometer. (laughs)

BO: Yeah.

HS: Okay, so this mass spectrometer sat still in the nuclear research building. We didn't really want to keep it in our lab because it needed to have too much kind of life support systems from technicians and liquid nitrogen supply and so on. It was a funny old machine that was rather cranky. But eventually we moved it to the, uh—a new laboratory was built for me in a brand new building that was constructed at about that time, and I now had my mass spectrometer in my own lab, and I think Pete started to use that machine before he finished his doctorate. I can't exactly remember the timing of this, but I think he did—any rate, his Ph.D. thesis was published partly as a paper in *Science* and it focused on the stable isotope record of the speleothem—of a stalagmite from West Virginia.

BO: That was seventy [1970], seventy-one [1971] or something like that?

HS: Uh, yeah, seventy-one [1971] or seventy-two [1972]. Something, something about that. Yeah, about that.

BO: So you also, uh, made all the dates, already?

HS: Yeah, we already were doing [that]. So, Pete was doing the uranium series dates—oh, yeah! By the way, that was another thing which happened. I think I left out a part of this story. This is the problem with trying to do this kind of a—

BO: Development of the—

HS: Development. I think, 'cause—let me just say that, uh, there was another foreign individual in this story who should be mentioned at this point, and this is this guy we were talking about a moment ago, Cherdyntsev.

BO: Yeah, from Russia.

HS: From Russia. Now, this guy—Vladimir? I can't remember what his first name was. Anyway—Nikolai, maybe. Nikolai Cherdyntsev [Viktor Cherdyntsev] had published a book, which had the curious title of *Uranium-234* [English translation of *Uran-234*]. And why would you write a book with the title of *Uranium-234*? Well, it wasn't really about uranium-234. What it was was a book about the way in which the daughter isotopes of uranium gradually grow into radioactive—or what we call secular equilibrium with their parent isotope. And the important point is that uranium chemically—the chemical element uranium is separated from its daughter isotopes when water—when the uranium is dissolved in water and moves through the Earth. And, as a result, there is a possibility for finding a sample which contains uranium that doesn't have all of its daughter isotopes attached to it—locked in the crystals of a mineral such as calcite. And this process of trapping uranium without its daughter isotopes in the crystal is beginning a clock—is setting a clock to zero. And the hands on the clock basically, or the movement of the clock, is recorded in the growth of the daughter isotopes back into equilibrium with the uranium.

Now this idea really hadn't been at all fully developed by any other scientist to my knowledge until then. There were some other people who had worked on radium disequilibrium and on radon disequilibrium, but looking at the isotopes of uranium and of its daughter thorium-230 really hadn't been really worked out until Cherdyntsev. So, he showed that you could determine the age of a calcium carbonate deposit.

BO: Looking at this—

HS: By looking at the ratio of the thorium-230 daughter to its parent uranium-234, and also, the ratio of the uranium-234 to its parent uranium-238, et cetera. So, these two simultaneous decay processes gave you a clock with which you could determine the age of some kind of calcium [deposit]. But he hadn't really applied the method very extensively. So, realizing that this was another way of telling time which was much better than the $^{234}\text{U}/^{238}\text{U}$ ratio of measurement, which I had originally in mind, Pete Thompson set to work doing these measurements. And the 1971, or seventy-two [1972], paper on the Virginia caves—and, furthermore, his thesis—contained the first uranium series dates on speleothems, uh, that actually were really pointing towards some useful kind of paleoclimate information.

BO: Information, yeah. (speaking at the same time)

HS: Okay. So that was the beginning of everything.

BO: But, you know, I was at the very early time when I start[ed] doing alpha counting.

HS: Yes.

BO: Because even in ninety [1990], when I went to Stein-Erik Lauritzen in Norway.

HS: Yes.

BO: And he was at your lab for a while.

HS: Yes. He was still doing alpha counting.

BO: Yeah, he was still doing alpha counting. So I had this column preparation, column separation.

HS: Yes. Yes.

BO: Purification. How was this stuff at the very beginning? Because—

HS: Okay; well, I'll tell you there's another story that—as usual, I'm leaving out little bits of this story. In the—during the Second World War, the U.S. developed the technology for making nuclear weapons. Now, in order to do that, it had to perfect a huge amount of radiochemistry of the nuc—of the, uh, transuranium elements and of the elements of—in the family of uranium. And all of the information coming from that project, from those projects and the related information, was eventually published in technical manuals which you could buy from the U.S. government, which described in great detail how you could use ion exchange resins to separate uranium and thorium from other elements. And at the same time, there was a marvelous chemist in—I think he was Vienna—by the name of [Johann] Korkisch, and he had picked up on this thing and he was expanding on it and developing even more elaborate methods. So, between the work of Korkisch and of the various researchers from the—

BO: Army. Okay.

HS: —from the Manhattan Project, you now had some techniques for separating uranium and thorium isotopes and even protactinium, which was an element we didn't really pay

that much attention to, from the other constituents in a rock; and, therefore, be able to concentrate them and put them in front of the uranium—of an alpha detector and measure the uranium—the alpha spectrum with a multichannel analyzer.

BO: Yeah.

HS: So, it was really thanks to the, uh, atomic bomb project that we were able to do this, because if we had had to develop this, or, you know—

BO: It would have taken some more years.

HS: It would take many years, and frankly, I would never have done it because I was not about to become a radiochemist, which was for the—now, fortunately for us also, Pete Thompson has his background in chemistry, and if I'd had a biologist or a, you know, whatever, or a geologist, even, for a graduate student, it would never have taken place either. But because Pete [was] smart enough to realize what steps in these procedures were important and what you can change and modify and fool around with, he got the process going to the point where we were able to actually do this.

No. (laughs) You know, the funny thing about this is that back in those days, although we knew pretty much how to do this, we were pathetically inefficient at doing it. And by that I mean that—there's a quantity called *yield*, and yield means what fraction of the total number of atoms of what you're looking for that were in the sample eventually end up on the—on your detector or in front of your detector. And we would typically get yields which are on the order of 5 percent, 10 percent, 15 percent. And with yields like that, it was very difficult to count enough activity to be able to do this. And even to the very end of the days in which we were doing alpha spectrometry, I would say we were very happy if we got an analysis which gave us a yield of 50 percent, and I don't even now know why it is that our yields were so poor.

But in spite of these terrible drawbacks in the chemistry and the fact that the technique was working in such a ridiculously inefficient way, we still were able to get precisions on the order of 5 to 10 percent in the uranium—in the uranium series dates, in the alpha dates, and that was pretty good for telling the age of stalagmites back to, let's say the last interglacial. Now, let me just—can I—

BO: Sure.

HS: If I could just go on talking about some of the background of this thing. At about this same time another group, interestingly, totally unbeknownst to me—and I eventually got to know these guys very well—was developing, uh, U-series [uranium series] dating of speleothems. Actually, there were two groups.

There was a laboratory in—at the University of Pisa under the direction of—well, the person who actually did the dates was a woman named [Giuseppina] Fornaca-Rinaldi. I forget her first name, but that was—she had a hyphenated name, Fornaca-Rinaldi. And she and her supervisor who, I think, was named [Ezio] Tongiorgi—yeah, I'm pretty sure it was Tongiorgi. They extracted uranium and thorium from speleothems and they claimed to be able to determine their age; and they actually applied the method, interestingly, to speleothems from some archeological caves. Now, I'm mentioning this now because—

BO: We will go to the—

HS: This—we are going to get to that a little later on.

BO: Yeah. (speaking at the same time)

HS: But I'll just say that this is one of the ironies of my life, that I always find myself at the right place at the wrong time. In 1968 and sixty-nine [1969] while Pete Thompson was laboring back at McMaster to finish his doctorate, I took my first sabbatical leave, and where did I take my sabbatical? I take it—took it in the Istituto di Geologia Nucleare at Università di Pisa, Via Santa Maria 22, in a building which was built probably in the fourteenth century and had walls which were about two meters thick. So, it's rather difficult if you ever wanted to run a pipe from one room to another in this building, because you have to drill a long way.

Any rate, so, in this laboratory I was working not on stable isotopes and speleothems, strangely, because I had—back home I had a student who was working on it. But I was working on oxygen isotopes in sulfates. Now, I must admit that in my whole research career, my contribut—the various things that I've made contributions to, I would say probably the least important study I ever did was the oxygen isotope studies of sulfates.

Uh, but I had a good time in Pisa because it's in Flor—in, uh—

BO: It's not far from—

HS: Not far from Florence.

BO: Florence. (speaking at the same time)

HS: And it's in—

BO: Close to the sea.

HS: And it's in Tuscany, and we ate very good food and my wife studied how to play the cello, and generally I had a very good time, and I was on a Fulbright Fellowship, too. And I'm sure the Fulbright people got their money's worth from what happened afterwards. But in the course of this, I got to know Fornaca-Rinaldi and saw what she was doing, but I was not really that interested in what she was doing at that point because this was just before we made our own—

BO: Karst contribution.

HS: Karst contribution to this thing, and I realized only afterwards that she was doing it—doing the right thing by doing it the wrong way, and that the results that she was getting were probably rather—rather screwed up, shall we say.

Uh, listen, if we could take a break, I need a glass of water.

BO: Uh—

HS: Is it possible, possible to get a drink of water somewhere? [If] I've got to keep talking like this, you have to—ah! There we go. Perfect.

BO: Brand new.

HS: A brand new bottle. This [is] a very important piece of equipment for this interview. “Zehpher-rillis?” Zephyrhills, okay.

BO: That's just across the campus, on the other side, a couple of miles from here.

HS: Okay, there we go. All right, so, um—so, I've just mentioned this in passing, because there I was in the same building with Fornaca-Rinaldi and I wasn't really paying that much attention to her. But a little while later, obviously, we became interested in this and were doing exactly the same things that they had been doing.

And then the second laboratory [person] that I want to mention, which is much more relevant to what we're talking about here, was that Jean-Claude Duplessy, who is in the—well, at that time was in [the] University of Paris, I believe. I'm not exactly sure, but somewhere in France, anyway. And he had picked up on the work that Chris Hendy was doing, and so he found a cave in France, which had a stalag—at least one stalagmite and he did an isotope profile in it, and—

BO: The quarry was Clamouse [La Grotte de Clamouse], if I remember well. The paper

HS: Was it Clamouse!?

BO: It might be.

HS: Oh. Well, maybe; it could be, could—very likely, because it was certainly a cave you could get a—easily get a stalagmite from.

BO: Easy access and lots of speleothems in.

HS: Yeah, okay. So, there he did this paper and he showed that there was a kind of isotope signal in the thing. So, uh, this was at about the same time that we were doing our work. Now, the interesting question was what the isotope's signal was telling you?

Um, you know, there's a curious thing about all the work that has been done on speleothems in those years and every subsequent year to the present, which is that people have very widely made use of plots—graphs of the relationship between the $^{18}\text{O}/^{16}\text{O}$ ratio, the $\delta^{18}\text{O}$ value of a stalagmite, and time, and said that this is a record of paleoclimate. The problem is that with some graphs the high point of the $\delta^{18}\text{O}$ represents a warm period and the low point represents a cold period.

BO: (inaudible) Cold period.

HS: And on other graphs it's the reverse. So, it appears that this is a very ambiguous signal. It tells you something about climate, but you have to know how to interpret it at each particular location, and in particular at, uh—if you have a very short record which happens to include, let's say, a glacial and an interglacial transition, uh—and in those days we didn't even know what the date of what we now call [Marine Isotope] Stage 5e is. So, you might have some argument as to whether the top of your peak represents the warm period—

BO: The warm period. (speaking at the same time)

HS: —and the bottom of it represents the cold period or the other way around. And, in fact, Duplessy and Emiliani got into a bit of fight over which was which, because Emiliani was having an argument as to what the date of the last—the warmest part of the last interglacial was, and in those days he believed that it was 100,000 years and not, as we know it today, as 130,000 years. And so—and that—it's kind of getting off—

BO: History.

HS: Hmm?

BO: It's history.

HS: It's an interesting bit of history, but we were kind of just sort of innocent bystanders of this story. But at the same time we were realizing from this the kind of problems that you would get into as you got further into the study of these isotope variations of oxygen in the calcite component, all right.

So, in this same period, and actually, still while Pete Thompson was—I was surprised; the other day I was looking at Pete Thompson's Ph.D. thesis, and I had forgotten that he was still completing his thesis when we began to do fluid inclusion work. And in fact, he included some early fluid inclusion studies in his thesis.

So, what about this fluid inclusion story? We are talking now about the problem of

determining paleotemperature using the concept that Chris Hendy had invoked of applying the calcite water fractionation to a speleothem. Sometime around 1970—maybe seventy-three [1973] or seventy-four [1974], I can't exactly say when—I guess I was looking at a book on—fluid inclusions, water fluid inclusions. Geologists, for a very long time going back into the nineteenth century, had known that some rocks contain small, uh—

BO: Bubbles.

HS: Bubbles. Pockets, little enclosures of—which are filled with water and, if the rock is in a rock form that a high temperature as in deep in the earth's crust, the water might be very salty; it might contain a lot of crystals in it as well as the water. But, uh, apparently it hadn't occurred to me or anybody—why should they think of it?—that there might also be fluid inclusions. We call these things, by the way, fluid inclusions. There might be fluid inclusions in stalagmites, and in fact in speleothems in general—flowstones (inaudible)—and even stalactites. Well, that might be an interesting thing to look at because the fluid inclusion is believed, generally, to be a sample of the water which was present at the surface of a crystal at the time the growth surface was just passing the point where that fluid inclusion was forming. And so, therefore, it would be a trapped sample of the water—

BO: From that time.

HS: —from that time. And if you have a stalagmite which grew over 50,000 years, the fluid inclusions could, in principle, give you a sample of the history of the water through that period of time. Now, what can you learn about that water? Well, there's not much dissolved in the water. It's not a question of determining the change in the salinity of the water or trace elements, but certainly the important thing for us was the isotopes in the water.

Hydrogen and oxygen make up the water. So, we could learn something about the hydrogen isotope ratio and the oxygen isotope ratio if we could extract the water, or so it seemed. The first trick we had to do was to develop a way of extracting the inclusion from the speleothems. By looking at the speleothem—at some samples of stalagmite under a microscope, I concluded that they must contain about a tenth of a percent of fluid inclusions by weight. I was just guessing on the basis of the area—of what fraction of the area of a thin section of stalagmite was hollow, was—

BO: (inaudible) Emitting!

HS: An inclusion. And so, I figured we needed to crush about one gram of stalagmite to have enough water to analyze. Ah! (sighing). So—

BO: This was already in seventy [1970]?

HS: Well, I'm not sure whether I started to do this—I know—well, okay; the point is that by 1976 we published the first paper on fluid inclusions and—how were we measuring the hydrogen isotope ratios? See, this is what I can't remember. Ah, this is a good question. (laughs)

BO: 'Cause I'm puzzled because recent—nowadays you are also trying to develop a better method to do extraction of the fluid inclusions.

HS: Yeah. Yes, right, right. (speaking at the same time) Okay, yeah, but—okay, what I'm trying to figure out now is, where did I do the hydrogen isotope measurements of this? Because at that point I don't think we had a hydrogen isotope mass spectrometer. We must have had, though. Oh, that's weird. You know—hmm. I'll have to go back and think about that.

At any rate, at that point we first developed a kind of weird crushing device which consisted of a kind of a bellows, a copper or flexible metal bellows, and a screw, which allowed us to screw down this bellows and crush the stalagmite, the piece of stalagmite, until some of the inclusions trim out.

BO: Are released. (speaking at the same time)

HS: Were liberated. And then that didn't work so well, and particularly, the thing began to leak very quickly. So, this was the stage of Pete Thompson's thesis. And then, later on, when Russell Harmon came to do a Ph.D. with me, we moved on to a more elaborate method, which actually worked pretty well and which we stayed with for some time, and that was to take a length of stainless steel tubing and to put a piece of stalagmite in the middle of the tube with some glass wool on either side so it couldn't move around, and then we evacuated this tube with some kind of flanges, which he put on either end. And then we put this under a hydraulic press right at the spot where the stalagmite was located and the hydraulic press crushed the stalagmite and then liberated the water, which was trapped in the fluid inclusion, and then we were able to analyze the δD . And—okay, so—this is really baffling me now because I can't—I should be able to remember this. Any rate—

BO: You are right; there are so many things happening.

HS: Yeah, happening in my lifetime that I can't even remember what I was doing.

But in any case, uh, we showed, first of all—well, what I should say is that it was pretty obvious from thinking about the question that we shouldn't be looking at the oxygen isotopic composition of the water, because the water itself had been trapped inside of, you might say, a bottle which was made of calcite for 50,000 or 100,000 years and had probably had continued to exchange its oxygen isotopes with the bottle so that it no longer remembered the original oxygen isotopic composition. But there was no hydrogen in the bottle. So, therefore, the hydrogen isotope ratio, the ratio of deuterium to hydrogen in the water, would still be preserved and remembered from the time when the stalagmite had grown.

Now, it turns out that there's a very good relationship between the hydrogen isotope ratio and the oxygen isotope ratio in drip waters in caves. We showed that ourselves measuring drip waters from caves, and it's also known in—since the drip water is basically a sample of meteoric water, it's much better known in meteoric waters from the work of—

BO: Craig and—

HS: Craig, and of [Will] Dansgaard before that. That—

BO: Meteoric water line.

HS: There was this so-called meteoric water line, which relates to δD and the $\delta^{18}O$ of the water. So, we could, therefore, use the δD value to infer what the $\delta^{18}O$ of the water must have been, and we had to assume that the meteoric water line had remained constant through time. And we did this. We measured the paleotemperatures of speleothems by this means, and the first paper on this was published in *Geochimica* in 1976, and I was the senior author and Derek and Pete Thompson and Russ Harmon were the co-authors on this paper because they had all contributed to the analysis and, of course, the samples and the dating. And then we tried to move on with this method, but things got a lot more complicated very quickly.

First of all, I had a marvelous graduate student, or Derek and I had a marvelous graduate student, named Chas Yonge, Charles Yonge. Now, Charles—

BO: “Young” or “Young-gee”?

HS: Yonge.

BO: Okay. Because he was in Romania [at] one of the meetings I organized.

HS: Yes.

BO: And everyone was calling him “Young-gee.”

HS: “Young-gee.” Well, no, his name was really Yonge.

BO: Maybe. But he spent some time in Indonesia and—

HS: But he was a very quiet guy, and if you called him “Young-gee” he wouldn’t bother to correct you.

BO: Okay. (laughs)

HS: Very quiet guy. Very nice person, and he was a physicist by training. So, he set to work to build a deuterium-hydrogen mass spectrometer for our lab, and not only that but to do a lot of work on figuring out exactly what was going on with fluid inclusions in speleothems. Now, in fact, a lot of the work that he did ended up being more of the drip water aspects of—because with the hydrogen isotope measurements, we can now further confirm that we had done—we were doing the right thing by assuming the meteoric water line for the drip waters because, in fact, the drip waters we could show were on the meteoric water line.

But then Chas did a whole bunch of experiments [in] which he heated the stalagmites and showed enormous excursions in the deuterium-hydrogen ratio as the water came out of this stalagmite while it was being heated, and this scared the hell out of us because we didn’t know if these were real variations in—like if there were multiple populations of

fluid inclusions which were giving different isotope ratios and which came out at various times or what was going on. And it wasn't really only until, I would say, probably last year—a very nice study, which was done, that really has led to some understanding of what goes on when this heating is taking place.

But, at any rate, Chas and I worked together on this for a few years, but unfortunately, not very much of what he did was published. In fact, people still refer to his Ph.D. thesis.

BO: Yeah, [it was] at least one more paper that I, well, actually recently used.

HS: Yeah.

BO: Because it's one of the first that compares the precipitation a little bit—and you got a lot of samples from throughout North America and Canada in that paper.

HS: Yeah, yeah. (speaking at same time) Yeah, and then we published—there was an International Atomic Energy Agency symposium on the history of—isotopic history of water or something like that. I think it was organized by Peter Fritz, at that time, who was at Waterloo [University]. And Chas and I gave a paper at this symposium in which we presented some of these fluid inclusion data, and I guess that was about the major publication on this thing. And then there was one more paper that Russ Harmon and—oh no, there's another marvelous person I should—whose name I should mention here, which is Jim O'Neil.

Jim O'Neil is, of course, very well known in the isotope—stable isotope community for having done a lot of calibration of isotope fractionations in silicate systems. But he was also a student at [the University of] Chicago when I was doing my postdoc and so we've been old friends. And he was—at the moment that Russ Harmon and I were working on this stuff, Jim was a postdoc at Caltech. And so, I went to Caltech—oh! Yes, I think that's right. Yes, I went to Caltech, and Pete Thompson was also at Caltech at that time, by the way. That's another story. He had finished his doctorate and he went there as a postdoc to work with Sam Epstein on something totally different; he left speleothems altogether and went to work on cellulose, on hydrogen isotopes and oxygen isotopes in cellulose.

At any rate, so I went to Caltech and we did the analyses and published a paper, which kind of firmed-up our understanding of the isotopic composition of fluid inclusions, at least the hydrogen isotopic composition of fluid inclusions. And out of that we, I guess, concluded that it was in theory at least possible to do significant paleotemperature

measurements. The problem was that until Chas Yonge came along, we didn't really have a stable hydrogen mass spectrometer to do this on, and after Chas, left the machine really wasn't working very well. He seemed to be the only person who was able to make it work—

BO: Knew how to run it. (speaking at the same time)

HS: —because it was a homemade machine. Again, one of these machines that had been put together from bits and pieces that had been sitting around. And so, I kind of stopped doing stable isotopes with speleothems, or let's say fluid inclusions, and went on to doing other things. All right, so that's, you might say, the end of chapter two, which is the end of my early work in fluid inclusions; and we'll come back to fluid inclusions a little latter on.

BO: More, yeah, more recent.

HS: Okay.

BO: Let me ask you something about Chas, because I'm not sure if he was still with you or he was somewhere else when he—or if you are behind this stuff, actually—when he had done the stable isotopes in ice.

HS: No. That was definitely not me at all.

BO: Okay.

HS: No. I didn't—in fact, I wasn't even aware that he'd done such stuff.

BO: Yeah, he [did] have some—one of the glaciers—cave, uh—ice deposits in one of the caves in Canada.

HS: Ah, right. No, that's totally—I think he went off on his own at this point because—

BO: He had some oxygen and hydrogen measurements.

HS: Because he went off to Alberta to work in the laboratory of John Gray, who is a professor—was a professor at that time in physics, and he worked—helped John to do analyses of stable isotopes in various types of materials and that's probably when he did this work.

BO: I see. I see. So, basically—

HS: Okay.

BO: Basically, McMaster was [at] the forefront of dating, beginning of dating, regular datings on speleothems.

HS: Yes. (speaking at same time)

BO: I mean, Fornaca—they tried something in Pisa. They—I think Duplessy—[there] was only the one paper that he had.

HS: That was the only paper he did on that.

BO: Yeah. So—

HS: After that he dropped it completely.

BO: Yeah, I would not say that was a real laboratory working on speleothems.

HS: No.

BO: So, basically McMaster is the pioneer.

HS: I guess we were, right.

BO: Yeah.

HS: And we kept doing it and kept doing it, because after that we had Mel Gascoyne, who came and did his Ph.D. on more speleothems.

BO: Geochemistry and some other things.

HS: Yeah, but he was really working on dating of speleothems.

BO: Yeah.

HS: And he developed the protactinium method, which we didn't really take too seriously, but he showed how you could use this other isotope protactinium, which was the daughter of the rarer isotope of uranium, U-235.

BO: That's very used in coral dating.

HS: Yeah. It has been used; it's never really caught on in a really significant way because there's all kinds of problems with, uh—

BO: And Larry [Lawrence] Edwards, in his lab, was doing quite well.

HS: Yeah, yeah, right, yeah. (speaking at the same time) I mean, now that you can do it by mass spectrometry, I guess that it sort of picked up a little bit, but—at any rate, so, he did a bit of protactinium work. And then the other person who I have to mention, just while I'm kind of reviewing the people who worked with me, was Alf Latham, who did this amazing study of paleomagnetism on speleothems.

BO: Mexico.

HS: Yeah, working in Mexico and various places, and showed that you could reconstruct Holocene paleomagnetic variations very nicely with speleothem samples even though we didn't really know, and probably even to this day we don't know, what the, uh, (sighing) magnetic carrier is in the speleothem. We assumed it's very finely divided magnetite particles, but—

BO: Or hematite.

HS: Or hematite, but we could never really do the convincing study, which would be to do a—like a heating experiment to tell what happens, because when you heat stalagmites other things happen to the stalagmite before the signal begins to change. (laughs) Like, it becomes decarbonated. All right. Anyway, so—

BO: I just have one point to finish with your chapter now, because this is actually your history, and then I just wanted to tie it to something else. When I moved back from Norway, doing part of my Ph.D., and go back to Cluj, I was talking to Derek. I said, “Hey, Derek, you have an alpha spec. I know you moved on [to] TIMS [thermal ionization mass spectrometry]. What are you doing with that?”

HS: Oh! I remember this.

BO: He said, “You want it!?”

HS: I remember this so well.

BO: And he said, “Yeah, I have it.”

HS: And so Derek said to me, “You know, we have these alpha spectrometers. Why don’t we give them to our friends in Romania?” and I said, “Marvelous idea! Pack them up and send them off!”

BO: Yeah, and unfortunately, they came to Budapest and they [had been] shaken on and off and something like that.

HS: Oh, no.

BO: So, we got it into Cluj, finally.

HS: Yeah, right.

BO: We thought—it was a kind of smuggling over the border because if I would tell anyone what it is—and, actually, I told one.

HS: Oh, God. (speaking at same time) Oh, right.

BO: You know, the small chambers for measuring that—

HS: Yes, yes, right.

BO: And somebody asked me and I said, “Well, I’m [being] frank, I can’t tell you what is it,” and he said, “Okay, in my list, it doesn’t appear to have this kind of [harmful] device. So, if I will send [to] Bucharest a mention that you are want[ing] to cross, they will say, ‘Okay, we don’t have it in the list you have so [you will have] to wait months.’ So, [I am] pretending I have seen nothing.” So, we move everything to the university, put it down into the basement [and] manage to find the source because all the chambers came without [a counting] source.

HS: Yeah, that’s right.

BO: And the only problem we run into was the multi-chamber analyzer because of this shaking off and on.

HS: Shaking. [It] wasn’t working.

BO: It was lighting up, but the big screen to show you the spectra was not.

HS: Oh! Right. Oh, God. (laughs)

BO: So we had to find the physicist to look in it, and we managed to put it [to] work for a couple of weeks and then something [would] break down finally.

HS: Oh.

BO: Yeah.

HS: All right, well—

BO: That's the end of the alpha spec.

HS: So, you never got dates from it?

BO: We managed to get, like, three dates or something.

HS: All right.

BO: And then it's gone.

HS: Ah!

BO: Yeah. Well, we'll move on to ICP [Inductively Coupled Plasma] [spectroscopy] now. (laughs)

HS: Okay. You and everybody else moved on. All right, so let me move on to, should I say, chapter three. (clears throat) This is Henry the archaeologist.

Around 1974 or seventy-five [1975], let's say, my wife, who was watching me do all these strange things, said to me one day, very casually, she said, "Rocks are boring," and I said, "You know, you're right. They are rather boring for most people, but for me they very interesting." [And] she says, "Yes, but why can't you work on something which would be more interesting to me?" And since I was a very agreeable husband, I said, "Well, I'll look for something to see if there was anything I could work on."

And just about that time—I can't actually remember the exact circumstances, but there was a cave in New Mexico—and you know, again, it's going to be difficult for me to reconstruct the exact sequence of events. But this was a cave called Sandia Cave. And from the name you would guess that it's probably located in the Sandia—

BO: Mountains.

HS: Mountains, which are just next to Albuquerque. Indeed it is. And there some time in the 1930s, I think, a Boy Scout who was climbing around there discovered some Indian remains in the Sandia Cave. And so archaeologists began to dig in the cave, and by the 1960s or so, they had uncovered a long sequence of deposits. But the interesting thing was the deposits were capped by a stalagmite—actually, by a flowstone, I should say. Not a—by a flowstone. And so, I had been talking to someone—I don't even remember how, who or where—and this subject came up and I said, "You know, maybe it would be possible to date this thing."

So, very soon afterwards, I think it was Vance Haynes, who was a very noted archaeologist in those days in Arizona, managed to get a sample of the stalagmitic layer from Sandia Cave and send it to me. And I dated it and we put it through our mass spectrometer—through our alpha spectrometer system, and it turned out to have an age, pretty clearly, of 250,000 years. Ta-da! What? We have humans forming deposits, which were covered by a stalagmite 250,000 years ago, [an] epoch making discovery.

Well, of course, it didn't turn out to be that way. What we're looking at here, in fact, was a crevice in the cave where the flowstone had been sitting on top of some softer material; the softer material had been eroded away and then the Indians who—I mean, not the Indians—the people who had been living in the cave had created deposits.

BO: Shelter. (speaking at the same time)

HS: Under the shelter. [They] had inserted themselves under this level, and that was pretty obvious to me. But there was at least one crazy archaeologist who unfortunately heard about this discovery that I made, so-called discovery, and began to claim that this was a natural age, and so I had to fight pretty hard to make sure that this was never published as a statement that the age of this deposit was 250,000 years. But it turned out [that] a light in my head [went on] somehow. [This was] the idea that one could use uranium series as a method of dating archaeological stalagmites.

So, I first went off to the libraries to start to look for other examples of caves where there were stalagmites associated with—or flowstones or whatever—associated with speleothems, associated with archaeological sites, and indeed there were a lot. And it sort of came back to me at this point. I remembered good old Fornaca-Rinaldi, who had been dating a cave. I think it was Arene Candide, in fact, which is in Italy, which is one of the caves which has been dated more recently by uranium series. And I thought, well, this is interesting, because it opens the possibility of doing research in Europe and the Old

World, which is something I kind of had in the back of my mind since my family's roots go back to Hungary and family in Israel, and there were some caves in Israel.

So, I thought about this thing, and I began to do some analyses first in a very preliminary way using samples that other people had given me. In particular, there was a professor at the University of Paris, a very famous man named Henry de Lumley, who—has you—I don't know if you've heard of him.

BO: I [have] heard of him.

HS: You've heard of him. And when I told him what I was doing, he very happily sent me a whole bunch of bits of stalagmite, but he never told me exactly where they were from. And I found out that this was sort of a typical thing that de Lumley would do, is that he would find out whether you were doing anything useful and then he would get his own—

BO: (inaudible)

HS: Then he would do it himself.

BO: Yeah.

HS: Get his people to work on it himself, I suppose. So, it was the year 1975 and I had another sabbatical coming up, my second sabbatical. So, I set up my sabbatical as follows: that I was going to go—(laughs) what a complicated story this is. I was going to go to three countries. First of all, in those days McMaster University had a relationship with the government of Hungary through a thing called the Institute for Cultural Exchange. Okay, it [was] “Ka Ka Ka”—I can't remember what the words —“Kapsolata”—uh, anyway—“Kulturalis,” uh, anyway—

BO: Something.

HS: Something, something. [At] any rate, and these people had money, and so they could pay for me to come to Hungary and spend a month at their expense studying the caves of Hungary and collecting samples of stalagmite, which I would then take back to Canada and date. And my wife and I and my son, who was in those days about five years old, flew to Hungary. We were very happy to be in my old—my parents' old home country,

and I even had some relatives who were still living there at that time. And we lived in an apartment in Buda, and then I visited archaeological sites.

Now, what were these sites that we looked at? There's one very important site, which goes by the name of Vértesszöllös, and—I don't know if you know about—have you heard about this site?

BO: Yeah, of course. I know all your papers there.

HS: Okay, you know my papers. But Vértesszöllös was the type locality of a very curious lithic industry—oh, and by the way, at this time I was also having to teach myself archaeology, and particularly Paleolithic archaeology, because this is not exactly a part of my education at Caltech or University of Chicago or anywhere. So, I was going to the library and reading all these books. So, at Vértesszöllös there was an industry which made tools out of tiny little bits of flint, and they figured that it must be very, very old. And there was a particular Hungarian geomorphologist—oh, boy, am I going to remember his name? Uh, I should remember his—

BO: Jakoc?

HS: No.

BO: No?

HS: E, it starts with an E. Uh, anyway, I can't remember his name. [Pecsi Marton] At any rate—who argued on the basis of the number of terraces of a particular—

BO: It's on the Danube?

HS: It's not the Danube—it's a tributary of the Danube. But there was a particular little stream there which had these stream terraces, and by counting up from the terrace—from the bottom and arguing that each one of these represented an interglacial that you could count up, and you'd say that the site must be 450,000 years old, or whatever. And so that was one site which we measured, and then the other site was a place called Tata. And Tata was also a prehistoric site from the Paleolithic, which was a flowstone—not actually a flowstone; it was a spring deposit. So, I have to admit that—

BO: Tufa.

HS: Tufa. So, I have to admit that we're getting away, a little bit away from karst here, because—unless you want to include karstic areas which have tufas in them. At any rate, it was a very important part of my history. So, we did dates on these two sites, and then that was my first—the first month of my sabbatical, and then we packed up our things and we drove in our little Fiat, which we had bought when we arrived in Europe.

We drove down to Greece and to a marvelous cave called Petralona, and there I met a guy named, um—oh God, here goes the memory; the memory's going crazy again. Uh, Aris Poulianos, Aristoteles Poulianos. Now, in the cave of—the cave of Petralona is a very fine—

BO: It's a very famous one, yeah.

HS: A very famous cave. It's in the Khalkidiki Peninsula, which is these three fingers that point—that stick out of the northeastern part of Greece, and I guess they're pretty well all karstic landscape. And in the westernmost of these fingers is this cave of Petralona, and sometime in the 1960s a skull was found in this cave of a very primitive hominid. When it was first discovered, it was covered with layers of calcite, but the calcite was finally removed, and then they revealed early species of the genus *Homo* with very large brow ridges and a relatively small brain—anyway, something on the order of *Homo erectus*.

So, the date of this thing was not known, but Aris Poulianos had, I guess, had heard something of what we were doing. I don't remember how the connection was made, but he invited me to come down and go into this cave. Well, when I got to the cave, I realized that this was a disaster zone because the—although the cave was full of—

BO: (inaudible)

HS: Speleothems. Many, many interesting—it was a highly decorated cave, but the place where the skull was supposedly—had been recovered was a little niche hidden behind a kind of a wall of flowstone, which was filled with sediment, and the skull, of course, wasn't in this site anymore. It had been removed, and there was some argument as to exactly where in these layers of sediment the skull had been sitting. But Poulianos thought he could point to the spot, and he said, "We have to date the flowstone, which is in this horizon."

Well, later on I found out that Poulianos didn't have the slightest idea where the skull had been found, because it was found by a shepherd who had lost some sheep and had gone into the cave looking for them. And (laughs) it was only many months later that Poulianos had been into the cave, and there was that argument that went on for some time as to [the] exact location of the stalag—of the skull.

BO: Skull.

HS: And then in addition, there were another series of layers that had flowstone horizons and that he felt were correlated with that location, which he had me date. And these things also—the relationship of them to the exact time of hominid occupation was not exactly clear. Even more frustrating was that there were no artifacts in the cave. So, it may be that the cave—that the skull was dragged in there by a carnivore.

BO: Yeah.

HS: A hyena or some other animal, and that it didn't really belong in the sense of an archaeological—

BO: Population living there or something else.

HS: There was no population; it was deep inside the dark zone of the cave.

BO: Oh, okay.

HS: And so I began to understand with my studies that people only lived in the mouths of caves, which is bad news from some points of view but good from others.

The third leg on my three part sabbatical was in Israel. We spent the longest time—I was a visiting professor at Hebrew University of Jerusalem working with a guy named Yehoshua Kolodny, or Yeshu, as we all called him. [It's] funny, you know, Yeshu nowadays—most people, when you say somebody's name is Yeshu they say, "Oh, you mean Jesus," because there's these crazy people who are Jews for Jesus who think that—but they call Jesus by the name of Yeshu. But Yeshu was just—

BO: Yesu.

HS: Yehoshua, and he was my buddy. He is my buddy.

At any rate, so he had a student named Paul Aharon, who was just finishing his doctorate, and he had—and Paul had built an alpha spectrometer setup for doing uranium series measurements on stuff that was found some evaporated pools in southern Israel. But at any rate, the equipment was there, so I took it over and I began to sample stalagmites in—flowstone in stalagmites in caves in Israel in archaeological locations. And the person I was most indebted to for this phase of my work was Paul Goldberg, who is a guy who works on cave sediments and still does work on this subject; he's a professor now at Boston University. So, Paul took me around to virtually every important cave site that was known in Israel, and wherever we could we collected samples.

BO: There were no problems with collecting at that time?

HS: Absolutely none! Well, first of all, the archaeologists were delighted that there was somebody that was going to help them know what the age of these sites were. And second of all, the Department of Antiquities and these kinds of people were much more relaxed in those days. They had other things to worry about, so we could just go in and hack away at these things to our hearts' content.

So, the sites that we worked at were—well, I guess the one where we got the most interesting results was a site called Zuttiyeh, which is in the Valley of the—well, it's some place near the Sea of Galilee; and we got some dates there which showed that a particular horizon where they had particular interest in archaeological assemblage. Now, these were artifacts; these are stone artifacts imbedded in the—actually imbedded in the travertine that they—I can't remember the exact ages, but I think they came out around 180,000, which was kind of neat because nothing was known of the age of these things before then. And there were various other sites that we looked at.

The most important cave site in Israel, unfortunately, turns out to be very poor for this kind of work. This is a site called Tabun, and I have actually done a lot of work in Tabun, subsequently. I don't know how much I should talk about that because it's—well, it's a cave site.

BO: It's a cave site, and it's—well, most of the things in Israel has something to do with the migration of humans from—

HS: Oh, yeah. Very, very much, very much. Okay, okay.

BO: And it's really, really, really well connected to the—

HS: Yeah, okay. Listen, by now it's now a quarter after three, and I don't know, I'm not moving very fast. Maybe I should move through time a little bit faster and—

BO: Why, as you please. I mean, we have it recorded and then—

HS: (laughs)

BO: We'll see what we do. I mean, this is history.

HS: Okay, all right. (speaking at the same time) Well, I might skip over some of the details here, because—I mean, you're making me remember things which I had almost completely forgotten, and I'm glad you're making me bring these things back to my mind.

Okay, so I spent this year in Israel and the result of it was I published at least a couple of papers on U-series dating of archaeological sites in Israel, which sort of started that process. And it became apparent at about this point that the uranium series dating had its limitations, because the number of sites where you got stalagmites in critical association with—

BO: Culture.

HS: —culture was relatively limited. The other important area that I should mention that I worked on—we went back to Europe in subsequent years to France and worked in an area of the southern central part of France called the Charente district and worked in a cave called Lachaise and did some uranium series dating there. Lachaise is a cave which has at least five different levels, which have remains of Neanderthals in it. So, that was quite an interesting cave from that standpoint, and we got dates from pretty much every level that had Neanderthals associated with it. We were able to show a history ranging from about 250,000 to about 100,000.

And at this point, by the way, I should introduce another person into this record who worked with me on Lachaise and did her master's degree, but not her Ph.D., on this. It was a woman named Bonnie Blackwell. I don't know if you know—do you know this name?

BO: Well, I know—yes.

HS: Yeah, you know who—

BO: I just want to know where you go with it.

HS: Okay, because Bonnie was a very energetic, young woman. She's still an energetic woman and I think of her still as very young, and she just wanted to do everything and, unfortunately, sometimes she wanted to do more than probably was good for her. So, uh, eventually she went off and did a Ph.D. on something which was totally stupid. I won't even mention what it is. But she ended up coming back to work with me in 19—the late 1980s, I guess. I think some time in the eighties [1980s]. Yeah, eighty-three [1983] or eighty-four [1984], something like that. [She was with me] as a postdoc working on electron spin resonance dating. That was even before Rainer Grün came to work with me. That was the early days of my ESR [electron spin resonance] dating.

BO: So, how did you get involved in this one?

HS: In ESR?

BO: Yeah.

HS: Okay, that's another story. I'll get to that. Let's leave that for a moment. I just want to go back to say where else U-series dating of archaeological sites took me, because this kind of a method, you know, once you've realized you had this tool in your hand, you want to start looking all over the world and telling everybody you've got this thing.

Unfortunately, the one part of the world you can't really use it very well is in North America, because the archaeological history of America doesn't go back far enough; let's say the Americas, period. So, you really have to be working in what we always refer to as the Old World. And so, in no particular order, I'll just mention that we worked in

northern Spain in caves on the northern coast of Spain near the town of Santander, where I worked with Les Freeman and various other people in their whole series of caves. Of course, the best known of them is Altamira, but we didn't go into Altamira because, you know—not very happy about that, but we got samples from other caves there. And the main one, which we got dates from, is a cave called, uh (laughs)—anyway, leave that. I'll say it if it comes back to me; I'll remember what it is.

BO: No problem.

HS: [It was] a very interesting cave, which had—oh, El Castillo! That was the name of the cave, El Castillo. All right, and then—I mentioned that I worked in the northern part of France, and then we also went down to—I went down to the southern part of France to a famous cave called Arago, which was excavated by that same Henry de Lumley, where they had found some of the most primitive hominid remains known at that time. And they had a series of flowstones in this cave, which we tried to date.

And I'll just throw out another part of this story at this point, which is kind of relevant. This is the notion of the “dirty calcite problem.”

BO: Yeah, I was thinking to ask you when you start doing in Hungary the tufa.

HS: Yeah.

BO: Because most of the tufa sometimes act as an open system.

HS: Okay, correct.

BO: You will need an isochron for that.

HS: Okay, all right. So, what happened was, uh, there is an interesting problem in uranium series dating, which is that what you're measuring—uranium series dating, what is this, anyway?

We're measuring the ratio of a daughter of uranium-238 and the thorium-230, and we're measuring its ratio in relation to its parent uranium-234. Now, the daughter isotope thorium-230 is an isotope of the element thorium. And on the surface of the earth there's

a lot of thorium floating around, which includes some thorium-230, but which isn't the daughter of the local uranium but is, rather, there because it's been carried in the form of detritus. So, watch out! What if some of this detrital thorium-230 gets into your sample? Well, if it gets in there it will make the sample look older because, basically, the higher the thorium-230 content—or in relation to uranium—the older is the sample. So, we needed to correct for this.

And I guess the people who got me interested in this was the people from Lamont, back in the 1960s, I suppose, particularly a guy named Dave Thurber who was doing a Ph.D. thesis on trying to date uranium—uranium series dating of something. He wasn't really doing uranium series dating at all; he was just measuring uranium disequilibrium of things. But he suggested the idea of a kind of an isochron plot, and so this is what we picked up on. And I think a number of different people were trying to develop isochrons at about this same time.

Alf Latham, who was my graduate student and then later became a postdoc with me, he and I turned to this problem and we kind of wrote a couple of papers, which were called "Dirty Calcite." Actually, we wrote one paper called "Dirty Calcite," and then a Polish postdoc I had—worked with me later, uh, his name was—[I can't] remember his name. It was a very complicated name, [Wojciech] Przybylowicz. Przybylowicz and I and Alf Latham tested this out experimentally by creating dirty calcite artificially from clean calcite plus dirt, and we showed that you could construct isochrons from these mixtures, which gave the correct age for the clean component, which was kind of neat.

All right, so this technique—we applied it, first of all, to the site that I mentioned before, Vértesszöllös, and we were able to come up with a date for this site—which, by the way, was radically younger than the date that the—

BO: Geomorphologists.

HS: —geomorphologists wanted it to be. And this is one of the really weird stories of my life. [It] is that this paper—the people working on Vértesszöllös published a symposium or—

BO: Proceedings [of a symposium].

HS: Proceedings volume called *The Archaeology of the Site of Vértesszöllös*, which included two papers by us: one by Alf, principally by Alf, on the paleomagnetism of the sediments showing that they were normal—the normal orientation. And the second was

uranium series date of the travertines, and of course, it came up with this very—for them, very young age. And in the paper on the uranium series dating there was a footnote added by the editor saying, “The editors have been chosen to include this paper, even though they do not believe the results presented in it.”

BO: Interesting.

HS: (laughs) And you can see that if you look up the volume on Vértesszöllös, yes.

Okay, so, um, Alf and I did this work, and then from that point on this opened up a whole different area of investigation, which was trying to date stalagmites—or not stalagmites, but speleothems from situations where there might be a lot of detritus present. And this turns out to be a very common problem in archaeological sites because people drag a lot of dirt into the caves with them. In fact, the significant amount of the detritus in some caves is probably the result of humans living in the caves, and that detritus gets mixed up with the speleothems, particularly flowstone, since there’s not much current washing the flowstone surface so it just acquires this stuff. And so, this makes a real messy problem, but we could get through it by multiple analyses. Of course, it means that to get one date you now have to do four or five or more uranium series analyses, but the method did seem to work out.

BO: Be accurate by the end.

HS: But later on, or just a little bit after this time, a guy named [James] Bischoff working at the U.S. Geological Survey, and also Teh Lung Ku—Richard Ku [as we call him].

BO: Oh, and Fitzpatrick.

HS: Yeah, Fitzpatrick. They—

BO: Yeah.

HS: They also worked on this problem, and they were able to do slightly more elegant solutions that I think—

BO: Polishing.

HS: Hmm?

BO: Polishing.

HS: Polishing it, and I think, really, the final—in the final analysis, the best approach was this method called—so-called total dissolution method, in which you dissolve everything, the calcite and the detritus, and mix them all together and analyze them all in one run. And that might give you the most accurate method; but even now, to this day, I'm not convinced as to whether this is the right approach.

And then there's another aspect of this thing, which was developed by a guy named Ken Ludwig, who's at the—was at the Geological Survey and now is at the Berkeley Geochronology Lab in Berkeley, California. And Ludwig showed that you could optimize the analysis by using a certain kind of statistical procedure, which allowed the error on both the X and the Y axes to be free, because usually you assume one variable is independent and the other is dependent. So, you minimize the error on one and maximize the error on the other, and he showed that there was another kind of software you could use—another kind of statistics you could use, which would give you a better statistical evaluation.

Okay, so this is again getting a little bit far away from the main stream, but this was, I would say for me, a very interesting period of working on the dirty calcite problem. And perhaps the last place that we applied this thing to was not really a karst situation at all, but these were carbonate sediments that were forming in paleolakes in the middle of the Sahara Desert that I worked on with a guy from Texas named, uh, oh, God (laughs)—what is this? [Fred Wendorf] Uh, anyway, it'll come to me. Yes, anyway, (laughs) paleolakes.

Okay, so, uh, where am I? I think I'm going to skip over—well, do you want me to talk about ESR dating? Is it—I think this is kind of—it's kind of marginal to the karst story and it really is more about archaeology in general.

BO: It's—yeah, it's more archaeology. I remember there were some studies that were [done] trying to do speleothems.

HS: Okay. Yeah, I'll talk to you about that. Okay.

BO: Yeah.

HS: Let's talk a little bit about ESR. Okay, ESR. What does that stand for? That's electron spin resonance dating; and this is a method—well, electron spin resonance is a method of analysis that was developed by the chemists to look at what are mostly called free radicals in organic substances, but it's a way of detecting the presence of an unpaired electron in a crystal or a material of any sort.

And sometime in the early eighties [mid-1970s], I guess, a Japanese scientist named Motoji Ikeya began to be interested in using ESR as a method of dating. And the first material he tried dating, oddly enough, was a stalactite. Now, why he picked out a stalactite, I don't know, (laughs) but I guessed he figured it was a nice pure substance and it was some calcite crystals, and he seemed to be getting some kind of an age from it. So, he then turned to some archaeological sites to do this on, and which archeological site did he go to? None other than Petralona, the very same site which we had been dating by U-series. And so, he got some supposed dates from there. I'm not—I don't even remember what kind of numbers he got from there, but it seemed as if it would be possible to determine the age of a stalagmite by electron spin resonance, anyway.

So, in the 19—let's say—what year would've that been? Hmm, I actually don't even remember. I think it's in the period right after I was in Israel on sabbatical doing ESR dating; that would have been in the early eighties [1980s]. When I came back to Canada I was starting to do ESR dating on teeth from archaeological sites. But also, I thought we should try going back to stalagmites, since stalagmites were my first love for dating; and, besides which, we knew so much about them; and, besides which, we could tell what age they were supposed to be, so we could compare the ESR date to the U-series date. And so, I had a graduate student at that point from Greece, whose name was Jake Karakostanoglou. Unfortunately, Jake was not able to continue in the scientific discipline because he had a family business which he had to take over in Piraeus, Greece. The last I knew, he was living in Piraeus and—

BO: Doing something else.

HS: Doing something very unscientific. But at any rate, I gave him the task of constructing an isochron. Now, the idea of an isochron is that in ESR dating you have two variables that are very important. One is the dose rate, and the other is the dose. The dose rate is the rate at which radioactivity is bombarding the crystals and generating little trapped electrons, and then the dose is the amount of trapped energy which has been built up in the stalagmite.

So, it seemed to me that with a stalagmite you had the poss—well, with a—particularly if you're looking at stalagmites, you had the interesting possibility that you had variable uranium concentration and that meant that you had variable dose rate and, therefore, you could make a graph of the signal intensity versus the dose rate, uranium concentration, and use this to calculate the age, because basically the slope of that line would depend upon the age. The uranium, once it gets into the stalagmite, stays there forever, and the only thing which is changing the radioactivity is the U-series growth of the thing. So, you can sort of correct for the fact that there was some uranium series growth and then you can make a correlation.

So, we did that and we published—we wrote a paper on this thing, which we actually ended up publishing in some rather obscure journal, it seems to me. It was published in *PACT*, which was—as a journal it doesn't even exist anymore.¹ It was a journal which was almost entirely populated by articles by people doing either thermoluminescence dating in those days, or later on a little bit of ESR, but mostly thermoluminescence dating. So, you won't even find this journal, *PACT*. You won't find it in your library anywhere; and probably I should go back and get this paper and republish it somewhere else than a journal, because it's still basically the right story but it disappeared from view.

And interestingly enough, I presented this at a conference on thermoluminescence dating, because in those days the only people who were interested in these kinds of methods were really using this other technique called thermoluminescence and—oh, and by the way, you can also use thermoluminescence to date speleothems, it turns out, though I never ever tried to do that. Some other people did, but I didn't go that route.

So, this showed that in theory it was possible to date speleothems by ESR, and then (laughs) along came Rainer Grün and his supervisor at the University of Cologne, in those days, who was Professor Brunnacker. And they tried to do ESR dating on speleothems, and they crashed in flames, so to speak. It didn't work. It wasn't working at all.

BO: But he has a lot of papers, I think.

HS: No; he has a lot of papers, subsequently, on ESR dating in other things.

BO: Yeah.

¹ *Journal of the European Study Group on Physical, Chemical, and Mathematical Techniques Applied to Archaeology.*

HS: But his first attempts were trying to ESR date stalagmites and it was definitely the wrong thing to date, and it took them a few years of failed experiments to find this out. Maybe we were lucky with our isochron dates, and we were just working on the very few samples that actually did have good, uh—

BO: Signal.

HS: Signals, and didn't suffer from the problems that other samples did. And I wouldn't even say we know today what the problem really is, but it's probably most likely due to a well known difficulty in the general field of physics, which is fading, which is the notion that once the signal is formed by trapping electrons that the electrons have some tendency to disappear, to become retrapped—to be untrapped and then retrapped where you don't see them, and then the signal just drops away with time. And that could be the explanation, though that would always give you signals which were too young; whereas in fact some of their data were giving samples which were too old, and they were just all over the place. They were upsy-downsy, upsy-downsy.

So, the ESR dating of stalagmites had a short and (laughs)—short life. Every now and then, Derek would keep coming back to me and saying, “Listen, why don't we try dating this stuff by ESR since it's too old to date by U-series?” and I would have to take him to the Faculty Club and buy him another beer and say, “Derek, you know, ESR really doesn't work on stalagmites. Let's try not to do that anymore.” Anyway (laughs), so, that was it.

Okay, now I think we're going to move briskly on to the later part of the twentieth century and the early part of the twenty-first century.

BO: Yes.

HS: And come back to fluid inclusions, and then I think that's going to bring an end to my—

BO: Yeah.

HS: Okay, because I'm really going on a bit too much. Sometime around—oh I don't know when. Sometime in the late nineties [1990s], perhaps, a group at the University of East Anglia, Tim Atkinson in particular and Peter Rowe—

BO: Peter Rowe, yeah.

HS: —got interested in the idea of going back to the old fluid inclusion story, and together with another guy whose name I should know very well, but it just—oh, God, again. [Paul Dennis]

BO: I have it in my mind myself.

HS: Okay, yeah, right. Actually, he was the key ingredient to this, because he designed the equipment. He built this marvelous magnetically operated crusher that would crush stalagmites in a vacuum system and then liberate the water, and then they proceeded to analyze this mass spectrometry—mass spectrometrically, but they also analyzed the oxygen. And in their opinion, it was possible to—first of all, using this machine that it was possible to get very good δD measurements—very good, let's say, plus or minus a certain error. And also, they could measure the $\delta^{18}O$, and they did this on some Holocene stalagmites, but they really didn't publish any paleotemperature data based on it.

But at this time I had a student—Derek and I had a student named Freda Serefiddin, and we'd gotten back into the speleothem game after a bit of a lapse and then we thought we'll get Freda to do the fluid inclusion stuff. So, she actually learned—I think she went to East Anglia, and she saw the equipment in operation and then she got the design for it and brought it back. And we constructed one of these things and it worked, and then she did some analyses and published one paper, at least, in a special paper of the *Geological Society of America [Bulletin]*. But you could see in this paper that there were huge errors in the very poor replication in serial samples of the same material, and this worried me.

And then at about the same time, my old friend in Jerusalem, Alan Matthews, was starting to do fluid inclusions by decrepitation, which is a totally different method where you take the sample, heat it up in a flame and then the water comes off and you collect the water in a—just by cooling the tube downstream. And he was getting reasonably good values, but they were offset by about 20 ‰ from the correct value that he knew to be.

So, it—I don't know; sometimes I would lay awake at night trying to think of how to do things better, you know. And one night, one sleepless night, I got this idea and I said, "There's a thing called the mean free path." If a water molecule is moving around in vacuum, it has a very long mean free path, and there's a pretty good chance that it's just going to keep going until it runs into something solid like the wall of the crushing

chamber, and if it gets there it's going to stick.

So, what you want to do is you want to get something standing in its way so it can't run into the wall of the chamber. [It] can't run into anything until you get it to the place where you want it to sit down, and so what you do is put other things in its way in the form of helium atoms. So, what if we passed a stream of helium through the crushing chamber while it was being crushed, and this would whisk the water molecules away from the crushed calcite before they had a chance to settle down anywhere. And so, Freda had finished her thesis, [and] by this time it was too late for her to do anything.

By this point, a new student came on line, who was Ren Zhang—and he's just graduated, really, so we're really up to the very recent times. And my technician in the lab, Martin Knyf, and Ren and I constructed this new line with a pipe on the top of the crusher, which carried the stream of helium into the system, and after some false starts we discovered that we could reproduce the δD value of a fluid inclusion with a precision of about 0.1%.

BO: Oh, yeah, that's great.

HS: Yeah, which is really pretty damn good considering how little water you have to work with. And so, from that point on I realized now we have a thing we can actually make use of. So, in the last five years or so, essentially, we've been applying this to measure Holocene paleotemperatures, and that's really where we're up to right now. And the paper's, in fact, just in press in *Geochimica*, and that is, you might say, the last word on the subject up to now. And I'm not going to tell you, since this is only a recording of past history and not future history, where we're going to go to next—

BO: It's secret?

HS: —because God knows if any of that will work. Right.

BO: Yeah, science is unpredictable!

HS: Yeah. Okay, so I think that's it from the mental records of Henry Schwarcz, but I'm sure you might have some other questions for me.

BO: Well, yes, I was just curious. Is the—well, I know that you got rid of [the] alpha spectrometer, but you have a TIMS or ICP now?

HS: Oh, God, yes! Oh, I forgot entirely about TIMS. Oh, right. Oh, thank you for reminding me about that. Yes. Ah! Right.

Larry Edwards, in—

BO: Minnesota.

HS: What is—no, no. Well, first of all, Caltech.

BO: Okay.

HS: There is a guy at Caltech who has to be remembered in this whole business named Gerry Wasserburg. Now, everyone knows Gerry Wasserburg; he was the man on the moon. He was—he built the Lunatic Lab at Caltech for the analyses of returned lunar specimens. But he was—he is, was, and is possibly the most brilliant scientist ever to work in earth sciences; he could do anything.

At any rate, so amongst other things, sometime in the 19—what?—eighties [1980s], I guess, he got the idea that since he had these very high precision mass spectrometers that he could measure the age of corals more precisely than anybody had ever done before by using mass spectrometry to measure thorium-230 and U-234. Now, dating of corals has nothing to do with karst as such, but because the chronology problem is exactly parallel to that of stalagmites, namely determining ages in the Pleistocene, there is obviously a very important feedback through here.

So, he had a student named Larry Edwards, and really, it was Larry and Gerry and Dimitri Papanastassiou who developed the U-series dating method using—

BO: Mass spectrometry.

HS: Mass spectrometry. Now, we refer to this as TIMS. This is thermal ionization mass spectrometry. The difference being that the sample, instead of being vapor—passed through the system as a gas, as we do for oxygen isotopes—is loaded onto a filament and evaporated from the filament at a high temperature .

Now, the next story—part of the story—is that Larry Edwards went to Minnesota, set up a TIMS lab there, and began dating everything under the sun. But at about the same time, we had a graduate student named Li. I can't remember what his first name is, but he was a very fine young man; [he was] a Chinese national who came to us to do first a master's degree, which he did on something involving radioactive waste disposal, and then he started to do a Ph.D. And for his problem—for his Ph.D. problem—we gave him the problem of applying TIMS dating to stalagmites. And so, he worked with Joyce Lundberg, who was at that time working with Derek—she was really Derek's student, not mine—as a graduate student working on stalagmites and flowstones in caves in the Bahamas, I guess, or in some—

BO: Yeah, in the Bahamas.

HS: In the Bahamas. And, uh—

BO: Was it Ko?

HS: Hmm?

BO: Li Ko?

HS: Uh, no, no. He was—Li was his last name, but I, uh—

BO: Yeah, 'cause I know they have a paper of the first mass spectrometer date coming to date on the Bahamas.

HS: Yeah, okay, okay, okay. So, what happened was that Li, working together with—Bob McNutt at this time was our head honcho on doing thermal ionization mass spectrometry, and we were using a homemade mass spectrometer, in fact, for doing this. It was built at the machine shop that built all the other mass spectrometers at McMaster, but it worked fine. It was a great little mass spectrometer.

He was using it mostly for strontium isotopes, but we fixed it up to use it for U-series dating, and we got measurements that were surprisingly precise; I mean, way, way more precise than we could ever get by alpha spectrometry. And then, later on, Joyce and Derek published a much more detailed paper on the dating of a particular flowstone from

the Bahamas that had Stage 5e record, sea level record, in it. I didn't even mention any of the stuff about the blue holes and the, uh—

BO: Sea level changes.

HS: Sea level change. Well, there's not enough time to talk about everything, but you know, how much can I talk.

Anyway, so TIMS came along at that point as the great white hope, the great new discovery, and so we proceeded to train, or retrain, our alpha spectrometer technician, Nicki Robinson. She stopped doing alpha spectrometry and she started doing TIMS dating, and it was marvelous because she could get dates with a precision of about 1 percent, which was sort of what Larry Edwards was getting. Sometimes it wasn't quite as good as that, but other times, it would be even better than that, and we never looked back.

That was about the point that the alpha spectrometer took their vacation to—they went on sabbatical to Romania.

BO: Yeah.

HS: And then we kept doing that until about two years ago, and I guess what really—yeah, maybe about two years ago. The end of the story is that I began to see the writing on the wall because first of all, it was taking Nicki Robinson a very long time to do each TIMS analysis, and time is money. You know, we were having to spend that much time getting her—paying her salary. And, in the meantime, a new method had come online, which was multicollector ICP-MS, and so, I was beginning to get the feeling that we should—oh, and also the other thing which was happening [which] was very important was that Derek, who had really been paying Nicki's salary laterally and doing a lot of—taking in a lot of other's people's samples for analysis, that Derek was retiring and his NSERC grant was running out, so we couldn't afford to keep her. So, the lab basically shut down.

BO: Mm-hm.

HS: Leaving us without a really viable way of doing uranium series dating for a while. So now, thanks to having a little bit of money in our research grant and the fact that other people can now do uranium series dating for us for about as much as it would've cost us

to do then ourselves, we can keep doing this.

BO: Well, yeah. The good thing with McMaster and with you and with Derek and with others is that you don't only create a school or people who are very well educated, but all these guys brought over and over—their knowledge—and they continue the school, but some of them move into business but not so many—

HS: Some of them, but others of them kept moving and going off and doing—

BO: Yeah, and I—you know, you just—

HS: Well, look at Stein-Erik [Lauritzen]. You know, I don't know what happened to Stein-Erik when he was—

BO: He's—yeah, he's doing okay. Not that good like [a] few years ago when his lab was populating and lots of money.

HS: Yes.

BO: But that's something else. But what I'm looking [at] is that if you go into the karst environment, I would say—

HS: Yes.

BO: You will always find people, like, you know, Paul Aharon was friend with you.

HS: Yeah. (laughs)

BO: Now he's in Alabama supervising a student of mine from Romania.

HS: Yes, right, right, right.

BO: And Jack Hess, who was [with] Will White, and you know, it's only a bunch of

people who actually create all these lines.

HS: Yeah, but this is how science should be. You know, that there should be a few places which—

BO: Focus and—

HS: They seem to get stuck in doing a certain kind of thing, and then eventually that thing begins to leak out into other places.

BO: Yeah.

HS: If it's interesting enough. I mean, it should be interesting enough that it makes it worthwhile. Now, let me just say in closing, if I could just sort of close this thing off from the standpoint of my personal contribution, that I've always looked upon the karst environment as being a very special kind of environment for study because nature has provided us with an almost, but not quite, closed system—

BO: Protected.

HS: —which is protected from all kinds of other problems that mess up the story when you go out into the outside world. And it also has provided us with a system which, at least over part of its time scale, can be very accurately dated.

What I'm distressed to see today is the increasing number of people who say that they're going to go and study isotopes and speleothems, and in their—because I get a lot of these grant proposals to review, and I sometimes get even papers to review, but grant proposals are what worry me the most, where they say and, "We will study stalagmites" from some caves. Nowhere in the papers do they mention the fact that stalagmites are extremely precious objects that, unfortunately, don't occur very often. Oh, let's say some caves are full of them, but other caves have many fewer of them, and they're really meant to, as much as possible, to stay in the caves.

BO: Be preserved.

HS: And be preserved. And what I'm afraid of is that this beautiful and precious

environment will be more and more trampled on by the next generation of isotope geochemists and speleothem—well, paleoclimatologists and so on, who are not really paying very close attention to the conservation ethic of the speleothem community.

BO: You're right here; but actually, if I'm looking back, ninety-six [1996] when Stein-Erik organizing in Bergen the first karst meeting on paleoclimate, it was one issue that was actually coming up from Peter Smart and Andy Baker.

HS: Yes.

BO: Saying, "Well, we should be careful because now we have instruments. We can date very small amounts," because on the time of alpha spectrometry you never mentioned, but we needed hundreds of grams of uranium—

HS: Oh, yeah, I didn't mention that. Right, yes.

BO: Okay, so now we have just milligrams to do all this, and every single meeting afterwards we try to emphasize this, but you are right.

HS: Yeah.

BO: These are the karst people, not the geochemist people.

HS: Right.

BO: Geochemist people will just go out, pick up a sample, send a student and say, "Go in that cave and pick up the—"

HS: But it's remarkable. I mean, I'm not going to name any names, but I can tell you grant proposals I've received from very imminent geochemists, who at least had their names on them, who were just talking about stalagmites as if they were like outcrops.

BO: Yeah, yeah.

HS: It's, say—[like] they could, you know, go to the Rocky Mountains with their geological hammer and knock off a piece of this rock formation. Why not? Anyway.

BO: Yes. Thank you very much, Henry.

HS: All right. You're welcome. It's been a pleasure, and I don't know if this will be—I guess this will be useful for your—

BO: It's going to be useful, and we have a good experience with [Alexander] Klimchouk and the other ones that we had and all—it was online somewhere on our Karst Portal.

HS: Okay.

BO: And people around the world ask permission to use the portal already.

HS: (laughs)

BO: Yeah, there's so much history in your story that—

HS: Yeah. Now, you watch out because I mentioned some names in here, and these people are going to come back and they'd say, "Wait a second, you didn't say that about me. That's not true at all." Right.

BO: We'll figure out that.

HS: You'll figure that one out, okay. (inaudible) You may have to delete that. Thank you.

End of interview