The Oral History of Gene E. Likens

October 24th, 2019

Interview conducted by Mark Madison

Storrs, Connecticut

Oral History Cover Sheet

Name: Gene E. Likens

Date of Interview: October 24, 2019

Location of Interview: Storrs, Connecticut

Interviewer: Mark Madison

Approximate Years Working in Science Field: 60.

Positions Held: Faculty at Dartmouth College, Hanover, New Hampshire, (1961-1969). Faculty at Cornell University, Ithaca, New York, (1969-1983).

Most Important Projects: Spent early time in his career in Antarctica where he took radioisotopes and conducted research on water movement; worked with three colleagues on Hubbard Brook Ecosystems Study discovering acid rain in 1963.

Mentors: G. Evelyn Hutchinson, (Vladmir) Verdnadsky, Eugene Odum, Howard Odum, Margaret Davis and Tom Winter.

Brief Summary of Interview: Gene Likens is an American ecologist and limnologist. He begins describing his early background years where he grew up in the Midwest on a farm in Indiana in humble beginnings. He was connected to the outside world through lakes, forests, and places he liked to spend time in. As an undergraduate, he went to a small liberal arts school called Manchester College in Indiana. He initially contemplated a career in sports but heeded the call to science as the result of an insistent professor. Gene went to the University of Wisconsin-Madison where he completed both his master's and Ph.D. degrees. In 1963, he was hired as an instructor at Dartmouth College, and he later moved to continue teaching at Cornell University in 1969. While at Dartmouth, he traveled to and from Hubbard Brook in the White Mountains of New Hampshire. It is here that he, along with three others conducted a long-term ecological research study looking at forests as ecosystems and analyzing the chemistry of stream water. This became known as the Hubbard Brook Ecosystem Study with the co-discovery of acid rain. Gene's early work has influenced governmental policy and included giving testimony to Congress along with other colleagues describing what acid rain was and the actions that they thought should be done about it. This briefing was instrumental in the signing into law of the Clean Air Act Amendments in 1990.

Transcript:

Mark: Today is October 24, 2019. We are in Storrs; Connecticut on the UConn (University of Connecticut) campus and Mark Madison is doing the interview of Gene Likens. Even though we had you sign a piece of paper Gene, we usually get it on tape also. Are you okay if we transcribe this story?

Gene: Sure.

Mark: Alright, great. Well, Gene an obvious place to start is what was your early background?

Gene: Well, I grew up on a small farm in the Midwest, Indiana. As a kid, I spent a lot of my time outdoors, barefoot in the summertime. I was a depression baby, so we didn't have any money. I actually lived in a log cabin without electricity or running water for about six years. So, I grew up very much connected to lakes and forests and things that I just loved to spend time in, and I did. I graduated from high school in 1953, Sidney High School. I went onto a small liberal arts school from there, Manchester College, which is in North Manchester, Indiana and then I graduated from there in 1957. Let me back up a moment. From my earliest recollections, all I ever really wanted to do was play professional baseball. So, I was wanting to do that, I didn't even have a bat or a ball because we didn't have the money for one, so I would use small stones and a stick. I figured if I could hit the small stones with my stick, I could probably hit a baseball. So, when I was ready to go to college, I was offered a scholarship at Michigan State University in athletics. I talked to a friend of mine, a neighbor friend and we drove to East Lansing and got the full tour. Very impressive. I'll never forget being in the huge facility where they had sports, a name for it, I don't know what it was called but they had a giant painting of Robin Roberts, who is a Hall of Fame pitcher for the Philadelphia Phillies, and I think that's who he played for. I just stood there and looked in awe and on the way home, I told my friend, I said, well, that was really impressive and wonderful, but I would be lost there. It's just too big and too complicated. So, I made my decision to go to Manchester College instead. It was probably one of the better things I ever did in my life because I had professors that cared and helped me go on to the career that I eventually had.

Mark: Yeah, how did you get interested in science? You were, you were a jock!

Gene: Well, my major was biology, but like so many kids, boys at that time in Indiana. We were trying to become coaches. Basketball coaches with a major in biology. So many of them would teach biology or some subject like that, and then they would coach basketball, and I was on that track, and I got my opportunity to play professional baseball. I played two years on something called the rookie leagues in Kansas, The Ban Johnson League, and I did well. I made the all-star team one year, but I decided that that really wasn't what I wanted to do. The funnel, as I call it

was huge. Many, many, many people that wanted to make the very few spots at the big leagues, the show as it's sometimes called. I was confident that I was good, but I didn't know if I could possibly survive that funnel and make the big time, and if you don't make the big time then you don't do very well. It's a real struggle. I decided that I would give in to what this professor at Manchester College, Emerson Niswander had been pushing me really hard to do; and that was to go on to graduate school. So, I had to go take the graduate record exam. I had to drive to South Bend, Indiana at Notre Dame to take the graduate record exam. It was a horrible experience, absolutely horrible. I obviously did alright, but it was not a good experience. I applied to three universities to go to graduate school, University of Wisconsin, Madison, Indiana University in Bloomington, and Cornell University in Ithaca, New York. I was accepted at Wisconsin and Indiana, but I was turned down at Cornell and I always took a lot of pleasure years later when I was chairman of the department at Cornell with an endowed professorship, reminding them that they had turned me down when I had applied. And the reason they told me was, it wasn't because of me, but they couldn't evaluate, they said, people from small liberal arts schools, they didn't know anything about the small liberal arts schools in terms of how someone from there could compete with the larger institutions, students in the larger institutions. Any rate, I enjoyed reminding them of that for years.

Mark: Good for you. Rachel Carson went to a small liberal arts school, too.

Gene: I'm a huge fan of a liberal arts education and my first teaching job was at Dartmouth College. I was at Dartmouth for about eight years and then I was invited back several years ago for a conference on the future of liberal education. It was called, but it meant Liberal Arts Education. It was a pretty interesting conference and I argued from my point of view strongly that that is a very important way of educating many people in our country and needs to be maintained and nurtured.

Mark: Excellent. We need to backtrack to the big plain gray university in Madison. What did you study there?

Gene: So, I ended up deciding to go to the University of Wisconsin, totally naïve. I had no idea what I might want to do. I had heard of the ologies in my undergraduate schooling. Ornithology, mammalogy, ichthyology, whatever, but I didn't really know what I wanted to do and so I was assigned a major professor, his name was Arthur Hasler. I was so naïve; it was really funny. I met with him when I got to the campus and he said: "Well, you should probably take my course" and it wasn't called Limnology when I was there, it was called Conservation of Aquatic Resources. There was no Limnology course in Madison at that time, which is a very interesting history too, and "you should take my journal club and you'll do fine, and we'll see you." I walked out of the meeting, and I saw someone, another grad student that I had met, and I said I was just told I should take a journal club. What is a journal club? I have no idea what that is. He said, oh well, you usually read a paper and then present the paper to the group and discuss it. Okay, never done that, didn't even know what a journal was because at Manchester College we didn't, we didn't read that literature, so journal was new to me. Well, Hasler was kind and he scheduled me sort of mid-semester and when I started to do my presentation, my first sentence

was, well, the data show and he stopped me, just stopped me, he said, the word is data. It's not data it's data. Here I go up front of this group trying to make my presentation. I didn't know if it was data or data, or whatever. Another graduate student, a senior graduate student of his sitting in the back of the room said no Dr. Hasler it's data, he's right, so they proceeded to argue for maybe five minutes about whether it was data or data; and I got my composure back and when they were done arguing I went on with my seminar to the journal club and that's how I got my introduction.

Mark: The academic Squabbling.

Gene: My first introduction to academic squabbling, right. And then I was taking his course, the Conservation of Aquatic Resources, and I was sitting in the back, shy, naïve and he was talking about the work that he was doing with students in northern Wisconsin, and I thought it was just terrific. So, I waited after the class was done for everybody to clear out and I went up front and I said to him, I'll never forget it. I said Dr. Hasler, I really enjoyed what you are talking about today. Can you really get paid for doing that kind of work? And he chuckled and he said, well, yes you can. He said I have another graduate student that's going to northern Wisconsin this coming weekend if you would like to ride along with him, you could and you could see firsthand some of the work that we're doing, and this was in the northwestern part of the state around Chippewa Falls. And, I said, oh I'd love to do that, so I did. By the end of that trip, I was hooked as a limnologist, and so I pursued my thesis, which was in physical Limnology, the study of the circulation and mixing in lakes and I used radioisotopes as a tracer of water movement and it is some work I'm really very proud of, actually. That was the way I started as a physical Limnologist. But at the University of Wisconsin, I took a course, a graduate course in ecology, and in the course, they were using Odum's second edition of his textbook on ecology, which was an evangelical bible for ecosystem ecology. Ecosystem ecology was very new, but Odum was pushing it as the way that you could make real progress in understanding the complexity of nature. So, I got hooked in that too because I was a double major. I majored in zoology and botany; they weren't combined there at that time. They were separate, and I liked both of them and I didn't know how to make a decision of whether I wanted to be a botanist or a zoologist and ecosystem ecology and limnology, which is really ecosystem work because you had to study the system of a lake or a river in order to understand it. The choice I didn't have to make. I didn't have to become a zoologist or a botanist because in ecosystem ecology I had to be a botanist, a zoologist, a meteorologist, a hydrologist, and a geologist, and that suited me just fine. I love that complexity.

Mark: That's great, but you just tipped the whole history of ecology. Stephen Forbes kicks it off as a brilliant limnologist in the U. S. and then the Odum brothers.

Gene: That's right.

Mark: I had that textbook even when I was an undergraduate and that was, that was a big change. Systems ecology really was the thread that ran through that.

Gene: Every year at the institute, now the Cary Institute of Ecosystem Studies, I teach the opening session on the history of ecosystem ecology, and I always bring Odum's second edition, I have a copy and wave it around and say, this is the bible that really started the field.

Mark: It is. It is.

Gene: I think there's no question and it also, it combines terrestrial with aquatic. You didn't have to make decisions about separating; in fact, he was arguing that they function from an ecosystem point of view in the same way. His push was from energetics, but whether it's a lake or a forest or a meadow or whatever, they all have the same characteristics and function in the same way. I thought that was great because that allowed you to integrate over such a large system.

Mark: It transformed the field.

Gene: It really did.

Mark: They were early pushers of using radio isotopes, you know. So, your study with radio isotopes in Chippewa Falls where my parents had a cabin.

Gene: They know the area well then.

Mark: That must've been pretty early on too?

Gene: It was very early on. I used phosphorous 32 and sodium 24 and I used iodine 131, for different purposes, but I added them to lakes. I had to develop how to do that. I would get the radio isotopes in small glass vials, arrived by airplane at the Eau Claire airport in big lead shielded boxes that I could barely lift. I usually had to have somebody help me with them because it was really hot dangerous stuff. Then I developed this crusher I called it. I put the glass bottles into the crusher, and I then could lower down them into the lake and then turn the inside rod and it would bring the jaws of the basket together and crush the glass bottle. I wanted to release it as gently as I could, because I was studying circulation and water movement and I didn't want to generate water movement, so I had to invent and develop all that stuff. There was nothing like that available. So, I did, and it worked, and I found that I could measure water movement in small lakes and then I took radioiodine to the Antarctic and measured water movement in a very large deep ice-covered lake. A permanently ice-covered lake in Antarctica.

Mark: How did you find that project? Well, it's the same climate as Wisconsin.

Gene: Just as I was graduating with my Ph.D., a colleague in meteorology, Robert Ragotzkie and I had decided that we'd like to try this method in Antarctica, because we found in small lakes in Wisconsin that the circulation of water was counterclockwise under the ice. We wondered in the southern hemisphere if it would be clockwise. I hope I have that right; I'll have to think about that for a moment. So that was one of the ideas we got National Science Foundation support for that. I had to take a whole Curie of radioiodine to Antarctica because by the time we were able to use it, it had decayed significantly. I don't remember exactly. I thought about this a long time, maybe a quarter of it was left because of the decay of the radioisotope. I had to take a lot in the beginning. Well, we worked all that out but at the last moment the

commercial jet airliner said you can't bring it on our planes and so we went military air transport, and they weren't too happy about it either. So, we actually carried our container to the plane and said to the pilot, can we tuck this in the back somewhere? He said, it's okay with me, I never heard the request. So, we tucked it in the tail compartment of the airplane and away we went. That's a long, long story and I hadn't thought about it in a long time. The details were a little rough but…

Mark: Yeah, but it was worthwhile hearing it. So, after that, is that when you went to Dartmouth?

Gene: So, these days our graduate students have such competition to find jobs and whatever. Well, I was in a graduate student office in Birge Hall on the University of Wisconsin campus right across the hall from Hasler's office. There was about six of us in the office. One day he came in and he said, I just had a call from my friend and colleague at Dartmouth College and they've had a sudden departure of one of their faculty and they need to have somebody fill in a course in aquatic ecology this fall. This was in the summer when he walked across the hall to our office. He said if any of you are interested, let me know and turned around and left. We all looked at one another and we said, and I'm not kidding, where's Dartmouth College? None of us knew. We never heard of Dartmouth College, talking about being insular in the Midwest. So, I had a Webster dictionary on a shelf above my desk, and in the back, it listed all of the universities in the U.S. So, I pulled it out and looked and I read it out. It said something like founded in 1769 or whatever the date is, men only, and then we all really laughed. What kind of a place is that? Well, I was going to finish my Ph.D. and defend in January, and I didn't really have any specific plans of what I was going to do after January. So, I said yeah, I'll do it. That might be interesting. I've never been in New England and so I took the job, that was 1961. I moved to Hanover for the fall, they have a trimester system there, and I taught this course in aquatic ecology. Well, it was there, that I met Herbert Bormann and started the Hubbard Brook Ecosystem Study. So, if it weren't for that little bit of serendipity; my whole life has been influenced by serendipity. I define serendipity as, keeping your eyes, ears and mind open. And, if you see something or hear something interesting, you jump on it and go with it. So, if it hadn't been for that little bit of serendipity, it's probably fair to state that the Hubbard Brook Ecosystem Study never would have started because we originators, there were four of us, always said that none of us would ever have done it alone. We had to do it together jointly as a group and so how did that happen? I don't know, but if I hadn't gone to Hubbard Brook almost certainly, I would have never discovered acid rain and on and on and on. My life had not been planned where I set out to, oh; my first job is going to be in an Ivy League school and in New England. No, it wasn't planned like that.

Mark: Well, any graduate student hearing this today is going to tear their hair out about hiring practices. So, you went to Dartmouth in 1961. Did you come back and have to defend then?

Gene: Yes, and I had to come back to Wisconsin and defend, so I was finishing writing my thesis while I was at Dartmouth and teaching the course and then I came back and defended, and then I went to the Antarctic. That's when I went to the Antarctic. Then Dartmouth offered me a permanent position as an instructor, no less. I don't think anybody does that anymore. You

know, it's usually an assistant, associate, full, but they hired me as an instructor in 1963. So, when I got back from the Antarctic, I went directly to Hanover, to Dartmouth College, and started my time there. I liked Dartmouth very much. It was about 70 miles from Hubbard Brook, so it was very convenient to make that trip and to start the research there. The project that we were trying to start at Hubbard Brook was very complicated and expansive. We wanted to try what may have been for the first time for a terrestrial system to understand how it all worked together. How does a forest system work? And so, the Forest Service had these gauged watersheds at Hubbard Brook Experimental Forest in the White Mountains in New Hampshire. We thought we could use those watersheds as ecosystems. So, we called them watershed ecosystems and then tried to understand how they functioned as a system, but at the scale of a watershed. But we weren't quite sure how to do that because the complexity is absolutely enormous. We thought well maybe we could use a medical metaphor. We could measure and analyze the chemistry of stream water much like a physician uses the chemistry of blood and urine to measure the health of a patient. And then if the chemistry of blood and urine changes, you need to go inside to the pancreas or the kidneys or whatever to try to understand why. So, we though that could be a useful metaphor. I guess that metaphor might be how we could think about the complexity of the watershed ecosystem if the chemistry of stream water changed then you had to go inside and try to find out what was causing it, the sugar maples doing it or was it the soil microbiology that was doing it or whatever. So, we wrote a proposal, and we submitted it to National Institutes of Health, and they just turned it down flat. They just thought that it was totally naïve and would never work and so they just turned it down flat. We then revised it and sent it to the National Science Foundation, and they funded it. They funded it for three years. I'd have to look up the number, but I think they gave us something like $65,000.00 for three years, and I'm talking about a total for three years, and there was that amount of money, that may not be the exact amount. It was a three-year grant, so it wasn't much money. Dartmouth didn't have a robust graduate program. They only had students at the master's level, and the masters' students often couldn't compete in classes with the undergraduates. Undergrads were very smart, very very good. There was no Ph.D. program, but we started the program. And to do what we wanted to do we thought we had to calculate a budget for the system, a mass balance for the system. So, we had to measure all the inputs and outputs. We had a model. We actually published our model in Science. One of the first papers that was published on Hubbard Brook was just our model, which was amazing to think that we could publish that in Science, but we did. So, we had this model and we needed to measure all the inputs and outputs, and the inputs were coming primarily through precipitation, and the outputs were occurring through the stream water, and the stream water was gauged, and the precipitation was gauged so that we could do quantitative budgets. Well, the very first sample of rainwater that we collected in July of 1963 had a PH that was about 100 times more acidic than we thought it should be, but we didn't know what it should be, because there weren't other data for a comparison. And we did know that theoretically distilled water in equilibrium with carbon oxide in the atmosphere should produce a pH of about 5.6, but we knew the rain wasn't distilled water. But that's what we knew. It was about 100 times more acidic than we expected, so that was the discovery in July 1963 of acid rain in North America. Then we had to answer questions of, well, is this something unique to the White Mountains of New Hampshire? Does it occur anywhere else? How long has it been that

way? What causes it? What effect does it have ecologically on the system? And a whole series of major questions like that. Which I've now spent decades trying to answer.

Mark: Did that transform the whole focus of the experiment, the discovery of …

Gene: Yes and no. Not really because the study went on as we had planned, but the acid rain issue became very visible. Two things became very visible very early in the Hubbard Brook Ecosystem Study. One was we experimentally cut down all the trees in an entire watershed. We were saying, okay, if we deforest the whole watershed ecosystem, what will that cause? We were quite sure that it would increase the flow of water because the trees transpire a lot of water to the atmosphere, but if the trees are gone that formerly transpired water vapor then would run out as liquid water in the streams. Well, that experiment also showed that there was a huge chemical loss from the deforestation, particularly nitrate in stream water. The nitrate concentration shot up to levels twice what the Public Health Service said were suitable for drinking water. Well, that was totally unexpected, and that along with acid rain, are the findings that are in the textbooks about Hubbard Brook. The biology textbooks usually show a photograph of that deforested watershed, then the results. So those two things, the acid rain and the forest disturbance affecting the chemical cycling. The acid rain we discovered was in 1963, July. We didn't publish the first paper on it for nine years because we were trying to answer some of those questions that I mentioned and again serendipity came into play. In 1970, I moved from Dartmouth to Cornell University, largely because of the graduate program that I mentioned before. So, I had worked very hard at Dartmouth to start a graduate Ph.D. program and I had the first Ph.D. student at Dartmouth which is surprising to me, but I did. We had master's students but not Ph.D.'s before that. And then I had a second student, so I had two students, and they were the only Ph.D. students on campus, and they didn't particularly like one another. They didn't dislike one another, but they didn't interact closely at all, and they were working on very different things. My first student worked on salamanders, interestingly enough and my second student worked on streams at Hubbard Brook. Well, I had thought that I had gotten so much learning from the interacting with fellow graduate students at the University of Wisconsin that I thought these students were being cheated, if you will, in not having that opportunity and so the faculty we had there, there were about five or six of us that were ecologically oriented in the department. So, we tried to work with the students and that was good. But it wasn't like having your fellow graduate student that you could interact with and discuss things with. So, I decided to change jobs and that's when I went to Cornell, because they had a very strong graduate program there. So, my move to Cornell was largely because of the desire to have a more active graduate program. I moved to Cornell in 1969/70. In 1969, I received the NATO Senior Fellowship. It was a fellowship that allowed me to visit Great Britain and Sweden. I was going to learn more about aquatic chemistry and what was being done there and the newer procedures that they were using. I had a very nice visit at the Ferry House on Lake Windermere, in the UK. I was there maybe a week and then I went on to Uppsala Sweden and met with…

Mark: Do you want to take a break and get a glass of water?

Gene: Yes.

Mark: Let me just pause this.

Transcript Resumes:

Gene: My host in Sweden was Dr. Torsten Ahland as we were chatting in his office, he said, there's a friend of mine that you really should meet because I was telling him about acid rain and what you are doing His name is Svante Oden, and would you like to meet him. I said of course. So, Torsten called him, and Svante came over and we then talked about acid rain for an hour and Svante said, I'm going tonight on the overnight train to Oslo and I'm giving a lecture in the botany department tomorrow on my work. You're welcome to go along if you like. I said, well, okay sure. So, we sat up and talked most all night long in his train compartment about his work and then he gave his lecture the next day in Swedish to a Norwegian audience. And of course, I don't know Swedish or Norwegian that well, but I'd heard the lecture the night before, so I was able to follow. He was suggesting that Sweden's lakes were being acidified, and he thought it was because of pollution that was coming through the atmosphere from the industrialized Ruhr Valley and from Great Britain. It was being transported through the atmosphere and then falling on southern Sweden. They were seeing larger effects in southern Sweden than they were in northern Sweden on the lakes. I said, wow! I wonder if that could be what's happening with us if the emissions in the Midwest, from the large coal-fired and oil-fired power plants in Indiana, Illinois, Ohio, West Virginia, Kentucky. I wondered if that's what's giving us the acid rain in New Hampshire. I want to make a very long story short; Yes, that was exactly what was going on but that was one of those questions that we had to try to answer. I'll just mention briefly two of the questions we tried to answer. That was one. We didn't know how to address that question and remember this was a long time ago. This was in the early 1970's. So, I contacted a colleague in NCAR, the National Center for Atmospheric Research, in Boulder and said how could be trying to get some information on this? They had a small plane and so we actually flew in this small plane in plumes that were coming from the big stacks. On certain days you could see the plumes when conditions were right. We literally held out the window of the small plane funnel collectors, trying to collect samples of this pollution in these plumes. I mean, talking about early on. But that's all we could do. Then we also had a van on the ground, and we would be in the van below the plume hoping it was going to rain and if it rained, we'd stop the van and get out and hold our funnels and try to collect samples of rain below the plume. What we found was, yes, the plumes were very polluted. There was high sulfur pollution in those plumes, and they were headed toward the northeastern United States. Later, we and others were able to use isotopic tracers but that's what we had to do in the beginning. Then we were able to correlate changes in emissions with changes in sulfate concentrations we were measuring way out there in Hubbard Brook and the White Mountains of New Hampshire. The correlations became very convincing but that's the kind of steps we had to go through to answer the question, where is it coming from? Because we didn't know. And the serendipity of that trip to Uppsala, Sweden was what helped me understand that. The other story is, how long has it been that way? So, two colleagues from the University of Virginia and another colleague and I set up precipitation monitoring stations in the most remote human locations we could find. We put one near the southern tip of Chile, one on the southern tip of South Africa, and one on an island in the middle of the Indian Ocean called Amsterdam Island, a remote site in Australia, a remote site in China and so forth, and we ran those sites for about 10 years to get the answer that the pH without

human activity polluting the precipitation was about 5.1 and we didn't know that before. But it took that effort to get that one number and we needed all that information to try to piece together and answer those questions of where it was coming from? How long has it been that way? And so forth. So, it took a long time and a lot of effort. The first paper that we published was titled just "acid rain". It was authored by myself, Herbert Bormann and Noye Johnson. We argued a long time about what the title should be. We said, well, it could be "the chemistry of precipitation in the White Mountains of New Hampshire". I argued really hard and won the argument that it needed to be impactful. That's the right word and so we settled on Acid Rain, just two words, acid rain, and that was the title and it really caught on to the media and to the public. I gave a lecture at the University of California Davis, in the early 1970's, and there was a reporter in the audience. I didn't know he or she was there. The next morning in the Sacramento Bee, which is one of California's major political newspapers because that's where the capital is; had a headline, Prof says Rain on Acid Trip. Well, not quite what I was talking about, but I guess bad publicity, as we're seeing now in the U.S. is as good as good publicity in terms of trying to attract awareness about a problem. So, it took us a long time. That first paper was published in a journal called *Environment*, and then the second paper on the issue was published in Science. We didn't know if this phenomenon was just something that was happening in the White Mountains in New Hampshire. It is the granite state so maybe the chemistry there is strange for some reason. So, when I changed jobs from Dartmouth to Cornell, I set up precipitation collecting stations around the Finger Lakes, primarily Cayuga and Seneca Lakes, and I found that the chemistry of the precipitation there was very similar to what it had been at Hubbard Brook. So, we published a paper in *Science* that talked about the regional aspect of acid rain, and that was the first indication that it was not just something at Hubbard Brook, but more widely dispersed. We also talked in this paper about the power plants contributing to the pollution and that raised a lot of comment. The day after that paper appeared, an article about it appeared on the front page of the New York Times, and my life has never been the same since. Back then, I had calls and letters from colleagues and people I didn't even know from all over the world saying, Likens what is this? So, you asked if the research at Hubbard Brook changed as a result of the acid rain discovery? Yes and no. I mean it became so visible and so politically charged. Very much like today with climate change, with a whole group of deniers. I could tell you stories for the rest of the day about things that happened. I always used to say we were trying to understand how the Hubbard Brook ecosystem functions and acid rain is just one of those components that is affecting how it functions. I think the legacy of acid rain on how it functions, is very great. One of the things that has happened is that with now 50 years of acid rain, we had determined as best we can that the phenomenon of acid rain in North America started in the mid 1950's. So, from 1950 till now acid rain has leached very large amounts of calcium and magnesium from the soils. The buffering capacity of the soils has been greatly diminished at Hubbard Brook, where we have detailed information. More than half of the exchangeable calcium has been leached from the soil during this time. It is much more than we or anybody else had expected. Which means then, that these depleted systems are much more sensitive to acid rain than they were previously. That's another story I need to fill in. So, let me backtrack. This research, and that of others finally led to the 1990 Clean Air Act Amendments to the 1970 Clean Air Act. And so, the result is what I would call a real environmental success story. At Hubbard Brook the decline of the acidity in precipitation since the peak in the late

1960's or early 1970's is about 83%. So, it's really declined a lot, but with this loss of buffering capacity in the soils, it's made those soils much more sensitive, so even though the acidity is less, the soils are more sensitive. So, acid rain is still having an impact on the soils and if politically, which is being proposed right now, we cut back on pollution controls and start increasing the sulfur dioxide emissions again. There was a news report in the newspaper, and I haven't had a chance to check it out, that West Virginia's emissions are now going up, the Adirondack Lakes people are very concerned about that because of the impact there. Well, if that is happening, then these now more sensitive systems are very vulnerable, and it could produce a very large impact.

Mark: This is great, and you've really given us a sense of science in action or in process. I had two questions. First before Sweden, before your trip to Sweden you knew that water in the lake was more acidic than it should have been. Did you have some working hypotheses about the cause of it?

Gene: What do you mean by the lake?

Mark: Wasn't that at Hubbard Brook where you measured the acidity was in the stream or a lake?

Gene: No, no, it was the precipitation.

Mark: Okay, The precipitation. Did you have a working hypothesis about why that was abnormally acidic?

Gene: No, no, not until Sweden did I have the idea that the pollution was coming from the Midwestern power plants. Yeah.

Mark: That's amazing!

Gene: I mean we had obviously talked about it at the time, and we were speculating about what might be the cause. We knew that there weren't large emission sources nearby Hubbard Brook, at least upwind of Hubbard Brook. But no, putting it together with power plant emissions, which in terms of sulfur dioxide, it's 70+ % of all the sulfur dioxide in the U.S. comes from those power plants in the Midwest. It's a huge source and the winds of course go primarily from west to east so then we have to also answer more questions. So we were working with atmospheric scientists, modelers, we actually modeled every trajectory of a wind path that produced a precipitation event at Hubbard Brook and then we had to do a cluster analysis to see what the major clusters were and then we had to follow those cluster trajectories over the states and Canada, upwind of Hubbard Brook because in the U.S., the emission inventories were done on a state-by-state basis so as the trajectory went over Pennsylvania or over Ohio or it went over Delaware or wherever it went, we had to do that in order to calculate what the deposition was from 24-hour back trajectories and 36-hour back trajectories, a huge amount of effort to get answers like that, that were relevant to Hubbard Brook.

Mark: The other question I had was acid rain. The term acid rain. Before the article, and was that article, was that 1972?

Gene: Yeah.

Mark: Was that a term you used? Your group?

Gene: Yes, we did. It had been used earlier by a British scientist, Robert Angus Smith. He was studying the air pollution in highly polluted areas of Great Britain around the large, industrialized areas in cities and he used the term acid rain. We didn't know that at the time, but he didn't use it in the way that we had used it. It had been used before, but we didn't know it.

Mark: Because it took off? When I was an undergraduate that was our concern.

Gene: Well, yesterday or the day before my son actually sent me this article and it apparently, it had just been published this week in PNAS, but I haven't seen the article. They're claiming that acid rain is what actually killed the dinosaurs and so I want to read that.

Mark: Volcanic eruptions?

Gene: No, a meteor, the meteor that before they thought was causing nuclear winter.

Mark: Right.

Gene: This article is saying it was acid rain. So, I want to read the article.

Mark: So, you said after the articles came out, it transformed your notoriety.

Gene: Well, yes, my life.

Mark: Yes, exactly. So, did you become active then once you knew the causes and lobbying to reduce emissions?

Gene: No lobbying, but yes, I became very active. I gave a lot of lectures. I wrote a lot of papers. We've also written, three or four books about Hubbard Brook. The most recent one was written in 2016 and it has this story in it to document it in writing. I did some testimony to Congress, and I was on a program at the Smithsonian with Senator Wirth and Senator Heinz. John Heinz was Republican, Tim Wirth was Democrat, but they worked together. They were good friends, and they worked on a number of issues, including something called Project 88, which was an environmental ecological effort. So, this was an all-day session at the Smithsonian, and somebody dug that out. Who was it? Who did all the videos? CNN or somebody did.

Mark: C-SPAN?

Gene: C-SPAN. Not CNN. And somebody dug those out just two or three weeks ago and sent me copies. I've never seen them before. But there was testimony I had done in Congress and this one for the Smithsonian and I looked young. Actually, I'm going to be less than humble, but I thought I did pretty well in communicating and in, you know, telling a story that was compelling. So, you know, then I led a small team to brief President Reagan and the full cabinet in September of 1983. We had a full hour with the president and the cabinet. It was organized by William Ruckelshaus, who was the administrator of EPA. We presented what acid rain was and what we thought should be done about it. The reduction of emission, which is very similar to

what the 1990 Clean Air Act Amendments did. This was 1983. President Reagan listened very attentively and then at the end of the presentation, sat back in his chair and said, And I'll never forget this quote. Well, gentlemen, and there were ladies in the room. Well gentlemen, it's clear to me that my undergraduate education didn't prepare me for such complicated issues, unquote. I thought, good grief. And then I wondered where he went to undergraduate school. But I think our presentation went well, but that was September. By January, the director of management and budget announced that it was too expensive to deal with acid rain so we're going to study it. So, the NAPAP [National Acid Precipitation Assessment] Program, which had been started during the Carter administration, was then planned to study the acid rain issue for 10 years and then we'd know everything we need to know and could make wise decisions. Of course, Reagan wouldn't still be president after 10 years, so it was just putting it out of his domain. But then George H. Bush, the senior, came in, and one of the very first things he said publicly was, we're going to deal with the acid rain issue. It's been around, it's serious, and we're going to deal with it, and his new EPA administrator and his new head of the Department of State said the same thing. Baker and the EPA administrator, William Riley said that acid rain was high on the agenda to deal with, and so President Bush signed it into law in November, I believe, but the NAPAP report didn't appear until the following February, so it was signed into law before the study was completed. The Study, that was to give us all the answers to help us know how to deal with the fact that acid rain had caused all that damage. It was an interesting time. Very interesting time and of course it was a conflict with the Canadians because they were saying more than half of their sulfur was coming from the U.S., even though they had big sources like the copper nickel smelter in Sudbury, which is a huge source. But mostly that plume stayed within Canada. So, it was an amazing time with deniers and meetings, and I was spending a lot of time working with European colleagues and going to Norway and Sweden and Great Britain for meetings, and it was amazing.

Mark: Besides acid rain, the other pioneering part of Hubbard Brook was it was a long-term ecological study. Tell us why that is important.

Gene: Well, I think there are just some insights that you can't get any other way. With modeling or with deep thinking or whatever, and without the context of what has happened in the past, it's very difficult to put what's happening on an individual event now into real terms. So, something very simple like yesterday was very warm. Was that unusual? You don't know if that was unusual without the context of the long-term data to say, yeah that was, I'm making this up. That was the warmest day that we've ever had on the record of measured temperatures. It wasn't, but I'm j making it up, you know? But you know, you need that long term perspective so the same is true about so much that we know about ecology. Ecology is such a complicated field. It's very difficult to understand relationships on many levels and to try to put all that together into an understanding. So, without the long-term nature of what happens or what the trends are, you know, if we've got a trend that's going upward and then you've got a big spike in it, you don't know if that big spike is unusual without that long term trend. So, a colleague and I, Dave Lindenmayer in Australia and I have tried to spend a lot of time thinking about long term monitoring and we argue that you need at least 10 years of data to call yourself or to call the study long term. We wanted to try to identify that, you can argue with whether 10 years is correct or not, but that's the number that we used. Our research at Hubbard Brook has been

continuous since 1963: continuous monitoring of precipitation amount and chemistry, stream water amount and chemistry, lake water chemistry, ice cover on Mirror Lake at the bottom of the valley, forest growth, and on and on. Those long-term parameters then allow us to have the context for what we may want to study experimentally like an experimental treatment on an entire watershed or whether something, as I said, that we suddenly see is unusual. With climate change this is very obvious, isn't it because there's a lot of argument about well, is this unusual or are the trends going up or down. We know because we have the long-term patterns. David Keeling's carbon dioxide measurements are some of the most important environmental records that are available and the struggles that he went through to try and keep those studies going. He was threatened with termination, I think six times. But the long-term CO2 record is really important. I would argue again not humbly that the long-term record of atmospheric CO2 and the long-term record of precipitation chemistry are two of the most important environmental records.

Mark: Did you envision back in 1963 that Hubbard Brook would be a long-term study or was it just a three year?

Gene: Not at all. We thought we'd be lucky if we got three years in and I think we spent up most of the grant money in like two years, so we had to go back in for another grant and we did. We've had continuous funding from the National Science Foundation since 1963. 1962 was when the grant became available. It may be some record, I don't know. But having the funding through the National Science Foundation was crucial during the period I call the acid rain wars of the 1980's, because it allowed us to do really high-quality science with funding sources that were not questionable in terms of political action or activity and that gave us credibility. I had a contract put out on me. A group offered $400,000.00 and the call for proposals said, Likens' has said, and they listed about six things I had said in the published literature, and the proposal call was, show that he is wrong. And they made that award to a consulting group for $400,000.00, and one of the things that I am most proud about in my career was that they weren't able to do that. One of the main reasons was that our data were impeccable. We very carefully had tried to do the right thing in terms of collecting our data and with standards and procedures, and we don't change methodology without overlapping it with new methodology for a year or whatever is appropriate. So, to be challenged, the reason that we were able to survive that challenge was because the data were not able to be proved wrong.

Mark: I never heard that before about the $400,000.00 contract.

Gene: Well, it wasn't a threat on my life, it was a threat on my career. I was a young scientist and if they had shown that there were just all these problems with the Hubbard Brook record (data), which was an important record in this whole debate about acid rain, a very important record. Well, then we might not have had the 1990 Clean Air Act Amendments, I don't know, but my career could have been destroyed. It was something I'd never experienced before.

Mark: Hopefully never again. Let's change tracks a little bit. Were there previous scientists that inspired you in your career?

Gene: Oh Yes! G. Evelyn Hutchinson at Yale as a limnologist. His big treatises in limnology always inspired me, not only the size of them, they're each about two inches thick, but the information that is there, and this was before the computer age. So, this was information that he was generating from the library and from his brain. He's always been an inspiration to me. I got to know him, and I talked with him before he died several times. Vernadsky, a Russian biogeochemist, is another person that inspired me. I think his ability to think conceptually and deeply is truly inspirational. Eugene Odum. Howard too, his brother, but Gene primarily with the ecosystem concept and approach. That influenced my career in a very major way. Those three. Margaret Davis, a paleoecologist, I actually worked with Margaret. She's inspired me in many ways. Tom Winter, USGS Groundwater Hydrologist has inspired me. So those are some examples.

Mark: It's a good pantheon. You've been in academic ecology for 60 years or so. How has it changed in your career? You've covered over half of America ecology in your own career.

Gene: Well, you know that's a difficult question to answer, I think, because the field has changed dramatically. We're now using a whole new set of tools to address questions. Genomics. Genomics didn't exist in that way then. More sophisticated statistics that are available through high volume and high speed computers. I heard yesterday that Google has just developed a new super supercomputer that can do things that, what I read would solve a problem that would have taken 10,000 years, now in three minutes or something. I mean, so the tools are very different, I used radioisotopes. There's now a whole series of stable isotopes people use to tease out an understanding of the complexity of ecology that weren't available to me as a young ecologist at all. As I mentioned earlier, I had to make many of my own tools. I physically made them. I built them myself because there was no place I could go to buy them. I didn't have a machine shop that I could have make them for me, so I made them myself. That's all very different. I think the fact that we can now communicate so easily and broadly. I can work this afternoon with a colleague in Australia, with a colleague in China, on the internet with no problem whatsoever. I couldn't begin to do that before. There is a downside to that, and that is that I think if my email string is any example, I can't keep up with it. It's also taking away from my productive time in a major way. So, I frequently ask students and young scientists, are you saving time to think deeply because you can't do it all in the computer? You have to think as well, in my opinion, and I'm afraid the busyness of the email and the cell phone and whatever often detracts from our ability to think. If I were to receive an invitation to go to a meeting in Brazil next Monday, I could go to that meeting. Back when I was a young scientist, that would have been unthinkable. One, I couldn't have arranged the flights to do that and two, I would have had to receive a letter invitation inviting me to do that and I would have written a letter back, yes, I will come or whatever, but I need to know this before I can come. Now all that can happen quickly. I have a colleague here at U. Conn that I teach with, and he went off for a one-day meeting in France this week and is back. That's hard. I mean that travel is hard physically on us. So, I'm kind of rambling. I said it's a difficult question to answer but I think the modeling, the new tools, the internet access are all fantastic ways that advance our field much more rapidly. I'm going to sound old, and I apologize, but I hope advances are done accurately and well, rapidly for sure, but I hope accurately and well.

Mark: You teach a course here; I saw called Nature Science and Society.

Gene: I do.

Mark: I love history of science. What is it, an undergrad or grad course?

Gene: Both and townspeople. We have five or six townspeople that sit in on the course and several professors and we're all over the place regarding topics. We talk about whatever seems to be of interest so the last couple of sessions have dealt with the climate strike issue that has happened all over the world. It happened here on this campus as well. So, we brought in two undergraduate student leaders of the climate strike on campus to discuss with all of us about why they are doing it, what they hope to achieve, what their goals are, the future, that's been fascinating to hear from them. They're very smart, dedicated young people and so I will crowd-source, I call it, can read a whole book quickly. We read Jane Mayer, Dark Money and different people read different chapters and then they report on what they read in their chapter. So, we can do all that in one week. Whereas if you assigned a book of that size to the class, you couldn't do that. It's been great fun. I already retired twice. I can't quite figure out what that word means. I love being here and being challenged to think and to act and I love this aspect very much.

Mark: And you've been working with bright students as you commented since Dartmouth, you been teaching probably close to 60 years. What type of message do you want to convey to your students?

Gene: Well, I think they need to find time to think as I said before. I think that's very important, and I think they need to be sure that they develop the credentials in science, and that mostly means publication in peer reviewed journals with your results and ideas. And then, if you've done that and you have that credibility and those credentials, then I think when there are issues that need to be communicated to the public and to the media, you should do it. You should speak out, but you should do that when you have something to say, and you have the expertise to say it. I think sadly, we're in a situation right now in our country where there's a lot of things that are being said and written that are not factually correct at all and we had a session in my class this Monday led by a philosopher on campus, a very smart philosopher, talking about the difference between opinions, beliefs and convictions, and how if you have those firmly as a part of your tribal existence and your make up, you're probably not going to change them. So, a person like me, that deals with facts, I try to collect information, so I know I'm searching for the truth and, I don't find the truth, but I am constantly searching for the truth. So, as I get facts in that search for the truth, the contest between trying to argue from a factual point of view versus a belief point of view or a conviction point of view even more so is very difficult. I think we're having that problem in the country right now in a way that I'd never experienced before. So, I think what I can do as a scientist is to continue my search for the truth and to speak out and to say what I know about what I have learned and what I want to communicate with others about what I've learned.

Mark: That where we should end because it's so eloquent. As a historian of science, I've got one more process question. It just occurred to me you mentioned about publishing. When you are ready to publish and you have an amazing number of books and articles published and going

back to Hubbard Brook, a great collaboration and I wonder with your background what you think makes effective collaboration. I think it's one of the most challenging things in the sciences and that so much of our work is collaborative.

Gene: Oh, and I agree. Well, but some of it isn't as collaborative as it should be. Some may be multidisciplinary, rather than interdisciplinary as often as should be, it's multidisciplinary. Well, I actually have written about this. I feel deeply that if you're going to work collaboratively and interdisciplinarity with others, that the absolute most important thing is trust. Another very important thing is you actually need to like each other. You don't have to like each other but that really helps. The trust is so important because when you work in large teams, as I have done a lot in my career, and we're working on a project and maybe you're doing this statistical stuff mostly and someone else is doing the titration work in the lab mostly, and someone else is doing the digging of the soil pits mostly and whatever. And yes, I can do all those things myself but if I were going to do it myself then I wouldn't need you. But if you're going to do the statistics then I have to trust that you've done them correctly and honestly and properly. Because otherwise I would have to do them myself. And then that takes me back to that loop. Why have you involved them if I'm going to do it myself. So, trust to me is just the absolute most important thing. Now, if I like you, I suppose some might argue that could tarnish a bit your view of trust, but if I like you, like for example Noye Johnson was one of the four originators of the Hubbard Brook ecosystem study, he died sadly way too young. He was a dear friend of mine. I miss him today. We would take car trips and talk the whole time about science. We discovered that we could measure the temperature profile in the muds below a lake bottom surface and measure the heat flow to the center of the earth from pushing a probe into the sediments five meters deep, which is easy to do and hadn't been done before. All that came from the discussions we had as friends. Wouldn't that be interesting to do? Wouldn't that be fun to do? Ah well, let's go do it, and so there's all the other stuff that you bring expertise to that I don't have, maybe you're a better statistician that I am, you may be a better hole digger than I am and whatever. But for me, it's trust. But in what I've written, I listed the features that I thought were necessary for teamwork. But for me trust is the big one at the top of the list.

Mark: That's perfect.