Eville Gorham

Interview with Eville Gorham

Interviewed by Professor Clarke A. Chambers
University of Minnesota

Interviewed on September 15, 1994

Eville Gorham - EG
Clarke A. Chambers - CAC

CAC: I'm conversing this afternoon with Eville Gorham, Regents professor at the University of Minnesota, who has been here since the early 1960s and has been very active in biological sciences generally, and his own discipline in research and teaching, and in other university-wide affairs.

Eville, as I suggested before I turned this machine on, it's very useful to get kind of an intellectual and academic autobiography, not too extensive, but how on earth did you, as a boy, get interested in the range of things that came to be your career? Then, how did you get trained? Then, we're off and running

EG: When I was a boy, my family had a small cabin on an island in a lake in Nova Scotia. I grew up wandering around that little island and the surroundings and paddling about in a row boat on the lake; so, I developed an interest in natural history. But, that was really stimulated very greatly by a teacher I had, Donald Crowdis in grade eight, and another teacher I had, Charles Allen in grade ten, both of whom were biologists who had taken master's degrees at my, later, University of Dalhousie. They sparked in me an interest in natural history and biology that directed me in that path when I got to the university.

CAC: How on earth in intermediate school could they do that?

EG: I really don't know how they did it; but, they communicated their enthusiasm without much doubt. Charlie Allen used to take us on walks in the woods and turn over stones. We did field trips. Don Crowdis was just an enthusiast and it came across in his teaching. About forty years later, I got in touch with both of them to thank them for this.

CAC: [laughter]

EG: One of them, Crowdis, said that he'd left school teaching soon after he taught me and he'd never had the faintest idea he'd had any influence on any of the students he taught; but, he was glad to know there was one.

CAC: Ohhh!

EG: The other one, Charlie Allen, had become superintendent of a school for the deaf and thanked me ever so much for not waiting until it was time for the obituary.

CAC: What a nice story.

EG: Then, I got to Dalhousie in Halifax, Nova Scotia.

CAC: Which would have been a natural choice for you.

EG: Yes, I didn't come from a well-to-do family. It was essential that I go local. I went to Dalhousie and I bumped into Hugh Bell, the botanist, there. He was head of the department of four. He, again, was an enthusiast. He was probably somewhat out-of-date and in an old-fashioned field of plant anatomy; but, he could make that exciting as a teacher. From the day I got there, I was the one new lamb of the biology department. This was during the war years, World War II, and I was the one likely prospect they had to go on to graduate school; so, they treated me as one of the group. The group of four faculty doesn't want to talk to one another all the time. There are too few of them; so, they would talk to me instead and I was, more or less, part of the department from the day I entered. I had somebody teaching me a course in histology on the side, just for fun, because I was interested in how they made slides that went into the Biology I lectures and labs. I got very well treated. I got out of botany and into zoology because I had a quarrel with my master's adviser, who was a workaholic to end all workaholics.

CAC: As an undergraduate, how sharp are the lines between the various lines of the biological sciences?

EG: Biology then had a couple of botanists and a couple of zoologists. I started out in botany because of Hugh Bell, who was a botanist. As I say, I got out of that. I wanted to do a master's degree at Dalhousie; but, I couldn't get on with the fellow with whom I chose to work. He was a very bright fellow; but, as I say, a terrible workaholic. He wouldn't allow me any time off at all on weekends or anything; so, I moved into zoology where I met a much more rigorous and well-known scholar, Ronald Hayes, who had an international reputation at this small school of 800 students. He got me enthusiastic. He had trained at the University of Liverpool and while Hugh Bell thought he could get me into Harvard, through his connections there, as a Ph.D. student, Ronald Hayes and his wife, Dixie Paluay, who was British by birth, suggested I might do better to go to absorb a whole different set of prejudices and preconceptions and go across the water to Britain—which I did.

CAC: Oh, good. They enabled you with a scholarship support of some sort?

EG: I had no scholarship money at the start. My father said he'd stake me to a year and see how things worked out. I wrote for a fellowship, actually. I wrote for admittance to the University of Sheffield. I knew I couldn't go to London, or Oxford, or Cambridge; it would be too expensive. I'd go to a provincial university. I wanted to move away from animals because I'd gotten fed up with killing them; so, I moved back to botany. I wrote to the University of Sheffield because I wanted to move from experimental embryology of [unclear] into plant ecology. I knew nothing about it.

CAC: But, you knew that they had that there?

EG: Yes, I looked at the *Journal of Ecology*, which had a group of officers, and I saw that the secretary was at the University of Sheffield, a man named Clampimus. I wrote to him—never got a reply. Then, in the meantime, I picked up an 1851 Exhibition science research scholarship for colonial students, so to speak . . . thanks to . . .

CAC: [laughter]

EG: . . . Prince Albert's foresight and using some of the profits of the 1851 Exhibition to support students. I got one of these and they wrote back to say, "Where would you like to study?" I wrote, "Tentatively, I've applied to Sheffield; but, they haven't answered my letter." Thereupon, for reasons I don't understand yet, they wrote back, "We would like to inform you that Professor Pearsall moved from Sheffield to London two years ago. We suggest that you write to him there." I'd never heard of Pearsall; but, I wrote to him and he, more or less, replied by return mail, "Sure, come along, by all means." It was the biggest break in my life really because I learned a lot in London, not just academically but in all sorts of ways.

CAC: You knew already at that point that ecological concerns wanted to be where you were?

EG: Yes.

CAC: Where did that come from?

EG: It came from this adviser with whom I quarreled who took me on a summer field trip to study the ecology of pastures in the Maritime Provinces and along the way, we did a lot of plant collecting. As I say, he worked from eight in the morning till eleven at night and wouldn't allow me any weekends off to go see my girlfriend or anything. In the end, he told me, "Look, Gorham, you don't really know what work is. When I was a graduate student doing my master's degree at McDonald College, there were five of us in the group, three of us got ulcers. We knew what work was."

CAC: [laughter]

EG: I said, "If I have to get ulcers to satisfy this guy, I'm going to get out."

CAC: At least you picked up a sense of ecology?

EG: Oh, yes.

CAC: Would that have been widespread at that time throughout the . . .

EG: Not really, not really, no. A few departments would have an ecologist . . . some big ones might have even a couple; but, it wasn't a recognized part of the natural complement of biologists, particularly at a small college.

CAC: The revolution that came in the biological sciences was how many years off?

EG: I think that ecology began to take off after the war. It switched from being a purely descriptive science, people looking at what plant and animal communities were . . .

CAC: Classifications.

EG: . . . and how they related to the climate, and topography, and geology to looking at how ecological systems function, and the processes that went on in their productivity, their nutrient cycling, decomposition processes, things of that sort.

CAC: So, that's in place beginning in the post-war era?

EG: It was just developing in the 1950s, yes; so, the natural break point was World War II.

CAC: Having in mind people who are going to be listening to this down the line, what factors internal to the science or external in a cultural or social sense delayed that sense until 1950, plus or minus?

EG: I think it's hard to know; but, once you're describe everything, then the natural inclination is to see how these systems work, how the plant communities work.

CAC: [unclear] was in place by this time?

EG: Yes. In Britain for example, Sir Arthur Townsley had written *The British Islands and Their Vegetation*, which essentially described the plant communities of Britain and how they related to climate, topography, geology, and so on. If you read that book, you got the sort of sense that everything in plant ecology was known; so, why go on describing another minute facet of some plant community and how it related to geology? That had sort of naturally come to a fruition and there was room for new thoughts about how these communities actually operated and some biologists were beginning to study chemistry and look at things . . .

CAC: Ah ha.

EG: . . . like nutrient cycling and so on. That, essentially, is what I did. I got interested in the chemistry of these ecological systems.

CAC: But, that early on, there were not cultural, or social, or political issues that would have led one to think of ecological systems?

EG: There was very little environmentalism. Paul Sears had written *Deserts on the March* and things like that; but, they hadn't made much of a dent in academia. There was no sense of environmentalism on the campuses that I knew at that time.

CAC: Isn't that interesting?

EG: They were just beginning to get started in Britain then. The Nature Conservancy was founded during the 1960s, I would guess, when I was there . . . the late 1950s and early 1960s.

CAC: That's a story we want to trace out as we go along in our conversation this afternoon.

EG: I became interested in the chemistry of ecological systems. Again, most of what I did was serendipitous, just like my going to London to study with Pearsall. I happened to be writing a personal letter to someone in Sweden for reprints at the suggestion of my professor Pearsall who said, "Don't type one of those little postcards that they fashion in America. That will only go in the wastebasket. Write a letter and write it by hand."

CAC: Good.

EG: So, I wrote a letter to Lars Gunnar Romel in Sweden and at the end of the letter, I said, "It must be wonderful to study these systems in Sweden. Here in England, we have so much of the complications of history to deal with that all the ecological systems are mucked about." He wrote back and said, yes, the reprints were coming by separate post and, yes, Sweden was a great place to study these things, and why didn't I come for a year.

CAC: [laughter]

EG: So, I went and while I was there, I happened to go up to the University of Upsala where N.R. Duryea was the *doyenne* of plant community ecologists. He took me around to peat bogs . . . actually, he and one of his students who was also his mistress at that time, and later became his wife. He was about sixty odd at the time. He took me out on some of the local peat bogs and while there, he was telling me that this student, Margaretha Vitting, was showing that the bog pools were essentially derived from rainwater, that these were raised bogs, domed up above the surrounding landscapes so they got their mineral supply solely from the rain. I thought, yes, that sounds logical; but, why haven't they ever analyzed the rain to make the comparison exact

between the pool chemistry that Margaretha was analyzing and what was really actually in the rain as it fell? So, a few years later, when I went to the English Lake District to work at the Fresh Water Biological Association—again, by a happy accident . . . although, I'd done by Ph.D. in that area—I thought, I'll take a look at this question; so, I went and got some bog pool water and I went and collected rainwater for several collections and analyzed it. Much to my surprise, I found that I was able to confirm Duryea and Vitting's observation that the bog pools were slightly altered rainwater; but, I also found that when the wind was blowing from . . .

CAC: Ohhh.

EG: . . . the Irish Sea that we were drenched by sea salt and when the wind was blowing from industrial Lancaster, and Northumberland, and Durham, we were being drenched by sulfuric acid. So, I started off on a long series of studies on acid rain, essentially with no thought in mind of that when I began. Fortunately, one of my colleagues at the Fresh Water Biological Association was looking at the chemistry of lakes in the Lake District where I was then and finding that the lakes on the hard rocks of the Central Massif were very dilute, not much more concentrated than rainwater, and they were surprisingly acid and, of course, I was able to tell him why. That started me on some studies of acid rain that are still going on.

CAC: You must have been one of the first really to observe these connections and start working on it?

EG: One of the first. There were three different groups that found acid rain in rural areas in 1955, published in 1955; but, I was the only one who kept on with it and really looked into what it meant. Then, twenty-four years later, somebody asked me, "Who had first used the term 'acid rain?'" I looked in my papers and I'd never used the term. [laughter]

CAC: [laughter]

EG: I talked about rain being acidified and that sort of thing; but, I had never used the phrase itself. This sort of stuck in my mind and, later, somebody asked me to do a little historical research for a paper on the history of acid rain. I recalled that somewhere in my reading, I had come across a book called *Air and Rain* by Angus Smith, published in 1872. I thought, I ought to have a look at that because I knew Smith was a chemist. I'd seen some reference to his work on the nitrogen content of rainfall and that was what gave me the clue. I looked this up. It took me four months to get it through Interlibrary Loan. Then, I found they had it in the athenaeum down in the Minneapolis Public Library, later on. Angus Smith, back in 1872, described acid rain.

CAC: Using that term?

EG: Using that term. What he did was look at the rainfall falling in cities. He was a chemist by training; but, he was Queen Victoria's first inspector of alkali works, and he became interested

in what these were dumping out into the rain, and he also thought that the chemistry of the rain might give the clue to rain-borne diseases. He was devotee at first of the Miasma Theory, and then, he very gradually gave that up in favor of [Louis] Pasteur's germ theory. He actually first described acid rain in 1852 in the city of Manchester.

CAC: Now, when you picked this up in the 1950s, you know that it's new? You're doing a new gimmick?

EG: Yes, and I thought it was very interesting and I published a whole bunch of papers on it, none of which made the slightest ripple in the field. [laughter]

CAC: Really?

EG: They were met with thundering silence.

CAC: They must have been part of a bundle when you came to Minnesota though to recommend you?

EG: I had done a lot of research of various kinds including acid rain research, studies of lakes, limnology, studies of peat lands, and so on; so, it wasn't that in particular, I think, but just that I'd done a lot of ecology and what's now called now biogeochemistry.

CAC: I want to talk about that later—remind me. We start stringing these words together, don't we?

EG: Yes.

CAC: The kind of work you do just has to be cross-disciplinary.

EG: I gave a talk to Gown and Town not too long ago about the field of biogeochemistry. You may not remember that Gown and Town has a penchant for obscure titles? I thought I would give a talk entitled, "Trinitarian Science: What's Past is Prologue."

CAC: I see.

EG: Essentially, it's the linkage of biology, geology, and chemistry and what I was giving was a history of how this discipline arose; so, I thought that was a rather good title. Nobody twigged. [laughter]

CAC: [laughter] You're saying that nobody really took up the significance of what you were doing at that time? They would come to see the significance later?

EG: Yes. Actually, the person who brought it to wide-spread public attention was a Swede, Swanto Oden, who in 1968 wrote something in the bulletin of the Swedish Ecological Society detailing some of the effects in Sweden. He also wrote an article for *Dagens Nyheter* [unclear] . . .

[telephone rings - break in the interview]

EG: *Dagens Nyheter* is one of the more prestigious Stockholm dailies and this caused quite a debate in Sweden and became the basis for Sweden's case to the United Nations in the first United Nation's Environmental Conference in Stockholm in 1972. Then, Oden did a tour of North America, and talked to Gene Lichens who had done some work at Hubbard Brook on rain chemistry, and started looking into its acidity after his meeting with Oden. They held the first International Conference on acid rain in Columbus, Ohio, in 1975 and that's when it first became a generally popular topic.

CAC: That's a long time from your initial questions.

EG: Twenty years, yes.

CAC: How did you train yourself . . . formally or just by osmosis in chemistry and geology?

EG: Totally informally.

CAC: Did you have any mentors who would help you and guide you in this?

EG: I did. When I left the University of London and moved up to the Fresh Water Biological Association in Windemere in the Lake District, the chemist there, a man named John Macinrath, was one of those chemists who was interested in the ecological questions and talked to ecologists. I learned most of my chemistry from him. More or less, it was cookbook chemistry.

CAC: And geology the same way?

EG: I still know very little geology but enough to make sense of what I need to know.

CAC: There would be other fields later on as your career goes along that you have to move sideways to have at least a basic understanding in in order to do our own work?

EG: Yes, ecologists have to be Jacks-of-all-trades, perhaps, not so much as geographers; but, close to it.

CAC: As that process goes on, it's a self-learning process?

EG: Oh, yes, very much.

CAC: In your case and in all cases?

EG: Nowadays, our students can be fairly broadly trained. Our graduate students in Ecology can go and take courses in Soil Science, and Climatology, and Advanced Chemistry, and Statistics, and Forest Hydrology, and God knows what.

CAC: But, these were not options available to you?

EG: Not to me, no. I should have taken more math, physics, and chemistry; but, my Dalhousie teachers would have thought I would be a classifier or taxonomist and I wouldn't need that sort of thing, which was a great loss to me because I've been behind the curve ever since . . . the sorts of things that I could have used very effectively. I took every botany and zoology course at Dalhousie. I'd been much better off taking half of those and then spending the rest of the time on math, physics, and chemistry.

CAC: Right. Now, you're in England and you have to find your way back to the North American continent and into the academy.

EG: Again, it was largely matters of happenstance. My father died at fifty-five. I was an only child. My mother wasn't well. Neither of us had enough money to cross the ocean; so, we would never see one another again effectively; so, I had to move back. I sent out my résumé to fifty or sixty different places in North America.

CAC: This would have been the late 1950s?

EG: This is in 1957, I guess it was, that I was looking to move. I moved in 1958. Eventually, I got a job. I had to start over again. I'd had about seven years of experience post Ph.D. and published thirty or forty papers and so on; but, I had to start over again as a lecturer at the University of Toronto.

CAC: You did take on there?

EG: Yes, I got hired there to teach General Botany to non honors students, science students, which was a terrible chore . . . classes of 200. You had to supervise the lab all the time . . . a full year course with essay exams to grade oneself.

CAC: [laughter]

EG: After I'd taught it for the first year, some of the students got into my office and stole my notes; so, I had to do it all over again.

CAC: [laughter] What do you suppose ever you would do with stolen notes?

EG: He had stolen them just before the final exam.

CAC: I see . . . so, then to throw them away probably.

EG: Throw them away. I'd sent an open notice around and I spoke to the class and said, "For God's sake! leave them somewhere where they'll get back to me'" but, they never did.

CAC: Some of us, later in our career, would benefit from such a theft.

EG: [laughter] Yes. Yes, me, too, I think; but, in those days, it was a real trauma. Toronto was a very authoritarian university and a couple of years after I got there, the Botany Department lost its head. He went to U-Mass[achusetts] at Amherst. The deans, without any reference to the department, imposed a Dutchman out of South Africa on us as a head, who was extremely authoritarian and had no use for ecology; so, after a couple of years of this, I was just mad . . . keen to get out. As it happens, I had a colleague at Toronto, Roger Bray, who had spent a year, or maybe even two, as a fill-in lecturer while people were on leave at Minnesota in the Botany Department and he unbeknownst to me had told Tom Morley in the Botany Department that I was looking for a job, that I really wanted to leave Toronto. Again, it was a happy accident but even more so because I had a summer appointment at the University of Berkeley in the Department of Plants and Soils working with a very eminent soil scientist, Hans Yennie. One day when I was there, I got a call from Tom Morley who introduced himself as a friend of Roger Bray from the Botany Department at Minnesota. He was in California where his father lived for part of the summer and he would like to visit me. I was just <u>desperately</u> busy; so, I said, "If you'd like to come and talk to me while I'm fixing up some experiments in the greenhouse, I'd be glad to chat with you."

CAC: [laughter]

EG: He turned up and started asking me about what I did. I thought, this is sort of strange, a taxonomist interested in ecology. Roger really must have done a number on him to make him so interested. At the end of an hour or two chat, while I was busy working, he said, "You know, it's all very interesting. Would you send me some reprints when you get it back?" As soon as I got it back, I sent off some reprints and thought nothing more about it. Then, two or three months later, I got an invitation to give a seminar at Minnesota.

CAC: Ahhh.

EG: And they hired me. Again, it was largely a chapter of accidents.

CAC: It wouldn't surprise you that with the number of people I've talked to so far chance has played a major role in the shaping of everyone's career.

EG: Oh, yes, serendipity. Of course, Pasteur had it right, "Chance favors the prepared mind."

CAC: [laughter]

EG: You have to be ready. I have two aphorisms I like. One is that one. The other is ascribed to Linus Pauling; although, it maybe apocryphal. "In order to have good ideas, it's first necessary to have lots of ideas."

CAC: [laughter] It would fit him, would it not? He had lots of ideas, a couple of which were pretty good.

EG: Yes, that's right.

CAC: So, you show up at Minnesota in 1962?

EG: Yes.

CAC: Say something about the department that you came into. This is the Department of Botany?

EG: This was the Department of Botany.

CAC: In the College of Liberal Arts?

EG: In SLA, as it was.

CAC: Science, Literature, and the Arts, yes.

EG: I came in to teach General Biology. What they hadn't told me was that I was going to teach it on television. [laughter]

CAC: That's just barely experimental at that time?

EG: Just starting out. I was part of the first team to do it. That was a real shock. It was interesting to do it for two or three years; but, you shouldn't teach the general course on television without a warm body in front of the class. We gradually realized that over a long haul and it's now no longer taught that way.

CAC: It did give you the opportunity to focus the TV on experiments of some sort?

EG: We had an extremely good director who was willing to do almost anything you liked by pointing the microscope down to camera. The trouble was, we didn't have enough budget to do it well. It was interesting, as I say, to do for awhile to see what you could do with it. We started out with a small audience of about a dozen students; but, we couldn't keep the director

from having the cameras swung around onto them and they didn't like their peers seeing them on TV. They got kidded a lot about this.

CAC: I see. It went out to hundreds . . .

EG: Oh, thousands.

CAC: . . . but only a group of six or seven were there?

EG: [unclear], yes, we were processing, and I use the word advisedly, 2,000 or 3,000 students a year. These things would run day and night. We did it one year on public television. You could take the course lying in bed in the evening and watch it. As I say, it was interesting. It had its funny moments. The cameras were so old—I don't know where they got them—that when parts failed, which was not infrequent, they had to be handcrafted. I remember vividly one day in full flight in the middle of a lecture, I saw a little spiral of smoke coming out of one of the TV monitors.

CAC: [laughter]

EG: I sort of looked over and checked. I had looked at the monitor and it was still showing up all right; so, I trudged on through my lecture. Then, eventually, a great cloud of smoke leapt out of this thing. The monitor went dead and I heard the other cameraman lean over to his buddy and say, "My god! I've heard of faces that would stop a clock but this is ridiculous!" [laughter]

CAC: [laughter]

EG: But, I had my revenge on them. A few weeks later, I did a lecture on plant dispersal and I brought in a pod of milkweed seeds, and I huffed and I puffed and I blew them across the camera, and they were floating across the camera for weeks thereafter. [laughter]

CAC: [laughter]

EG: They would settle up in the beams and woodwork and anytime anybody opened a door, they would be dislodged and float around some more.

CAC: Did you have other assignments than this thing?

EG: That was all I did.

CAC: I see.

EG: But, that was a big assignment.

CAC: How large was the department that you came into?

EG: Oh, it must have been around a dozen or so, I suppose, something of that order . . . twelve to fifteen at the most.

CAC: Did it have intellectual relationships with other related departments?

EG: We had a fairly close relationship to Zoology. When I came to the Botany Department, it was sort of natural for everybody to join the Campus Club, which was just adjacent to the Botany Department.

CAC: Oh, you were right next door.

EG: The zoologists did likewise and we would . . .

CAC: Zoology was also in the Botany . . . ?

EG: No, it was in the building just across the road and, again, just next to Coffman Union; so, the botanists and the zoologists would, many, many, many of them, would lunch together. Often the botanists and zoologist would lunch at the same table; so we had fairly close connections with them . . . not so much with other departments.

CAC: Did you really talk shop when you were there?

EG: Oh, yes, sure. Yes.

CAC: You may not remember; but, I used to have lunch occasionally with Magnus Olson and you.

EG: Oh, yes, once in awhile.

CAC: Those are the only two I remember. I'm sure there were others.

EG: Ern Stille, I'm sure. It was part of the culture that you would lunch together and that you would join the Campus Club. That gradually died over the next two or three decades to the point where it became an old folks' home, so to speak.

CAC: It died because here we are on the West Bank. It's a long walk.

EG: Yes, and I think there are so many more small restaurants around where people can go for lunch as well. I think the university is just much less collegial than it used to be.

CAC: I want to come back to that because I find, talking with others, that's a persisting theme. I want to have your angle on it. Let's talk a bit more about the Botany, if it's okay?

EG: Sure.

CAC: How was the department governed when you came into it?

EG: It was extremely democratic and it was a wonder to me coming out of a very authoritarian background.

CAC: Yes.

EG: I had lived under a malevolent despot at Toronto who ran things to suit himself and I'd lived under a benevolent despot in London in Professor Pearsall where we had one staff meeting a year to parcel out the teaching loads and I sat on one committee in the four years I was there. But, Pearsall had everybody convinced that he was doing his best for everyone in the department and it was just wonderful. It was wonderful. I spent most of my time in research. I gave eight lectures a year and I spent two weeks in the summer with students out in the field. That was my assignment. Apart from that, I did research and I did what I damned well pleased. It was a shock to go to a malevolent despot in Toronto and to teach this huge class of 200 sort of all the time. By the time I'd finished grading one set of exams, it was time to start the next.

CAC: Sure.

EG: So, Minnesota was a breath of fresh air to me. The Botany Department was extremely democratic. It had a chairman who believed in being a chairman. We had weekly or, at most, fortnightly staff meetings. Everything the department was put up for vote which was recorded in the minutes and the minutes were taken by the junior faculty member, who in the beginning, turned out to be me. It was a wonderful way to learn who was who in the department and what was going on . . . it really was. The department head, the department chair, made sure that I knew what was going on. When I had questions about what this meant in the minutes, he would explain to me what the background was. It was he who took me over to the Campus Club. He would sometimes take me over and sit me down at other tables in the Campus Club with friends of his in other departments . . .

CAC: Bravo.

EG: . . . which doesn't happen nowadays. In those days, of course, Meredith Wilson, the president, was often in the Campus Club and might sit down almost anywhere and introduce himself.

CAC: You'd be surprised how many people have commented on Met Wilson.

EG: I'm sure that's true.

CAC: That's interesting. Did democracy extend to budget and to merit decisions?

EG: I'm not sure just how much. I think the chair consulted fairly widely about these things. There were discussions about how much money there was available; but, I can't remember in those days just how it was handled. I know how I handled it. I made out a list of the faculty members and asked people to fill in . . . Should these people be given the normal raise, something less than normal, something more than normal?

CAC: This is when you took your turn at chair?

EG: When I was chair, yes. What I did, actually, was to round off the numbers I got and whatever rounding left me, I would use myself and usually managed to pry a little money out of the dean for people I thought were particularly deserving. By and large, it was my experience that department was pretty bright about saying who deserved rewards and who did not.

CAC: Is there any way of accounting for that because this is not the normal mode of departmental governance at that time? I think it came to be in many more.

EG: I don't know. I think it had perhaps been a tradition. The person who would know is Ernst Abbe who is still alive and mentally fit at eighty-nine.

CAC: Oh, good. I should talk to him.

EG: You should talk to him. It was extremely democratic, sometimes to the point that we spent time talking about real trivia. One of our faculty members, Allen Brown, was well-known for shouting out the administrative detail when these things came along . . . leave it to the chair. We discussed the minutia of departmental affairs, sometimes at an inordinate length.

CAC: What was the reputation of the Botany Department at that time, nationally?

EG: I would think middling . . . between fifteen and twenty in the country probably. Ernst Abbe was the only one I'd heard of when I had come there, who did some quite distinguished things. Don Lawrence was well-known in the ecology field. He'd been editor of the journal for awhile. I don't think they had anybody really in the front rank. There were several people who might fit in the second tier. Orville Dahl was quite well-known as a [unclear]. They'd had two earlier persons, both of them ecologists, who'd been very distinguished: Frederick Clements was briefly at Minnesota and W.C. Cooper, William Cooper, was a very distinguished ecologist. Both of those people were very well-known as early founders of ecology. I don't know enough about the other members to know what their reputations were; but, I would say there were probably not many in the very top rank but in the second rank. They'd lost a lot of good people.

CAC: Many departments expanded very substantially just about the time you came, that is, from the mid 1960s to the mid 1970s. Was that true of Botany?

EG: That was true of Botany. Gradually, it and Zoology accumulated enough people to hive off a Department of Genetics, and a Department of Cell Biology, and a Department of Ecology.

CAC: Oh, so that once you got them in, then, they would split off and have their own department?

EG: Yes. Several of the people that are still in Genetics, and Cell Biology, and still in Ecology came out of either the Botany or Zoology Departments.

CAC: Would that have been the pattern elsewhere as well? We're talking more than administrative convenience.

EG: Yes.

CAC: We're talking about really disciplined perceptions.

EG: Yes. I don't know. I think gradually Ecology Departments came into prominence and gradually Genetics Departments were formed, probably in much the same way, hiving out of Botany and Zoology Departments or Biology Departments.

CAC: I see. So, you really have a creation of new departments as well as new faculty?

EG: Oh, yes, yes.

CAC: They could not have been happy or not effective working within a large umbrella of Botany?

EG: It's hard to say. I think that when the College of Biological Sciences [CBS] was founded in 1965, 1966, Dick Caldecott, who became dean—he'd been on the search committee and they settled on him as dean—had a vision, which was essentially that of the Irvine Campus in California, of hierarchical levels: Department of Molecular Biology, Cellular Biology, Organism Biology, Population Biology. He wanted those sorts of departments not the old generalist departments. Now, he couldn't have his way; but, he did get to found the Genetics Department and the Ecology Department. Essentially, Minnesota had a sort of crossed-up organization in the sense that you could either belong to the specialist departments of Biochemistry, Genetics and Cell Biology, or Ecology or you could belong to Botany or Zoology, the generalist departments. I chose to stay with the generalist department, not to move into the Department of Ecology when it was founded, as was true of . . .

CAC: But, later you do?

EG: I did move later, largely because I was coerced to do so.

CAC: Is that a story that comes right away? I would like to know more about the motivation, the dynamics of . . .

EG: The founding?

CAC: . . . the splitting away of Biological Sciences from the Science, Literature, and the Arts.

EG: My understanding of it, which I gained largely from Ernst Abbe and from departmental discussions . . .

CAC: Except that you were here.

EG: . . . when I came . . . yes, in the early years that I was here, was that there had been a long history of neglect of the sciences in SLA; and Physics had moved out and other sciences had moved up into IT [Institute of Technology]. Botany and Zoology were the only ones left in SLA. This was in the time when [Errett W.] McDiarmid was dean and we perceived him, rightly or wrongly, as not very sympathetic to the sciences, particularly the biological sciences anyway, which was all he had left. We wanted a new building. We couldn't get one. We wanted better supply budgets. We couldn't get that.

[telephone rings - break in the interview]

EG: So, Botany and Zoology essentially felt under supported in SLA, wanted a new building, wanted better budgets, more staff; and they agitated to hive off as a separate college. Eventually, they succeed in that.

CAC: With what kind of support from Central Administration?

EG: I don't really know the ins and outs—you'd get more information from Ernst Abbe or maybe from the history of the Botany Department—but, I think some support. It was somewhat ironic because just after this decision had been finalized, McDiarmid went out and [E.W.] "Easy" Ziebarth came in; and we had the impression again that he would have been much more sympathetic to us and might have averted the whole damned thing.

CAC: For one thing, the whole college was coming into more money by 1965.

EG: Yes.

CAC: These years, 1965 to 1972, 1973 are eight years of growth.

EG: Yes.

CAC: Your timing may just have been . . .

EG: Might have been fortuitous . . .

CAC: [laughter]

EG: . . . but certainly my recollection is that Ernst found "Easy" Ziebarth a lot of easier to deal with.

CAC: Oh, I'm sure, yes.

EG: But, by then, the decisions had been made. I left at that time. I knew there was an Ecology Department going to be formed. The thought was that Bill Marshall would be the head with whom I'd had some run-ins over graduate exams. He'd really twisted my arm about some of his graduate students that I didn't think much of and I didn't want to work for him. My wife was keen to return to Canada and I thought it would be nice to go back; so, I took a job at Calgary for a year.

CAC: I see.

EG: I took it as a permanent . . .

CAC: Just at this time?

EG: Just at this time. I was at Calgary from the fall of 1965 to the spring of 1966. Again, one of those ironies of fate . . . I had been corresponding with Herb Wright with whom I was working at the time . . .

CAC: Oh, yes.

EG: . . . and had a joint grant. I mentioned that I had gone to be head of the Department of Biology at Calgary and I mentioned that all those promises they'd made me, that I'd have time for research and so on, were not going to be fulfilled. We were in the midst of a huge expansion boom and I was going to be spending all my time in administration. It wasn't much fun; besides, it was a very factious department. I've never seen so many shifting factions of faculty. I didn't really like it there; although, the surroundings were great and the budgets were big. I'd written this to Herb who communicated it to one or two people. Of all things, Bill Marshall agitated to have me come back. [laughter] And he raised the issue of Caldecott. Marshall didn't turn out to get to be made head of Ecology, as it happened.

CAC: The college was to have incorporated all kinds of biological sciences . . . within the medical, within the Health Sciences as well?

EG: The Department of Microbiology was in the Medical School. It was to have a joint affiliation with the College of Biological Sciences.

CAC: But it's budget was still to be in the Health Sciences?

EG: It's budget was still in the Health Sciences.

CAC: Okay. Can you describe a bit more then how large an umbrella that was?

EG: We had Departments of Biochemistry, Genetics and Cell Biology, Ecology and Behavioral Biology, Botany, and Zoology, five departments. The staff was probably only about sixty or seventy, probably a tiny bit larger than the English Department.

CAC: But you met as departments and not as a college?

EG: Yes.

CAC: Really effective governance was still at the specialized department level?

EG: Oh, yes, yes. The college was to be a two-campus college. Botany and Zoology were to stay here and keep a presence in Minneapolis and the other departments would move to St. Paul and have a presence in St. Paul and link up with the Ag school.

CAC: That, in fact, took place?

EG: That took place.

CAC: Did you get a new building? <u>They</u> did, did you?

EG: Caldecott finally got a new building, the Bioscience Center. We did the major planning for it when I was department head in Botany. We stayed on this campus until the new building was built and then Botany and Zoology . . . What did happen? Botany moved over, I think, and Zoology stayed here—that's my recollection—because Botany moved into the new Bioscience Center, probably because it had more ties with Agriculture . . . I'm not sure why.

CAC: The rationales for these kinds of reorganizations are frequently administrative, this certain rationality or logic that seems to dictate. For posterity, whom we're talking to here, I'm interested in what the intellectual umbrella was. By having a College of Biological Sciences, did it give you a larger opportunity for you graduate students, for faculty to exchange what they're doing?

EG: I think it was more a matter opportunities for expansion. If you had new departments, they had to be staffed; so this was a way . . .

[End of Tape 1, Side 1]

[Tape 1, Side 2]

EG:. . . expanding, partly a means of tapping new sources of graduate students. For Ecology, this was a wonderful opportunity. I didn't move into the Ecology Department until I was pushed into it in about 1975, I think.

CAC: Why did you resist? So much of your work was really . . .

EG: I figured, I will always make the effort to go and talk to ecologists wherever they may be, on the other campus or whatever; but, I'd benefit from talking to botanists who were studying plants at all levels from the molecular up through populations, to taxonomy, and physiology, and all that . . . genetics. I thought I fitted better in the generalist department than the specialist department.

CAC: Reflecting back upon that, was that intellectually sound . . . wise?

EG: I've always been something of a generalist and I think it's good for you to be forced to talk to people in these diverse other levels.

CAC: And just by sharing department space, it forced you talk with each other?

EG: Yes, oh yes. In those days, of course, we were lunching in the Campus Club . . .

CAC: All right.

EG: . . . and meeting in the hallway of the Botany building. It had a big central hall and people would stop and chat. I think that was the rationale. For Ecology, it was really a godsend and for me it was a godsend, also later. I found, when I finally did make the change, that although I lost the benefits in mixing with these people interested in plants at all levels, the quality of the graduate students in the Ecology Department were just streets ahead of the Botany Department.

CAC: Now, we're talking about the mid 1970s forward?

EG: Yes.

CAC: How do you account for that? Was ecology a sexy way to go if you were in the life sciences?

EG: I think so. In the life sciences, there were two ways to go. These sexy things were molecular biology on the one hand and ecology on the other. A lot of bright young students want to be ecologists. They don't want to be a biologist. They want to be an ecologist; so, if

there's an ecology department around—there aren't too many of them even yet—they write there. It's the statement of our directors of Graduate Studies that we have the best graduate students in the university as judged by GRE [Graduate Record Examination] scores, the fellowships that go with them, and the Bush Fellowships they've been awarded, and so on. They're just superlatively good and most of them are native-born. We're not relying on Asians and others to fill our Graduate School. In Botany now, lots and lots of Chinese students . . . We have a few; but, we have a lot of the very best American students still. Engineering and IT has lots of foreign-born students. That's not a bad thing. I don't begrudge them; but, it's nice to know that there are people still in this country who want to become scientists.

CAC: These good students are good in scientific terms; but, they also—I'm guessing and you can correct me—have more of a political commitment or not?

EG: I think probably that most of them are interested in environmental affairs to a major degree. Some of them are more activist than others; but, a lot of them . . .

CAC: But, it would attract that kind of person.

EG: It attracts that kind.

CAC: [unclear] public policy?

EG: Perhaps, not so much in public policy but in figuring out what to do about the bio-diversity crisis or dealing with pollution problems, how to restore wetlands, things of that sort. We get a lot of students who want to do things like that.

CAC: Right.

EG: But, they're also interested in the subject. I always tell students, "I'm mistrustful of students that come to me and say they want do ecology because they want to help the environment." I tell them, "I think the best reason to come into the field is because you think it's the most fascinating puzzle you're yet come across." It's admirable to want to save the environment but if you don't have that feeling of problem solving or this is the most fascinating thing you can do with your life then . . .

CAC: I'm going to ask a kind of baiting question, perhaps. You understand I'm <u>way</u> out of line, way out of field. I have a sense that, let us say, from 1900 to 1950 is the day of physics and to a lesser extent of chemistry but theoretical physics, atomic physics, and so on; and that sometime in the 1960s and extending down to 1994, when we're talking, that it is in the various biological fields, as we say the cutting edge, the new ideas are the really important things. Is that a superficial perception?

EG: I think you could make it as a broad generalization. There are still fascinating things in every discipline . . .

CAC: Oh, of course . . . just think of astro physics.

EG: Yes, that's right. By and large, I think that the life sciences is where most of the action is nowadays, particularly in molecular biology and in the ecological field. I think the science of development though is coming up in that direction.

CAC: By science of development you mean?

EG: How molecular biology determines the development of organisms, and tissues, and organs.

CAC: This isn't too superficial then.

EG: No, no, I wouldn't think so.

CAC: You're restating the generalization. Then, for reasons both internal to the disciplines and more broadly social and cultural in that environment, can you account for this? I mean, why molecular biology in 1990 and atomic physics in 1940?

EG: I think physics probably topped out with the atomic bomb and all the underlying science that underpinned it. I think that, perhaps, physics wasn't quite so attractive after the two bombs were dropped, and all the fuss of the [Joseph] McCarthy era, and so on.

CAC: I see; so, it is in some part political?

EG: I don't know . . .

CAC: We're just speculating. That's all right.

EG: That's my speculation. I think molecular biology just had tremendous intellectual excitement when [James Dewey] Watson and [Francis] Crick discovered the double helix.

CAC: Yes, yes. But, there's a new technology that makes that possible?

EG: Crystallography, yes. Sure.

CAC: That work could not have been in 1930?

EG: No, no. It waited for the work of the Braggs [Sir William Henry and Sir William Lawrence Bragg] in X-ray crystallography and Maurice Wilkins and Rosalind Franklin [unclear] post doc, who provided the crystallographic data that clinched it for Watson and Crick. I must say, having

read both Watson's *Double Helix* and the biography of Rosalind Franklin, there was a lot of shoddy behavior that went on behind that, just shoddy. I'm glad that I've never been in such a competitive field . . .

CAC: [laughter]

EG: . . . that leads you to stoop to the sorts of back room [unclear].

CAC: The clusters of discoveries that came on with that revolutionized your field.

EG: Revolutionized biology, yes . . . not ecology but genetics and a lot of other biology and, of course, revolutionized medicine. Again, that's partly why it's so popular now.

CAC: Oh, my, my. Ah.

EG: It's the base of a new science called biotechnology. So, molecular biology, I think, is the leading attractor now; but, ecology, I think, would be the second. They're almost at polar opposites in biology.

CAC: Polars in [unclear] universe that's being dealt with?

EG: Yes, if you look at organismal biology, and physiology, and taxonomy, things of that sort, they're dying. Our Botany Department will soon be a department of Molecular Botany, once the present generation of older folks retire.

CAC: It won't surprise you, the life science I had at Carleton College before the second world war was almost entirely taxonomic.

EG: I was trained the same way.

CAC: And just deadly dull! I was not attracted to the field at all.

EG: You don't get interested in taxonomy unless you have an interest in wandering around in the woods and fields and you're forced to make own collection [unclear].

CAC: Or you have a very neat mind.

EG: That helps, yes. I think it just changed at the time of World War II. I was trained, essentially, in pre-war and World War II biology.

CAC: Say something else about the development of your own career. You said you were forced into the ecology college and then were happy that you made the move. Say a bit more about that.

EG: What happened was that the Ecology Department was very largely zoological. Ed Cushing, and Don Lawrence, and I had stayed in Botany. The dean really wanted to boost the Ecology Department. He didn't have much opinion on the Botany Department. He thought that we should move. Don was either just retired or about to retire; so, it was immaterial for him. The dean made a big push to get Ed Cushing and me to join Ecology because they didn't have any plant scientists. As I say, I resisted; but, his argument was, "Look, the Botany Department is now in the Bioscience Center. The ecologists are scattered through that and at Snyder Hall over on the St. Paul campus; so, you're all adjacent. You can see the ecologists any day of the week you want to. Why not join them? If you join them, the botanists are still just down the hall. You can keep your contacts with them. What's the point? Isn't it more logical for you as a plant ecologist to be in the Ecology Department?" I said, "I suppose you're right, as long as I keep in touch with the botanists." Of course, no sooner had I joined the Ecology Department . . . then Margaret Davis came in as head and she on the urging of some the members of faculty in the Ecology Department pushed and moved us back to the old Zoology building because the dean essentially abolished Zoology in a fit of pique.

CAC: [laughter]

EG: So, we moved back to Zoology and I was . . .

CAC: [unclear] what happened to all the people who were teaching Zoology?

EG: We had a Department of Ecology, Behavior Biology and Evolution, and Residual Zoology.

CAC: Is that really its official title?

EG: [laughter] No.

CAC: [laughter]

EG: It's Ecology, Evolution, and Behavior; but it should have added to it and Residual Zoology. Some of the zoologists moved into Genetics and Cell Biology. The rest of them moved into Ecology. The dean did this. It's rather interesting business. As generalist departments, we reckoned in Botany and Biology that we could take in graduate students at any level and [unclear]. The Zoology Department put out a flyer to this effect, which they circulated widely. The dean, when he saw this, went up in smoke. He said, "These damned zoologists are essentially calling themselves a College of Biology and they're preempting everything." He was furious. Essentially, he got mad and got rid of them. They resisted but he said, "Okay, you can resist; but, there will be no new appointments when people retire, or die, or leave. The positions will go elsewhere. You'll wither on the vine." So, they caved in. He held all the cards, which made him really rather unpopular with a lot of people.

CAC: You mentioned Margaret Davis joining you. I think perhaps it's unique in university history to have two Regents professors in the same small department, relatively small?

EG: Yes, I guess Physics had several when it was large. Economics . . . I don't know how large Economics is. They've had two or three.

CAC: I mean at the same time.

EG: Yes. But even so, Economics had two or three [unclear], and [Leonid] Hurwicz, and one other, whose names escapes me at the moment.

CAC: Say more then about your research as it developed and how it related to the instructional mission in your own life; and then we can probably see how Margaret Davis fits into that as well. She's a close parallel colleague?

EG: Yes. When I came here, I wanted to work on lakes and on wetlands, which I had developed an interest in.

CAC: It started with the bogs.

EG: Yes. I'd wandered around the bogs in Nova Scotia and part of my thesis work was on wetlands as well as on lakes and woodlands in the Lake District. When I got here, I went down and talked to the people in the Department of Natural Resources [DNR]. I wanted to do a comparative study of lakes across the gradient in Minnesota, which goes from relatively wet in the Arrowhead to very dry on the prairie border, where essentially, you have lakes that go all the way in chemistry from that of rainwater, not of sea water but in sodium sulfate rather than sodium chloride, if you get into the Dakotas. I wanted to do a study of this chemical gradient. I went down to the DNR and talked to John Loyal who had graduated from the Department of Zoology and was a first rate researcher sort of lost in the DNR largely because he stuttered terribly and couldn't take an academic job. He helped me out. I talked to people like Herb Wright and others and got some help from Don Lawrence. I gradually developed a research program on lakes here with some work on wetlands in collaboration with Herbie Wright. That developed and it turned out that, for me, Minnesota was just an extremely good place to be because the walls between departments are low and between colleges are low. You can do research here with any number of people in any number of places.

CAC: You're saying that in a comparative sense?

EG: Compared to other places I've been.

CAC: Or know about?

EG: Yes, that's right.

CAC: The lines are porous. Why is that so?

EG: I don't know. I think, perhaps, it has something to do with democracy. I really don't know what it is. I had never found it so easy to talk to people or to work across departmental and collegiate lines as when I got here. As I say, I really don't know what it is about the place; but, it's certainly clear to me. Actually, I made a little list of the colleges and departments I've worked in. If you look at them, I've worked with people in Civil and Mineral Engineering, and in Geology and IT, with people in Soils, and Vet Medicine, and Statistics, and Agriculture, the people in Botany, Genetics, and Cell Biology, and Ecology, and my own college of CBS, and with someone in Forest Resources and the Natural Resources College.

CAC: That's a pretty impressive list.

EG: Some of my students have done a lot of their work in other people's labs. It's very easy to move across lines here.

CAC: One of the questions that I've just got to press, not necessarily with you this afternoon, but almost everybody I've interviewed, wherever they come from, says the same thing, that there's a degree of elbow room, and an ease, and encouragement. When I wanted to be an adjunct professor of Social Work and really teach in Social Work, that was fine, even if they were a different college. They paid me different. That's really bizarre when one thinks of big bureaucratic institutions.

EG: I think the place has gotten more bureaucratic and I think that it does little to stimulate these interactions; but, if a faculty member has those wishes, it's very easy to do and it's very easy to find like-minded colleagues. I don't think it's supported or facilitated administratively. I think it's just a matter that the faculty somehow is more collegially in that sense and willing to . . .

CAC: It's something with cultural style.

EG: It may be. It may be.

CAC: Wouldn't it be fun to try to identify that?

EG: It would indeed.

CAC: If, indeed, it is largely university-wide.

EG: Yes, it even goes beyond the university, out to people in the Pollution Control Agency, and the DNR, and so on.

CAC: I see.

EG: I had great help from the DNR and some of our students have gone into the Pollution Control Agency.

CAC: Do you get funding from these places, too . . . your graduate students?

EG: We've had a little money from the DNR from time to time.

CAC: Where does your major funding come from other than internal to the university?

EG: Over the years, the largest share of it has come from NSF [National Science Foundation]; but, it has also come from NIH [National Institutes of Health], and DLE, and the AEC at one time . . .

CAC: AEC?

EG: Atomic Energy Commission, the predecessor to the Department of Energy . . . and from the Mellon Foundation. I've been able to get some money from Canada, from Environmental Canada, as well. I've had support from a wide variety of sources; but, the primary one would be the NSF. It's just an easy place for someone who wants to work with people around the spectrum. Partly what got me started was Herb Wright's limnology seminar, which he ran for many years in his living room in the evening, which attracted people from a wide diversity of departments. He was in Geology; but, a lot of the limnologists were in Botany and Zoology. At its height, it was attracting students from Geology, Botany, Zoology, Public Health, Soils, Civil and Mineral Engineering, all over the place. People from the DNR and the Pollution Control Agency would come. It was a very mixed group.

CAC: This was just a free-floating seminar? Non-credit?

EG: It was winter quarter. Students could take it for credit.

CAC: I see; but, the rest of you just showed up?

EG: Just showed up . . . a wide diversity of faculty and people from outside. It would have about twenty, twenty-five people there and very interactive. A lot of people developed interactions there. I developed a major interaction with Steve Eisenreich in Civil and Mineral Engineering simply because we would chat at these seminars of Herbie's and it really rejuvenated my research program on wetlands to work with those folks.

CAC: When Bill McDonald got in trouble with the classics problem of archaeology in Greece, he called on Herb Wright.

EG: That's right, yes. Herbie was a real catalyst in that sense. He ran the Limnological Research Center as a little vest pocket empire with no consultation; but those seminars were the glue that held the Aquatic Science community together.

CAC: Just marvelous. Ah, you called it Aquatic Science.

EG: Yes. We're now trying to put that together more formally with a council on water that has some administrative substance. Whether we'll get there, I don't know. Aquatic Sciences at the University of Minnesota . . . the whole has always been less than the sum of its parts. We have one of the best groups in Aquatic Science in the world and, yet, we've had no formal structures to focus it. What we have are seven, soon to be eight, small underfunded centers, institutes, you name it, which have had no formal links and which have never melded together to form a program. I think we have an Aquatic Sciences minor now in the Graduate School. We're going to try and get a major Ph.D. in the master's program. Despite that fact that we've never focused, Herb Wright's seminars glued it together. I think if you go around the country, you'll find that Minnesota has a very high reputation in Aquatic Science; but, I think it's not as high as it could be if we had a more formal program, if we were attracting students to take a Ph.D. in Limnology or Aquatic Science.

CAC: God! what an engaging story.

EG: That's really the story of science, I think. You have certain people who have the facility to develop a group. Herb Wright developed it in limnology. I can think of Gene Lichens at Cornell who developed the Hubbard Brook project, which is one of the major projects in ecology—it has a world reputation—and Dave Shindler who worked for the Canadian government in the Experimental Lakes Area Center just north of us in Kenora with the lab center in Winnipeg. These were powerhouse people who attracted really good people to work there. In one case, in a university, at Cornell, and in the other case, in a government department, in the Department of Fisheries and Oceans in Canada. These people somehow provide a focus and attract others to it. I don't know what it is about them.

CAC: Somehow there has also to be a green light if you're a young professor, a young scholar, that to do this kind of experimental moving around is okay.

EG: Yes, and I think the Botany Department was always open to that. I don't think they ever thought that I wasn't enough of a botanist. I had done a lot of work. I was doing as much botany as many of the people in the department, plus some ecology and biogeochemistry as well. I never heard a word to say that I should be more botanical. I don't think that's been the style of the departments that I have worked with. I've always been free to go my own way, as long as I could find the money to do it.

CAC: [laughter] And did it with distinction.

EG: In Ecology or in Botany, you could always fudge together a program for a student. There was never any pressure to . . . there were certain things you had to know because you would go out with a Botany Ph.D. and you had to be able to teach the general course. You could always fudge around a program to take odd ball courses into consideration for special people. It's never been a problem and in Ecology, it's even less of a problem. We take Physics majors and make ecologists out of them.

CAC: As I listen to you, a lot of the advantages and a lot of the affirming aspects of these developments were really informal . . .

EG: Oh, yes.

CAC: . . . and sprang, if not spontaneously, without formal program or planning?

EG: I think that's really true. I think it's just the fortuitous getting together of certain people who make these things happen. It's very difficult administratively to facilitate them. Probably the best thing you can do is find some good people and give them money.

CAC: The College of Biological Sciences was facilitating in that way?

EG: Yes, I think it improved the Biological Sciences here by allowing us to expand, and get more faculty, and also it allowed us to take in students who wanted more specialized degrees than Biology would give them.

CAC: Did it keep you open, however, to undergraduates in the Arts College, for example, who wanted to know a little something or explore down in some field in Botany or whatever?

EG: I think we made a big mistake in the College of Biological Sciences. I should say that I think our first dean, Dick Caldecott, made a big mistake. He really viewed the college as a pre-professional college, as a college to train professionals, [unclear] in Medicine, Dentistry, or in professional Biology. He really did a good thing in the sense that he forced us to develop a very rigorous curriculum; such that, people who graduated in Biology here did not have any remedial course work to do if they went for graduate study elsewhere. That was a good thing; but, he made it very restrictive. I thought we had a wonderful opportunity at Minnesota we never took advantage of, to have a B.A. in Biology and a B.S. in Biology. We do offer those two degrees; but, they're both very restrictive. I thought we ought to have restricted the B.S. from pre-professionals and had the B.A. as an avocational degree in the way that History or English would be.

CAC: Yes.

EG: We never did that and we froze out a lot of people who wouldn't face the calculus, and the physics, and the chemistry.

CAC: What does a really bright sophomore do if they want to sample the kinds of things you're talking about? Can they get into these courses and manage them intellectually?

EG: It's mostly a matter of having the prerequisites and it's difficult for them to get into those sorts of courses. They can take an Introductory Ecology course but not the rigorous one we give to our own CBS graduates. They can take a course in Plants Useful to Man but . . .

CAC: Have you given both?

EG: No, I've given Introductory Ecology once. Thank goodness, I've not had to do it more often.

CAC: What is the course Plants Useful to Man?

EG: It was developed by Don Lawrence and later followed up by Doug Pratt. Essentially, it takes what plants humans have used for what purposes, whether flax to make linen or food plants, look a little at their history, their biology, how they came to be used, what the patterns were of their spread around through the known world, things of that sort. It's a cultural course.

CAC: These courses are . . . ?

EG: Very widely subscribed. Yes, and I think the Introductory Ecology for non-majors is fairly heavily subscribed, too. There will probably be a course, if not now, in Conservation Biology.

CAC: But, then you're suggesting there's a big bump to get over if you want to really do this thing [unclear]?

EG: If you want to graduate in Biology, you have a pretty rigorous curriculum. I think we should have had a Biology degree that was somewhat less rigorous, that would attract people with a more avocational background or who wanted to be school teachers. We have a lot of phy-ed teachers teaching biology and we need more good biology teachers; but, a lot of them simply won't face the math, physics, and calculus, and chemistry that they need to get a degree in Biology here.

CAC: I bet a lot of them are still teaching taxonomy.

EG: Some of them may be. We could provide a good degree for non-majors that would enable them to go out and teach biology effectively in the schools, without any doubt. I'd be glad to teach in that area. I've taught lots of introductory courses. In fact, I'm teaching one now, which is really quite exciting. It's called Our Changing Planet, and again, it's interdisciplinary. Larry Rudnick in Physics, who is an astronomer, called me one day and asked if I was interested in getting together with him and a colleague, John Dickey in Astronomy, and a couple of geologists

to talk about a new course at the 1000 level with no prerequisites. Essentially, that course starts with the Big Bang and it ends up with a crises of biodiversity, and population, and ecology.

CAC: In ten weeks?

EG: In ten weeks, yes.

CAC: That's a real challenge.

EG: It is; but, it's great fun to teach. Larry Rudnick . . . and we also imported Fred Finley from the School of Education and Larry and Fred are experts in active learning techniques. We're having great fun.

CAC: How does that apply to the work you're doing in that course . . . active learning? How do you work it in?

EG: You interact with the class much more actively in lectures by asking questions. You give them three by five cards to hand in at the end with questions; so, you get some feedback about what's going on. You raise a problem and then say, "Talk to your neighbor about this for two minutes. Then I will call on somebody out here to say what they think." The labs are run the same way with all the active learning things that I am just learning about. I have the feeling that in the twilight of my career I'm finally learning how to teach.

CAC: Oh, you're in the twilight, are you?

EG: [laughter] I'm thinking about retirement.

CAC: It's not a bad thing. We've talked a bit about outreach, that is through the DNR and so forth. Do you have other things you wanted to say about that?

EG: I got involved in outreach at this university, again, in a rather interesting way. I had imbibed the British idea that somebody ought to pay me to do as I damned well pleased because I was doing interesting science. I had no thought that any of this stuff I was doing on acid rain, or radio active fallout at the time, or respiratory disease and air pollutants had any real relevance to the world. I knew it might have; but, I didn't think it was my job to be involved with all of that. I ought to be paid to do what I damned well pleased and follow my nose wherever it led. So, I had no interest in environmentalism or anything like that. I went to Toronto and had the same attitude. I came down here with that same attitude. In 1962, when I came, Rachel Carson published *Silent Spring*.

CAC: Indeed she did.

EG: Don Lawrence, who was in the Botany Department and who had been largely instrumental in bringing me here, held a seminar on Rachel Carson's book in his rec room . . .

CAC: Heavens.

EG: . . . in his basement in the evenings. He had been a Ph.D. student at Johns Hopkins while Rachel was there. He knew her. He didn't know her well; but, he knew her.

CAC: Who came to this basement seminar?

EG: Oh, people from the Ag school, from Forestry, from Botany and Zoology. I can't really remember.

CAC: Do you know what's happening right down the block from you? Professor's wives are gathering in basements to read the *Feminine Mystique* at the same time.

EG: Is that right?

CAC: Both equally revolutionary. [laughter]

EG: Yes. We would take a chapter at a time at these seminars . . .

CAC: Yes, same deal.

EG: . . . and talk about it. It suddenly became borne in on me that I'd been studying problems of practical significance all this time just because I thought they were interesting problems not because I thought they were practical. In Britain, to do practical science was to do second-rate science. Nobody would be caught dead doing applied science if they could do anything else. I was very much of that mind set; but, I'd been working on these practical things. Suddenly, I thought, Rachel Carson is out there telling people the real science behind all of this. Maybe I should be out there talking about the things that I know about. This sort of seed got planted in my mind; but, it didn't grow until Doug Pratt came to me one day and said he'd been asked to go to the park board and talk to them about using DDT as a spray for dutch elm disease. He was going to argue against it and he had acquired some literature; but, he wasn't feeling well or something had come up that he couldn't do it and would I do it? He gave me this bunch of literature. I went along to the park board and I talked about this. Then, somebody called me up who'd been at the park board and said would I go on and talk to the House Committee on Agriculture down in St. Paul. I said, "I suppose I could." I knew really very little about it. I'd just gotten it up out of the literature that Doug gave me. So, I went and got some more stuff from some people in the museum who had some information on DDT in birds and I went down to the State House. It was a very interesting experience and it was an interesting experience for the agriculture committee because they found there were two representatives of the university

there on polar opposites. [Lawrence] Larry Cutkomp was there from the Entomology Department saying that DDT was great stuff.

CAC: I see. All right.

EG: There was no problem at all with this and here was this fellow from the Botany Department saying, "This is poison!" They couldn't make head or tail of this . . .

CAC: [laughter]

EG: . . . because they'd been used to people from the Ag school coming with the full imprimatur of the university.

CAC: Oh, of course.

EG: This was the university speaking with one voice.

CAC: Right.

EG: Here it was speaking with forked tongue. That sort of thing happened often enough in the next few years—this was in the mid 1960s—that the university developed a policy where you're supposed not to speak for the university and you're supposed to make a disclaimer. A lot of people don't do it and I sometimes forget; but, I almost always say now, "I'm not speaking for the university. I'm taking off my scientist hat and I'm going to speak to you with my citizen's hat about what I think ought to be done about these things." That's university policy and it grew out of these sorts of things happening down at the State House. More often people were coming in with different views from the university, which was very strange for those legislators.

CAC: It would be strange for any citizen's group.

EG: Yes. Then, people started calling me up and asking me if I'd go and talk about this or that and then acid rain came along. It became a well-known problem and, here, I knew a lot about that; so, I went and talked about that. More recently, I've been talking about global warming and I have a talk on the scientist's view of the problems of the Twenty-first Century, looking at what I think ecology and biology can tell us about where we're going. I've talked to hundreds of groups.

CAC: Heavens.

EG: You name it, I've been there.

CAC: Do you work through any particular group or is this just kind of free-lance?

EG: The university had a speaker's bureau for awhile, which didn't get me very many engagements. I don't know why. I had a lot of them through the Minnesota Academy of Science, which had a very active speaker's bureau.

CAC: I see.

EG: But, it was mostly word of mouth.

CAC: Nature Conservancy?

EG: I've talked to them, yes. Again, somebody goes to this talk . . . meeting a friend . . . they learn that some group wants somebody on an environmental topic and they say, "Gee, I heard Ev Gorham talk about that a year or two ago. Why don't you call him?" I'd get calls from all over the place. The Minnesota Academy was particularly active in the schools. I talked to a lot of high schools. I haven't done that in recent years so much. I've talked to 3M's technical council, corporate lawyer's groups, and legion posts, and National Guard groups, and God knows what . . . church groups, children's groups.

The most exciting teaching I've ever done, Clarke, came in Richfield. This was in the 1970s maybe, after I'd come back from Calgary. Somebody called me and asked if I'd go out and talk. I'd been giving a talk on Biology and the Future of Man, which I later developed into a course with Doug Pratt. Somebody called and asked if I could go and give this to a group of grade six kids in Richfield. These would be the high achiever streams. They'd bus them in to some central school and I would talk to them. I went out there and I started my—I lowered it a little but not very much—little talk I'd been giving. I thought, I don't know whether I can get it across; but, I'll do my best to these grade six kids. I started talking. Within about fifteen minutes, it seemed to me that every hand in the class was up; so, I started answering questions. After about ten minutes of this, I said, "You know kids,"—all the hands were still up—"I've got some other things I'd like to talk to you about. Why don't you let me talk for a little while. Then, you can raise your hands again." Fifteen minutes later, all these hands were up. It went on like that for an hour.

CAC: [laughter]

EG: A few of the students were able, with one or two of the teachers, to stay around and chat to me a little bit after that. By then, it was lunch time. I went home; I was so rung out, I couldn't do anything else. I was so high on this, I just said, "To hell with it. I'm going home."

CAC: Did you ever go back?

EG: No, no. It went over really well. Actually, this must have been back about 1963 . . .

CAC: [gasp] That early?

EG: . . . because I went out later, I think, to Calgary in 1965 and somebody in the School of Education heard me give this talk to a Biology group in the university and said, "Could you give this to a group of our grade ten kids? We'll bus them in, these high achiever streams from all around, and you can talk to them." I gave the same sort of talk. I stood around for about an hour or an hour and a half answering questions with this group of students.

CAC: You bet. That younger generation.

EG: These were the two most exciting teaching experiences of my life. The second one had an unfortunate sequel. The person who had organized it said, "Oh, this was just great stuff. You must come and give a talk at the annual meeting of our high school Science Teachers' Association. That was the deadest group I've ever talked to. If it wasn't in the syllabus, they didn't want to hear about it. I thought, these people are teaching those bright kids? God!

CAC: My seventh grade grandson in the state of Virginia was able to participate in marine biology through Johns Hopkins in a three-week seminar in Chesapeake Bay. This sort of thing is going on and those kids are ready to move faster than we've ever realized.

EG: Oh, yes, that's true.

CAC: I wonder what will happen if that persists, and they move up the line, and they have that kind of challenge all the way.

EG: It would be great if it happened; but, it's certainly deadening not to have it. I went through school most of my high school . . . Most of my school career was just a waste of time. When I got into around grade eight and nine, I essentially would finish my work and be sent out to grade the ice rink while they taught the rest of the class.

CAC: We have talked about the Campus Club to switch into something quite different. I know and you remind me that you were also active in AAUP [American Association of University Professors], as I was. Did that open up contacts with other . . . ?

EG: I met a lot of people around the university outside the sciences through AAUP and contacts I've maintained to this day. Unfortunately, the AAUP is moribund on the campus. I don't know quite how it happened; but, it seemed to happen sort of in the same way that the Campus Club went downhill, that there was less contact among the faculty, less tradition of joining up when you were young when you came here in both AAUP and the Campus Club. I see a real parallel there. The main thing I remember about AAUP was that we, when I was active in it, finally managed to get the university to open up the budget, to make it available. When I was president of AAUP, the local chapter, the only way I could see the budget was through Sam Popper, whose dean would let him have a look at it surreptitiously and Sam would let me have a look at it surreptitiously. It became borne in on me then that a faculty member had both hands tied behind his back when he went to bargain with an administrator for an outside offer . . .

CAC: Yes! or for any other reason.

EG: Or for any other reason. You just didn't know how well you were being treated vis-à-vis your peers, whether you were being exceptionally well treated or exceptionally poorly treated in terms of salary. That seemed to me to be extremely inequitable and I agitated long and hard for this and so did many others in AAUP. I think it was our pressure that finally helped to tip the balance and get it opened up.

CAC: Approximately what date was that?

EG: This must have been the late 1960s or the early 1970s.

CAC: It must have been the early 1970s. In 1971, I became chairman and I didn't have access even to the college budget. I hired a research assistant, and she went over to the state archives, and used it there and xeroxed the things that I wanted. Now, that's just outrageous!

EG: Yes.

CAC: That's as late as 1971.

EG: I think this was the period of agitation. I think it may have started either just before my presidency or during that time and carried over some. Eventually, they opened it up. I remember we had a great fuss with Don Smith about not replying to our letters about some matters or other. I remember Gerry Shepherd coming in and really blasting me for getting crossed on Smith . . .

CAC: [laughter]

EG: . . . as did my dean. [laughter] Don Smith was a nice guy . . .

CAC: Oh, he was.

EG: . . . but, he just wouldn't answer our letter.

CAC: [laughter]

EG: I think what we did in the end was write something for the *Daily* complaining about the lack of information from Central Administration. That was really what triggered Gerry and Dick Caldecott to get after me.

CAC: Eville, I hear lots of things but two things that seem to be in opposition. One is that informally, in all the ways you've described—they really are engaging and exciting—you've been able, as many others, to move sideways, and into other departments, and really do cross-disciplinary work, not in any formal sense but very informal and very effectively. On the other

hand, I hear you also saying that over the last twenty years, let us say, or twenty-five years—your career and mine—that there is increasingly a specialization in all disciplines that has had a fragmenting impact.

EG: I think that's true.

CAC: How do you accommodate these two if they're both true?

EG: I think those people with personalities who favor interdisciplinary work can always find ways of making the contacts; but, I think all the disciplinary pressures are narrowing.

CAC: Say more about that. Would it be true in your fields?

EG: It's becoming, perhaps, less true in Ecology now because, as I say, you have to be a Jack-of-all-trades.

CAC: Sure, sure.

EG: When you're faced with practical problems of acid rain, or global warming, or toxification by chlorinated organic hydrocarbons like DDT, you have to know a little bit about a lot of things. You've got to know about air mass transport. You've got to know about what happens in soils. You've got to know about what the biological effects are, physiological effects, right up to the community effects; so, you have to know about these ancillary disciplines and you have to train your students in that way. In most of the things that I do, I rely very heavily on my students to learn the things that I don't know.

CAC: Good.

EG: Some of my colleagues have the view that they ought to be able to do everything their graduate students do so that the students can turn to them for advice. I don't feel that at all. I let my students take on problems that are well outside my area of expertise. I know where they can go for help and I send them there. I can tell them what the pitfalls are along the way in dealing with the thesis; but, I don't have the feeling that I have to know all these things. I don't have the training to do it and I don't have the math and physics to keep up with the computer revolution, for example. I rely on my students, and my post docs, and so on to teach me just as much as I teach them—perhaps, more.

CAC: There is that other side of, perhaps, not in your biological sciences but I've heard it so often, the intense . . .

[End of Tape 1, Side 2]

[Tape 2, Side 1]

CAC: We're talking about careers . . . increasingly, the rewards, the merits, and the distribution of rewards, tend to go—I've heard from many people—to those who are doing intensely specialized work, which, in turn, cuts them apart. Specialization has been fragmenting and there has been a corresponding loss of collegiality.

EG: I think that's true and I think the departmental structure favors that because some departments are not so happy about having their faculty move outside the department. For instance, I can think of a case where somebody was telling me just recently about a new hire. One of the faculty members said, "I don't mind what he does as long as he does it with corn." [laughter]

CAC: [laughter]

EG: That sort of attitude still persists, I think, in the more specialized departments. It is more difficult to do interdisciplinary studies than other more specialized studies. It's certainly much more easy to get them funded and that's an important area of the sciences. Without money, you're dead in the water. Hustling money for interdisciplinary projects is difficult.

CAC: I see.

EG: Again, with things like acid rain, and global warming, and those sorts of things that require interdisciplinary work, you have a better chance of getting funded to do it, I think, particularly if you're willing to work with teams. We've had some team projects that have been well funded, bringing in specialists from several different departments. We had a big NSF grant for awhile in which I was one of thirteen people at five different institutions looking at sphagnum bogs from here to the Atlantic provinces of Canada. We had all sorts of different people looking at them. We had taxonomists talking to geochemists, to people interested in productivity, or in [unclear], or goodness knows what. It's very exciting; but, it's not easy to get money for that sort of thing. There are many areas where you can fall between two stools. It's getting a little easier in ecology, I think, to cross these lines and get funding for it.

CAC: Particularly because there's public pressure?

EG: That's right.

CAC: These are issues that are so pressing, we just have to address them.

EG: Yes, that's right. I've been in a lucky position in that sense—although, I have had projects founder because they fell between the gambits of two different groups in NSF. I wanted to look at the influence of dust fall on rain chemistry with a view to inputs to ecosystems that might be of importance. I sent it to the Ecosystem Studies Program and they sent it to the Atmospheric

Sciences Program who said, essentially, "Look, the field of atmospheric chemistry has passed Gorham by long ago. We don't see much merit in this." They sent it back to the Ecosystem people and they still sent it to Atmospheric Sciences with one ecologist to review it; and he gave it an *A+* review and the others said, essentially, "No, this isn't interesting." I finally did get some money from the Energy, Research, and Development Administration to do it because it fit with their particular mandate. There was no way I could have got that funded through NSF because, essentially, it fell between two stools.

CAC: You want to speak of something . . .

EG: The thing that I want to speak of is what worries me about the future of the university. There is a drying up of resources. There is less willingness to fund graduate students. There aren't going to be jobs for graduate students on the scale that there have been. In ecology, it may be a little better than some because these environmental problems are not going away and the agencies like the Department of Natural Resources and Pollution Control Agency will still need staffing. But, I think the days of each faculty member in the research university training twelve to twenty-five graduate students is over. We are going to be forced to do our research with fewer students, less financial support as the competition gets ever more severe, and I think we're going to be asked to teach more. That, together with all the committee work and the paper work that is in the modern university, I think makes it a much less attractive occupation than it was when I got into it. Now, this may just be the effects of age. I had no committees to serve on when I was a young faculty member. I did mostly research. I had total freedom and there were no paperwork requirements to speak of at all; so, I could do science and not have any of these other things intrude on what I was doing. I could spend, essentially, full time at it. Also, in my day, of course, again in a rather old-fashioned way, I had a wife to look after me so that I could devote myself entirely to science.

CAC: Is your wife a professional person?

EG: No, she was a homemaker. She worked as a research assistant when I was a graduate student; but, after that, she looked after the home and I did science. Even with the best will in the world, nowadays with sharing of family responsibilities and other responsibilities . . .

CAC: Right, right.

EG: . . . nobody can spend the sort of time that I spent on science. I think it's going to be more difficult for males and especially for females because they won't shuck the family responsibilities to the degree that the men will. I think even for the best of the men, they're not going to have this sort of opportunity that I had. I had much more time and freedom from paperwork and hustling money. I write at least as many grant proposals as papers now, or have done in the last few years, and many of them don't succeed; and they have to be rewritten and resubmitted and they still may not succeed. Academic life just doesn't have the charm for me that it had when I was younger.

CAC: Has the university been effective in facilitating for awarding these kinds of research grant requests?

EG: I think it's very much a matter of independent action. There's very little that the university does to help.

CAC: There are all kinds of offices around here.

EG: I've had very little help from any of them. The main thing that I've had help with is secretarial help. That's been the main difference for me. I've had all the secretarial help I want; especially now when most people are typing their own letters on their computers and I still rely on the department secretarial staff. I think it's becoming a less attractive discipline. I don't think we'll have access to as many or as good graduate students in the future. A lot of the best students are going to turn away from science, I'm sure. They're doing it already in IT.

CAC: Where are they going?

EG: Business, law, medicine, things that will pay them well and where they'll have help to deal with all the paperwork and the other things that go on.

CAC: Do you find increased paperwork other than in your research grant activity?

EG: Oh, yes. I'm sitting on ten or eleven committees now in the university and outside. There are more forms to fill out. I get questionnaires all the time with people saying how urgent it is to fill them out. I toss most of them in the bucket because I simply don't have the time to deal with them. I'd rather spend my time answering prospective graduate student letters . . .

CAC: Sure.

EG: . . . or collegial letters about science than filling in the latest questionnaire from Central Administration or some unit of the university that has a particular ax to grind. I do my best; but, I just can't deal with it all.

CAC: You had one term as chair; otherwise, you've not had administrative positions as such?

EG: No. I was the chair of Biology at Calgary for a year and then here for five years. I decided that I would controvert the Peter Principle and retreat from my level of incompetence. One term as chair was enough for me. I don't like dealing with people problems and that was essentially what I dealt with. My feeling at the end of it was that these people were transferring all the knots in their heads into my stomach and I would lie awake and pop Rolaids.

CAC: [laughter]

EG: I didn't like it. I didn't see that I made much of a difference as department head. I filled out the forms and I did what I could; but, I didn't feel that I advanced the department very much. Those were the days when there was lots of money about and we were hiring new faculty; but, we weren't getting the best faculty. They were going to one of the coasts. We would simply not attract the best faculty whatever we had to offer them. I think we're still living with some of that as that faculty is aging. I didn't really feel that I made that much of a difference. I also felt that I was, perhaps, too much of a democrat. As I say, I paid a lot of attention to faculty opinion on merit raises. I also had a practice, which I don't think I've heard duplicated yet. At the end of each year, I would have the faculty drop in a box with a slot in it a slip marked "Yes" or "No," which I would take as a mark of whether I should continue as department head. I was fairly successful in getting "Yes" votes. I did it because I'd seen what happened when department heads grew unpopular without knowing it.

CAC: You bet, without knowing it.

EG: Without knowing it. I thought, I want some indicator that I'm getting on the wrong side of the faculty and I thought this was the best way to do it, where it would be totally anonymous and the secretary would then hand me the box.

CAC: I hear what you're saying in the last three or four minutes; but, I also have heard—I've watched your body posture and signals—that it's been a pretty rewarding career and a pretty rewarding life?

EG: I've been paid very largely to do what I like.

CAC: [laughter] At a place that has allowed you and encouraged you to do that?

EG: Yes. Minnesota has another advantage for me and that is that it has this tremendous gradient of habitat within easy reach, which is just ideal for studying the ecology. In California, they have more; but, they're more difficult to get at. This has been a great place because the physical setting has favored my research, as well as the interdisciplinary mind set of the faculty. I wouldn't want to do anything differently.

CAC: And it's a good place to raise family.

EG: Yes, that's right. Part of the pleasure has been teaching at Itasca. I taught for several years up at our Itasca Field Station, which is set in the pine woods and where you get a faculty cabin. My kids have very fond memories of spending time there.

CAC: Are any of your children going into the life sciences?

EG: No, my eldest daughter started out in biology at Harvard and got fed up with it. She was in with a bunch of pre-meds and she said it was just so competitive she couldn't stand it . . .

people hiding books, taking them out of the library and hiding them and nobody would collaborate; so, she then got into the history of science, and finished a degree there, and then did a master's degree at Brandeis in environmental history with the historian Don Wooster, who was at Brandeis and is now at Kansas.

CAC: Oh, good. Right.

EG: He's a very interesting guy.

CAC: Yes, he is. He was a contemporary of mine at Berkeley as a graduate.

EG: Oh, was he?

CAC: I didn't know him well; but, he got his degree there.

EG: I've just had tea with him once at his house because of my daughter Kirsten. I've read his *Nature's Economy*[*: a history of ecological ideas*]. I used to see the things that Kirsten was doing for him as his research assistant. He's just a very interesting person.

I have this feeling that the academic life is not what it used to be and I realize that it may be just an old man's view of the past; but, I don't think it is. I think the difficulties of keeping up with all the committee work, the difficulties of hustling money all the time are eroding the satisfactions. I think some of our graduate students are seeing that. The way I see it manifested is that several of my recent graduate students have decided to go to small liberal arts colleges rather than to the big research university. They don't want the hassle of *publish or perish* or hustling money all the time or all the paperwork that's involved. They want a more interesting life. I think a good small liberal arts college is probably where the happiest life is in academia because you're mixing with a broader mix of faculty.

CAC: Maybe you should retire . . .

EG: We would talk to one another if we lived in a small liberal arts college where we don't talk to one another here.

CAC: Right. This has been one of the great advantages to me having this project. I've learned all kinds of things about different disciplines.

EG: Yes, I'm sure.

CAC: I've just got a floating seminar all for me. It's pretty nice.

EG: How do you feel about this? Do you think that it's just as attractive, the academic life, as it used to be?

CAC: As I neared retirement, months and maybe a year before retirement, I realized that I was beginning to behave as all of my children behaved their senior year in high school, that they had to leave home and they had to find some reason that they didn't like their home.

EG: [laughter] Yes.

CAC: That their parents weren't teaching them right or something. It wasn't quite that severe; but, I was beginning to get not hostile but very skeptical about many things. That was four years ago. I guess I feel better now; but, I think what you're portraying is one that I've heard from twenty-five or thirty people in one language or another and with one intensity and another—and a lot of these people are younger; so they aren't yet facing it as you are and I did. I don't think it is a nostalgic reaction. I think this institution has, indeed, changed. It may be that you and I were lucky in having our careers roughly from 1945 to 1990 . . .

EG: Yes, I think so.

CAC: . . . in the academy . . . That was a golden. . . not a golden era . . . there never is such a thing.

EG: There's another aspect to it that I think is important, Clarke, and that is that nowadays in the sciences, knowledge has just exploded so much that nobody can keep up with it.

CAC: Yes! Yes, yes.

EG: When I was an undergraduate at Dalhousie, I had the feeling that by scanning the journals that came into the library in biology, I could keep up with what was going on in biology. Now, I was wrong. I couldn't really; but, I could have that illusion. There were two journals in anatomy. I scanned them both for titles and I'd read an occasional article. I think there were two journals in physiology. There were two or three in ecology. I would scan those titles, read an occasional article and think, I have a feeling for what's going in biology. Nowadays, my graduate students can't even say they have a feeling for what's going on in ecosystem ecology. There are so many journals, so many papers written. They feel snowed under. The result is that when I beat on them that they ought to know something about the history of their discipline, they haven't got time to read it. I've had a sort of avocation of doing history of ecology, history of biogeochemistry. I've written several articles on it. It's been fun for me; but, I've had the time to read those things and my students just don't. They just don't have time.

CAC: I think you're just absolutely right. That does come to be a terrible psychological burden . . . particularly . . . you remember that fellow you spoke of earlier that you had to work real hard. You're going to work ten times harder than he and you can't do it.

EG: That's right, yes. I think it's really important not to just get a snapshot of a field but to get a moving picture of how it developed. The history of science is something I think all our students ought to have in their armamentarium because, damn it! students repeat these things.

CAC: Yes.

EG: I had an example of this with one of my graduate students, Jessie Ford, who was working on the gradual acidification of ecosystems over time, as the bases were leached out of the soil by organic acids produced by plant decay and so on. She was working on this subject from the basis that had been developed, she thought, by a couple of Scandinavians, Iverson and Anderson who had done some pollen analysis to show the change in plant communities successively upwardly in lake sediments. This had shown that the uplands had gradually got leached of these bases, and the lime loving flora had gotten restricted, and the acid loving flora had expanded. She thought this whole idea, which she was exploiting, came from 1968. When she came to write the introduction to her thesis, I suddenly thought, damn it! this is the sort of thing I put in my Ph.D. thesis and it wasn't original with me. I got it from Pearsall; so, I went back to my old Ph.D. thesis and I read it again—I'd forgotten most of what I'd put in it—and I looked at the reference list and there, in 1922, a fellow named Edward Salisbury, who later became Sir Edward and the director of Que Botanic Garden, had written about this exact same thing. He'd been studying the flora of the woodlands in Hartfordshire and some other places where he'd found that the uplands had an acid loving flora and the bases that were leached out of the uplands down along the stream lines and out along the shore and had a base loving flora down there. He then went to some peat deposits that had been investigated by somebody and found the same sort of sequence upward. This whole idea had come at least as far back as 1922.

CAC: But, had been entirely lost?

EG: Well, Jessie had never heard of him, never heard of him. Iverson and Anderson made their reputations on this idea in paleoecology. Of course, I had the same experience. Angus Smith who had done this early work on acid rain was totally forgotten. It took me twenty years to find him.

CAC: Right.

EG: I'm very concerned about the lack of historical appreciation.

CAC: It comes to be a larger cultural problem. How on earth does our society, facing the overwhelming problems that it has, get on top of information and, then, turn it into any kind of salvation policy?

EG: Yes.

CAC: It's terrible. Do you have any further reflections on our common enterprise?

EG: No. Let me just turn the light on and see whether there's anything on my list.

CAC: Oh, please. We're sitting in the dark, in part, because rain clouds have settled in very heavily as we talked

EG: Yes, that's right. [pause] No, I think that's it. I've covered just about everything I had on the list.

CAC: My friend, that was a very engaging conversation. I thank you.

[End of Tape 2, Side 1]

[End of the Interview]

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