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## Derek Ford oral history interview with Dr. Philip Van Beynen, January 24, 2008

Derek Ford (Interviewee)

Philip Van Beynen (Interviewer)

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**Spencer Fleury:** Okay, you are rolling. If I could just get from both of you, just to say who you are, and then, Phil, it's all yours.

**Philip van Beynen:** Okay. Hello, everybody. My name is Dr. Philip van Beynen from the Department of Geography here at the University of South Florida. Today it's my pleasure to interview Dr. Derek Ford from McMaster University, who is a world-renowned karst geomorphologist. And it's my pleasure today to be speaking to him about his time as a researcher in this field. So, Derek, maybe you want to tell us a little bit about your background, where you come from as far as your childhood, your old stomping grounds, before you really got into academia.

**Derek Ford:** I was born in 1935 in the limestone city of Bath, which is now a UNESCO [United Nations Educational, Scientific and Cultural Organization] World Heritage City. If you're a fan of Jane Austen movies, nearly all of them have some shots from the upper class part of Bath—not the lower class eastern end where I was born.

But there I was born on—approximate to limestone, you might say. In fact, the world's first geological map, by the canal builder William Smith, is entitled *A Map of the Geology Around Bath*. And the home, the row of terrace houses that I was born in, is actually on Joaquin Marcus founded terrain, meaning it's an unstabilized landslide. (laughs) So I've been unstable from way back in my [life]. You might say it was in the Fuller's Earth or clay underneath oolitic limestone.

I went to the local school during World War II and then won a place to the local grammar school, the City of Bath Boys' School it was called then. They split the boys and girls into separate schools at age eleven. And by the—

A critical moment: I guess I was a passionate reader as a schoolboy. Probably when I was about ten years old, or eleven at the most, I picked up a book by a pre-war rock climber, a well-known English rock climber, Colin Kirkus, called *Let's Go Climbing*. And something snapped and I said, "That's what I want to do!"

But there weren't many very good rocks to climb around Bath. I bicycled twenty miles, hilly miles, on a one-gear bicycle when I was, I guess, [in my] late twelfth or early thirteenth year, out to Burrington Combe on the north side of the Mendip Hills. If there are any Anglicans or Episcopalians listening to this, Burrington Combe is where in 1776—a notable year in American history—the Reverend Augustus Toplady was caught out in the Combe [during] a thunderstorm. So he took refuge in a cleft in its limestone walls and wrote the celebrated hymn, "Rock of Ages, cleft for me. Let me hide myself in Thee." Which I guess is what karst geomorphologists have been doing ever since. (laughs)

Anyway, with my school friends, I sort of organized an informal group; we scrambled up the rocks, but they weren't very exciting. We only had clotheslines for safety lines. And we went down the local caves instead. They were two little spring caves called Goatchurch Cave and Sidcot Swallet. Both of them required a lot of wriggling and bending that elastic thirteen year olds can do, using candles and the front lamps off of our bicycles to see our way around. I ruined my first pair of long pants in that first season, I remember.

And of course, as the years went on, you got more ambitious. I continued to go rock climbing. I bicycled up north Wales to the big stuff. But I got more and more interested and moved up into big caves. On the top, I formed a little caving club at my school, affiliated it to the biggest caving club on the Mendip Hills so that we could use their ropes and their ladders. We used cable ladders and aluminum rungs and piano wire asides and safety lines to get down the cave. So by the time I was sixteen I had done all of the tough stuff that didn't require anything other very short free dives, I guess, in that area, and began to branch out a bit into South Wales and to visit Derbyshire.

PB: Was there a caver during that time that was leading you, the bunch of young people, through these caves?

DF: No. No, we were more or less self-starters then, I think.

PB: Yeah.

DF: The way it is in retrospect, no, there was no—nowadays you'd have to take a training program.

PB: Yes.

DF: And you'd have to work your way through a certificate course to be allowed to lead underground. But then, it was a—

PB: A free-for-all.

DF: Well, there weren't many. It wasn't—"all" is probably—I wouldn't underscore the word "all." There weren't many people caving. It wasn't a great, great pastime. But it was great fun. I thought my first—we had a little [group]. The Wessex Cave Club that I affiliated our little group to had a caving hut up on the top, and I had my first drunken orgy there. I got drunk on sour cider—rough cider, as it's called, a local Somerset brew. I've never touched apple cider since. (both laugh) I went back home white as a sheet, lied to my mother that I had a bilious attack. My father took a look at me and he knew. (both laugh)

And so by the time I finished high school I was quite an experienced Mendip caver and quite a good rock climber. Then I had to do two years in the Royal Air Force. My eyesight wasn't good enough for aircrew, so I was put into radar. And I was very fast at dropping the right position that was reported and attacking the aircraft in our simulated exercises on the big map so that the officers could decide how to intercept them. So they sent me up to the Eastern Filter Station, which happened to be just south of the Derbyshire caving area, where you also had rock climbing on the Great Derbyshire Gritstones. So I—and the chief cook at the camp was a passionate climber—used to bicycle out every evening and go rock climbing and then went down the caves in the weekend.

Then, ironically, a curiousness. The U.S. had established a northern air base at Thule on the northeast coast of Greenland. And there was a proposal to second a small group of RAF radar people up there for six months, to see what arctic conditions were like and to see how the systems operated, the radar and radio systems, in those climatic conditions. The people who were being sent, most of them were young men. They'd just got their girlfriends pregnant or they were getting married or something. They were bitching like

hell down the wire. So, I volunteered to go. The consequence was perhaps predictable. I was posted at the Southwestern Fighter Interception Center, eight miles from my home. (laughs)

PB: (laughs)

DF: So back I went to Bath. I then really hit into caves passionately every weekend that I was free. And I was free most weekends because the Russians weren't attacking very hard in those days, particularly not in the southwest of England. There wasn't much to do in the radar. I went caving, hard caving, and began cave mapping. And once you start to map a cave and see the joy of laying out the passage patterns, then you can ask the question: Why is the pattern like that? So then I go up to be an undergraduate at Oxford.

PB: Was there—people think of Oxford as a very difficult institution to get into. Was it that case in those days?

DF: It wasn't. I won a couple of scholarships, so it wasn't too bad.

PB: Were those scholarships because of some of the work you had done?

DF: No.

PB: No? Just purely academic.

DF: I won scholarships, curiously, in English literature and history. I read geography because physical landscape history was really what I was interested in, I guess. And then I transferred. By modern standards, I took a minor in geology while I was up there as well, for the three years.

I joined the mountaineering club. I remember that year, four who were already quite skilled climbers joined, three others and me. I was the only one to graduate. One died in Scotland, one died in the Alps, and one died in the Himalayas. (laughs) That slowed down my climbing ambitions a bit. And I joined the Exploration Club. And Peter Crabtree, a guy from the north of England, and I founded the Caving Club, which still goes, I understand. It is still going strong.

My second year, I organized an expedition of old school friends up to North Norway. In the first summer we mapped the most northern ice cap in Europe. It hadn't been mapped since 1896. We were doing it in 1956. So that's sixty years on. And I published my first paper, not in karst, but from just commenting on what had happened to the ice in the sixty years—how it had receded and what the pattern of recession meant in terms of what I thought, my very simplistic flow dynamic. But for thirty years—twenty-five years thereafter, I was a member publishing in the *International Journal of Glaciology*, a member of the International Glaciological Society, very interested in that. Because of that, I wanted to go down to Antarctica. When I graduated, I was offered a position in the so-called Falkland Islands Dependency Survey, where I would have accumulated information and data to write up a Ph.D. when I got back after a three-year term of service down there.

But I had also been caving hard. And I had become enamored with a certain young lady. And neither of us ever really faced the prospect of a three-year separation with too much glee. She was coming up to her twenty-fifth birthday. Free love was not as widespread in those days, you know, particularly in those days amongst uptight Englishmen like me. So the old Adam took over, and instead of going down to Antarctica to become a professional glacial geomorphologist and glaciologist, I decided to work on the caves in the Mendip Hills. Marjorie Sweeting was newly established at Oxford. She had written her own Ph.D. thesis on interpreting the development of caves in Yorkshire, England. And I was an obvious candidate in her eye to work. So it was agreed I would do a thesis—it was supposed to be a master's, thesis initially—on some aspect of the caves in central Mendip, the ones that I knew well, the ones that were complex and richest.

And, uh, we married. I started work on the Ph.D. There was no coursework in Oxford in those days. And the scholarship was £400 a year, which is not exactly very much money. My wife Margaret began school teaching. And Marjorie took off for her first sabbatical, taking a slow-boat to Australia where she and Joe Jennings—the other great name in English karst geomorphology, who was at the Australian National University. And the two of them went to work on the semi-arid karst in the northeast—northwestern Australia. So Marjorie was away during my first year and not there to supervise.

By June of the following year—we're now looking at June of 1959. I got my bachelor's degree the year before. My wife was pregnant, and in those days you didn't have pregnant ladies teaching. So we were broke. And my predecessor at McMaster University—a four-person geography department—a geomorphologist, died unexpectedly under heart surgery. He was a young man, but things went wrong. So they desperately needed somebody to teach that coming year. They knew that somebody was traveling in England at the time, and this somebody caught me at the geography school as I was just coming back from buying some black ink to draw up a cave map. And they said, "Would you like a job at McMaster University in Hamilton?" I asked, "Where's Hamilton? I haven't done the research geography."

PB: (laughs)

DF: And, well, we couldn't exchange letters, no email in those days. It was set up, so we decided we'd go there for eight months. I spent my career there.

PB: Now, how far along were you in your Ph.D. at the time?

DF: I had done essentially eight months' unsupervised work when I came to Canada. In the following summer, with our first baby, we came back to England. We flew back to England in a turbo-prop in April and flew back to Canada in a turbo-prop in late August in order to take the field camp out in September. And I did that for two years. [I] finished the Ph.D. fieldwork, went back at the end of the academic year—the North American academic year—in sixty-three [1963] to type up the thesis—no word processing, just typewriter onto carbon paper—and [then] present and defend it, which I did that in June of that year.

And then I had actually intended—I had thought I had a job set up at the University of Kentucky in Lexington, in the karst, because Hamilton's a long way from caves. But that job fell through. So, very quickly, just summarizing a bit more of the academic career before we get on the active stuff, I wound up teaching at California State College in Los Angeles, which is located on the boundary between East Los Angeles and Alhambra. We had then two little boys, my wife had two little boys, living in an apartment. It wasn't a very nice place to teach. It was very interesting: some local, vigorous local people; vigorous local students; a lovely multi-racial, multi-cultural campus. A real—it's much more commonplace now; it was really rare then.

Some local students decided they wanted a climbing club. They needed a faculty adviser, so I was asked to be the adviser. So I quickly perverted their climbing club into a caving club! (both laugh) We went caving in the desert, the winding stair up into the Sierra Nevada. It was good fun. We had some very good friends there. It was a pleasant time.

My successor at McMaster was not a success. I was telephoned by the chair in April or May of sixty-four [1964] and was asked, basically, "Please come back." We decided we'd put down roots, we'd made friends there, so back we went. Since I was a long way from caves at the time, I said, "Okay. I am going to start a karst research group." I got my Ph.D. here. There was nobody doing karst in Canada. Will White was about the only name on the books in the United States at the time. He and I met for the first time at the National Speleological Society meeting in Indiana in sixty-five [1965], I think. And I got



a very warm welcome from the many Americans, (inaudible) and other notables who are now even older than I am. (laughs) So that started me into a close association that's followed all my life with the States. I've directed, undertaken, or supervised research all over the United States, as well as Canada now. But there was the basic decision to head in that direction.

You want to ask me what did I find from my Ph.D.? The first was typed out at 1500 pages!

PB: (laughs)

DF: The only advice Marjorie Sweeting really gave me was to cut it, (both laugh) which I did. My wife and I chopped it in about three weeks to 500 pages, I think, the great file of maps and paper. It was, looking back, and I don't mean to brag, but it was the most detailed, systematic attack that anybody up to that time had launched on a set of caves: to try to figure out why each passage twisted and turned, why some were big, why some were small, why some went steeply downhill, why some were flat, while some were horizontal, et cetera, et cetera.

By the end of the thesis I had done enough work for—by conventional standards—two Ph.D. theses, I think, in terms of discovery of how to sort the main cave networks out. They were back of Cheddar Gorge, which is well known for its cheese. But I still hadn't—the penny hadn't quite dropped. It was only a few years later that I realized (snaps fingers) snap, how meteoric water cave systems—at least in steeply dipping rocks—are put together. And I wrote that up in a series of papers and published it in the early seventies [1970s], and then went on from there.

I've jumped ahead of myself a bit, haven't I? Should I stay on cave genetics for a while?

PB: One of the things I was interested in is the—obviously in the sixties [1960s] and the early seventies [1970s], there was a change from maybe a more physiographic approach in geography to maybe a bit more of a quantitative approach. Where would you see yourself fitting in that?

DF: Well, the quantitative revolution swept through geography, okay. I wasn't trained in quantitative geography at Oxford, but Oxford was the home of regional geography. My supervisor Marjorie Sweeting was very proud that she'd—of the fact that for about £20 she got a calorimeter—that's a crude pH meter—for me so that I could do the pH of the waters. And I did swaths of that type of cases; parametric titrations to figure out the

amount of dissolved solids in the water. [That] work was in the early fifties [1950s], so it was novel then, but certainly not quantitative in the modern style. But I did 2,000 water analyses on the Mendip that I never incorporated into the thesis, but I used [them] to write a critique of a piece of work done at University of Bristol by a trained chemist to point out that it was much too simple. The reality was much more complicated. And I had all the stuff and I took it from that.

Then I came to Mac. When I came to back to McMaster from the United States in sixty-four [1964], the quantitative revolution was beginning. And I should mention that in my one graduate year at Oxford, another—having been born in Somerset—another Somerset geomorphologist came back, who had trained in the United States. His name was Richard J. Chorley, who could be said to have introduced the quantitative revolution to English geography and geomorphology. He came back—he trained under Arthur Strahler. He'd worked on badlands doing Hortonian analysis on badlands in—Utah, I think it was, if I remember rightly. His accent was a wonderful mixture of Bronx, Somerset, and Utah. (both laugh) And his poor soul, having been five years in the United States, knew nobody at Oxford.

He came back on some miserable kind of scholarship to run the meteorological instruments and to write up a second doctorate on the history of geomorphology. But he strung around with me and my wife because we were all Somerset, you see, and we could talk to one another in the jargon. He really fired me up. He was all—he was hot on to (inaudible), the streamlined shapes of drumlins, at the time, and there he was in the Oxford Map Library measuring that. So, I tried a bit of quantitative analysis, Hortonian analysis, on my cave maps on Mendip. But I published an article in the National Speleological Society during [this time], one of the first things I wrote, in an attempt—it wasn't successful. It wasn't very imaginative; it wasn't formally trained.

But when I came back to McMaster, two things happened: A guy called Gerry Rushton, who is now an eminent—I guess he's retired—an eminent quantitative urban geographer, was hired to teach urban geography. And my first—well, one of my first two Ph.D. students was an English physicist—got his undergraduate degree in classical physics, therefore was highly quantitative and analytical in his approach—called Michael F. Goodchild. He was another guru of GIS. Okay, Mike got his Ph.D. with me. Mike and Gerry Rushton really hit it off. I think Gerry is substantially responsible for swinging Mike into the map-based human geography, in which he's now such a luminary, really. Those two had a big impact at Mac, and of course they had a big impact on me.

I quickly began—I started teaching my students statistics from year two, year three. The eminent sedimentologist Gerry Middleton was at McMaster. At the time he got—everybody was now measuring the properties of sand grains, the numerical properties that you could enumerate, so he went that way, too. Gerry and I, we had our children, our

children and our families were coming on at the same time, so we worked together on that. The quantitative revolution in the sixties [1960s] really turned geography around, and it turned me around.

Can I add a last thing (inaudible) as a historical review? I'll change topic a little after that. Nineteen sixty-four was the first international conference I went to. It was a one in four years International Geographical Congress, which was held in London. The opening reception was in front of the monkey house at Whipsnade Zoo, I remember, where we were introduced—where we met the great L. Dudley Stamp, who was shaking everybody's hand. And I met Paul Williams there for the first time; I met Joe Jennings for the first time. Paul and Joe and (inaudible) went, and a number of others, the European karst luminaries, Kramer from Germany and some of the inter-war year big names, all went up to Marjorie's patch in Yorkshire. It was the first time I'd looked at the Yorkshire karst properly. And that decided—it persuaded me that I had to open up my interest in karst. I had worked on the dolines and so on, on the top of Mendip as part of the thesis. But I really had to open up into all of karst morphology, surface morphology, as well as being a cave specialist. Hitherto, I'd been, obviously, preeminently a cave specialist.

So there we are. We're looking at roughly 1965. Rushton's at McMaster, beginning at McMaster the quantitative revolution that blasting through geography—and geology; geologists were pretty qualitative before then, as well. And I'm set to run away and start my research team a long way from big karst, in Hamilton, Ontario. Ironically, there are beautiful little caves just fifteen kilometers, twenty kilometers, away from the campus, on the subsidiary escarpment south of the main escarpment, and dolomite there. Over to you.

PB: And, um—so you've mentioned—

DF: Am I talking all right, because I ramble—

PB: No. You're doing absolutely perfect. One of the—you mentioned very briefly Paul Williams. You met him during that trip back to England. He was there when you met—obviously the two of you are synonymous with *Karst Geomorphology*, one of the bibles of karst. Was there something at that time when you realized that the two of you would actually be, you know, put down in history together as—?

DF: Not immediately, no. Paul had just finished his Ph.D. He went to teach at Trinity Dublin. He had done his Ph.D. on the Irish karst. He was Marjorie's second Ph.D. student; I was her first; Alastair Pitty was her third. And we next met one another when I came back to England on leave in 1970 and went over to tour his patch in Ireland. A Volkswagen coming over the hill in the other direction driven by a happily drunk

Irishman clobbered the car in front of us, and wrecked our car, too. So we had to see the western Irish karst without a car, on foot. But we had a good time. We got along well, and we dovetailed well because shortly after that, of course, Paul got his big breaks in New Guinea. He did his big Hortonian-type quantitative analysis on the distribution of dolines. I'd already done the basic framework for a quadrad approach, using my Mendip data.

And John Drake, one of my Ph.D. students in the later sixties [1960s], came from Oxford, worked on water chemistry for his master's and Ph.D. thesis in subject areas in the Rocky Mountains and up in Wood Buffalo National Park, gypsum and dolomite terrain up on the Northwest Territory frontier to the east of the Rockies. John and I worked together, and we did a quadrad analysis of the parent/daughter doline distribution problem that gets you thinking about what must be happening hydrologically, what linkage must be taking place down in the rock. Paul, at the same time, was thinking both—he was working on a larger scale. He was working on the giant dolines and glades of New Guinea and other areas, working from aerial photographs. And he was also beginning to formulate his ideas on what we now call epikarst; he called it subcutaneous karst. And the French group was also beginning to invent the concept of epikarst.

So I sort of got two or three meters below the epikarst with my quadrad analysis; he was coming in there. He was tropical. Obviously, working in Canada—and I'll tell you a bit about the Rockies and so on later. I worked in northern Norway on ice, and I was working in the high Rockies and was in the early seventies [1970s] sent up to Nahanni [National Park]. I was telling you about that this morning. So I was clearly the cold climate man, and working in highly glaciated terrain. So, the two of us dove together very well.

So it was that, when in—now I'm jumping forward many years—in the middle eighties [1980s] an English publishing rep came along and said, "Why don't you write the big book in karst geomorphology, because Marjorie Sweeting's book of 1972 is considered a bit too soft and qualitative. And Joe Jennings is a bit soft and qualitative." He was coming out with a second edition. "We need something a bit more solid." And I said, "Well, I certainly wouldn't dream of doing it alone. Paul and I fit together."

PB: Yes.

DF: So I wrote a letter to Paul and he said yes, and we took it from there. So we've been very close friends ever since. We are—he is just a few years younger than me. He has only two stents in his heart; I have three on that front. (both laugh) We swap notes by email now, and Christmas letters. Our wives get on very well together. We're all politically the same sort of animals, and socially the same sort of animals. They have three boys, and we have three boys and one girl, similar ages.

PB: When you were doing your research, what would you consider to be some of the—besides the introduction of the more quantitative look at karst, with regard to the Mendips and then moving here into Canada, what would you say were some of the major scientific breakthroughs of your career or your time in karst?

DF: Well, what did I do?

PB: Yeah.

DF: Let's focus on that to begin with. I think that by 1970 I understood how you hook meteoric water caves together, where the streams are coming in from different sinkholes, or from focus sinks below the epikarst in other cases. In the dimensions of length and depth, it had always been considered a problem. I had considered that a previous generation of cave genesis faced is one that is the relationship between the cave and the water table. I expressed this several times. Do caves develop above the water table where water will flow faster under gravity, maybe? Or do they develop dominantly below the water table where they can be pushed by a headwater above? Or do they develop along it? All three arguments had been advocated by previous workers. In fact, the water table one had one advocate, [Allyn] Swinnerton, saying that they developed along the water table from the upstream and downstream, and [Roger] Rhoades and [M.N.] Sinacori said no, they developed downstream and up backwards and into the rock! Right?

And so the problem became which of these hypotheses is true, if any? And my solution is to say the answer—are they correct? Yes. Are they correct? No. They're all true in the right geological circumstances, and you sort it out. So I sorted out those two dimensions. And I worked on dolines with John Drake, and the quadrat analysis. And the next step, clearly, was a step that should have been done first, historically, and that is to try to sort out the planned patterns of meteoric water change.

Ralph Ewers, a man with just a bachelor's degree and working as an assistant at Cincinnati Museum where he built the first cave in a museum—it's a lovely one, modernized since then—had done, for a master's thesis, had done a bit of work with salt rocks: pushing water, making a fake bedding plane and pushing water through it. He wrote to me, I wrote to him, and we agreed that he'd come up and do a Ph.D. in which we attacked the problem of pattern building in the dimensions of length and breadth. That was his plan. And so he modeled it in the working with plaster of Paris, putting planes on top of a plastic pillow [that] we could see through so that we could see the evolution of the patterns. Systematically, between 1971-seventy-two [1972] and seventy-five [1975], I guess, and we—by seventy-five [1975], essentially we had solved the problem, I think.

But Ralph didn't get around to writing up the thesis until 1982, and he's never published a big paper from it because he was running the science museum in West Palm Beach. And the first job as director of the science museum in West Palm Beach was to raise the money for his own salary, that of his assistants, and to run the whole damn museum. So he was very much shut down those years before he bailed out of that and became an assistant professor, ultimately professor, at Eastern Kentucky University and a very successful consultant in Kentucky and Tennessee groundwater problems.

But we had got it sorted out, when along comes a very bright German physicist working in spectroscopy and spectroanalysis, Wolfgang Dreybrodt, whose real passion in life is exploring caves. Like me, he decided to try and put his work to work. And he basically took Ralph Ewer's thesis and put it onto a computer at the beginning, in the 1980s. He and I worked and exchanged a great deal of letters. We met, and we each got along very well together. Wolfgang and I get along very well together as well. So Wolfgang's taken off in his gigantic model. He's now the world's greatest, or his team is the world's finest computer modelers of karst hydrogeology with Franci Gabrovsek, who was in Slovenia, and Douchko Romanov, who is Romanian but is now at the Free University in Berlin with another great German, a fine German modeler, Georg Kaufmann. They really took off.

And at the same time, rather less focused on developing caves but more focused on trying to understand how the existence of caves in rock focus water and the behavior of water coming up at springs in karst areas, is a group in southern Germany, Martin Sauter. I had been the external examiner of the habilitations and doing sort of (inaudible) for several of them, Martin and Georg and others over the years. So what I—the real breakthrough there, again, it sort of comes along. It's historically not very neat and tidy. Ralph dissolved salt in ice, and I said, "That's not very realistic; the crystallinity is too big and (inaudible)." Ralph (inaudible) and he never gets around to publishing it, but it gets known anyway. And Wolfgang takes off and does brilliant work with it.

I should mention, since we've jumped into the area of Ralph, that at the same time, Jim Quinlan, an American, came to McMaster and studied for a post-doctorate, my first post-doc, for the three years on the McMaster scholarship I had gotten him. He hadn't actually got his doctorate at that time. His doctorate was a review of everything that had ever been written about karst in any language that anybody could read, except Chinese. (both laugh) It was an immense piece of work that eventually, after nineteen years, he completed satisfactorily at the University of Texas. Bob Folk, the great limestone petrologist and carbonate sedimentologist, was one of the supervisors. (laughs) In fact, as one of the supervising committee, I should mention that Jim Quinlan outlived his supervisor. The poor guy died before Jim could finish his thesis.

He was a highly trained geologist. He worked in uranium exploration in breccia pikes and so on. He focused me yet more. I focused on structure; he focused me on the properties of the rock between the individual bedding planes and fractures. And of course I'm—the two of us got along very well together, and together we worked on producing the first shot at what all is known about karst and the distribution of karstifiable rocks in Canada. So Jim was an important role at that time.

So that brings me into the second area I've contributed in. I have been talking speleogenesis, right?

PB: Uh-huh.

DF: And an interest in modeling, in hardware modeling, because I am not much of a computer man. Wolfgang has taken over that. But an example of a successful hardware model is to get a very good practical hands-on man called John Grue, who had been a technician at the University of—Queens University in Kingston, Ontario—who built a rainfall simulator machine for use outside on eroding soils. I got him to build one in the basement of the Burke Science building [at McMaster University] and rain over plaster of Paris slabs oriented at different elevations and produce rillenkarren. So we—John's master's thesis really is the definitive work on rillenkarren; it still is.

PB: Yes.

DF: A lot of people have copied it, and Wolfgang has modeled it.

I should have mentioned that Mike Goodchild's Ph.D. thesis, my first big modeling epic—we had the power drive of a washing machine. We had a recirculating over-flume built of marine plywood, painted to be watertight. And we just drove water at high velocity around this—using the washing machine—around this apparatus and drove it over inclined plaster of Paris slabs and produced scallops. It was a field test of the scallop theory, and also applied spectral work to it. So Mike did the fundamental three-dimensional extra work on scallops. The chemical engineer Rane Curl had come up with a very good theory, a two-dimensional theory, from his base at the University of Michigan in Ann Arbor. And we had basically probed it out, and that again has stood the test of time. But it is more range work.

So I've had that modeling experience. I just mentioned one other model. Of course I have played around with a few other things since then, have published a bit on river models as

well. But let's leave that and move on to the secondary, if I may, if you—unless you want to pull me up short.

PB: No, no. I'm sure the—one of the other aspects that you're very well known for is dealing with moving out of speleogenesis. Well, partially related, I guess, to the speleogenesis would be the looking at the secondary deposits within the caves.

DF: Can I come back to that?

PB: Absolutely, sure.

DF: It all—I took you to about sixty-five [1965], when the quantitative revolution comes in. And Henry [Schwarcz] was established at McMaster by then, so we'll get back to Henry later. Having decided that I was going to settle down at McMaster, I had to get field stuff going. So with a local student, Charlie Brown, one of my first Ph.D. students; and Mike Goodchild, my first Ph.D. student; and June Ryder, my first M.Sc. student—she had already got her master's thesis with me before. She was a glacial geomorphologist, measuring proglacial lake beaches around Hamilton. We took off to the Selkirk Mountains, west of the Great Rocky Mountain Parks, where what was then the longest known cave in Canada was to be found, Nakimu Caves, in very steeply dipping metamorphose limestone between quartzites. And [it was] ultra-dynamic, because there is a small glacier melting up in the valley. It's a rainforest area. Water comes thundering down, comes crashing through this cave so violently that the whole cave shakes. Well, we extended the 5,000 feet of mapped cave to 20,000 in fairly short order, found two or three more entrances. It's a great sporting cave, and I worked on that.

And by chance, as we did so, it had been—part of it had been opened historically as a show tour or show cave, when there was a big resort on the Canadian-Pacific Railway on the head of a pass nearby. The rail had—the train had to climb up to the top of the pass, and they put a hotel there. People could walk up to glaciers and take a horse and buggy up to the cave. We met two senior parks officers from the headquarters in Ottawa, quite by chance, who were coming around to evaluate the possibility of reopening the cave to visitors in some limited fashion. It had shut down in the 1920s when the hotel burned down. And they gave me money—gave us money—for our second and final season, a real working season, at Nakimu, and that established a connection with the national parks so that, in 1966, I went back into the parks to start Ph.D. work there. Mike Goodchild was working on a model experiment in the laboratory, but he was coming out—students have a laboratory in the field in the summer to get their arses in gear, you see.

The biggest sinking river in Canada is the Maligne River, sinking into Medicine Lake in



Jasper National Park. So we gave that problem to Charlie Brown for his Ph.D. thesis. It's a hell of a system, really. Between the sink point and the putative spring, it was ten miles and a drop of 1600 feet. Under high-water conditions, the dye goes through in eleven hours. You go figure. It's real express train stuff. We scoured the mountains for side passages, entrances that might take us down into this ultimate dynamic cave. The sinkholes are in the bottom of the lake; the springs all come up through the floor of the canyon. Neither of the springs or the sinkholes are accessible, so we tried to break in in the middle, little unsuccessfully. I hit with geophysics later on, electrical resistivity, and drilled 10,000 feet of core on the targets and got nothing out of it.

But that started me in the Rockies. I then went down to the area of Crowsnest Pass where there were rumored to be—I had scouted and had seen a few cave mouths in the walls. And we spent the later seasons of the sixties [1960s] focused mainly on Crowsnest Pass. We were helping Charlie to finish up at Medicine Lake and making the first discovery of the big caves—it's now one of the main [ones]—around Crowsnest Pass, one of the big alpine cave areas, the most concentrated number of caves in Canada. You can see our impact from the names on the things. There's the Mendip Caves and the Yorkshire Caves and Derbyshire Pot and so on and so on. Steve Worthington wrote his Ph.D. thesis on the hydrogeology of those caves years later on.

But that established me at the south end of the Canadian Rockies. I was working in Jasper National Park in the north and we picked up stories of a cave that poured flood waters out of its mouth in the summer every afternoon at four o'clock. So I sent Mike Goodchild in on a long trek—it's eleven miles from the road up a grass hill across an alpine meadow—with a couple of other guys to check this out. They said, "Yes, there's a cave there." He went in a hundred meters to a twenty-foot drop. And so we returned in force and began the exploration of Castleguard Cave, which is now one of the world's most famous caves. It's a great sporting cave, et cetera, et cetera.

The first exploration was done in the summer by Peter Thompson. We'll talk about his role later—a very hard caver. Michael Boon, a very, very hard, brilliant cave explorer came over to work as an assistant, paid a pittance in the summer field campaigns, and got a bachelor's degree in English literature at McMaster in the winter. And Mike Boon and Peter Thompson made the first really big push deep into the cave. They got trapped by a sudden flood on the way out. I took the rescue party in to find them. The flood had just rebated, but it was—we only just got out again before the flood closed the route. So we shut down summer operations, and ever since then, we have explored Castleguard in the winter to cave.

But I have done a lot of work on surface in the summer. It's a wonderful meadow in late July. You can't put your foot on the ground without crushing a flower, and the place is just gorgeous. In some places, you're right up on the grasses and you can hear a

subglacial river rumbling along, but nothing comes out at mouth of the glacier. It's all going down into the karst underneath. Ultra-dynamic, very, very exciting. And Chris Smart, another Ph.D. student, did his Ph.D. thesis on that in the late seventies [1970s], early eighties [1980s]. He is now [a] professor at the University of Western Ontario. I always fit them together.

So, the second thing I'm doing, then, is working on karst in Canada. I'm looking—obviously looking around in Ontario a bit, which has extensive spreads of limestone and dolomite. As my name begins to spread, a couple of keen cavers from Quebec come along to study with me. One decided he wouldn't make enough money as a professor of karst, so he went and joined Quebec Hydro instead (inaudible). The other one, Jean Robert, a passionate *séparatiste*, was a very good caver, good company and scholar, did a lovely master's thesis on Anticosti Island, a limestone island in the Gulf of St. Lawrence.

I also put a first thesis—and two theses by the later seventies [1970s]—down into western Newfoundland. I did a bit of work on Nova Scotia. And I had early theses in the early eighties [1980s] on the West Coast of Vancouver Island, where you've got steeply dipping deep marine limestone, a quite different character with volcanic inter-beds in places intruding, volcanic intrusions, fascinating and different.

So by that time, I knew I had seen great spreads of Canada, largely south of latitude 60°, and I began to try and put them together. And I read a bit on what had been done on the prairies, where the geologists really had no clue what karst was—although until the 1920s, the city of Winnipeg got all of its water by pumping up the local epikarst. Then it pumped up too much, drew in deep salt. So here what I am gearing up to is what's my next breakthrough, okay. By the late seventies [1970s], I began to publish my ideas on what—how the hell glaciers beat up karst, how karst survives in glaciers, how the two interact.

Also, 1971, the national parks were gearing up, as I told you this morning, to declare these beautiful river canyons and waterfalls on the South Nahanni River a national park preserve. Nobody was going to build hydroelectric dams on them. And the Quebec Park had explored some caves in the wall of first canyon. And so my Parks friends—who knew me well now from working at Maligne in Jasper Park, Castleguard Cave in Banff National Park, stuff in Yoho [National Park] and all the way down, and as well as the Nakimu Caves in Glacier National Park—I had written umpteen reports for them, thousands of pages of reports, which don't count as publications. Stay away from this, not a way for an academic to go.

They asked me go up and suss out these caves. So I looked at the geological map, checked out the limestone, saw it went further north. Went up to (inaudible), looked at the

air photographs. Bloody hell! There was this fabulous labyrinth karst north, well north of the area that was proposed to be protected as a national park. But I just saw it on the air photographs.

Joe Jennings was touring in North America that year, and I invited Joe to come along to see Nahanni with me. So I got two or three of my great field assistants, and one is now the leading antiquarian bookseller in Guatemala. (both laugh) People end up in diverse occupations. Mike Shorecroft is a brilliant linguist. We had a wonderful cook, a glamorous blonde called Lucinda. She was agile as all hell; she was just beautiful to know. But you always knew where Lucinda was, because of the sound of crashing rocks. She was slim and agile, and she always managed to hit the rock that was going to take off on a ski slope or something like that so that [it came] thundering down the mountain.

We went up there and the party from Quebec was returning to that area, and they brought with them a Belgian geomorphologist who had an interest in caves, Jacques Schroeder, a longtime professor at the University of Quebec à Montreal now. Jacques and I hit it off immediately. We have published quite a lot together. They're fine caves. And on the way out I flew further north over the great north karst. And Joe rode out with me and he said, "Bloody hell! We've got to see this." So the next summer I got a bit of money from the national parks, beginning to stir the tub to try to expand the area of the park. And I got—took money out of my own check grant and out of my own pocket and anywhere else we could get it.

We drove a couple of trucks all the way up to Fort Simpson on Mackenzie River. It's a hell of a long drive from Hamilton, but we couldn't afford the airfare. We took all of our food freighted up in bands from a Volkswagen bus from Edmonton and flew into the nearest lake on a floatplane. The pilot on the first landing—nobody had ever landed a floatplane on it. We flew around carefully looking for snags; that's logs just under the water that can arrest the floats. We couldn't see any. We landed; we pulled up just in time. The lake was only just long enough for landing with a heavy load and taking off with a lighter load. The pilot threw the cockpit door open and said, "We'll call it Musky Joe Lake!" (both laugh) But we picked the right spot.

We set up camp there and tramped down into the karst. Of course George Brook, who is now Merle C. Prunty Professor of Geography at [the University of] Georgia and chair there, that was his Ph.D. thesis. And I had a couple of masters' theses subsidiary there. George worked on the karst north of the park while I did all of the geomorphology: the glacial, the fluvial, the glacial lake sediments, the caves all the way down in the park itself.

Having all that, I was in a position, more than anybody else, I guess, alive at that time,

certainly in the—outside of the communist region. The Russians were doing a lot of good work on permafrost, but they never got out to tell us what they were doing. We never got in to tell them what we were doing, or hardly. I was in a position to spell out what I think were the interactions of karst and glacial topography, just as Paul Williams makes a contribution to understanding deep doline and patterns in tropical and temperate regions, which is why we two, Paul and I, dovetail together quite well.

So there's a second issue of my breakthrough-type contribution. It sort of appears spread out over two or three papers. And then later on I get a contract to go up and look at a lead-zinc mine in Baffin Island that you can only look at in February. It's a touch chilly in Baffin in February. So, I go up and study [for a] week, because they're ventilating the mine by blowing in air from outside. In summer, the warm air is blown in and it covers the walls, which are minus 30 degrees Celsius with ice. You can't see the geology, just a layer ice, except in the blasting places. But in the winter the ice breaks off, and it's a fabulous hypogene ore, Klimchouk-type topography that nobody else has seen. This one really is very exotic. It's a hot temperature, very acidic, sulfuric acid fluid situation. But it was also in permafrost. I could see where permafrost had melted and part of the lancing core carried the cavity containing lancing core and collapsed into giant boulders—what do you call it?—a mega breccia, and was cemented by ground ice. If you melted the ice, the collapse would renew. So it would put the mine out of action.

So with that I was able—that gave me, formulated my ideas on permafrost and karst development. As I said before, exposure to the Russians. I published on that as well in the later eighties [1980s]. Then things were really opening up with Russia. I was able to talk to the Russians and find that, yes, we were all pretty much on the same wavelength. So there's another breakthrough-type thing, if you like.

How are we doing? Shall I try to carry on?

PB: Uh—you can keep carrying on (inaudible).

DF: Framing historically?

PB: Yeah, absolutely.

DF: Yeah.

PB: Your work, obviously moving towards maybe a paleoclimate type of research—

DF: Right, we'll come back to that. That's sort of a separate thread for much of my life. We'll come back to it later.

The sixties [1960s], the later sixties [1960s], and to a much lesser extent in the first half of the seventies [1970s], then I was working in the Rockies and then swung into Nahanni. I ran a hydrological camp on the Ram Canyon that I showed you this morning in the mid-seventies [1970s], funded by the Hydrological Institute of Canada to get a grip on the behavior of the river. And also [I] was getting a bit more into tropical stuff, warm stuff. John Fish came up from Texas. He got his master's degree in geophysics there, in magnetometry, and did a Ph.D. on his back region, the Sierra del Abra, which is very beautiful, in Texas. I also supervised work in Jamaica. I was branching out there with the dating. I was branching out into working in various parts of the U.S., but we'll come to that later.

PB: Yeah.

DF: That was widening my scope. So we had John Edward Fish. So in the later seventies [1970s], I was broadening. Still focused in Canada, but I had research going all over Canada. I had two master's theses going in Newfoundland, one master's thesis going on Vancouver Island, a real jetsetter. (laughs) Good seafood on both coasts.

And then, in the late seventies [1970s], I began to go back into Europe more. I had made—I had taken my party over to a speleo-congress in Olomouc in Czechoslovakia. This was in 1973. And we made good friends there: Vladimir Panos from Czechoslovakia; Pavel Bosak, who's now head of the Czech Geological Survey; and a few others. And key was perhaps the directors of the Slovene Karst Research Institute, which is even bigger than the University of Southern [sic] Florida Karst Institute in terms of number of staff at the moment. (laughs)

PB: Yeah.

DF: The only one that I know of that's bigger than you are. I was getting more contacts, was branching out with them. That led me to play more of a role in the International Speleological Union. I was elected the first vice president in the meeting in Kentucky in 1981.

I took an international party. Michel Bakalowicz and (inaudible) came to me for post-

doctoral studies after that. And I had [Jurji] Kunaver and others from Slovenia; Andrej Kranjc and his wife. I had Stein-Erik Lauritzen, whom I met in seventy-seven [1977] from Norway, came along. Paul Williams. I took them to Crowsnest Pass, up to Banff Hot Springs karst and caves, and then up in a heli—oh, I didn't have a helicopter that summer. We marched up the glacier, our rucksacks on our backs, saw the surface of the surface karst in Castleguard. And then we went up to the Maligne Medicine Lake, a giant sinkhole area. They had a grand trip. I had got more international contact and exposure that way.

And then Russia was beginning to open up, okay. So in the eighties [1980s], my wife and I—she is very much of a peace activist and wanted to establish links. By the invitation of the Russians from eighty-three [1983] onwards, we were in Russia for two weeks [or] a month every summer. I was still conducting research in North America. I've got other research interests elsewhere in Europe, but preeminently I am concerned with bringing the Russians [and] Ukrainians into the international scene. It was great fun, a teeny-weenie contribution to the collapse of the [Berlin] Wall, I suppose. That's the way we, you know, happened to be in the right place in the right time in history. Certainly another story, but that was very significant.

I became president of the union in eighty-six [1986] and chaired it through to the next congress in Budapest in eighty-nine [1989]. I got to know the Hungarians very well. I did quite a lot of research there with them. That got me again into hot water caves, after visiting Wind and Jewel [Caves] again in South Dakota with Michel Bakalowicz and (inaudible) in particular. In eighty-one [1981], we looked at them and we said, "These are hydrothermal caves." And so we did the stable isotope work needed to demonstrate that in the succeeding years. So that made me international. Of course the Chinese then invited me for the first time in eighty-four [1984], and again in eighty-eight [1988] and ninety [1990] I was back in China getting stuck into the world's greatest karst.

I was thoroughly internationalized by the mid-eighties [1980s], and really in a position with Paul Williams. Of course, it's not dissimilar from the other end of the planet—the bottom half, as we call it, or the top half as you guys call it. Really in a position to write up the first version of the big textbook we wrote up.

Okay. Shall I call it to a halt there and you'll put the next question [to me]?

PB: We can just keep going on in a similar vein, and the fact that obviously, you know, you're well known as a preeminent scholar when it comes to the founding principles of development of caves and karst environments. And you have given a great background as to how that happened and who were some of the players in that field. But you have also made a major contribution to a new field of research that started in the seventies [1970s]

and maybe late sixties [1960s] with someone like Chris down in New Zealand, in Waitomo. But from their group—

DF: They beat us to the draw. Okay, I'll tell you about it.

PB: (laughs) So, you were very well known also with regards to work done in karst with regards to speleothems. Maybe you'd like to tell us a little bit about that second—well, not second, probably third string.

DF: Okay, well, the course has been a very important one. Henry Schwarcz and I have published more than forty papers together. I think he can truly be said to be the chief, although he didn't do the original stable isotope work or the first uranium series measurements on stalagmites, where the guys who demonstrated that were our graduate students. Preeminently, they did the work on (inaudible) that the thing would work. And they laid down much of the framework of it.

So let's come back to, you know, great accidents of history. I happened to turn up at McMaster University. The leading scientific figure of McMaster University was Henry George Thode, a Canadian who had done major contributions to defining the sulfur isotopes. In those days, if you wanted to detect different isotopes, you had to bash a gas or a solution around a mass spectrometer. They were big and clumsy. There were no commercially built mass spectrometers. You had to design and build your own. McMaster became the preeminent—one of the world's preeminent centers of design of mass spectrometers, done by physicists and chemists. They held patents for many applications for helium and deuterium, hydrogen mass spectrometers and so on, near and whatnot.

So, quite by chance, I had shown up at the hotbed of mass spectrometry or isotope studies in Canada, one of the hotbeds throughout the entire world. And they hired a young isotope geochemist from Caltech, one of the other hotbeds, called Henry Schwarcz, Henry Phillip Schwarcz, or H.P. Salz for short, as he was known by his students for many, many years. (both laugh) He is a year older than me; we were much of an age. It's 19—I had just come back from the time in Los Angeles; it's 1964. I'm all gung ho about caves and stalagmites, and Henry mentioned, "You know, the father of uranium series dating, Cherdynstev, has measured some tufas and stalagmites in Russia, and he's got dates." Obviously the key problem with geomorphology in those days is that you couldn't date a damn thing, except by carbon-14. Carbon-14 is so limited in range, nothing has happened—

PB: Yes. (laughs)

DF: —in the lifespan of the carbon six—half-life of carbon. But an American U-series man had also worked on them and said, “No, the system is not closed.” You leach, preferentially leach, uranium two, three, four. Therefore you couldn’t (inaudible) on the basis of his results.

So Henry and I agreed that we would investigate this. McMaster, with all its powerful capability—we had a nuclear reactor next door to our building, the only one on a campus in Canada. And we could irradiate samples to see how much uranium was in them. And there were all sorts of people around with alpha spectrometers who could count the alpha decay. That’s just how it was done until the late eighties [1980s], of course; you dated by counting the number of decays in a week or something like that, on a concentrated solution.

So we agreed in 1964, sixty-five [1965], that we would start a program with my stalagmites that I had collected. And I knew the geomorphic setting, therefore the significance they would have for dating a cave. We would start on it. So my interest was then strictly limited to dating for geomorphic purposes, okay. But Henry knew, of course, having been at Caltech, [where] the great work was done by another Canadian, Sam Epstein down at Caltech, on the oxygen paleothermometer in oceanic carbon coral, et cetera. But you could potentially get paleotemperatures out of it. So we agreed this was Henry’s drive. I wanted the dates—

PB: (chuckles)

DF: Henry wanted the dated isotopic records—stable isotopic records to say “warmer/colder.” So we got our first graduate student, who had been trained in industrial chemistry at the University of Bradford in England. Peter Thompson was a gung-ho, really tough caver climber. He came over in 1966 and we probably nearly drowned him in the Maligne River the night he was driven in. He flew from Britain to Edmonton, which was a slow job in those days, and changed planes several times. Charlie Brown picked him up there, drove him for five hours to the camp. And he sat around the camp and immediately volunteered to go on some crazy rubber-rafting trip through rapids. (both laugh) We nearly wrote him off, anyway.

Pete was our first grad student. We didn’t have any counters, but we managed to borrow access. And slowly, over the course of his five years working for a master’s that was upped into a Ph.D., we got by with a home-built mass spectrometer still for the stable isotopes and borrowed alpha spectrometric equipment scrounged from other people. We got a working dating system going. And we, of course, were investigating the problem—of we established that, yes, you can date stalagmites validly by alpha spectrometry. Some



samples are certainly not open to replacement or preferential leaching of the uranium leaving the derivative thorium too enriched. We established that, I think, by 1970, beyond a shadow of a doubt.

We didn't know that a guy in New Zealand, a geochemist in New Zealand, Karl Wilson, and a graduate student of his, Chris Henley, were working on the same thing. They didn't have the right dating. They were carbon-14 dating. It was crappy stuff. But they solved the fundamental, the basic equilibrium problem with Chris Henley's test. So we were beaten on the draw there. And one thing that had escaped our notice completely, all of us, was that the great paleoclimatologist based in New York—his name I cannot remember, and I'm always forgetting names. But Donna had looked at a polished-section of a stalagmite in the—

PB: Broker?

DF: Yeah, Wally Broker. Yeah.

PB: Yeah.

DF: In the—that's right, in the early sixties [1960s]. And he figured out that there was probably annual banding, got a rough approximation to support it from carbon-14, published a letter in *Nature*, and nobody took it up. I didn't read *Nature* as a matter of routine, but we all missed that. So many, many years later it came back to haunt us, as you know very well indeed. (both laugh) So there we are. There's Wally Broker's—this is the way science (inaudible). That one just disappeared off the map for me. Henley beats us, but we'd have got there anyway. But we led the field in establishing that uranium series dating was okay.

Peter Thompson finished, and at the same time Russ Harmon, who is now president of the International Geochemical Association, came in to do a second Ph.D. And he was real gung-ho, hard driving. We worked some samples from Nahanni, but Peter worked on samples from Virginia, from West Virginia. Russ went on into Kentucky. He made a first foray down into Texas as well. We were trying to get a geographical range, you see. Some stuff in the Canadian Rockies, in the Mackenzie Mountains in the Northwest Territories on the east coast. I'm a geographer, not Henry. But I tried to get the geographical scope spread of the thing. And so Peter's thesis and Russ's thesis, both completed and published by 1975, seventy-six [1976], I think, established that you had a viable dating system and a viable paleothermometer in speleothems.

Henry at that time was frustrated, because he could only say “warmer/colder.” So in 1974, [19]75 we were thinking about it but it was preeminently his drive to study the fluid inclusion work in the first fluid inclusion papers published in 1975, *Geochimica*. So Henry is pushing for that. Henry is opening up that frontier, a new one with fluid inclusion.

So that’s followed up later on by Chaz Yonge, who we have—Chaz worked a transect all the way from Texas to upstate New York, to McFails Cave in upstate New York, through Kentucky and West Virginia and so on, looking at the drips on the stalagmites, the water in the stalagmites, trying to establish that indeed the highly—the rainfall varied so much seasonally in all its isotopic—its oxygen and carbon preempted the oxygen isotopic concentration. This homogenized by the time it gets a stalagmite—which by in large it is. Not in every case, but the large majority. So Chaz does that now.

Another very good chemistry student comes along from England, Melvin Gascoyne. And Mel breaks his heart on the speleothems of Jamaica, because we have hardly any uranium and they are difficult to date. But he does excellent dating work on Yorkshire, so that the speleothem Quaternary chronology—Upper Quaternary chronology of the last 300,000 years, say, in Yorkshire is worked out by Mel Gascoyne in a couple of big papers in the *Proceedings of the Royal Society* and so forth, working with Mel.

And we also—Mel is now—he then went on to work with Atomic Energy of Canada Limited, looking at the mobility of uranium in fractures in granite. Canada wants to put all of its high-level radioactive waste—well, in those years they were thinking of doing it—while in a big chamber one kilometer underground in a nice [plutonic rock] where there aren’t many voters. (both laugh) Up in the Canadian Shield, where the land is largely populated by mosquitoes. They ought to see whether it’s safe to put this hot stuff down there, supposing that it were to leak out of the immediate container, the container that you take it underground in. Would it get into the groundwater?

Well, you look at the mobility of uranium in vein fills and fractures in the groundwater, and know that a lot of interesting work there ultimately was published in big reports on the viability of that. That was when we were independent referees from the Canadian Geoscience Council on that proposal. The whole thing was shelved by the politicians much later on because of political consideration, not that that is not a safe place to put the damn stuff. It’s a lot safer (whispering) than Yucca Mountain, where you guys are going to hopefully dump your crap. But that’s just my opinion.

We also got Alf Latham, another ex-pat Brit caver—you see, I keep on attracting cavers and caver-types. Russ of course is American, not British. He studied with Will White for a master’s in hydrochemistry and then came to us for a Ph.D. in speleothems,

hydrochemical deposits. Alf Latham was a physicist of magnetic, so he did the first paleomagnetism on speleothems. And with Chaz Yonge, we completed what are going to be our main original contributions.

And Joyce comes along—Joyce Lundberg comes along. We give her a nice piece of speleothem to carry on. We convince her to do alpha spectrometry and—yeah, the U-series alpha spectrometry and the stable isotopes dated. Our other mass spectrometrist in the department, who worked in exotic isotopes in rare elements and exotic isotopes in metamorphic rocks in Northern Ontario, Alan—

PB: Dicken?

DF: Alan Dicken, yeah. Thank you. [He] made a first attempt to date on the DT354 to date the speleothems by mass spectrometry, but he failed. And then the group at Caltech—Larry Edwards, doing work for his Ph.D. thesis, succeeded with corals, so we immediately switched Joyce and a Chinese student, Huang Chin Lee, to cracking the problem, in so far as there ever stood a problem with speleothems. And they did the first—we published the first speleothem mass spectrometry dating (inaudible) ionization to TIMS [Thermal Infrared Multispectral Scanner], back at the end of the eighties [1980s]. Joyce was very scrupulous, and a very elegant piece of work.

And during the course of my incursion into the Soviet Union in the 1980s I attempted a big meeting in Georgia in the Caucasus, the Russian—well, now independent Georgia, not American Georgia. There was a weird guy from Bulgaria. He looks like everybody's dream of a mad anarchist. He has a great big straggly beard, crazy long hair, a big black coat. His name is Professor Yavor Shopov, professor of physics specializing in fluorescence and luminescence, as you know far better than I. And he reckoned that—he was working in an astronomy department dominated by astronomers and astrophysicists, as it happened. He reckoned that he could see eleven-year sunspot cycles in the varying luminescent laminations of selected speleothems from his neck of the woods.

He couldn't date them; he hadn't any money. He had an old 1950s (inaudible), and manufactured a piece of equipment for tracing slight variations in the density of the photo image. He was a real ace at getting that with the photograph. It was desperately poor equipment. But he reckoned he had eleven-year sunspot cycles. He gave that—gave his story in 19—I had already heard it. He gave it in eighty-six [1986] again at the speleocongress in Buda—no, eighty-six [1986] in Spain, where Paul Williams heard it as well as me. So Paul got hooked in and got you [referring to PB] to be in there investigating, Phil van Beynen down in New Zealand.

Yavor wrote to me to say, during the collapse of communism—his lab, which I suspect was never very well funded, was wrecked. Basically, he was in a holding pattern teaching a few students on fifty or sixty dollars a month. Could I help? So I brought him over to Hamilton in a post-doctoral capacity, supporting him in a post-doctoral capacity for a couple of years. He got decent cameras and other equipment for him to get back into luminescence. Of course he was always too much focused on endlessly finding cycles in speleothems that have not yet been precisely enough dated to know the duration between the two endpoints on his image. And that remains Yavor's problem, I think; he runs before he can walk. He has become, at least, a more cautious scientist. But he was the breakthrough pioneer there who rediscovered, in essence, what Wally Broker found without luminescence.

But back in the beginning of the sixties [1960s], talk of the early buzz about Yavor's work in Bulgaria got out into Western Europe, too, and a couple of Belgians, Dominique Genty and his supervisor Yves Quinif, had the very intelligent idea looking at a stalagmite that had formed in a water canal tunnel: they knew when the canal was cut, so it had to be older than it. And they got annual laminations in places where we now expect to find annual laminations. And so that started the independent rush. And (inaudible) started, with a student down in Baker Bristol as well. That started that whole area.

I think—I don't know what Henry said, but Henry and I agreed that [it was] stimulated by this exercise. The two of us are going to have to sit down and agree with one another what our narrative is, you know? (both laugh) Who did what, with what and to whom and when? Because we never did keep tight records. We've just got the papers and surviving notes and theses to tell us in what sequence and how things arose.

But I think in hindsight there are two things. The suspicion that you've got annual laminations you can resolve down to a year, and presumably inside it and down to seasons, speleothems, arising out of Yavor's work. Because Wally Broker's was forgotten; it was lost. He had never followed up on it. And the demonstration that you can get beautiful reproduction, high precision with TIMS dating blows it wide open. And now it's a field that's a bandwagon, isn't it?

PB: Yes. Absolutely.

DF: You guys are into it; New Mexico is into it, four or five others. Boston University is into it. All sorts of places here, the University of Saskatchewan is into it. The University of Quebec-Montréal is now into it, and all over Europe. The Chinese heavily—the Chinese are doing some superb work, I must say.

PB: Uh-huh.

DF: It's exploding.

PB: It is.

DF: And the next big meeting is in Chongqing in June. I don't think I am going to go, but  
—

PB: Of all work you've done, is there anything you feel the most proud of? Is there any, like, one paper or piece of work that you say, "This is my defining work," or "This is the work that I have—"

DF: I don't have a—that's a good question. No, I think there are a handful. It would have to be—the one that's best known is Ford and Ewers—I put Ralph's name on it because he discussed the matter, though he didn't write part of it—on the development of caves in length and depth. Published—it was my third revision. I had published it earlier, but I polished it up in 1978. That's obviously a big one.

An explosion of publications in the *Journal of Hydrology* in the early eighties [1980s] in which I talked about the very short paper "The Effects of Glaciation on Karst in Canada," that illustrated from Newfoundland, from Anticosti Island, from the Bruce peninsula in Ontario, from Winnipeg, from Crowsnest Pass—sweeping across the continent like that. That outpouring, as Will White called it when he asked me for reprints—"Could you send me reprints of your recent outpouring?" (both laugh) That would be a second bunch. So that's the key contribution there.

PB: Uh-huh.

DF: I suppose when it comes to speleothem papers my name is rarely first, quite rightly. I am one of the movers and shakers and thunderers. But the grad students slugging away in the lab are doing the work. (inaudible) So, quite rightly, they go forward.

I did write a paper solo author on using the first—the earliest uranium dates out of Nahanni to date canyons. That's the first use, as far as I am aware of, of uranium dating seriously anywhere in geomorphology. So from that point of view, it's a benchmark paper, I suppose. It's not a particularly good one. I don't necessarily believe it anymore.

But it's there. It's got the germ of the idea. It shows how you can begin to play this game. And I wrote that. I wrote an essay—I have got a number of essays and chapters of books and conference proceedings that don't fit so well. There's a big one in one of the Binghamton symposia in which I ponder how you can interpret stalagmite records going under the sea here and going above there, that would be another important one.

On my sabbatical, seventy-seven [1977]-seventy-eight [1978], I wrote up a review. I put many other people's names on it as well. I took all of the uranium dates we got from the Rockies and tried to figure out—and John Drake's water chemistry work figuring out how particles dissolve in the Rockies. I tried to marry them together in the paper, an article in *Alpine Research* asking the question how old are the Rockies? Not the tectonic structures but the actual shapes you see. That summit over there, at one time it was below a valley floor, wasn't it? How much time has elapsed? The typical relief for the Rockies is 1500 meters between the top of the mountain and the valley. And I tackled that one. That's one I've been proud of.

PB: Are there any—obviously, when you get to, for lack of a better word, a more advanced stage of your career, are there any regrets?

DF: Decrepitude. (both laugh) Well, I regret that I didn't really take a very powerful hand lens or microscope to a stalagmite and see in 1966 that they had what would probably have been annual banding. Alf Latham and I logged a heavy chunk of—you have this pioneering dating, doing magnetic work. To measure the magnetism of a stalagmite, you need a big—you need big cubes, so you go take big samples. So, we're coming down the Rocky Mountains and off of Crowsnest Pass with bloody great big rocks in our sacks. We really were sweating buckets. And we slice up one of these, and it's got some poor laminations in it. But we're only interested in the magnitude. We didn't pay attention to those damn laminations! Oh, how I wish I had done so.

PB: Will White said exactly the same thing. He was looking at these under microscopes, looking at the fluorescence and all the rest, and he never thought about what these laminations were, or actually that they were annual. So—

DF: I suppose one of the points is that pioneers are scattered, and I've certainly been scattered around. Frequently I am—you know, a graduate student comes, and he's got a good record. We accepted that graduate student. Sometime I say, "You're going to do this." But sometimes the student wants to work in a particular geographical area, so we go off and find a good problem there. So you get, particularly perhaps in geography and in types of geology, you get this kind of geographical scattering necessary there for intellectual scattering.

PB: One of the things that's really struck me from this conversation is the way you've framed it is through the grad students—

DF: Oh, of course.

PB: —that you've had. And are any of those the most kind of memorable? And like I know from discussions we've had in the past, there are a few that really stand out, not necessarily for their academic aptitude but for their character. Are there any that you would like to just quickly go through?

DF: I wouldn't like to really leave out names. I don't know what you thought of me when you came along to me late on in my career. I'm an old man now, or much older, but [during] the grad years in the sixties [1960s] and the seventies [1970s], we had a farm up in Lancaster, a fifty minute drive from the university. We used to have a meeting once every couple of weeks. I'd bring in a two-four or two two-fours of beer, and we'd sit around and we'd have a damn good seminar, and entirely unstructured. Or one Ph.D. or master's student would be talking about what he or she was up to.

So, I have always liked team. I've always liked to get my people out into the field, and to get them to conferences too, as you know, more so, perhaps, than most supervisors. So because of that, I have always prided myself—I don't know how well—on having exceptionally close relationships with students. I think that began to tail off in the eighties [1980s]. I was getting tired and I'm spread out all over the world by the time you came along, to be honest. So the ones you remember—I guess it's not surprising anymore—are the earlier ones.

PB: Uh-huh.

DF: Mike Goodchild was how Somerset became (inaudible). So going up, there's a 2,000 feet ascent from the camp we were in to the first cave we worked on, Nakimu. The national park wouldn't let us camp at the cave because the grizzly bear problem was considerable. So, we had to go up every day. So we'd race each other up this old cart track.

PB: (laughs)

DF: Mike and I get on very well together. Obviously he's a star. Then there's the much

quieter, rather overall best in the beginning is Charlie Brown. To sum up his early personality when we were working together, sweet it is, but he was clever. He had a real flair for thinking of infinities, thinking outside of the box. Unfortunately, he's lost his job to alcoholism, one of the problems of academics amongst many, many other professions, right?

Another man who you didn't know in the early years was Tom Wolfe, who did a lovely thesis—never published it properly—on sediments in West Virginia caves. He was [a] dead-keen West Virginia caver. Born and raised in Pennsylvania, he went to the University of Pittsburgh for his master's thesis, and there he met an Oxford graduate, Jackie Welch, Jacqueline Welch, who was studying also geography at the University of Pittsburgh. They became husband and wife and had two boys.

Tom came up to me for a Ph.D. He was ebullient, vigorous, very different. He dressed in gorgeous raiment. He was a fabulous cook and a great party host. I gave—Margaret and I gave big parties, and he would be the life and soul of the party. Jackie was pretty good, too. He never got a really solid job around. Jackie became a professor in geography at Guelph, was very successful. After seventeen years of marriage, Tom declared himself really a homosexual. He came out of the closet, a very, very courageous thing to do in those days. The marriage broke up, of course, but not with acrimony. He used to ring me up occasionally thereafter when he was a bit sauced and talk about the old times and ask me for advice on his love life. (both laugh) I am the last person. He was really very, very attractive.

Let me run through them, just through the major names with the Ph.D.s. Peter Thompson, steady, stable, passionately devoted to climbing mountains [and] skiing. Goes out when he got his Ph.D. from us and does a post-doc in stable isotopes trying to look at oxygen and carbon, particularly oxygen in tree rings at the University of Edmonton. Of course, he goes climbing in the Rockies every summer. Then, he eventually gets thoroughly pissed off with running mass spectrometers, and invents *Canadian International Caver Magazine*, which went bankrupt when he was sued for libeling a notable British caver, or publishing something by a notable British caver who libeled somebody who liked to think he was a notable British caver.

PB: (laughs)

DF: That was more like it. But then Pete developed—invented a magazine called *Explorer*, which did very well until he sold up a few years ago. So he had become a magazine publisher.



One of my field assistants is Mike Shorecroft, a brilliant linguist who goes down to help Johnny Fish down in Mexico and picks up Spanish. And he is now an antiquarian—apparently, the biggest antiquarian bookseller in Guatemala, which is not the kind of profession you read about when learning programs in the university. Mike was good, with lovely Lucinda there.

Russ Harmon: very driven, hard working. He has become quite eminent in his fields. He was a (inaudible), very solid. His marriage broke up for the reason that marriages often do. He came over with an English wife. She couldn't get a job in Canada to her liking. The Canadians wouldn't recognize her qualifications. She wouldn't recognize the Canadians. That broke up, and he married a lovely Dutch girl who was doing a Ph.D. in hydrology and engineering, civil engineering at Mac, and they became very successful. She is still employed by the Atomic Energy of Canada Limited. He has been unemployed from them; he has been consulting for many years because of the shutdown of the uranium part.

Mel has been good around here. Alf Latham, in his sold way, has just written a novel. He's about to retire as a professor at the University of Liverpool. He's gone into writing novels again. He's asked my wife when we were in England, a very skilled editor, for her opinion. She said one of the stories, one of the components, is not bad at all. So maybe something will publish there. Who knows?

Uh, Joyce, our lovely flaxen-haired, or dark-haired—raven-haired, I should say, when her hair was its natural color. Our Irish colleague was good fun and, I thought, a very hard worker. I'm running down the list. I am coming towards you now, so—

PB: Well, I was thinking more about people like Craig Malis, some of your other students who were interesting characters.

DF: Yes. People are always fascinating, and a guy like me who's focused on landforms and landscapes really doesn't appreciate too much. I didn't realize that Charlie Brown's broken up with his first fiancée, apparently. Now, this was thirty or forty years ago, and I was making wholly inappropriate remarks to the two of them at parties and things like that.

Yeah, Agnes Pluhar, a very, very self-centered and obstreperous Hungarian. She came out of Hungary—she's my age. She came out at the time of the Hungarian Revolution in fifty-six [1956] and into the displaced persons camps, and then eventually wound up in Canada. Came along as a—hired as a technician or got a job as a technician in a geology department, declared her intention of wanting to do a master's degree as a geology

student. She wanted a supervisor, so she was waiting for me when I came back from Los Angeles in 1984. I put Agnes onto working the first serious attempt, other than what Paul had done in Ireland, on trying to figure out what was happening on limestone (inaudible). Sorry, I take that back, Marjorie was also working on that at the same time.

She was—Agnes did very nice work on the dolomite plains around Hamilton, Ontario. But she was so difficult to work with. Most of the grads I got along with easily, but Agnes was just hard. I can't—I won't tell you too many things, because I might get sued for slander. (laughs) She was one of the difficult ones.

Coming down to the many graduate students, John Glew built the bloody ray machine and did the—really, with hindsight, he put plenty of imagination into it, although it was originally my idea. The work was exquisite, technically exquisite. That was great fun.

Craig is a nice, sweet gentle soul, lacking—Craig managed a master's thesis on coastal karst in Newfoundland, where the coastal karst—I was there again with my wife in May this year—is wonderful. Very cold seas, the coldest seas we've ever studied from the point of view of how are seawater marine animals—how are they going to react on a soluble medium rooted on a soluble medium. He really wanted to study sand and sand dunes with Brian McCann, but Brian, who was chair of the geography department at that time, didn't want him. We'll each jump if we want somebody. If we've got somebody from Winnipeg who wants to study a saltwater coast, Brian isn't available to—Brian McCann was a great sand, beach and sand dune barrier beaches (inaudible) man, and my close friend and colleague for many years.

So Craig gets put to me, and he worked away. And he was a passionate sports fan. We'd decide—we had just began getting interested into baseball then. The Blue Jays, the Toronto team, were just beginning to—well, they were on their way to what was ultimately going to be two consecutive winnings of the World Series. So you can say that Craig gave me my basic instructions in the finer points of baseball as we drove around and picked his research sites on the west coast of Newfoundland.

PB: Yeah.

DF: Uh—

PB: Steve Worthington, obviously, was a person who was very into, like you said, a breadth of things such as, you know, groundwater hydrology but also the speleothems—

DF: He was, yes. Steve came over in the early eighties [1980s]. He did a master's thesis that he's never properly published on Friar's Hole—the Friar's Hole Complex System is a very, very interesting, significant cave down in West Virginia—and then did his Ph.D. on Crowsnest Pass. He really wrote two theses. One was on the hydrogeology of Crowsnest Pass based on hydrochemistry, based on dye tracing and quantitative measurements of the water coming out—uh, chemistry. And the lady who became his wife started a master's thesis on the stalagmites there, but never finished it.

But he also wrote—he read deeply, did Steve. He also wrote a thesis trying to overthrow Derek Ford's hypothesis of four states of cave development in the dimensions of length and depth. He had an idea that came—the deeper the water goes, the warmer it is going to be, therefore its viscosity is lower, therefore it will flow faster. And so he reinvented, really, you could say, with much more sophistication, William Morris Davis's deep phreatic hypothesis and got some data. I have never given it perhaps the due that it—you've got me there—the credit it should have. I don't believe it. I think I have sorted out the question of why caves are where they are in those two particular dimensions sufficiently for my limited imagination. If the structure's right, yes, it will go deeper. If it ain't right, you ain't going to go deeper. And the hydrologic orientation. Steve and I sort of politely continued to disagree on that.

Steve married, and his wife Jane is the very, very dynamic daughter of the mayor of a big local town, Burlington City, a politician. They had two children. The marriage has broken up long since, but because of Jane's attachment to the region for political reasons, Steve had to stay there. He didn't vigorously seek a professorial job elsewhere. He was also slow to finish off that Ph.D. thesis, because like I said, he was writing two theses at the same time, supporting himself by consulting. What, of course, has happened since then is that he and I have done a great deal of joint consulting, and it is very enjoyable. We work together very well. I can never persuade him to do anything he doesn't want to do. (both laugh) He's a stubborn bugger, to put it mildly. But then, I suppose he could say the same of me. (laughs)

PB: In the last few minutes, 'cause we're wrapping this up, what do you—just very succinctly, what do you see are the major challenges in the future for karst research?

DF: One of the major challenges, clearly, is to persuade the ordinary run-of-the-mill hydrogeologist to believe in it.

PB: Uh-huh.

DF: In my opinion, at the very least hundreds of millions, if not billions of dollars, of work are thrown away each year on wasted modeling, wasted drilling of cores where you don't need to, in karst areas. Will White once said it very nicely, "You take a typical hydrogeologist; give him a karst base and \$100,000. He says, 'Great! I can drill a hundred wells. Okay.'" The short answer is, "Bollocks. Find out where the springs are. Monitor them tightly, then decide where you're going to put the dye traces in and then, and only then, decide if you need any wells to augment the information."

The big job now is going to be to tie together conventional hydrogeology, marry it to karst, to caves, to speleogenesis, to understanding a meteoric water cave and some of the others later, so that we have much improved predicative power. And that means, I'm afraid, educating the great unwashed, the standard hydrogeologist who reads Dominique and Schwartz. That's Frank Schwartz, another Canadian, not—Freise and Carey, two more Canadians from just up the road from me in Waterloo.

The one thing that's being done these days is superb, and Ed Sudicky has taken a serious interest in the karst problem. His student Bill Annable got his Ph.D.; I was on the supervisory committee and Martin Sauter was the external. He's now in civil engineering at Waterloo, and I hope that he'll carry it along further. But really the modeling—the modeling has moved more to Europe with Wolfgang Dreybrodt's team. I heard the USGS [United States Geological Survey] has finally and seriously got into trying to adapt mudflow, their great program to put realistic karst models into it. They've not yet succeeded, I think. But they are hot in pursuit of this and that. Steve Worthington played a very valuable role with Gary Shingle advising Dan on the work on the Edwards Aquifer and getting in with the USGS people arising from that. That is clearly a preeminent area.

And getting that information out to other and learning as well from others. Preeminently leading are the Chinese. They know a lot, they do a lot. They still look to us for equations and models that are going to solve their problems, but they have solved a great many problems by slugging away, doing the kind of intensive fieldwork that typically hydrogeologists do not do. Karst people do intensive fieldwork. Going into a cave and crawling around in it is intensive work, right?

PB: (laughs)

DF: So you're used to working hard from the start. That's one. Another area is clearly the one that Sasha Klimchouk has blown up is the role of hypogene in speleogenesis, the many roles of hypogene in speleogenesis. There's a lot more still to be done on that. But I think, preeminently stimulated by Sasha, a lot of advances have been made in the last twenty years.

Henry would have said as much as anyone needs about the future of speleothem studies. There's a lot more to be done, I think, with the trace elements and ever-higher resolution and looking at the laminations. Clearly you pioneered—did excellent work with the (inaudible). That's fascinating. The role of biogeochemistry and biofilm—revolting crap on cave walls, if you want to call it that—or weathering them—that is another interesting area, one that fascinates us in this.

Clearly, a very important question is what is paleokarst? To what extent are great gas and oil accumulations trapped in paleokarst? Or is it really paleokarst? Is the damn thing evolving? Are the cavities enlarging or clogging, and filling and diverting the flow as the oil matures or the gas field expands or contracts, as the case may be? What causes that?

The whole nature of paleokarst is an area that only late in her life, talking of Marjorie Sweeting, saying, "This one fascinates me. I wish I had paid more attention to it." And I have to say the same thing. I've been crawling around in caves; I've seen fabulous, crazy paleokarst sections inside the depositing limestone. I've just tried to figure out their effect on the orientation and structure, the morphology of the existing cave I'm studying, rather than studying them in their own right. What the hell happened to create that back in the Mississippian or whenever, you see, or the Lower Carboniferous, if you like.

Paleokarst is a very rich area. The paleokarst that I know of in Florida—a lot of work has been done—are interesting. They are comparatively simple. There are some very rich ones, particularly when you get into Proterozoic and Archean rocks, very, very interesting stuff to be looked at there.

Does that sound okay?

PB: That sounds fine.

DF: That's more than enough to be getting a grip on, and obviously speleothems and cave sediments generally are (inaudible) I see of using magnetic susceptibility on sediments. Pavel Bosak's team doing the paleomagnetism and magnetic susceptibility of classic sedimentary fills. In Slovenia you've got (inaudible) clastic rocks over thrust on the limestone, so they dump all this crap on—they erode quickly, dump all the crap into the cave, fill them up and flush them out again in the interglacials, fill them in the glacials. You've got six or seven cycles like that in parts of Postojna. That is a whole area that merits being investigated.

For example, take the Appalachians. You've got sandstone ridges, then typically you've got shale, and then you've got limestone benches. Water comes bubbling off of the sandstones, picking up a bit of sand that picks up a lot of clay on the shale and kicks the whole lot into the cave. So, along the edges, along the contact with the clastic rocks in the Appalachians, you've got some beautiful paragenetic caves that have been—they've not been understood. North Americans really haven't gotten a handle on paragenesis yet. That is the dissolving of the ceiling upwards on a rising column of film, which may be highly irregular underneath. Ralph Ewers has done some very nice ones in Kentucky (inaudible) up there.

PB: Uh-huh.

DF: There are one or two in the Appalachians that are just gorgeous. And I saw one in Wangtian Cave in Liaoning Province in China just last November. But if I were young, I'd love to go roam and crawl around that with my compass and notebook and little trowel. (laughs) That's a whole new problem there.

PB: Yes. Well, Derek—

DF: Have I chatted on long enough?

PB: I think so. On behalf of the Karst Information Portal and the University of South Florida karst community, thank you very much for taking the time to talk with us.

DF: Once I get started, you see, you can't shut the old geezer up.

*End of interview*