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Description: **2752 James Fastook,** interviewed by Adam Lee Cilli, November 5, 2013, in his office in Neville Hall at the University of Maine, Orono. Fastook talks about the beginnings of his career studying and modeling glaciers; his beginnings at the Climate Change Institute; conducting research in Antarctica; the CCI's change of focus with the arrival of Paul Mayewski; and the reality of anthropogenic climate change.

Text: 10 pp. transcript
Recording: **mfc_na2752_audio001** 52 minutes

Related Collections & Accessions

Restrictions

Formats Included Document: Original= .docx, Master= .odt, Access= .pdf; Sound: Original= .mp3, Master= .wav, Access= .mp3

Notes

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Narrator: James Fastook

Interviewer: Adam Lee Cilli

Transcriber: Adam Lee Cilli

Date of interview: November 5, 2013

ABSTRACT: This interview took place in James Fastook's office in Neville Hall, which is located at the University of Maine in Orono. In the first half of the interview, Fastook discussed how he became interested in studying and modeling glaciers, how he got involved with the Institute, and his research expedition in Antarctica. Later, he considered how the focus of the Institute changed when Paul Mayewski arrived and shared his views on the so-called climate change debate.

Note: This is the transcriber's best effort to convert audio to text, the audio is the primary material.

Cilli: This is Adam Cilli, PhD candidate in the Department of History. Today is November 5, 2013, and I'm here in James Fastook's office to interview him about his experiences with the Climate Change Institute. I'm wondering if you could tell me how you got attracted to glaciology.

Fastook: Sure. I was a PhD in the Physics Department here at the university. And my field of study in physics was atmospheric physics. We had a big cloud chamber in the basement and we could put various pollutants into the cloud chamber, and we were measuring how water droplets grew as a function of humidity and trace pollutant content. And I was under the impression that we were doing weather physics. I thought we were studying how releasing unburned hydrocarbons into the atmosphere was going to change the nature of the cloud. That's what I thought I was doing. But I met my adviser's adviser at a conference, and I asked my adviser's adviser what he was working on. You know, just idle conversation. He said, "I can't tell you." And I said, "What do you mean you can't tell me? You're doing cloud physics." He said, "I can't tell you. Think about it, taking water drops, and with trace pollutants, and move them from a low humidity environment to a high humidity environment, and know how they're gonna grow." And what he was describing was chemical warfare, was the delivery of the maximum amount of pollutants (poison) to the depths of the lungs. And if the drops didn't grow fast enough, they would stay small and you would inhale them and exhale them back out. If they grew too fast they would be swept up by the cilia in your nose and you wouldn't get it into the lung, and so we had to design this water droplet (we'd grow it at just the right rate) so that it got into the deep, penetrating part of the lung, and was then big enough to impact the surface and stick there and deliver the toxin to the worst place. And so what I realized was that the work I was doing was weapon research, and I was not very happy with that. At the same time, I had one course to finish for my PhD, one class necessary to fulfill the requirement for the PhD. And I had a choice between a numerical analysis class in the Math Department (some kind of dry, numerical method thing), but I'd seen a flyer for Terry Hughes's glaciology class, and in the flyer he talked about using tensors. And I had just come from a relativity class and was full of tensors; I understood them, I liked them, I thought they were a cool device—way beyond vectors as a cool mathematical construct. And so I signed up for Terry's glaciology class on a lark, rather than the numerical analysis class that would have been a more traditional path. Terry was in the

Earth Sciences Department, and then Geology, and so everyone in the class was a geologist, except me. I was a physicist. And Terry was very mathematical. He did a lot of equations; he put a lot of things up on the board. He'd cover the board with equations, and write things out, and cancel terms, and do all this stuff. But the homework assignments, because I had just come from this relativity class, where I learned a particularly powerful notation for manipulating tensors (called the Einstein Notation), I was able to do the homework that he assigned in three or four equations. Whereas everybody else, using a more primitive notation, had to write out page, after page, after page of equations. My notation was powerful enough that I didn't have to do all of that. I did have to write out two pages to explain how the notation worked, because I wasn't sure that Terry knew the Einstein Notation, since he didn't use it in class. I didn't know that he knew it. So, my homework, instead of being five pages of equations, was four lines of equations and two pages of how the notation worked. And so I kind of impressed Terry. The other thing that got me involved with the Institute was that Physics got one of the first desktop computers of the day (this was '77 or something like that). And it was a Hewlett-Packard, it had a one-line screen, it was programmed in Basic, and it was connected through an XY plotter. At the time, Terry was doing the CLIMAP reconstruction of the Laurentide Ice Sheet, and in order to do this, because the models were relatively primitive then, you had to tune the model to the shape of the present ice sheets. So, what Terry was doing was he was taking this deck of cards that was the program of his equations, and he was putting parameters for the equations on cards, and he was handing it over at the desk at the computer center and waiting two hours for the printout to come back from the computer room. And the printout was a list of numbers, and he would take that list of numbers, and he would graph it on graph paper by hand and see how well it fit the profile of the glacier. And if it was too high he would change one number in the deck of cards; if it was too low he'd change another number. And he would have to sit at a terminal and type it out on a card, and stick it back in the deck, and put it over the counter and wait two hours for it to come back with another list of numbers, [and] graph it by hand again. And I had this machine that you could type the number into the computer and it would draw the picture on the XY plotter right then. So I showed this to Terry. I programmed his equations from class into this desktop computer; I could interact with the program directly—as opposed to cards, wait two hours, and the results come back as numbers. I could put the number into the computer, and it drew the picture right away. And so, one, I was very powerful mathematically. Terry liked that. And two, I had access to these computers that made the Mainframe look like a virtual antique: interactive computing. And so, Terry offered me a job right then, while the class was still going on. He said, "As soon as you graduate, come work for me." And so I did, and I was happy to go from cloud physics (that I thought was weather, but turned out to be chemical warfare), to glaciology, where, to my knowledge, no one has ever weaponized a glacier. There's no weapons-applications for glaciers. And so I came and worked for Terry. I did the tuning for the CLIMAP book that Denton and Hughes (*Last Great Ice Sheets*)… has all the reconstructions in it. I did all the reconstructions for that, and I did it in a matter of weeks, where Terry, with his card system, would have taken him months, or even years to do the same thing. 'Cause I could do it in three seconds, what took him two hours to do.

Cilli: So that book, in glaciology, is regarded as a fairly important book?

Fastook: Yeah. It's old, but at the time… George Denton is a very broad thinker, and he kind of went through all the literature, and where all the ice sheet margins were known to be in the world, and he and Terry drew maps of the outlines of the ice sheets. And the CLIMAP project, what the intent of it was to provide boundary conditions for some climate modelers who were doing models of the Ice Age world. Anytime you're a modeler, one thing you want to do is model more than one situation. Because if you model one situation, your parameters that you used to tune it, to get it to fit that one situation, may not be broadly applicable to another system, another world. And so, the climate people had done fairly successful models of the current climate. Their weather-predicted devices predicted when it was going to rain, [and] where it was going to rain. They had good forecasting ability, but they didn't know if it was going to work for a different world. And they chose the Ice Age world because, A) it's very different from the present world (much, much colder; snow and ice everywhere), and B) it's fairly close to the present in time (it's not 65 million years ago; it's 14,000 years ago, 20,000 years ago). It's fairly recent in geologic time. And because it's fairly recent in geologic time, there's a lot of evidence for it. There are climate proxies in the forms of beatles and pollen and bog records. So, it was a world that they had accessed information about, but one of the pieces of information they didn't have was what the topography of the planet was. Because, with ice sheets, ice sheets are big. They're like mountain ranges; that are not there now. So, during the Ice Age there was a mountain over what's now Hudson Bay. And so the circulation of the air was much, much different. It flowed down around the south edge of this thing, and up around the north side of it. It couldn't flow straight across it like it can now. And so they were interested in having a topography of this Ice Age world. They wanted to know the height of the glacier, cause it affected their climate models… how the air flowed over the planet. And the glacial geologists of the time knew pretty well what the glacial footprint of the glacier was. They've got morraines, piles of dirt, that they've been able to date with carbon 14, that they know this pile of dirt was built at a certain time. And they've traced these piles of dirt around the periphery of the ice sheets. So they know what the footprint is, but they didn't know what the elevation was. And the only way to get at the elevation was with a model. And so the part on [CLIMPAP] that we took was we had a set of equations that described how ice deforms, and what we didn't know was how hard the ice was. How much it deforms for a given force. So, we took this set of equations, programmed it to make profiles (slices through a glacier), and we compared those profiles with the two existing ice sheets that we have: in Greenland and Antarctica. And with this comparison we were able to tune in what the ice hardness of the current ice sheets was. Then we made the assumption that the paleo ice sheets had the same ice hardness. We programmed slices through this ice sheet; we knew where the edges were. We just didn't know how big it was in the middle. And so we could then predict how big the ice sheets had been, and we provided it as input data for these GCMs that were running as part of the CLIMAP project.

Cilli: So when Terry Hughes hired you it was what, as a technician?

Fastook: I was a postdoc. And it was originally for two years, and it involved a trip to Antarctica. We went down to Antarctica and measured the velocity of the Byrd Glacier. We laid out targets for a photogrammetry flight that came over and took pictures, and then came back and took pictures three months later. And we were able to compare the photos and see how much the ice had moved in that time. And from that we got the velocity of this ice. Now they do it remotely with a satellite, but in that time velocities were mostly measured by putting stakes in the ice, and serving in their position, and then coming back three months later and serving the state to see how much it had moved. And it was very labor intensive. And this photogrammetry was kind of revolutionary, because it gave us hundreds and hundreds of velocity measurements from the photographs, as opposed to from people crawling around on the surface of the ice, risking their lives (to fall in crevasses). It was all done from airplanes. So that was a bigger deal. We still had to put in the control for the photographs. We had to survey in the mountain peaks. The rock outcrops were stationary; we had to know where they were, so that when they referenced some moving target, a crevasse or a serac or something that could be seen in the photos, when they moved we'd know how much they moved relative to something that was fixed. And so we surveyed mountain tops and other things that stuck up through the ice. And I was there for one season. I didn't like it a whole lot. I'm not a real good field person.

Cilli: One season.

Fastook: One season, yeah. Terry went a lot. Three or four seasons.

Cilli: So, a few months you were there.

Fastook: Yeah, I was there from middle of October until middle of January.

Cilli: Can you walk me through a typical day in Antarctica?

Fastook: A typical day in Antarctica?

Cilli: Sure.

Fastook: You should really talk to Brenda Hall about typical days in Antarctica. She still goes there. I went once, and didn't like it a whole lot.

Cilli: Yeah, I mean a typical day for you when you were there.

Fastook: A typical day for me? Well, we were in field camps. We were living in tents, Scott tents, which are about six and a half feet square, and you can stand up in the center of them. A pyramid-type tent. And they're the kind of tents they've been using since the heroic age of Antarctic exploration. Scott and Amundsen and all these guys used these kinds of tents. I don't know why. They're stable in the wind I think. The surveying teams were two people. I was with a guy named Tad Feffer. He's still a friend; he's out in Colorado. And we were assigned a survey spot at the top of a cliff. We could see the whole glacier out in front of us, and we had a theodolite set up on top of the cliff. And every day Terry, who was the PI of the project, would arrive in a helicopter, and the helicopter would fly out into the glacier, and we'd track it into the theodolite. And it would land somewhere on the glacier and lay out a target. And we would measure the angles to that target out on the glacier. And reference them to some other target on the ice, and so we did this the whole way around the edge of the glacier, so that we had targets all around the edge, and a line of targets up the middle, that were these big Mylar crosses that we had to put out. And the big Mylar crosses were going to show up in the photograph; they were gonna be reference things. We had an early version of GPS…it was about the size of a beer cooler, and it took three or four days to figure out where it was. And it didn't figure it out in real time. It recorded its information on tape and you took that back to the lab in the United States, and with the supercomputer you could process that into a position for this object. So we had these beer cooler things with GPS units that we had to put out on these moving targets; that we would measure where they were as they moved down the ice. So, Tad and I spent most of our time in this tent. We moved along the edge of the glacier; Terry flying in the helicopter overhead, putting out targets. The Mylar targets had to be weighted down with rocks. And so every day or so Terry would arrive in the big Huey helicopter and Tad and I would spend the afternoon loading rocks into the helicopter. Which then Terry would fly away with to deposit on the glacier, to hold down the Mylar.

Cilli: Terry himself wasn't flying the helicopter?

Fastook: No, he was a passenger. There were Navy pilots. The Navy did the support in those days. And so there were Navy pilots who were flying us around. They were Huey, Vietnam-era type helicopters.

Cilli: When you were out there, did you or someone you were working with have any close calls, where you almost got hurt?

Fastook: Yeah. No one got hurt really seriously, but we did have a helicopter mishap. We had a helicopter that took off from a nunatak (a mountain that sticks up through the ice), and we dropped a team of surveyors off on a nunatak, and were flying to another place to drop off Tad and I. Terry, Tad, and I were in the helicopter. We dropped off Mark Highland and I can't remember the other guy's name. It was a grey day; the sky was grey instead of blue. And when that happens in Antarctica it's sometimes difficult to see where the horizon is, cause the grey snow turns into the grey sky. And I think the pilot was probably watching his radar altimeter instead of out the window, and the radar penetrated the snow, so it was giving him a false sense of elevation, and he basically flew it into the ground. And so the helicopter hit and rolled and came apart. We thought Tad was hurt badly. He wasn't, in the long run. But at the time we thought his back was hurt. And so we got him out, put a tent up over him, and we got the radio out and called for help. And a rescue mission came and picked us up and took us back to McMurdo. That doesn't happen very often. The helicopter pilots were really, really good. This was the '70s, and so the Vietnam war was just winding up, and there were a lot of pilots who had thousands and thousands of hours flying under really bad circumstances. So these guys were really good. But this guy just made a mistake, and wasn't watching out the window…watching the altimeter instead.

Cilli: You say you thought Tad was severely injured.

Fastook: Yeah. We thought he had broken his back, and treated him for that. We immobilized him, and apart from dragging him away from the helicopter, [we] didn't move him. And when the rescue people came they wired him up in a proper stretcher and took good care of him. It turned out not to be a broken back; it was a cracked vertebrate. But he was hurt. I was pretty much unscathed. Terry didn't have a scratch on him.

Cilli: The three of you were in the helicopter and it was tumbling around?

Fastook: Yeah. The crewman had a broken rib and one of the pilots had a big cut on his forehead that made a lot of blood but wasn't serious. So, I really lost my interest in helicopters at this point in time.

Cilli: I imagine you did.

Fastook: Yeah.

Cilli: So, when you and Tad, it was just you two, you had to share a tent. And then, what? When you got up in the morning, did you have some kind of a stove to cook food on?

Fastook: Yeah, we had a regular camp stove. [A] one burner camp stove. And we made tea and oatmeal. The food was pretty good, because it was well below freezing, so we had boxes and boxes of frozen food. Frozen steak, frozen lobster, frozen vegetables. It was like having a walk-in refrigerator outside the tent, in this wooden box. We were not light weight. We had a thousand pounds of gear. We were not hikers. We had generators, we had solar panels, we had two tents: the tent we lived in and an emergency tent in case we burned down the tent we lived in. We had sleeping bags that were huge. You could barely get them in a full-sized duffle bag. And they were all feathers; no synthetics. We had crampons and stuff like that for negotiation rough terrain, but we never did. Cause it was too scary.

Cilli: And you had cots in your tent?

Fastook: Yeah, we had cots in the tent.

Cilli: Did you have a separate tent for preparing food?

Fastook: No. We cooked and lived in the Scott tent. Sometimes we would cluster up together into a larger camp with multiple tents, in which case we would have a sleeping tent and a food tent, but we didn't cluster up that much. We were mostly in teams of two, in a single Scott tent.

Cilli: Prior to going to Antarctica, had you read any of the accounts of the early Antarctic explorers?

Fastook: No.

Cilli: Were you interested in doing so afterwards?

Fastook: Yeah.

Cilli: Did you?

Fastook: I have, yeah.

Cilli: When you were out there, did you see yourself as a kind of explorer?

Fastook: Of course. And one of the guys on our team was an actual explorer from the Heroic Era. A guy named Swithenbank (was his last name). And he's still around. Terry communicates with him. He's an English guy. And he was there in the '40s as one of the Heroic Age explorers, before IGY in 1959, which was when the first real expeditions went to Antarctica (with lots of money). He was there as an explorer, and because he was from this era, he would always come prepared to walk out. We couldn't have walked out. If the helicopter wasn't going to come pick us up, we were going to die where we were. But he was ready to walk out. He had all the survival gear, and a sled, and crampons and snowshoes, and whatever he needed to walk out if he had to. If the helicopters didn't pick him up, he would have trekked the 150 miles to McMurdo and gotten out alive. 'Cause he was of that era, where you had to be prepared for that. We were the younger scientists who were dependent on helicopters.

Cilli: The Heroic Age of exploration… what was the rough chronology of that?

Fastook: Around the turn of the century, around World War I, was when it starts.

Cilli: Up to about the 1940s?

Fastook: Yeah. You know, Scott goes to the Pole and doesn't make it in time cause Amundsen beat him there. And Shakleton and Perry (well, Perry's kind of later)… all these guys who went there first. You know, first to the Pole and stuff like that. That's the beginning of the Heroic Age, and it ends kind of in 1959 with the International Geophysical Year. That's when the first real scientific expeditions go to Antarctica and they perform a lot of mapping and a lot of real science. Before that, it was just guys wandering around and making maps. But the science starts in the late ‘50s.

Cilli: And I understand that Hal Borns was one of the early…

Fastook: I think both Hal Borns and George Denton were there in IGY. George Denton was a graduate student at the time, but I think he was there in IGY. And Hal definitely was, yeah.

Cilli: So, at what point did you become a faculty member?

Fastook: I was a postdoc for something like seven years… a really long postdoc. Most postdocs are two years, but I liked it, and so when the CLIMAP grant was over I wrote a grant of my own and got funding and was doing that for I think seven years. I was on soft money, external funded money, for that time period. Then the money dried up, partly because the Antarctic Treaty was renegotiated. But prior to this NSF had a dedicated line to Antarctic research. And consequently they had a certain amount of money they had to spend in Antarctica, and it made getting funding kinda easy. Cause if you were willing to go, and you were willing to work in Antarctica, there was money to do it. The Feds wanted to spend money in Antarctica, partly because when the treaty came up for renegotiation, the countries that spent the most money would be able to say, “Look, we’ve spent a lot of money here. We want a big part in writing the new treaty.” And the new treaty came up in ’83 (or something like that), and when they renegotiated, NSF lost its line. And the program then had to compete for money, just like all the other directorates of the NSF. And it got significantly harder to get funding, and I ran out of money. And I was contract teaching here at the university. I taught in the Math Department; I taught in Physics; teaching for people who were on sabbatical; and [I] adjuncted. I was doing this in computer science, and the guy for whom I was covering didn’t come back from his sabbatical. It was one of the dot.com run ups, and he found a better job while he was on sabbatical. He didn’t come back, and I slipped into his job, which is how I ended up in Computer Science.

Cilli: At what point did you become a member of the Quaternary Institute?

Fastook: When I was a postdoc, so ’78.

Cilli: You joined right away, then?

Fastook: Yeah. I mean, my postdoc was in the Institute. I was a member of the Institute from the beginning. I’m a Cooperating Member now, only because my whole salary line is in Computer Science. Some of the faculty over there have joint appointments, which means they get money from both departments. But I get all my academic salary from Computer Science, so I’m a Cooperating Member. But I have the same voting rights and committee responsibilities as everybody else.

Cilli: When it was still the Quaternary Institute, did you participate in research with other members besides Terry and George?

Fastook: The Kellogs. I wrote a paper with the Kellogs at one point. Hal. I’ve had a number of collaborations with Hal.

Cilli: Could you give me a specific example of a project you did with Hal?

Fastook: We were looking at the Younger Dryas re-advance of the Loughmolmin [?] Ice Sheet. So I was doing reconstructions of what the ice sheet would do. Hal was mapping the margins, and I was figuring out how much the climate would have to change in order to make the glacier grow from nothing, which it pretty much went away during the period before the Younger Dryas re-advance. And it grew out to this position, and how much the climate had to change in order for the glacier to grow that much. [That] was the kind of project I did with Hal.

Cilli: Walk me through the human side of a collaboration of that nature. Did Hal approach you and say, "James, I'm thinking about doing this. Do you want to work together on it?"

Fastook: Oh, God. I have no recollection of how that happened. We have regular meetings at the Institute. Every month at 7:30 on a Wednesday morning, we meet for a couple hours, and the director talks to us, and afterwards there's coffee and doughnuts. (Well, not doughnuts anymore, but there's still coffee.) And conversations occur at those kinds of meetings. And at various times in the past we had a relatively active speaker series where, on a Wednesday night we'd meet in the classroom building and somebody would give a talk. Not necessarily from the Institute, but somebody from away, that we would bring in. So, there were those opportunities for a conversation. We have a regular field trip that we go on every fall, where we are locked up in a bus as we drive across the landscape, looking at sandpits, and moraines, and archeological sites, and historical landmarks. Sometimes we go as far as Cape Cod, or Quebec. They took a field trip to Newfoundland a couple years ago. And so, you're locked in a traveling vehicle with the gang for that much time. You have a lot of chance to talk. We have an annual seminar series, a two day mini-conference where everyone is expected to give a conference-style talk on what they're doing at the time, including the graduate students and the faculty. The Institute's gotten too big for a two-day session now. We really need a three or four-day session in order for everyone to have 15 minutes. But when the Institute was smaller everyone would give a talk on what they were doing. There's lunch and a meal an plenty of time to talk between sessions, so there's plenty of opportunity for collegiate conversation. And at least in the glacier side of things most of us knew what we were doing and how it could be useful to our research. And I've always served the geologists. Modeling by itself is not that interesting. But modeling in support of somebody collecting data is. And the geologists are collecting data. And one of the things that modeling can do is provide a way of looking at the field evidence that they're seeing, and help them to interpret. It allows you to kind of interpolate between data points. If the boundary was here and here, then it had to be here in between. You didn't have to measure the whole boundary. You could measure points on the boundary. A model would interpolate physically in between. Because climate is one of our concerns, how much the climate has to change, in order to make the glacier do what the geologist think it did, was a way of kind of measuring the climate in the past.

Cilli: How do you think the Institute has changed since you first got involved with it?

Fastook: One of the biggest changes is that I no longer have to explain what it's for. When it was the Quaternary Institute, people would say, "What's that?" And I'd have to explain that it's the current geologic epoch; it's the last two and a half million years; what went on in that epoch was namely the coming and going of glaciers. We study the glaciers and we're trying to figure out why glaciers come and go. But as we change our focus from just studying Quaternary, to a broader evaluation of climate change…and this occurred when Paul came to the Institute. Paul brought this climate change emphasis to the Institute. And it gave us a focus to our research that was much more societal impact-oriented, and not so much just for the abstract knowledge: of why ice ages occur and why did the ice go away, and where did it come from to begin with, and will it come again… those kinds of academic science questions.

To actually being able to predict climate change in the future and understand what the consequences of it would be. And even back in CLIMAP days, what those people were trying to do was validate their climate models. They work great now, but how will they work when the system changes. In other words, when CO_2 loading occurs. They were making these predictions about climate warming back then. The original greenhouse effect was a back-of-the-envelope kind of calculation done in 18-something. It's basic elementary thermodynamics, that if you trap more solar radiation than you reflect it gets warmer inside. And it's a simple equation that describes it. But the climate models of the day were beginning to have these kinds of predictions. And nobody would believe them, because they said, "oh, your model's tuned to the present. It's not appropriate for this changed world that you're describing." And so they were looking for changed worlds to do models of, to confirm that their models worked…. But at the time it was not couched in terms of climate change, so much as understanding climate. And as climate change became a confirmed occurrence, of course now we're looking at rates of change. And [we are] looking for analogs from the past, when the climate changed as rapidly, or maybe almost as rapidly as it's changing now, so that we can understand how the landscape changed then (from evidence), and that will inform our prediction of how the landscape will change in the future. It's been warmer in the past that it is now, but it didn't get that warm that fast. It got warm gradually and then it cooled off again. The Medieval warmth period is a time that was warmer than now, but it didn't warm up like the hockey stick. It warmed up gradually. And it cooled off gradually. That's kind of natural climate change. What we're doing is we're changing it faster than it has in the past. But the consequences will be the same. It's just that, from the animal's point of view they don't have as much time to adapt to it. When it warmed up the last time the polar bears had a chance to move someplace else and find some other way to catch seals. Now, in the span of one polar bear, it's gone from a place they can eat to a place they can't.

Cilli: What do you think the Institute's most important contribution to climate science has been?

Fastook: We have a lot of outreach right now. A lot of our students are not so interested in process as they are in policy. We have a lot of students who want to become informed climate scientists, so that they can affect policy. And whether they affect climate policy as scientists or whether they run for Congress, one of the ways that the old paradigm gets swept aside is that the old people who hold that paradigm die and are replaced by young people who have a new paradigm. And so, it would be really sweet to see a number of our Climate Change graduates, who understand climate and are aware of the realities of the situation, in positions of decision-making in government and other agencies that have to do something, or things are going to get rather strange. Things may already be gonna get strange, but we have a lot of students who are interested in it, not so much from doing the science as from the policy point of view. And they came up with this themselves. We had the students themselves actually (this was three or four years ago) formed a group to study climate policy. And they asked for a course in climate policy, and I think that… I'm not sure who teaches that. It might be Kirk.

Cilli: Outside the scientific community, the human role in climate change still is very much up for debate.

Fastook: No, it's *debated*. It's not *up for debate*.

Cilli: Yes, it's debated. I'm wondering if you can comment as to why that might be the case.

Fastook: One very simple answer is that the companies that own the carbon would like to sell it. And they would really rather not have restrictions put on them selling the carbon that they have in the ground. The oil companies that have big reserves of oil, they would like to sell it. The Canadians that have all the tar sands, they would like to sell it. Canada just pulled out of the Kyoto accords, because since they signed the Kyoto accords they found all this tar sand and they became one of the big petroleum reservoirs of the planet. And it's partially a technology, that they can extract petroleum from the tar sands; it's also the price of oil, which reached some level where it became economically feasible to take petroleum out of tar sands, but Canada has become one of the oil oligarchs. They got as much oil as Saudi Arabia (don't quote me on that; I'm not sure of the exact numbers). But they got a lot of oil, and they pulled out of the Kyoto accords. Cause, if you got a lot of carbon you'd like to be able to sell it. It's money. The deniers, they're sometimes encouraged by interests. I don't know how much of this I want to say on a recording, in terms of specific names, but there are people who are funding climate deniers.

Cilli: Such as large oil corporations?

Fastook: Indirectly I suppose, yeah.

Cilli: Well, that's all the questions I have, but before we conclude the interview I do want to give you a chance to add something that I didn't think to ask you about.

Fastook: I came to the university as a graduate student in 1971, did a PhD here, had a postdoc that I still look back on as the best years of my career. I had no teaching obligations; I had no meeting obligations. It was just seven years of purely doing science. I have a really good teaching gig here. Working in the Computer Science Department is a lot of fun. They give me a lot of resources; I'm surrounded by more computers than I can use. I'm given a lot of time to study new things; part of my job assignment is [to] learn something new. I'm not locked into doing the same thing over, and over, and over again. I'm encouraged to branch out into new things. A lot of my success as a graduate student I owe to the Institute. The mentors in the department—Hal Borns, and Terry Hughes, and George Denton, and George Jacobson—the mentors who encouraged a young scientist to go to meetings. Terry would come in and say, "Well, I submitted an abstract for you at this meeting. You've got three months until the meeting. Go write the paper." And he'd have already submitted the abstract for the paper I was supposed to write. I hadn't written it yet, but he submitted the abstract, he provided me with money, he sent me places. I went to Holland and England and Sweden, Belgium, Switzerland, all on travel money from the Institute or from a grant that I got through the Institute. So I really owe my career to the organization and the source of inspiration, and the whole Institute. I'm very happy with my association with the Institute. Have been all along. It's been great.

Cilli: Well, thank you once again for agreeing to do this interview.

Fastook: OK.