This is transcript of an interview of Michael (Mike) Church [M], Professor Emeritus at the University of British Columbia, conducted on the 23rd of January, 2020. The interview was conducted and transcribed by Leonora King [L].

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Square brackets [] are editorial insertions.

L: This is an interview with Michael Church by Leonora King and we are talking about his career. The questions, the three broad themes, are: your early life, and then maturation of your career, and retirement. So thinking about your early life and how you got into geography and geomorphology, what were the personal, disciplinary, and institutional forces that guided you towards geomorphology and brought you all the way through your PhD in that subject?

M: Okay. Well, the first thing to understand is that I didn't make a conscious decision to do anything at any stage. Random events simply impinged on me, and I took the path of least resistance each time. So when I was growing up, my father worked in the aircraft industry in Canada, which after the second world war was a fairly important industry for about 25 or 30 years. And I had thought that I was going to study to be an aeronautical engineer. My father took part in the building of an aircraft called the CF-105, which, at its time in the mid 50's, was by far the most advanced military aircraft in the world. But in about 1957, when I was in grade 11, the conservative government of the day cancelled it on the ground. It was too costly. I think there were probably other reasons but that's something else. So that sort of pulled the rug out from under me a little bit. At that time I was taking a grade 11 geography course in high school, a compulsory course that turned out to be one of the few really intelligent courses in high school in the sense that it taught you stuff that you could continue to use in afterlife. We learned to read topographic maps, to make topographic maps, we learned about physiography and things. So I thought that geography might be an interesting alternative. So when it came time to go to university, instead of enrolling in engineering, I enrolled to do geography.

During undergraduate summers I worked for the federal government in a group called the Geographical Branch, which was part of the old Department of Mines and Technical Surveys, now Natural Resources Canada. I was assigned to work on what amounted to exploratory studies in Baffin Island. I think the main point was to have Canadian boots on the ground because the Americans in those days were running around there with their radar stations and all sorts of defence-related nonsense. So I was assigned in my first year to accompany a Branch officer on a traverse of Northern Baffin Island and that was interesting. After that, I was given charge of a field party. So a fourth year undergraduate in charge of a field party, marooned in isolation for four months on Baffin Island at the edge of a glacier to study the mass balance of the glacier by measuring the runoff. It turns out the river was bigger than the fellow who sort of set us up thought - that was a Norwegian scientist who was visiting Canada - and the equipment they left us with didn't enable us to make successful measurements in the first year. So we began to study the smaller streams on the outwash

plain in front of the glacier. My party leader in my first year, 1962, had given me a copy of a classic monograph by Åke Sundborg, a famous Swedish geomorphologist, on the River Klarälven [Sundborg, 1956], and I read the Klarälven study and applied some of its principles in that sort of lost summer on Baffin Island, and decided that rivers were an interesting thing to study. And so that's how I got into rivers.

We went back to the Lewis River in subsequent summers by the way...

L - Lewis River was the one on Baffin?

M - Yes. That was the site at the edge of Barnes Ice Cap. We built a suspension bridge and launched a pipe over the over the river on the suspension bridge. We deposited salt solution into the river from the pipe and measured its dilution downstream and calculated the discharge that way. And so eventually we did get our records, and of course we had the stage records from the year before so our work was eventually a success. But it involved me in thinking about the project in an original way when the original plans didn't work out. It was just outstanding training - people couldn't get that kind of training anymore. It just wouldn't happen. So that was interesting.

L - Can I quickly ask for clarification? You did your undergrad where?

M - This was the University of Toronto.

L - These were summer positions that you applied for...

M - May to September. The federal government still runs such a program. Every year, lots of university students go to work for the government.

L - And you applied for that position specifically or was there a pool?

M - I applied for a position in the Geographical Branch, and subsequently in the Geological Survey which eventually absorbed the Geographical Branch.

L - So you were up in Baffin doing that during your undergrad and then when you went back and you built that suspension bridge. Was that after undergrad?

M - That would have been after my fourth year, so in the summer between undergraduate studies and graduate studies. I continued to work on Baffin until 1968, then went back again in 1972. There's another thing having to do with my undergraduate training and that is that, once I started going to Baffin, I realized that modern so-called physical geography involved a good deal of knowledge of computational mathematics and physics and so on, but the geography course I landed at the University of Toronto was very conservative and old-fashioned. Regional geography of wadi this and economic geography of Astrakhan and so on. But Toronto was a collegiate university, the only one in Canada. In those days, it worked on a so-called honour course system. You named your subject and they laid out all the courses - you had no choices. So I was in geography. But I realized I needed to study math and physics. So I went to my college registrar and explained the situation and the registrar kindly arranged for me to sort of enrol in math and physics. The math, physics and chemistry course at Toronto was a legendarily difficult course.

So they enrolled me in that and I dropped all the regional geography courses, much to the fury of the Geography Department. In the end, I graduated with kind of a mixture of geography, geology, math and physics, which made me the most highly trained physical geographer in the country in those days. I suspect I still am, which is a bad comment on training in this country. But when I finished undergraduate work, I applied to go do graduate work at Cambridge, which, in those days, was visibly the leading school for physical geography in the world. Well, Cambridge wrote back and said I could join Tripos Part Two - that's part two of their undergraduate curriculum. That's an insult! I'd just done an honours course at the University of Toronto. So I thought, well, I'll do a master's degree and then apply to Cambridge again. And I decided to come to UBC because, during my summers in Ottawa, I'd met Ross Mackay, who was doing a sabbatical leave at the Geographical Branch. I was impressed with the fact that he was an Arctic geomorphologist, and I was planning to do a career in arctic geomorphology at that stage of the game. So I came to UBC and ran into not only Mackay but also Mark Melton and William Mathews. Mathews was the Head of Geology and a geomorphologist. At the end of my first year here they offered me direct promotion into the PhD program. So I just simply abandoned the thought of going to Cambridge.

L - So, did you come to work with Ross?

M - Yes, he was my supervisor on record, but Melton was much more influential. One of your questions is who is Mark Melton?

L - Yes, he's a hard guy to pin down. There's not a lot of biographical information on him or even reflections on his disciplinary imprint.

M - Well, Melton was like me. He was a... well he was an Okie, from Oklahoma, which means something if you know American regional cultures. His father, Frank Melton, was one of the discoverers of the Oklahoma oil fields in the 1930s, so the Melton family was independently wealthy. Mark was trained in maths and physics and geology and was about 30 years ahead of his time in his work. His PhD supervisor was Arthur Strahler. He was doing brilliant work, but it was not being particularly appreciated because most people couldn't understand it. And he had a quite a short fuse - didn't get along with Ross Mackay, which was a bit awkward for me. I learned a lot from Mark and he taught me a lot, so fundamentally Mark directed the work I was doing, though Mackay remained the supervisor of record. But that led to an interesting outcome for my PhD studies because when I presented my PhD thesis, which was a study of Arctic outwash plains, with the work done most recently for the Geological Survey, there was a disagreement in my examining committee about whether the work was suitable as a PhD thesis. Melton said, well, it was good work, but it was not suitable as a PhD. Mackay said of course it was a good PhD.

I resolved the conundrum by withdrawing the thesis and I never actually took the PhD.

L - So it's not just an urban legend.

M - Well, subsequently I was asked to join the faculty at UBC, which we'll get to in a few minutes, and after some years the Dean of the day decided he couldn't stand to have somebody on his faculty who had only a BA degree. So the university arbitrarily assigned me a PhD. They broke all their own rules to do it, because I neither presented a thesis nor applied to graduate. So I've never acknowledged that.

L - Yes, on the UBC Geography website, you have no PhD listed, is that why?

M - That's right, yes. I do have a DSc now from Durham University, so I can be called Dr. Church. But, I mean, that's all rubbish. So yes, I did my PhD on Arctic outwash plains, mainly a big outwash plain in east Baffin Island, at a place called Ekalugad Fjord.

L - Actually before we get to the actual content of the PhD, could I ask just a bit more about Mark and Ross and their differences - was it a personality difference or did they have sort of philosophical disagreements?

M - It was mainly a personality difference I think, but Mark wanted to establish a science program for physical geography, and Ross, who was very much entrained in his research and wanted minimum administrative hassles, simply didn't want to hear anything about it. Ross, as a senior Professor, sat firmly on Mark's attempts to establish the science program, which led Mark to leave in the end. Oddly enough, after they got me on faculty, by then there was a different head of department, and the first thing I had to do was design a science program.

L - Can you speak a little more about what Mark's objections to your PhD thesis were and what his reaction was to you retracting it or withdrawing it?

M - I don't know what he thought of me withdrawing it, to be honest. I remained in contact with him for years afterwards. We were on good terms, so I assume he wasn't upset. His objection was there wasn't a thesis in there. I mean, a thesis, in the proper sense of the word, is a proposition which you're going to try and prove or disprove - controvert. There wasn't a single thesis - there wasn't any particular thesis in my study. It wasn't really a regional study in the classical sense, but it was a descriptive study of the phenomena attached to a particular landform. So Mark looked at it and said "where's the thesis?", and one or two of the other examiners agreed with him, and a couple of them agreed with Ross. So, like I said, I withdrew it.

L - Do you think that was a philosophical stance that still exists in the way examining is done these days?

M - Well, first of all I think the quality of supervision has improved to the point that it's highly unlikely the student would get to defend a piece of the kind that I submitted. The piece was done for the Geological Survey, and in those days in Canada it was standard for the Survey to take in graduate students from universities and give them a topic that would become their thesis. Many of these were sort of 'box of rocks' theses or regional geology theses - there was no difference between my thesis and a regional hard rock thesis, really. But that's all history. The thing was subsequently published as a GSC bulletin and won a prize. So I'm kind of confident it was good work. And it's been cited hundreds of times.

L - Yes, so let's talk about that thesis, then. The thesis topic was given to you by the Geological Survey?

M- It was more or less negotiated between us. I mean they knew I had become interested in river channels and they wanted to support my graduate work. And so they suggested I look at the structure of the outwash plains.

L - And this was back up in Baffin?

M - This was in Home Bay, on the east coast of Baffin Island, latitude 70 north. Ekalugad Fjord is one of the big fjords on the East Coast, with a glacier at the head of the fjord and an outwash plain in front. And so I spent three seasons there in the late 1960s.

L - And was this a mass balance question again, or what was the actual driving question behind it?

M - Well, that's the point, there wasn't a driving question. So I did the hydrology, did the sedimentology, looked at the sediment budget, looked at the history of the development of the outwash plain as far as we could, looked at the sedimentation in the deep water in front of the outwash plain. Once again, I was there as a graduate student with two or three field assistants, in charge of a party for four months at a time, with limited contact with the outside world. We had helicopter support by then - we didn't in the first years when I worked at the Lewis River. Once again just an outstanding opportunity to do interesting things.

L - And to just measure all the different parts of the system basically...

M - ... and measure everything we could.

L - When you look back on your thesis now, it seems like you do feel happy with it?

M - I was quite content with it. Amongst other things, I think my summers on Baffin Island were the happiest times of my life.

L - So, can you just tell me what Mark [Melton] did afterwards? He's hard to find biographical information on.

M - Well, I mentioned the family was independently wealthy. So, Mark went and lectured at Simon Fraser University for a short period of time. Then he started an ornamental tile company - it made ornamental tiles. I don't think it was successful - at least he didn't persist with that for long. He simply retired to Las Vegas, because there's no income tax in Nevada, and just basically lived as a sort of a wealthy retiree. I have no idea how old he was when he retired to Las Vegas. He might've been 45 years old, maybe approaching 50, living essentially on the family wealth and investments. He submitted one further paper about 10 years after he disappeared from the scene, having to do with a lava flow in Nevada. That's the only public utterances he's made. He took up photography and did a lot of very serious landscape photography. I don't know whether he tried to sell his photography or not. But he had a great big glass plate camera of the old-fashioned kind.

L - So, you were here at UBC Geography and you had Mark Melton, you had Ross Mackay. Can you talk about the dominant issues in the department, what the department was like at that time? This would have been 1964 - 1969. So, what were people excited about in geography and also in geomorphology. What were the controversies?

M - Some of the philosophical discussion or squabble between those who wanted to quantify geography and those who wanted to hew to traditional descriptive methods was lively at the time.

L - Would you say Ross fell on one side or the other of that?

M - Quantify. He was a cartographer who introduced statistical techniques into cartography.

L - And Mark, obviously on the quantify?

M - Oh yes, highly. Not only quantification, but physics as a basis for understanding landscape process, but Ross Mackay was the same way - his thing was ground heat flux and so on, freezing and thawing. But there were traditionalists in the department as well who were more interested in descriptive regional geography or descriptive urban analysis and so on, although the quantifiers were creeping into the urban geography scene as well, largely through the students and their theses. To be quite honest, I don't really know too much in detail about what was going on in the department in those years. I spent a lot of my time on Baffin Island and in Ottawa. And when I was here in the winters writing up, I pretty much kept to myself. I took courses in the university in the math department and physics department - fourth year level - and geology. When they professionalized geology in the province, I qualified as a geologist on my course background. So I really can't say too much about what were the driving issues in the department in those years. I didn't really take part in it.

L - So you finished your PhD - or you finished it but didn't finish it. And then what did you do?

M - They asked me to become a computer programmer in the department. John Chapman, who took over the headship in about 1968, was very well disposed toward me and asked me to become a computer programmer in 1969. So I didn't make a decision to stay here; it just seemed easy to do so. And then they gave me a sessional lectureship and then they gave me an assistant professorship. And so I sort of, like flotsam, floated to the top of the pile, without ever making a decision one way or the other. I've never looked for a job in my life! I don't think it could happen today.

L - So you stayed here for the rest of your life. Can you just say a word or two about the personal dimensions of that? Family?

M - There was none. June was a fellow graduate student here, in Geography. We started going around - well, I started helping her in the field. She was doing Quaternary stuff in the interior under Bill Mathews's supervision. I started helping her in the field and we started going around together and we married when we turned in our theses in 1969. So before we turned in our theses there were really no family complications of any sort.

L - Okay, and then staying here obviously was an easy choice. So then we get to UBC, you're faculty now, and then we get into the meat of your career. Broadly, can you talk about the personal, disciplinary, departmental, environmental, social, all of these different forces that shaped your career and how your interests evolved in response to that.

M - Well, I tend to be a bit of a loner, so I didn't kind of go in groups. I'd become interested in studying rivers, and so I pursued the means to study rivers. I initially thought I was going to continue to work in the Arctic, but federal politics became sufficiently complicated, in

terms of assigning independent initiative to Inuit and Inuvialuit people in the eastern and western Arctic for de facto control over their traditional lands. To work in the Arctic - unless you were a government scientist, of course - you had to go and talk to the community and get permission to work and so on. Well, doing that would take up more than half of your research grant to start with, so I thought it probably not worth my while to continue in the Arctic, because I've got the mountains here and all kinds of interesting geomorphology in the mountains. So after about 1972 I turned to working in the mountains instead of continuing to go back to the Arctic. And being interested in rivers, I found river issues in the mountains to look at.

One of the interesting things about my career, I think, is that I work on big rivers. Most geomorphologists until fairly recently have tended to work on things that they could wade in rubber wellingtons, in boots. I think mainly because they couldn't bring together the sort of equipment you need to work on a big river. With my background I was able to propose physically fairly rigorous programs of measurement and so I was able to get ship time to work on Fraser estuary, I was able to get grants to build an equipment inventory that enabled me to work on rivers like Fraser and Peace.

L- So you think that the quantitative nature of your proposals helped them be successful in securing the equipment that you needed for these big rivers?

M - I think so, though I don't really know.

L - So who were you applying to?

M - The money comes from the Natural Science and Engineering Research Council of Canada, which in the first years was part of the NRC, the National Research Council. NSERC was split out only in the mid-1970s, but that's beside the point. It was the same office giving the grants. And so I spent my career working really on three things: Peace River, Fraser River and small mountain streams. You ask with one of your questions: what was the motivation for the things you studied? And in my case, thinking about it, it was almost entirely applied issues. People would come to me and ask question about the impact of Bennett Dam on Peace River, or they would ask questions about the stability of steep mountain creeks and safety issues related to forestry activity around these creeks, or ultimately they came to me and asked me "what's the sediment budget of the gravel reach of Fraser River?". Because, of course, there's a war going on between fish people who want to preserve the environment for a fishery and developers who are concerned with the incremental flood hazard that results from the aggradation of gravel in that reach. Those sorts of applied questions. I was able in each case to turn into fundamental science questions. These people were asking questions that had no stock answer.

Throughout my career, I've done consultations. And my students - many of them have gone directly into consulting. My policy has been not to accept an assignment if it's something that there's knowledge available that ex-students downtown - trying to earn a living by being a consultant - can solve. If they can solve it, they should do it. Not me. If someone comes to me and asks a question that involves new science, then I'm willing to work on it and that's how the major projects that I've done developed.

L - So you came from outwash streams in the Arctic, where presumably the floodplain is not vegetated, and you moved to the coast where you have forestry issues and you

have salmon issues, and presumably those two issues evolved quite a bit over your career. It would have been a very dynamic time. Can you talk about that evolution over the course of your career? Was it something that always seemed important or that evolved as being important?

M - Well management issues have evolved over the years, yes. I think in both cases, the salmon fishery and the case of forests, the major point is that they are disappearing. And so questions arise about how to manage the environment in such a way that they will stop disappearing and in fact renew themselves.

L - When you started at UBC in 1969, were people worried about salmon at the time? Was that a big part of the conversation?

M - Well, people were concerned. It was not as fraught a conversation as it has been in the last couple of decades, with the almost complete disappearance of the Chinook for example. But people were concerned. Fisheries and Oceans Canada, which was probably the Department of Fisheries only in those days - the ocean was in a different ministry - was actively pursuing a hatcheries program to augment salmon numbers. It was well known that the salmon population of Fraser River was well below the historic levels of the first couple of decades of the 20th Century, for example. But the issues surrounding them didn't impinge on me very directly. I mentioned the salmon issue in relation to the Fraser gravel budget, but I was focused on the gravel budget, not on what to do about salmon.

L - When did placer mining come out as a big part of that story, because you did quite a bit of work on placer mining tailings.

M - The upshot of the gravel budget studies was that gravel is accumulating in the gravel reach sufficiently slowly that it is not, in the short term - that is a period of decades - a major concern for exacerbating the flood hazard. But the question arises then why does the reach have the appearance of a quite active gravel transporting river? One knows that in the 19th century, over the course of placer mining, a lot of sediment was dumped in the river off the terraces as placer washings, and so the question arises: was there a large influx of gravel into the Lower Mainland in the late 19th, early 20th centuries which led to substantial instability in the gravel reach? And so we did the studies that tried to determine what this gravel budget had been in mid-19th and early 20th centuries, and that involved some fairly original stunts because, of course, there were no measurements. So that was good fun, and new science to back up and make more comprehensive the answer to an originally applied question.

L - Looking back on that, what would you consider to be the highlight, if you had to pick one or two, of the projects that you worked on in your career?

M - Well, if I could pick a highlight just in terms of personal fun, it was a 2009 traverse of the Fraser Canyon which was part of that placer mining issue. To do a mathematical model of sediment transport from the placer sites through the canyons, we had to know something about the hydraulics in the canyons. So we put two rubber rafts in the river at Quesnel and strapped ADCPs and other instruments to them and we went all the way to Chilliwack, and we got an almost continuous record. We lost record in one or two places. We had to take the instruments off the rafts to shoot the waterfall at Bridge River rapids, and we lost contact in Hell's Gate because there was too much air in the water. So we simply lost signal. We actually went through Hell's Gate.

L - You were in the boat?

M - Oh yes. Well, people do that as adventure rafting. They're not allowed to do it above 8000 cumecs. But that was a purely personal highlight, I think. If one looks at the scientific highlights of my career, I would make quite different answers. I think collectively the papers on the application of 1-D morphodynamical models to Fraser River, including the placer-related one, and to Vedder-Chilliwack River - which involves Jon Tunnicliffe's [former Ph.D. student] work and so on - I think collectively those studies, which were deliberately designed to test the verisimilitude of the models would be a highlight, and an important contribution, I hope people think that. The other substantive highlight is the book on [the] Peace River [Church, 2014].

L - And that has come out in your retirement years, right?

M - Yes, but it's a 40 year study. And that's one of the reasons it's a highlight. To do geomorphology properly, you need to study something for a long time because geomorphology happens not on human timescales. And to do that you need to pick a project fairly early in your career and stick to it. Few geomorphologists do that, which really limits the significance of what they can accomplish. I was lucky when, early in my career, [BC] Hydro representatives came along. They were studying the possibility of building a dam at Site C in the early 1970s - it's finally under construction now - and they asked me to look at the issue of sediment transport in Peace River. That led me to realize that I could do this longitudinal study because I discovered the existence of a survey network with benchmarked cross-sections all the way down the river.

L – Who had established that?

M - A surveying company had done it for BC Hydro in the feasibility studies for Bennett Dam, which was closed in late 1967. I realized that, furthermore, Peace River was a very exceptional opportunity to do a quasi-experimental study, because usually when you dam a river you interrupt the water and sediment regimes in the river, which are the two major governing factors, whereas here, most of the water is coming from the mountains, and most of the sediment is coming from the Alberta plateau, and the dam is right at the point in between. So we've controlled the water flow, radically manipulated the water flow, and we've made no significant change to the sediment influx to the river. What happened? And so to answer that scientific question, I thought it was worthwhile to carry on - not because I was much concerned about hydro per se. But, oddly enough, that book was now the main substantive material for the environmental impact statements pertaining to Site C when it was finally proposed to be built several years ago.

L - How did you keep that program going for so long?

M - NSERC money. I deliberately stayed clear of [BC] Hydro funding so the work would be seen to be credible. Hydro was helpful in giving me access to sites and things like that, but there was no financial support or material support. They did things like giving me maps of their survey cross sections and things like that. But that was a good project and it could still be carried on by somebody if they wished from Site C down. And that led to some interesting times as well. We traversed the whole river in rafts at one stage of the game, and it's 1200 kilometres long. So that was good fun.

L - So those are some pieces of your research program that stand out as highly successful. Do you have any reflections on some projects or collaborations that stand out to you as not having been successful?

M - The non-successes that I've had.... I've not really had any serious non-success, I don't think. There's things I started and then decided weren't worth pursuing, but I don't count that as a non-success. I mean you look into something, you see whether there's something worthwhile here. No, there isn't, so cut it. The non-successes, if I've had them, relate mainly to students. I've had a couple of students in my career who were not getting on with other supervisors and I attempted to rehabilitate. But it turns out that in both cases the student had personal issues that prevented them from being effective, and I wasn't able to rescue them. I am sad about that. Other than that, I think my work was unusually lucky. I stuck to things I knew I could do, which is part of the game.

L - And being at UBC, being in BC, you have a pretty generous landscape to explore, I suppose.

M - Yes, and you must realize that during my tenure at UBC, the department became one of the leading departments of its kind in the world, and so departmental resources and things like that were supplied sufficiently to support the work, in the form of computer experts, a cartographer, in the form of equipment of one sort or another, in the form of colleagues with similar outlooks and interests. All those things were helpful.

L - I know you said you were a bit of a loner throughout your career, but can you talk a little bit about the department then? Who was there? You said it was becoming one of the top departments in the world. Why? What was setting it apart? And what were the conversations that were happening and the collaborations that were happening?

M - Well, I think it was the people that were hired. By the time I joined the department, or just before I joined the department, they were hiring people like Tim Oke, John Hay - who left eventually - Olav, who think along similar lines. So that guaranteed the presence of physical geography along with, of course, the presence of Mackay, who single-handedly revised periglacial studies. And they made notable appointments in human geography as well, people like David Ley and others. But I collaborated almost entirely with my own graduate students, not with those people. Of course I have several authorships with Olav, but they were simply convenient joint statements. They didn't reflect a particular project that we'd done together. So it was almost entirely with students. There was one important issue, or collaboration, however, and that was that when I was a graduate student, there was an interdisciplinary program in hydrology. One member of that program, Rolf Kellerhals, had his office in the Geography Department because his supervisor, Gary Gates, was in the Geography Department. And I got along quite well with Rolf, who was an engineer, a civil engineer. In fact, it was Rolf who put me on to the Peace River stuff because he was working for BC Hydro. Rolf and I had a number of projects together, of an engineering sort, which demonstrated the value of geomorphological work in a lot of civil engineering work. And I think that was very influential, over the course of my career, in the development of the notion that geomorphologists should be integral parts of engineering teams in environmental engineering, so that my graduate students could find jobs downtown - as the graduates of other professors now do. And furthermore in 1991 or 92, they finally professionalized us in the engineering organization, and I was one of the people who helped write the criteria for the

entry of geologists or Earth scientists - geoscientists as we are called - into what were the qualifications you had to have to be a professional geoscientist. I think that just simply followed on from my association with Rolf and my association with engineering projects. And I think that was very important for the development of engineering, particularly environmental engineering in the province. I don't really know to what degree my activities with Rolf are central to that but I think they were probably fairly central as a demonstration of the value of that kind of collaborative work.

L - And what do geomorphologist see that engineers don't see?

M - One thing is time - history. The notion that the landscape is not fixed, that it changes. Engineers are prone to measure the current conditions and design some project for the current condition. And then when the river gets behind their dike, they are amazed. Whereas a geomorphologist will start by saying "well, let's see what the river has done during the last 70 or 80 years". The engineer will say "how the hell can you figure that out?". And the geomorphologist will say: Easy - we've got lots of aerial photography. So one of my early graduate students established a consultancy in Victoria which consists almost entirely of interpreting landscape histories through aerial photography for engineering sites, and he's been highly successful.

I work quite a lot these days with Matthias Jakob, a PhD graduate of this department who is now a senior partner in BGC Engineering downtown. His degree is in geomorphology. And he is the country's and one of the world's leading experts on the stability of steep channels, particularly channels that develop debris flows and things. And he, as a geomorphologist, can go up the channel and he can look for sediment sources and things like that and he can appraise the hazard that the channel might or might not represent. Very few engineers could do that, because they don't see the sources of eroded material, they don't understand particularly well the way in which material becomes detached and moves. Sure, a geological engineer might be specialized in landslides, but doesn't know anything about the movement of stuff down the channels subsequently. And so Mattias has produced a major career by using his geomorphological knowledge to make more comprehensive the context in which an engineer has to plan mitigating works if there's something that has to be protected from this creek.

L - Can we train engineers to do that or do you think that what they do is just so distinct that there's no room in their training for that kind of more holistic vision?

M - You could train engineers do that, and increasingly they are being concerned with these sorts of things. But you can only bung so much information inside one person's head, and engineering technique ramifies, engineers also have to be increasingly concerned with the social consequences of the works that they produce, which is another avenue they've got to go down now that they didn't have to go down 30, 40 years ago. So it's perhaps unreasonable to expect an engineer it to be a geomorphologist as well. Far better to realize that increasingly we must do these things collaboratively because, as knowledge continues to expand and our capacity to understand things doesn't, we tend to know more and more about less and less, and to study big projects we have to collaborate. That's why 50 years ago, most scientific papers were singly or perhaps doubly authored. Nowadays, it's typically five or six people and sometimes as many as a thousand in big physics projects.

L - Do you think there's a shift in authoring philosophy as well?

M - The shift is directly a consequence of what I just said. It takes five or six people to do a project that could have been handled by one person 50 years ago, because it would have been handled much more superficially because so much less was known.

L - This is maybe a tactless question, but do you think that there is a change in how authorship is earned and awarded, as well, that reflects a shift in in the way research is done in universities?

M - Oh definitely. Authorship is earned and awarded on the basis of what one contributes to a project that somebody coordinates and puts together rather than simply being a member of the team that does the whole thing - so much so that I think novel problems arise in the issues of scientific authorship. A litmus test for being an author is supposedly that you understand the entire paper and can defend it in front of someone who asks questions about it. But practically it's very difficult to maintain that requirement in complicated projects. If you've got a project that involves something like cosmogenic dating and also the history of Tertiary rocks, it's hard to expect the cosmogenic dating expert to know everything there is to know about the Tertiary, or something like that. And it's hard for the Tertiary expert to be necessarily expert in contamination problems that might bias the cosmogenic date, which is up to the dater to tell the project team about.

And you face that very sharply over questions of giving credit to graduate students. Supervisors these days are very anxious to add their students to any paper that comes out of the group because the students need papers to get a job, which is one of the pernicious things that is visited upon us these days. It shouldn't happen that way. The students have done bits of analysis, so their names are added onto the paper. That's a difficult question, because it's one set of ethical criteria coming up against another set of life survival criteria - professional life survival criteria.

L - And is that something that's changed through time? Do you think that decision would have been made the same way 30 years ago?

M - Oh, years ago the students would have gotten a generous acknowledgement at the end of the paper.

L - So you just mentioned cosmogenic nuclides, and I know I've heard you talk before about how dating techniques and plate tectonic theory are two things that I think stand out to you as big disciplinary changes over the course of your career. Is that a fair statement?

M - Tectonics has been a substantive change that's changed the context in which geomorphology is done in an important way. Extensions of absolute dating - not just cosmogenics - extensions of absolute dating have been a very important revolution in technique, but as that 2010 paper of mine that you've read makes clear [Church, 2010], I think the changes in technique have been way more important in changing the character of geomorphology than any substantive result. And there's not just dating. There's computation, there's remote sensing, and remote sensing is not just things that happen with satellites and things, remote sensing includes sonar, ground-penetrating radar, things like that. I don't know. There's a list of technique issues in that paper, which you can read.

L - So most of your career has really been focused in the British Columbia landscapes, a landscape that would have been new to you when you started your PhD and you did most of your PhD work up in the Arctic. You are the foremost BC geomorphologists and it is quite a dynamic landscape. Do you have any reflections on how the BC landscape in particular has shaped the way your work is done and what you've learned about the BC landscape?

M - I've been interested in the BC landscape throughout my career but I've done nothing about it. My work has been specifically focused on rivers or reaches of rivers, which could just as easily have been on the prairies or somewhere else. What I appreciate about the BC landscape is the way in which here, in a mountainous landscape, the long-term issues, the tectonic issues, and the shorter term sorts of erosional development of landscape can be prominently seen working with each other. But it's harder to see that in a more benign landscape like the Canadian Shield or the sedimentary landscape of Southwestern Ontario, for example, where the Pleistocene is the last thing that happened, more or less. I'd always thought that when I retired I would perhaps spend several years trying to do an intelligent modern physiography of the BC landscape which would involve sort of compositing the digital terrain maps, filtering them for erosion surfaces, connecting those erosion surfaces to events and tectonic history and so on. I haven't done it because I've been too busy continuing the other things that I was doing all my professional life. I don't suppose I'll do it now. Someone will do it one day. There certainly is a very interesting story to be told here involving the synthesis of epigene processes with tectonic processes. I've watched that and I have read some things about it, but I haven't done it.

L - Which is a good transition into your retirement - the last phase of the interview. But before we go on there, are there any other final thoughts you want to talk about for your career and major forces, major accomplishments?

M - Yes, one summary comment I would make for public view is that there's an old saying, probably apocryphal, ascribed to Newton, to the effect that if we see farther than the ones before us it is because we stand on the shoulders of giants. Well my view is [that] we stand on the shoulders of students, and everything I've done has been successful because I've had a string of outstanding students. And if you're a university professor that's really the key to success: train brilliant young people.

L - All right, so your retirement years. You retired in 2007, so you've had 13 years to look back and reflect but also to continue particular projects. What have you mainly spent time on?

M - I've been continuing doing and trying to clear up the things that I was engaged in during my career. So I'm trying to finalize my view of the gravel budget of Fraser River. I'm supporting my ex-student Jeremy Venditti, who's really, since the 2009 traverse, become keen on the Fraser Canyon and we're now contributing, I think, some really important stuff to bedrock river studies. And those things are just a continuation of the research I was doing. It's beginning to wind down now, I'm disengaging myself from Jeremy's continuing stuff, and I'm getting close to the end of the Fraser revision. I'm writing various other little papers, and I'm not quite sure what I'll do from here forward. I've got a couple of book projects in mind. Whether they get done or not will depend on things like my health and whatnot.

Where geomorphology is going is interesting. You remark somewhere in those questions or in your letter to me that geomorphology or geography seems to be breaking up into various subdisciplines. And I think that's true. And as you know, I've got some interest in the history of the discipline. What I see is that three of four centuries ago, studies of the world around us, studies of natural history, were a single integrated discipline. Leonardo [da Vinci] looked at everything around him in the world. Geography was a very prominent part of that, because there is obviously a geography of the Earth landscape. People were primarily interested in that sort of thing. From the mid-18th century on, subjects began to specialize themselves as knowledge grew. Particularly through the 19th century, natural history began to split up into more specialized disciplines. Geography sat in the background, really, as the repository of what was left of natural history. And so, particularly as the universities got going in central Europe, Germany in particular in the 19th century, things like physics and chemistry and anthropology, and letters began to separate themselves out, disentangle themselves, define themselves more specifically and then become subdisciplines that began to split out from those things themselves. And again in the 20th century, geography continued to sit as a repository of what was left. What you're seeing going on in geography today is a continuation of the same thing.

Having said that, all of these disciplines have within them an element of geographic method to the extent that their phenomena are distributed in Earth space or planetary space. I mean, there's a geography of Mars. People in these disciplines would use geographic method, but wouldn't consider themselves as geographers. And so is there a core geography at all? I think there is, and it consists of the comprehensive description of the things that make up the workings of a place, that give character to a place on the Earth surface, or the Moon's surface, or Saturn's surface if you want. But there is very little geography being done. To the extent that there is geography being done, I think it's being done largely in some of global studies of resource endowment, of water resources, food and agricultural futures. If there's a geographer doing geography at UBC it's nobody in our department. It's Navin Ramankutty, in the Liu Institute, who is concerned with those global issues. And the papers in publications like *Nature* and *Science* on these big global issues invariably have one or two geographers amongst the authorship, bringing the geographical perspective. What we in the Geography Department are doing is continuing to participate in this split up into specializations. And that's not surprising, because this is related of course to the expansion of knowledge.

When I was hired in geography, I was not hired as a geomorphologist. I was hired to do resource geography. It was a big course called Geography 366 when I was hired - became Geography 310 (Environment and Resources), which is offered every term to 120 - 150 students. That's the course that I was hired to do and I did throughout my career. What they also had me do was a hydrology course because there was no hydrologist in the department. So I did that for half my career until Dan [Moore] appeared, who had originally been a student in the course. I never took a hydrology course when I was in school. And they also put me in in charge of the new field camp, and they put me in charge of designing the introductory graduate course in physical geography - when they split up the old Geography 500 [graduate seminar], which every incoming graduate student had to take...

L - So the seminar was together at the time (1969) [both human and physical geography grad students]?

M - It was together. When they split that up, at about the time I went on the faculty, they put me in charge of designing something for the physical geographers. That was my course load.

I didn't do any geomorphology. There was Bill Mathews who did the main geomorphology course for both departments. And they hired Olav, and then they hired Mike Bovis. It wasn't until really almost all those people retired that I got to teach geomorphology. So I saw this increasing specialization going on within the department. Within geomorphology, I think what is happening is on one hand, we're recognizing geomorphology as a geophysical discipline. If you really want to understand the way in which sediment moves around on the Earth surface and characterizes Earth's surface, you need to know the physics and geology and chemistry and to some extent the biology of what is going on. There is one kind of geomorphology that is typically expressed through organizations like the American Geophysical Union or the European Geoscience Union, which is becoming a geophysical discipline. But there is another, very much more tentative, and you'll know more, much more about this than I, kind of geomorphology, which is part of the kind of thing that Ramankutty does, which says that there's a geomorphology that has to be concerned with the implications for human welfare of the way landscape is and evolves. It's sometimes a critical geomorphology, it's sometimes a radical geomorphology, it's sometimes an integrative geomorphology. I don't know too much about that. That's a theme which Olav [Slaymaker] will talk to you about. And it's occurred to me that theme is probably closer to geography than the kind of geomorphology I do. The kind of geomorphology I do is becoming a separate subject. It is a separate discipline now, and is a legitimate one. And it continues to have a home in geography departments or geology departments depending on the history of the university.

L - So how do you decide whether its geography or geology for a department?

M - Mainly historically. In Europe, it's mainly in geography departments. In the US it's mainly in geology departments because of the peculiar history of Western exploration. In Canada, we generally tend to follow European traditions, but there is some arguing about that which has led to some ill feeling between geography departments and geology department in the past, including here, which I think is more or less gone now. So it's mainly just simply a matter of historical continuity where geomorphology is studied. Eventually, I suppose there probably will be semi-independent groups of geomorpholgists, or maybe they'll make common cause with the soil scientists or something like that, which we almost did here 20 years ago, but the cultural geographers objected. They were wondering what to do with soil science, and one thought was to fold it in with physical geography, which would have been a very good idea because they know a lot more about rock weathering and things like that than we do.

Anyway, you've got these two directions in which geomorphology seems to be going, and certainly the geophysical one seems to be the dominant strain at the moment. But you've got this more holistic kind of geomorphology that pays more attention to the human impact and the impact of geomorphology on human activity, which is going to become more important because humans are certainly the most important landscape changing element on Earth today. There is no doubt about that. I really don't know too much about that thread, about its philosophical underpinnings, about its justification for itself, except that it's sometimes been very intolerant of the kind of geomorphology I do. Maybe that's because it sees itself as fighting to establish a legitimate position for itself in the light of this geophysical juggernaut. But they're different issues.

L - Are you happy that you were in a geography department instead of geology department?

M - I was happy with the geography department because the people in the geography department were pleasant people to work with socially. I think the geology department, as far as I can tell, was a less happy place. But it wouldn't have mattered to me which place I worked. For years I collaborated with K. Fletcher, a geochemist, happily enough. It wouldn't matter to me which place I was in. But I worry sometimes about the attempts of the human-centered geomorphologists to establish themselves, in that they imagine that there is a different kind of geomorphology to be had. I don't think that's true. Geomorphology is geomorphology. And I think the physical kind of geomorphology is a substantive discipline. It's the way we look upon the findings, and the way we incorporate the human element that is perhaps special about this - whatever you want to call it - critical or singular geomorphology. It's not the geomorphology itself that is changing, it's the way we look at the landscape and the way we think about the things we learn about the landscapes, and what we do about it in society. That's what perhaps is a bit different or is a new dimension to the discipline, not unconnected with this outreach to engineers in recent years, by the way, though that's an element that doesn't seem to be much incorporated into critical geomorphology.

L - Can you say a little bit more about what you mean by that? Outreach to engineers?

M - Well it's kind of paying attention to the human element in geomorphology that you are usually thinking about when you are advising engineers. It's the application of geomorphology in the interests of human welfare. But that is, as far as I can see, if there's a legitimate aim to this new geomorphology, an important part of it. One of the interesting things is that, though I don't worry about it at all, I've had quite a lot to do, I think, with the operationalizing of these things with the fact that I do work with resource managers and engineers and lecture the general public from time to time on implications of geomorphological things.

L - So we've talked about geography. What about geomorphology? When you look at the trajectory of geomorphology, maybe moving away from the idea of its place disciplinarily [within geography], but within the discipline [of geomorphology], what are your reflections on how it's evolved in terms of its main concerns and then what's been good and bad about that trajectory?

M - I'll answer that in two parts. First of all, my personal engagement with geomorphology, since this is an interview about what my career has been about. Early in my career, I did quite a lot of statistical stuff and I have quite advanced training in statistics - at least I did 40 years ago, it has evolved since and I haven't kept up - I was somewhat in the vanguard of establishing the notion that things in the environment can to be statistical in character. There's variance that has to be handled. In my early papers, some of them are strongly statistical in character and that helped establish a trend initiated by Arthur Strahler, really, toward handling the morphological data in a more realistic way. From about 1980, I began to work with the first flume that we had, which was mounted in the space underneath the porch in the back of Room 100. And that was one of the early attempts to re-establish experimental methods - hardware experimental methods and geomorphology...

L - Since Gilbert?

M - It had kind of gone out after people like Gilbert, yes. In the early years people looked at us doing that sort of thing and starting to publish things and saying 'Isn't this obsolete, didn't

they abandon this years ago?'. Now everybody's doing it. So I think that we had some influence there, other people of course took it up as well. And more recently, with the Fraser Canyon stuff, we've not initiated, but entered sort of the mainstream thinking about, the evolution of the bedrock landscape, which is the long-term evolution of the landscape which connects back to plate tectonics. So there's been these three sorts of methodological contributions that have simply come out of demonstrating, by example, the way to work.

Now turning to the discipline as a whole, I think a fairly disastrous thing happened in the late 50s early 1960s, and that is that at the point at which plate tectonics was being enunciated, we turned away from Davisian style geomorphology, because a makeover of Davis fits exactly on top of the plate tectonics paradigm.

L - So you mean we turned away from this landscape evolution concept right when we should have been turning towards a landscape evolution concept?

M - We had it the first half of the century, and it was it was all sort of arm waving Davisian cant. We were at the point where we could have started to seriously quantify it and connect it with the tectonics. But we started measuring stream flows instead, which are things that happen on synoptic scales during which the Everlasting Hills are truly Everlasting and geomorphology ignored the whole issue until really the mid-to-late 1980s.

L - Was that a direct aversion to Davis?

M - It was a reaction against Davis, and it was largely due to the influence, I think, of the Water Resources Division of the USGS and Luna Leopold. But of course he had allies, people like Reds Wolman and other people in the Water Resources Division, Walter Langbein. But we moved to this sort of Newtonian framework, whereby if we couldn't measure it directly, it wasn't important. And so we spent most of the length of my career measuring contemporary processes and it was really only when the plate tectonics people came up against the fact that they had to know something about the erosional development of the landscaping in order to complete the cycle, that the geomorphologists came back into the frame of landscape history. And that would have been prompted by awkward questions from the plate tectonics people which began to impinge on us in the late 1980s, the mid-1980s. So I think that was a disaster in retrospect in one sense. In another sense it was no disaster at all, because that was one of the things that prompted geomorphology to change curriculum and to begin to become more quantitative and realize that you needed some kind of introductory physics course if you wanted to understand the way in which the problems in the landscape were being constructed. So it was by no means a complete disaster, but you can read all that in that 2010 paper [Church, 2010].

L - When you look back on your career and you look back on geomorphology in general and the history of geomorphology, I would be curious to know if you think that there's anyone who has been side-lined in the story or whose ideas deserves more recognition than they get?

M - I can't name an individual who is sufficiently prominent that they would matter to anyone listening to this interview, but there's a whole heap of geomorphologists who are not resident in the United States or Great Britain or northwestern continental Europe who've made major contributions, who are ignored because they're not part of the major schools of the discipline. That includes people in Canada, Australia, New Zealand, but also people in places like

Brazil, India, Turkey, some people from the old Soviet Union, Russia, who have done interesting things in different landscapes, whose work is simply not part of the mainstream because they are not in one of the mainstream places that aggressively define the agenda and who's prominent and who isn't.

L - Do you think that aggressiveness is through social mechanisms?

M - It's through control of the journals, it's through control of the publication process, it's through what editors do, it's through control of the professional organizations and the way they run their meetings, who gets to chair sessions, who gets to set agendas and so on, who gets the prizes. That's a warped game as well, which I know very well from personal experience. Once you win one prize, you win them all. It's just stupid. Prizes should be used to sort of encourage a range of people. One prize per person should probably be a good number, but if you win one, you win ten! And those are all things that probably contribute to the reason you're sitting here asking me questions today, which leaves a lot of people out of the big frame. Maybe you should be somewhere thinking of "who should I go and talk to" and digging into obscure stuff to find the people who seem to have an idea. I've run across more than one individual who's come to me with a very brilliant idea, and yet they never managed to get it articulated. And these are invariably people from somewhere strange.

L - Someone that I kind of wonder about - not that didn't get any recognition, because she did - but Barbara Kennedy. Did you know Barbara? She was one of Mark's students.

M - I was Barbara's closest professional colleague. She was not the kind of person to have colleagues because she was very, very acerbic. A very sharp critic, and indeed that is what she was known for. And to sort of be satisfactory to Barbara you had to appear to be very good. I'm not sure one had to be actually very good. Yes, I knew Barbara very well, that's why I thought an obituary was worthwhile and why I wrote it.

L - Her ideas were mostly on the history and critical history of geomorphology.

M - She was a commentator. She did very little original geomorphology herself. Her thesis work, both graduate degrees, was on asymmetric valleys, and that was interesting and useful but not earth shaking. And after that her major role was as a critic - a very good one. And very good critics are rarely very popular. She was probably the most intelligent geomorphologist of her generation, but she turned her intelligence to commentary rather than to doing work herself. And that's because one of the things about Barbara is, she was lazy. I mean just getting out in the bush and doing the hard work was just too much to think about. She was also in part a biogeographer, not entirely a geomorphologist by any means. She was essentially interested in Darwin, and actually made some interesting commentaries on Darwin - the great Darwin, Charles Darwin. She was one of the sharpest people that have come across the discipline, but I suspect the reputation she leaves behind is about the reputation she deserves, in view of her chosen way of dealing with the discipline. But the other thing is that at a time when women were not particularly welcome in geomorphology, she was a sufficiently assured personality to sort of blunder into the discipline and show that women could make good contributions. And there weren't many others when Barbara started her career. There's now a whole galaxy of young women in geomorphology. If you go to an AGU meeting now, and hang around the Earth Surface Process sessions, I would say they're almost dominated by young women.

L - Can you talk about that shift? I mean, it must have been very pronounced, obviously, in your career. How has the community responded to the changing demographics in geomorphology?

M - I think geomorphologists have proven to be among the more sensitive to the realization that you're wasting half of humanity here, and sort of began to invite women, to pick women out of undergraduate courses - I guess women enrolled in undergraduate courses to some degree. You pick the bright ones out and encourage them to do graduate work. I had a number of women graduate students who have gone on to various careers, and that's been true of others as well, such that they are now coming into their own. My experience is in many respects that women are better scientists because they're more careful and more particular about the precision and accuracy of everything. More fastidious about their work. I mostly hired women undergraduates as summer assistants because of that, because they do good science. You have to be very careful [in science]. I think women's dispositions - whether it's social training in our society or whether its inherent, I don't know, I don't really care - but the truth of the matter is women are more careful in their work, and that produces better science. So I suspect the fact that women are becoming increasingly important figures in our science means that the quality of the science generally will improve.

L - When you look back on your career, and you think about the big questions that you've applied yourself to, what is still left to do?

M - Well, the big question I've been most closely struggling with, stated as a big question, is "why do rivers behave the way they do?", and that's a dominant question that still is there. Behind that there's a question of space and time scales in the landscape. We do geomorphology by looking at various specific processes. How these processes influence each other has everything to do with space and time scales and other functions of material reservoirs. And how they connect together is a major problem. That's two big questions. That's enough for anyone.

L - So you said you don't know where you would go next with your retirement...

M - Well, if I survive long enough and am still doing work, I want around to write a book on experimental method in geomorphology.

L - A historical book?

M - No, methodological. Which of course would draw on experiments that have been done. It's rare that you can do an experiment in geomorphology because you don't control things and things take too long to happen. But there are certain circumstances in which you can construct a good experiment or at least quasi-experiment in the sense that you can control things in such a way that you can make definitive conclusions. And I think it's desperately important [that] geomorphologists understand those things so that when they do see the chance to do an experiment, they can take advantage of it, which they don't do now.

L - So you are thinking of some kind of natural experiment like the Site C dam.

M - That's a natural experiment, or quasi-experiment. But there's other cases where you can do actual experiments. Some very good examples were put up by Ross Mackay, who wanted

to know about the influence of snow cover on ground freezing in the Arctic. So he put up snow fences, so that he got little snow cover on one side and deep snow cover on the other within a few feet of each other on the same kind of tundra surface. That's a very clever sort of experiment. He artificially drained a lake to see about the pattern of the progradation of freezing into the sub-lacustrine soil.

L - It would be hard to get ethics approval for that these days probably.

M - Well, a small coastal lake in the Arctic tundra... no I don't think it was hard. There is a variety of kinds of experiments you can do and of course there are explicit ones you can set up, particularly in things like soil creep. There are certain areas of geomorphology that are more amenable to this than others. And so I would write not a long book, but a short book figuring out and discussing what you can do to establish experiments and when natural experiments should be taken advantage of. That would be my next project. But we'll see whether it ever gets done. If June has her way it probably won't get done. We'll spend our time traveling the world instead.

L - Doesn't sound so bad.

M - Aaah... I get worried about carbon footprints. I think people should stay where they live. The Hiltonization of the world is a bad thing.

L - This is a personal reflection, sitting here now and worrying about carbon footprints. This is something that has emerged over the course of your lifetime. As a practicing geographer in a geography department, how has that conversation worked its way into your life and your research?

M - Well, I think perhaps more now about the cost, particularly of travel. In my career I went to very few meetings - as I said I'm essentially a loner. So I think that's a good thing in retrospect. I can claim a much lower carbon footprint than most scientists. But it has mainly impinged on my everyday life, not my professional life. To get the things done that I need to get done I sometimes have used sources of major carbon pollution like helicopters, or stiff wing aircraft taking air photographs, or getting us into an isolated field area which is essential to visit. I've used small boats where I could have used larger ones that would burn a lot more fuel. But most of this was simply done for expediency rather than for any ethical sense.

L - You mentioned not going to a lot of meetings and you've mentioned a few times about being a loner. So how have you in your career, engaged in various roles with the community in terms of president, chair, editor, what kinds of roles have you taken on and if you haven't taken them on, why?

M – OK, roles I've taken on. I've done a good deal of outreach work here in the Lower Mainland, about the Fraser River and its development. Public speaking and so on. Talking mainly to the choir, natural history societies and things like that, but also engaging with government offices and providing advice to governments on request about flood management strategy and small stream maintenance strategy. I was a member of the scientific panel for Clayoquot Sound, for example, which was a landmark panel for forest management.

L - In the famous Clayoquot Sound protests?

M - Yes, and that led to the establishment of the scientific panel to recommend forest management strategy. The forest management strategy we came up with, which was not adopted in BC, is nevertheless a kind of a standard for the world.

L - But it wasn't adopted in BC?

M - No. Well, my opinion was that it should not have been adopted except on a trial basis in a limited area initially, because one had to work out the economic implications of it, which we weren't charged to do.

I've done a lot of that kind of thing. I've done a lot of editing work. I was an associate editor for about 11 years of *ESPL*, I was an associate editor of the *Canadian Journal of Earth Science*, associate editor of *Géographie Physique et Quaternaire*, and ultimately the editor-in-chief of *JGR Earth Surface*. Where I've failed is… I've had nothing to do with the formal geomorphological or geophysical organizations in Canada and, in retrospect, I can see that was a serious mistake because of my relative prominence in Canadian geomorphology. I probably could have made them much more dynamic societies than they are, particularly the Canadian Geomorphological Research Group. However, on the other side, I tried to join that group four or five times and never succeeded. They just never responded. I sent them my five dollars several years in a row and never got a response. So I thought "this is a waste of five dollars". However, I could have been much more active in promoting Canadian geomorphology than I have been. I've not been active at all, as a matter of fact. That was probably bad of me.

L - Why did you not get involved? Just time?

M - It was just the loner instinct. I just was more concerned with doing my own thing. Why should I spend my time sitting in a meeting room listening to things I don't want to hear? But to an extent you have to do that in order to sort of make sure the whole social enterprise that lies behind the science moves ahead, and I haven't contributed to that in Canada and I should have.

L - It's interesting that you were able to build your career without necessarily engaging in those same kinds of activities that are now seen as sort of professionally necessary.

M - Yes, I think the way you really build your career as a research scientist is you publish papers in journals that people have decided matter. I mean I've published papers without too much idea about how important this that or the other thing is, but some of my papers apparently are considered to be of value and that's where the reputation lies. In the end I think networking is becoming increasingly important. That's part and parcel of this move to multiple authorship as disciplinary tasks become more and more specialized. But, at least for most of the length of my career, the real litmus test was what you had to publicly say about the science. And so I've collaborated on a more or less continuing basis with only one or two people, beyond my students. I think until about four or five years ago, I'd not authored a paper with more than three names on it, and those names were almost always graduate students of mine or my colleague Rob Ferguson. And then all of a sudden I find myself on papers with five or six names, and that's these graduate students who sort of crowd into the act. We talked about that ages ago.

L - So do you think there's anything we haven't covered?

M - You had some impertinent questions that you emailed to me yesterday. Have you asked those?

L - Yes, so that was about forestry management practices, although the Clayoquot one was one I had thought to ask because it was one that would have happened during your career during this growing public consciousness around forestry, and the growing environmental movement has to be one of the shaping factors for environmental science in the 70s and 80s.

M - Yes my recommendations in the Clayoquot panel were the hydrological ones, and they had to do with work around small streams. They were the most restrictive and I think the most detailed of the recommendations from the panel and I think there's been significant movement toward meeting them - staying away from the stream. The old attitude was, well, once you get above fish, it doesn't matter. What we pointed out was that the quality of the water that comes into the fish reaches depends entirely on what you're doing in the headwaters. And so there's a lot more respect given to the headwaters of these streams now. And furthermore, most animals on the land, when they're travelling, follow the stream courses, because the declivity gives them shelter and protection, and so they are the routes that animals choose in the landscapes, even way into the headwaters, and so there's a lot more attention paid to headwater management now than there ever was before. And I think in part that's an outcome of what I wrote in the Clayoquot panel. But there are other factors involved as well, of course.

The World Wildlife Fund (WWF) took up the Clayoquot recommendations and used them as a standard for forest practice. And I went to the WWF to present our results. So I think that was an important exercise.

L - Do you see yourself as an environmentalist as well as an environmental scientist?

M - Latent. I've been very careful in my career to try and maintain, publicly, a neutral posture. The best way to get governments and private enterprise to do better in the environment is to show them that the environmentally sound way to do things is in the long run the cheapest way to do things because you don't have to pick up and pay to clean up a mess afterwards. So to be able to do that, to give that kind of advice, and at the same time to also talk to the environmental groups who are putting the pressure on to make them do that, you have to somehow try to maintain a neutral image. And so I've tried to be very careful to maintain a neutral image. So I've got sympathy for a lot of the things that an outfit like, say, Greenpeace does, but I've been very careful not to have anything to do with Greenpeace because that would immediately mark me as a kind of environmental loony, which would then mean I was useless from the point of view of advising industry. Whereas I've been able to maintain a position where I can advise industry and be taken seriously. So it's a kind of an odd position to be in but it's a help to be a loner. You have been running around obviously in the background trying to find out who I'm connected with and discovering nothing...

L - Yes, actually, it's been shockingly hard. Yes.

M - Well, that's good because that means that nobody else can obviously connect me with something and decide that I've got some bias or other, and that helps me if I'm called to sort of help BC Hydro decide what environmental issues are pertinent to Site C. They will trust

what I say, but at the same time I can go to Fort St. John and I can - I did - fill the movie theatre in Fort St John with a public lecture about the impacts of the dams on Peace River. So I can use science in the way it should be used, to almost any constituency. And I've been most concerned about that from a personal point of view. And that's one of the reasons I guess I am characterizing myself as a loner. Which doesn't stop you from talking to people, and particularly from talking to your own kind of cohort of ex-graduate students, whom I still work with quite a number of.

L - One of the impertinent questions was what was your biggest failure, but I asked it in a less impertinent way. I think we covered that.

M - Yes that question kind of flummoxed me in my view of projects because I can't really think of one. I've been lucky. Like I said at the beginning of this, I never made a conscious decision about the direction in which my career should go. It simply has unrolled satisfactorily, which is probably why I never made a conscious decision.

L - And now you're able to look back on your career and you feel...

M - Quite happy about it. What is most difficult is to be able to know whether you really have made a difference. I've implicated in this conversation several places where I think I've made a difference. When I say these things it sounds rather conceited, because I don't really know! The person who does things is the last person to know how important those things were.

L – 'What was your biggest failure' is a difficult question and it sounds like you've been very fortunate, but do you have any biggest regrets?

M - I think the business we talked about a couple minutes ago about not doing more to sort of promote Canadian geomorphology would be my biggest regret.

L - And so any final thoughts? Anything that we should cover that you'd want to have recorded in perpetuity?

M - No, I don't think so.

L - Advice for young geomorphologists?

M - Do your math and do your physics. But don't ignore the context in which you're working. Because it is possible to go into the math and physics and just get completely disconnected from what you should be thinking about. But to understand geomorphology as a science, I think you really do need to know your basic science. And that's the change that has overtaken and can flummox a lot of people. I think the decades ahead it will become a less important issue because it's becoming more and more obviously clear that you have to have some science background to do substantive geomorphology.

L - Is geomorphology a science?

M - Yes, geomorphology is a science, but like any other science it can be applied within a variety of contexts with broader human and social implications. And I think if we can recognize those things then this apparent 'two schools' business that seems to be slowly emerging will evaporate again.

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