Oral History Summary Page

Interviewee: John Harte, Professor Emeritus of Energy and Resources Group and the College of Natural Resources at the University of California.

Interviewer: Ann Brody Guy

Date: August 6, 2024

Location: Prof. Harte's home in Berkeley

Purpose, Scope, and Content: Prof. Harte was interviewed as part of the 50th anniversary celebration of the Energy and Resource Group at UC Berkeley. The interview lasts just over 2 hours and contains discussions of:

- His early interest in ecology and protecting the environment.
- Organizing an all-day, multidisciplinary Vietnam War teach-in at Yale.
- Valuable lessons from consequential studies on a proposed jetport near the Florida Everglades.
- Proposing an ERG-like new graduate program at Yale during the same period ERG was being imagined at UC Berkeley.
- Joining the Lawrence Berkeley Lab's fledgling Energy and Environment Group.
- Meeting John Holdren and agreeing to collaborate, leading to a career-long ERG connection.
- Memorably contentious ERG colloquia.
- A smooth move from adjunct faculty to core faculty.
- As chair in the 1990s, proactively protecting ERG from being moved into a college.
- Developing and teaching courses that were precursors to ERG 102 and 202.
- Origins and outcomes of his *Consider a Spherical Cow* textbook.
- Origins and outcomes of his 30-year Warming Meadow experiment.
- Rocky Mountain research on acid rain, and how it contributed to Clean Air Act amendments.
- Crushing Dick Cheney in a public forum – and protecting Clean Air Act amendments.
- ERG's extraordinary students, including the high number who became administrative leaders.
- How ERG has changed his life and the world.

Editorial note: This transcript has been lightly edited by the interviewee for factual corrections

Biographical Note: Professor John Harte trained as a physicist and became an ecologist, conducting consequential research on acid rain, climate change, and complex systems. He began his Berkeley career at the Lawrence Berkeley Lab, eventually moving to appointments shared between ERG and CNR. He is also an external professor of the Santa Fe Institute and has authored eight books and more than 250 articles. He is an elected Fellow of the American Association for the Advancement of Science, the American Physical Society, and the Ecological Society of America. He has won numerous awards and honors including a Guggenheim Fellowship, a George Polk prize in investigative journalism, and the

Leo Szilard prize from the American Physical Society. He has mentored ERG students from its earliest period of teaching and helped develop ERG's core courses.

John Harte Oral History Transcript

John Harte: My name is John Harte, and I'm a professor emeritus at the Energy and Resources Group at UC Berkeley.

Early environmental interests and activism

Ann Brody Guy: Let's talk about your shift from undergrad and PhD degrees in physics to becoming an ecologist. Tell me a little bit about that road.

JH: In a sense the road started when I was very young. I can remember as a 10-, 11-year-old being very upset by the destruction of open spaces in the suburbs where we moved to after we left New York City. I grew up in the Bronx, but then we left the Bronx when I was about eight and moved to the suburbs. I got really interested in bird watching and natural history as a kid. That was my obsession, and one of the things that that led to was anger at development, at forests being cut down, meadows cleared for new housing. And one of the stunts I pulled when I was about 11 or 12 was when people want to build a new house, the first thing to do is bulldoze the land and put in stakes, surveying stakes that will mark where the foundation of the house will be. So I would go out at night and move the surveyor stake just inward, like four feet, so they would barely, they wouldn't notice, but it would, in fact, create a trapezoidal instead of a rectangular foundation, and in my imagination, it would lead to the collapse of the house and the end of development. So I was sort of an environmental guerrilla, informally, as a kid.

But I was just obsessively involved in birding and natural history. More generally, I would try to collect seeds of every plant that grew in the area and try to photograph every species of bird that came into our yard and stuff like that. So that led, eventually, to my profession.

But in the meantime, in high school, I got interested in math and quantum mechanics, and I read books like [Bertrand] Russell's *The ABC of Relativity*, and got fascinated by physics and cosmology, and so I decided that would probably be what I'd study. Actually, when I first got to college, my thought was I might want to be a history major. My dad was a high school history teacher, and my mom was a high school English teacher, so I was sort of brought up in the humanities. And I realized, yeah, I love that stuff, but I wasn't that adept at it, whereas I was very good at math and physics. So I changed my major to physics with a minor in mathematics, and went on and got a PhD, and thought I would do that for the rest of my career. I could maintain my interest in bird watching and natural history all through this time, but only as a hobby.

At Yale, a pre-curser to ER 102

And then in Yale, I was on the physics faculty — I was an assistant professor and then an associate professor of physics — and I was slowly getting more and more upset at the same things that upset me as a kid — pollution and development. So, one of my teaching assignments was to teach introductory physics, and I found myself bringing more and more examples into the intro physics course from environment and from biological sciences.

ABG: You mean the overlap of environment and physics?

JH: Yeah, applications of physics to... in physics you want to teach something like Newton's laws or thermodynamics, so you pick a physical system, like a pulley or a container with a piston and gas in the container, and it would be a very abstract way to present the physics. Well, I found myself using more and more environmental systems to illustrate basic physics, to teach the laws of physics from the perspective of applications to the real world, rather than to made-up, fictitious, simple systems that physicists like to use. And I developed a more advanced course at Yale, which I co-taught with some faculty from economics and engineering. They didn't have a program, but we were allowed to present a new course under the rubric of combined sciences — that was a category in the course catalog at Yale, and it was meant for things like biophysics, where you combine physics with biology. But I wanted to combine physics with economics and energy and technology and other things, more broadly. This was 1969, 1970. So I developed this course. I co-taught it with other people.

A multi-disciplinary Vietnam War teach-in at Yale

A chance event, participation i a study of threats to the Everglades, led to my transition from physics to environmental science. The genesis of this event was, surprisingly, the Vietnam war. In 1968 a colleague, Robert Socolow, and I said, we've got to educate people here at Yale more about Vietnam. Things were escalating in Cambodia, and it looked like there was no end to this war. Nixon was going to be elected in '68 and we decided that we should have a teach-in where we would bring in experts — both people who supported the war and advised the Johnson and early years of the Nixon administration on the war, and also opponents of the war — but all from a scientific and technical perspective. We wanted people who were real experts.

So on March 4 of 1969 we organized a series of visits and lectures all day long, and essentially shut down all of classes, at least in the Yale sciences, chemistry, physics, biology, geology that whole day. It was an entire day of providing students an opportunity to learn about the war. And it was very well attended. Some of the talks were at the law school — even they got involved. All the departments ended up being involved in one way or another.

Florida Everglades project: A formative event

At that event, One of the people we invited was a Nobel laureate in physics named Murray Gell-Mann. He was involved in advising on Vietnam and could present the government side of the issue. At dinner that night, he asked if I would be interested in joining a small group of people organized through the National Academy of Sciences that that would conduct an environmental study. And I said, Study of what? He said, Well, we don't know yet, but we want to show that physicists can contribute to this new thing that seems to be building.

We didn't call it the environmental movement then — it was just beginning. The first Clean Air Act, Clean Water Act, Endangered Species Act, Environmental Protection Act — these things were just happening. It was a time of ferment in environmental action and policy, and there were some interesting big events occurring around that time, like the Santa Barbara oil spill and air pollution episodes that were beginning to get people riled up about environment.

So I said, Yeah, sure. And they said, Well, we're thinking possibly a study up in Alaska. But another option is the Florida Everglades, where, and I didn't know this, but they told me the government is thinking about building a jetport for supersonic passenger aircraft. Just like the Europeans built the Concorde, well we were going to build what we called the SST, the supersonic transport, and it would carry passengers faster than the speed of sound from the U.S. to wherever in the world. And there would be three major airports to land these supersonic jets. L.A. for flights from the Pacific, New York for flights from Europe, and Florida for flights from the southern hemisphere. But to land SSTs, you need six-mile-long airstrips. You can't land them on regular airports that were traditionally in use at that time. And the one in Miami was too small, so they would have to build a new supersonic transport jetport, and the idea was to build it right at the edge of Everglades National Park. And the park people were furious. Residents were also upset.

So we did a summer study. We got about 30 people together, and we spent the whole summer of 1969 learning everything we could about supersonic transport, about the Everglades ecosystem — because I knew a lot of ecology, I was especially involved in that — and the harm that would happen from the noise and the pollution and the development to the region.

It led to a study that Robert Socolow and I did together. We took a little chunk of this whole story and looked at it as physicists, but with a wrinkle. And the problem we looked at was, What would happen when you drain all the swampland that would have to be drained for this new development? And it turns out it would lead to salt intrusion into the water supplies of half a million people living along the Gulf Coast. So half a million people are going to lose their fresh water. And Florida, even though it seems like a wet state, is actually fresh-water-scarce. It doesn't have mountain streams and snow melt and the things that a lot of the rest of us have that supply pretty dependable water.

So we wrote this up. We published an article in a law review, and a chapter in a book, and a couple of other things, which reached the attention of the Secretary of Transportation for Nixon, a guy named Volpe. He and Nixon probably got together. We'll never know what they said, but I'm sure they said something like "We can't afford to lose half a million votes in Florida for the next election". So they canceled the airport. They just nixed it completely. It was a major victory. It was front page of *The New York Times* and it was based on — to a great extent, but not completely — on this study we had done that showed that all these people would lose their water. So even though my instinct initially, was to say, This airport is going to kill a lot of alligators and a lot of egrets and damage wildlife, and it'll destroy the integrity of the ecosystem — and we did say these things — the argument that worked at the time was one that appealed directly to human self-interest: drinking water.

Everglades Lessons: Ecosystem services; interdisciplinary teams, simple models, and the power of stories

The study taught me four major lessons. One was that there is such a thing as ecosystem services. We called it then by a different name — we said it's a confluence of interest between man and nature (man, because it was 1969; we don't say that today). We called it a confluence of interest, and it was the first time that a study had really made use of this concept and used it to good advantage to alter policy in a major way. Currently, today, everybody talks about this idea. They don't call it a confluence of interest between man and nature; they called it ecosystem services to humankind. It's the same thing, same deal, but we were really, I think, among the first to identify this as a key way to influence public policy and to exploit the notion, and to even just talk about the fact that humans and wildlife share a common dependence on natural processes like swamp water to keep salt intrusion out from aquifers, from drinking water supply. So that was one lesson, the confluence of interest.

Another was that we couldn't have done this study without having an interdisciplinary team of people working together to identify the issues. If it had just been physicists, which was, I thought, the original idea, it probably wouldn't have been as good. But we had economists, we had demographers, we had social scientists in the group, and it broadened our thinking and allowed us to go beyond just thinking about alligators and air quality to thinking more broadly about the whole system. The system boundary was as wide as it could be, and if it hadn't been, we might not have been successful.

A third lesson from this study was that in the papers we wrote, we used very simple models. An aquifer is a very complicated thing. It's heterogeneous, porous rock under soil, and it varies from place to place. If you look here, it'll look different than if you look three feet away. We said, Forget all that complexity. Let's make a simple, what I call a "spherical cow" model of the system. And the model was very simple indeed. It was basically just a U-tube with salt water on one end and fresh water on the other. And it allowed us to abstract away all the complexity and reach a very broad, general, irrefutable conclusion that we published. And it convinced me that making models overly complex to try to capture all the detail, loses the whole point of the effort, which is to influence people, that people don't understand complex models. Somebody else can build a complex model and put in different complexity and maybe get a different answer. But if you can strip away all the detail and show that very robustly, This is what's going to happen. And you can add complexity if you want, but this is the core, the essence.

ABG: Let me just clarify: That U-shaped tube, that was your simplified model that was in the paper that then got published and got a lot of attention?

JH: Yeah, and in a book that got a lot of visibility.

ABG: So you and Socolow had the *intention* of influencing policy?

JH: Oh, yeah. We wanted to stop the airport. We wanted to save the Everglades. Absolutely.

The fourth lesson came about when I gave lectures on this whole effort, the Everglades study — the approach, the hydrology, the aquifer, the salt intrusion, the egrets. I would give talks on this to the public, and at Yale, and other professors would invite me to talk about the study because it got a lot of attention. And I realized that telling a story is a very good way to teach science. The case study approach. Now, lawyers have known this for a century or more. And journalists teach by case study. Lawyers, especially: Today we're going to talk about Smith versus Jones case. Here's the background to the story. Here's what Smith did to Jones, here's what Jones did to Smith, here's how the court looked at it, and here was the decision.

And we don't use that method in teaching physics or biology. We take a much more abstract and sort of academicky approach. But I realized from this, this was lesson number four, that telling stories is very vital. So the next year, right after the summer study, Socolow and I began writing a textbook, which came out in 1971, titled *Patient Earth*.. We started writing it right after the study was over. It's a set of case studies of environmental successes and it teaches the science, it presents the simple, spherical cow models throughout, but it's in the context of stories. And one of the stories is the Everglades. And then we invited other authors to write some of the other case studies they were most familiar with, like banning DDT, reducing sulfur content of fuel oil in New York City, which greatly improved the air quality, and so on. In some ways, it was the first environmental textbook in the aftermath of the environmental movement, and it was used in classes and it had a lot of visibility. It's now so out of date that nobody would use it, probably.

ABG: I can see on the back it says, "These are partisan essays." And yet you're telling me that it was used in classes. Did you feel a certain subversive victory in that?

JH: Yes.

An ERG-like idea at Yale

Let me go to the next step in this trajectory from physics to ecology. Besides writing this book, Rob Socolow and I decided — this was late 1969 now — we want to start an interdisciplinary environmental major at Yale, a graduate program that would have undergraduates as well. And it may be a new department. But it would do things like this. It would teach all of environmental science, but it would also teach economics and political science and engineering, and it would bring the disciplines together, and it would have courses that made use of the case study approach as well as the traditional approach — some of each. And we would hire new faculty, and it would be the first of its kind anywhere.

We wrote up a 10-page document that summarized the program, we gave examples of career options for students who graduated from it, and we brought it to the president of Yale, a guy named Kingman Brewster, Does that ring a bell? He was old-school, classics-trained, very dignified, very New England, of-the-manner-born sort of gentleman. People liked him. I liked him. I thought he was good. And we brought it to him. His office was in his house, which was on the Yale campus. And he didn't read it while we were there — he said, Give me two weeks and then you come back, and I'll let you know what I think.

At Yale, unlike a place like Berkeley, where all of the faculty have a say in things like this, Yale was very much structured like a monarchy, with the President making these decisions. He had a lot of power, much more than the Chancellor has at Berkeley, where the Academic Senate has a big role. So we came back — I think I had to come back by myself. Rob was out of town or ill. I came back into his office and had the document, and he handed it back to me, and he said, "I'm sorry, at Yale, we solve puzzles, not problems." I'll never forget that quote. It really stuck with me because it was true. If you looked at Yale, their engineering department for a long time had lost accreditation ds, because Yale had starved it. They at one point had the world's finest center for the study of alcoholism back in the '50s, I think it was, and thought that was beneath the dignity of a liberal arts university. So they got rid of this amazing institute. They were not into applied and policy-driven, problem-driven, as opposed to puzzle-driven work in the College of Letters and Science, or Arts and Science.

There was the Yale School of Forestry, but in the 1960s they were more narrowly focused on forestry and classic pollution problems. A lot of their funding came from places like Weyerhauser,, and so they were not looking broadly at the huge, wide scope of environmental issues that the world was facing — climate change, acid rain, overpopulation, energy supply. These were not prominent on their agenda.

There were some excellent people there, superb people. But the program wasn't designed to do anything like what we had designed our effort to be. So Brewster nixed our proposal.

A new Energy and Environment group at LBL, and an early intro to ERG

I continued teaching — I developed and taught for several years a course at Yale that later became, along with what Holdren brought to it, ER 102 at ERG. But Yale would not budge on creating a new program. And then in late 1972, I heard about an opportunity at Berkeley to join a new group that was going to be interdisciplinary and dedicated to energy and environmental issues. It would be called the Energy and Environment division of Lawrence Berkeley Lab. They started this in, I think it was late ’72, early ’73. It wasn't called a division then, it was just a program. Jack Hollander was the program director. For my first year at LBL, 1973-74, my funding came from the lab director's slush fund he could use to hire one or two people each year, just for some unique purpose. So I was brought in to bring ecology and environmental science to this program that was mainly dominated by people like nuclear energy experts, and solar energy experts, and energy conservation experts. I and another guy named Tica Novakov, who studied air pollution, were the environmental scientists in this new program.

When I got to Lawrence Berkeley Lab in the fall of ’73, soon after I arrived, I had a visitor from campus who wanted to make contact with our program, and it was John Holdren, who had arrived from Cal Tech to Berkeley to help start ERG. I didn't know about ERG or about him. And then he told me what ERG was going to be all about, and I told him what this program at LBL was all about, and we decided to join forces and do as much collaboration as we could. At that time, there were no ERG students yet. It was just the beginning of a program, the first months with Holdren on campus to actually get ERG going.

ABG: I know there was a committee. Did you formally join this fledgling group as an affiliate at that point?

JH: I don't recall if they had affiliates that early. As soon as there were affiliates, I became an affiliate, and somewhere in the ’70s, maybe ’77 or ’78, I became an adjunct professor at ERG. Now, adjunct is not the same thing as a professor. Adjunct is not a tenured Academic Senate position. But you go through a screening process that's equivalent to what a regular professor goes through. You have to be approved by Academic Senate, and it goes through a process that's pretty rigorous. ERG now has two adjunct professors, Margaret Torn and Andy Jones.

So my connection with ERG began to build. But it wasn't until they started having students that I got very involved because I was the faculty member at ERG, adjunct still, but adjunct faculty who could mentor the students with an interest in environmental science and ecology. So I took on the role of being the primary mentor and research advisor for the incoming ERGies who wanted to work in some area of environment and ecology. And so Kathy Tonnessen, Mohamed, El-Gasseir, Laura King, were among my first students.

Berkeley precursors to ER 102 and 202

ABG: So when you came from Yale, you had the credentials to be the environment guy at Berkeley because you had already been developing all these courses despite the fact that you were in a physics department?

JH: That's right. The other thing I should say is, when I came to Lawrence Berkeley Lab in ’73, I wanted to teach, even though my appointment at the lab was 100% research. So ERG did not have slots for teaching. So I went to the physics department because they knew me and had confidence in me, and I developed two courses: Physics 80 which was "Introduction to Environmental Physics," and another course which was 200-level, a more advanced course on mathematical modeling methods in climate science and ecology. And the more advanced course became ER 202, and the other course merged with Holdren's 102 to become the current version of 102. John Holdren's early 102 was a superb course he had developed from his teaching at Cal Tech, and I had developed mine from my teaching at Yale. So I stopped doing Physics 80 and in some years co-taught 102 with John, adding a whole lot of climate and ecology science to the course that he had developed.

Teaching Climate change back in 1970, and the “Warming Meadow” experiment

ABG: Since you mentioned climate: People broadly, as they are today, weren't talking about climate change back then. So I want to know the origins of your work in that.

[time-keeping exchange]

JH: In 1969, when we wrote the book [*Patient Earth*], I wrote a section on climate change, on global warming, and how carbon dioxide from fossil fuel burning can heat the Earth and how bad that could get. We were totally convinced of the science. I was teaching climate change science pretty much the same way people would teach it today. So we knew this was a big issue way back when a handful of scientists were really outspoken about it, at most a handful. But our students were learning why global warming occurs and how serious it can be, and how to model it and understand it back in the early 1970s.

ABG: And your Warming Meadow project you started what year?

JH: The idea germinated in 1987. It's a 30-year experiment that ended in 2020. The idea began in the late ’80s, ’87. And in winter, my wife and I found ourselves in San Francisco, and we were hungry, and it was cold and foggy. We were near Ghirardelli Square, and we saw an outdoor restaurant. It was too cold to eat outdoors, but people were sitting there in their shirt sleeves, and the reason was they had these electric heaters out on the terrace, and you could sit under them and feel like it was mid-summer. Well, midsummer in San Francisco could be cold — it would feel like it was warm. And we were sitting there eating and enjoying, surrounded by the fog and the cold, but feeling quite comfortable and having a nice dinner, I said, You could do this in an ecosystem and study how it responds to warming.

Nobody had thought of that, as far as I know. Nobody had done it, certainly. So I started sketching on back of a napkin what a system might look like. And my first idea was I'd have this big central heater that would radiate out, and as you go further from it there's less warming, so you would have a controlled gradient of warming, and you could look at the response. And then I came up with a better design, but that was the genesis.

The experiment actually started in 1990. It took the summer of ’88 and ’89 — and the winter too, but a lot of the field work was in the summer — to build the apparatus. Well, first to raise the money. It was, you know, $40,000 of infrastructure: data loggers and towers to hold suspend cables, and then the heaters. We built this giant spider web of towers and wire cable covering a big area, like a quarter of an acre, of alpine meadow, and that was expensive. We had to get the money, find a site, bring electricity to the site, which meant trenching and bringing an electric line from far away. All of that took a couple years. So in 1990 we finally turned it on and began recording data.

ABG: Has anybody else tried to replicate what you did?

JH: There have been dozens and dozens of experiments since then that used our approach or other approaches too. We came up with what we think is the best, but there were other approaches. We came up with a design that's been used around the world to do heating experiments, but because we started first, we had the longest running one. Now I've been told that somebody in England started doing what we did three years later. And if it's still running, and I don't know if it is, it will now be longer, because we stopped four years ago.

ABG: And you stopped because...

JH: We would have had to replace at three times the cost of what we initially spent to build the system — all the electronic equipment, all the monitors in the soil. We had this network of probes and data loggers that electronically record every 90 minutes, they record a temperature and a moisture reading at 180 locations. So that's a lot of electronic apparatus, and it was like a patient in a hospital, you know, with probes and stuff. So we just couldn't pull that off.

NSF funded us for 30 years, but at some point, they said enough's enough. And one reviewer actually referred to my age as an argument not to continue funding. Of course it's illegal to use age as a criterion in federal funding and reviews. But they did anyway, and we no longer were able to get federal money., Eight or nine of my students got their PhD working on this experiment, another half-dozen students from around the world, working with other mentors, did their thesis work at my experiment. It led to, altogether, about 15 doctoral dissertations, trained several 100 undergraduates, led to around 50 journal papers and several book chapters. So it was very productive, and it had a big influence in that it really woke up people to the importance of feedback mechanisms, in which, as the climate changes, the ecosystem changes in such a way as to make even more climate change. We call that a positive feedback loop. There were many other conclusions from the study, but that was one outcome that really resonated.

ABG: You have other field work in the Sierras and Tibet in the Rockies. Out of all of that, what do you look at that had the most impact?

Acid rain and the Rocky Mountain Biological Lab

JH: Let me give you a quick progression of my research trajectory. When I first got to Lawrence Berkeley Lab, I got interested in lakes and the effect of toxics, chemical pollutants, on freshwater fish and algae and plankton. To study it, we built what we called a microcosm lab. We got a big empty room up at the Lawrence Berkeley Lab, and we filled it with these huge tanks. Some were as big as a cubic meter. We could make as many of them as we want, and they were filled with actual lake water.. And then we could add pollutants to the microcosms and see how the life in them responded to toxics or other perturbations. We didn't have big fish of course because you couldn't put big fish in even cubic-meter-sized tanks, but you could have algae and zooplankton and small fish, and we ran those for several years. We got funding from the EPA and NSF and the Electric Power Research Institute, who were all interested in how to develop systems that can allow deeper understanding of how chemical pollutants affect life, ecosystems.

Previous studies involved putting a fish or perhaps a species of zooplankton in a jar and adding some pollution, and the tank has just aquarium water in it — not real lake water. So these were unrealistic test systems on a single species. We wanted to understand how the whole ecosystem responded to the toxics, and that's what we were doing for a few years. And that led to my getting interested in acid rain. It had been thought to be a problem only back East, but we had discovered, and others at the same time also discovered, that it was a problem in the West as well, in the Rockies and the Sierra.

So this was where **Kathy Tonnesen**, one of my first PhD students, came in. She was really the one who took on the field work on acid deposition in the Sierra, and she did beautiful work. Led to a lot of interesting consequences. It eventually landed her a job as head of air quality for the National Park

Service. She was an ERG PhD student. And her work helped lead to California passing what was called the Kapiloff Act, which was a piece of legislation that set up a huge fund to both study and deal with acid deposition in California. Both Kathy and I served on an advisory committee to the Kapiloff Act, advising the state government on how to deal with acid deposition. We had several other students following on Kathy's work, who were doing Sierra acid rain work and looking at how it affects plankton and how water chemistry affects life in the Sierra lakes.

And then in 1977 Paul Ehrlich at Stanford, who also has worked very closely with John Holdren, invited me to come out to a place in Colorado called the Rocky Mountain Biological Laboratory. I came out the summer of ’77 for just a few weeks to help Paul Ehrlich teach a course at the lab to undergraduates on environmental science. And in teaching that course, I took the students up to a place high above the research lab. It's called the Mexican Cut, and it's an Alpine ecosystem. It's about 10 miles away, and it's up at 11-, 12,000-foot elevation. It was a beautiful place. And I said, I want to keep coming back to the Rocky Mountain Biological Lab, but I don't want to teach. I do enough teaching in the winter. I want to do research here.

So I wondered about acid deposition in the Rockies. We put out some buckets to collect rainwater and then in the winter, snow, and measured pH’s around 4.5, which is very low. It's acidic. It's like orange juice in terms of acidity. So I said, Let's study acid rain. And so for the entire decade of the ’80s, we studied acid rain in the high mountain ponds of the Rockies. And we studied in particular its effects on a species, a very unusual species of salamander, called the tiger salamander.

An influential neighbor helps bring science to policy

And we wrote papers, students did their master's dissertations, and their PhD dissertations. We studied the sources of acid rain. We were able to attribute it to the coal-fired power plant at Four Corners and a couple of other coal-fired power plants. And it led to a whole lot of interesting research, and it had an influence on legislation at the federal level.

And the reason it did is: A resident of the nearby town of Crested Butte was a guy named Tim Wirth. He actually was a summer resident, not year-round. He was the US senator from Colorado, and he also loved the outdoors, and we would go hiking together. He came up and visited our research site and one day informed me that he was working on some important air quality legislation ... Amendments to the US Clean Air Act of 1970. The Original Clean Air Act applied only to sources of acid rain in the Eastern US, where acid rain was first discovered. Well, we were discovering it was a problem in the West. So he said, We’ve got to include this. You’ve got to come and give some testimony in a congressional hearing.

A take-down of Dick Cheney (after a harrowing journey), and protecting consequential Clean Air Act amendments

I ended up getting invited to a hearing led by a Republican opponent of the Clean Air Act amendments, a guy named **Dick Cheney** from Wyoming. He was a Congressman from Wyoming. And up at Jackson Hole, Wyoming, he held that summer, summer of, I think it was ’89 — he held a big hearing designed to shoot down the idea that acid rain was a problem in the West. So I had to get up to Jackson.

I flew with volunteer guy who had a Piper Cub airplane that flew people places for environmental causes, and we nearly died. We almost crashed. We were running out of fuel, and I had to almost force him, on pain of physical harm, to land the plane and refuel. It was a new plane he had never flown before and he said, With a new plane, you don't really know ... even though the fuel gauge is reading empty, my watch is really a better fuel gauge. And I said, But you haven't flown this plane, you don't know. So we landed, and the tank was empty, and we filled up and then flew the last 30 miles to Jackson Hole.

So anyway we get there, and there was a lawyer there, an environmental lawyer from the Environmental Defense Fund in Denver. He had read our stuff about acid rain and was very concerned about it. Yuhnke — Bob Yuhnke was his name. So Bob and I got together the evening, before the hearing, which was an all-day thing, and we came up with a strategy. I presented the scientific findings that showed there was acid deposition in the Rockies and it was harming life. Unambiguous results to any real scientist. Cheney got some hack from the Bush administration EPA that came and said, We've gone to 70 lakes in the Rockies, and we've never seen acidity.

Now we knew about what these guys had done. They had done their work by helicopter. They didn't climb up in the late springtime, when the acid pulses occur at snowmelt. He went in late August, when you could safely land a helicopter on the shore of a lake, and they would go pond to pond, lake to lake, take a sample, measure the pH: perfectly normal. Well, we saw the same thing in our ponds, and what happens is you get this huge pulse of acid as the snow melts; and the salamanders lay their eggs in the spring, so the eggs are bathed in acidic water in June and by August, the lakes have recalibrated. The chemistry is buffered slowly, and by August, they're back to normal pH, pH 6 or 7. But in June, they can be down at pH 4.9, 5.0, which is lethal to developing salamander eggs.

So after this guy presented his thing, Bob Yuhnke said, I have questions for both of you. What time of year did you do your studies? And he was forced to say August. He didn't say it in his presentation. And then he asked me, What time of year did you do your studies? And I said June and July and August. And Yuhnke asked,was June the same as August? And knowing the answers, he led me through this thing that just completely crushed Cheney. His hack was completely demolished, and the hearing had no impact on boosting the case for opposition to Wirth’s amendments. And the bill passed. The legislation eventually was implemented, and the Four Corners power plant has basically shut down. Other plants in the West have cleaned up their act. The Douglas smelter and Arizona moved to Mexico, and the problem of acid deposition in the Rockies has greatly ameliorated. And Tim Wirth deserves enormous credit for his work.

It was a beautiful success story. As satisfying as the Everglades work had been. It got dozens of students trained to do environmental chemistry, salamander biology. The students who did that all went on to important positions. One of them became a top scientist in the EPA. Another became head of air quality for the National Park Service. These are all ERGies.

Salamanders: The germ of a 30-year global warming experiment

At the end of the ’80s, I said, Assuming that this all works as we hoped it would — and it did — then what's going to happen to these salamanders, which I had come to sort of love? And I realized global warming will do them in. And what else will global warming do to ecosystems? In the late ’80s, people were not asking that question. But at ERG, because of the way ERG is structured, we're not confined to just looking at a sub-discipline within the discipline of ecology. We could look more broadly. And I got very interested in, more broadly, what climate change will do — not just to temperature and to human suffering directly, but to ecosystems. And so I said, how would we learn about that? We could make mathematical models. But you know, garbage in, garbage out. How do we do it responsibly? And I said, we’ve got to simulate global warming, just like we simulated acid rain with microcosm studies. But anyway, I said, Well, how will we do that? And then the Ghirardelli Square inspired this experiment.

And then for 30 years we were heating a subalpine meadow and watching enormous changes in the amount of carbon systems stored, the dominant vegetation, the timing of flowering. Basically the heating experiment turned a wildflower- dominated area into a sagebrush-dominated area. It was hugely dramatic.

Consider a Spherical Cow

JH: The next thing on your list is spherical cow. This is the new edition. it just came out, and it's 30% bigger, more material. It includes toxics and radiation and pandemics.

ABG: So this is a revision that you did?

JH: Yeah, I did this all last year.

ABG: Well, let's consider a spherical cow, then, since I'm holding it in my hand...

JH: The concept came out of the Everglades work. Remember: Simple models, the U-tube. So this is a book of analogs of the U-tube for practically every conceivable environmental problem; how to think about them in a simple way that gets you a clear, direct understanding of the science behind it, so that you can think responsibly about what to do about it. It doesn't tell you what to do about the problem. It's not about solutions. It's about understanding environmental science.

ABG: And when did you first write this?

JH: This is the first edition [shows book], and that came out in 1985. This is a second printing or a third printing of the first edition, but this [shows new book] is a brand-new edition. Much more material, and 101 new homework problems, and a solutions manual is about to come out with the solutions to all the homework problems in the book.

ABG: So it's a straight-up textbook.

JH: That's right. And it's used around the world, and it's been translated into Japanese.. It sells a few thousand copies a year, but it's done that every year since 1985, so it doesn't go out of date.

ABG: I saw in the box of material that I went through at ERG, there was a retirement card that a lot of students had written you notes on, and all of them were riffing on the spherical cow in some way. What do you think captures the imagination of students about that book?

JH: Certainly, they have a spherical cow award every year at graduation. Do you know about that? It's a rubber spherical cow [laughter] that you can sit on and bounce. It goes to staff sometimes, sometimes to a faculty member. I was the first recipient of it, but now it's an annual thing. It's become a name like "Jello" or "Xerox," a generic name for approaching problems by abstracting away the unnecessary complexity and reducing it to its essence, and then using relatively simple math to draw conclusions. That's the idea, and it really originated with the Everglades work, and it's the required textbook in a whole lot of courses around the world. I've written eight books. This is the only one that really keeps selling.

ERG memories from the 1980s and ’90s

ABG: You talked a little bit about how you first heard of ERG when you went to the lab and John Holdren came to see you. Can you talk about your earliest memories of interacting with ERG? You said you became an adjunct professor there in the ’70s. What are your earliest memories of what they were doing, what the students were like?

JH: For most of the ’70s, I would spend most of my day, Monday through Friday, at Lawrence Berkeley Lab, because that's where my lab was. I had a group of about 10 research assistants working in my group under my mentorship. So my main responsibility was the Lawrence Berkeley Lab, and I was very involved. I was more involved in helping the Energy and Environment Division at the lab survive and prosper, which, as with ERG, was always an issue. There were people at the lab who thought it was crazy and who didn't have respect for what we did — just like ERG. So I put most of my effort in the '70s into working with the Energy and Environment Division at the lab.

My second most amount of effort probably went into helping in the College of Natural Resources with what they called the CRS program, Conservation and Resource Studies. I taught a course for them pretty regularly, in addition to co-teaching with John at ERG and then teaching 202 on my own. But my third most effort was with the nascent ERG program.

ABG: Did you have ERG graduate students?

JH: I had students, and that was my major connection to ERG in the ’70s … to work with students, not to be involved in the committee structure of ERG. I did serve on the prelim committee and the admissions committee and the executive committee of ERG itself, but not the campus-wide committee that created and oversaw ERG. I did not work with that committee.

ABG: Did you go to the faculty lunches?

JH: Oh, yes, I went, not all the time. Sometimes I found the faculty lunches a little boring — it was very rare that they talked about specific scientific issues. They would talk about who's advising the AEC about nuclear safety issues... it was all very high-level stuff. And I was maybe in more of a down-to-earth mood at that time. But I would go. I went to the colloquia. I don't remember any particularly memorable colloquium from the ’70s. I remember much more from the ’80s and ’90s.

Taking down scoundrels with sharp arguments

ABG: What are your biggest memories?

JH: I can't remember the year but one of the things that got everybody fired up in the late ’80s was a paper that appeared in the journal Science by a Berkeley professor named Bruce Ames. Bruce Ames was a biochemist, and he developed an absolutely brilliant test called the Ames test, of course, to test for whether a chemical could cause cancer. And the idea was, chemicals that cause cancer, often, not always, but most of the time, also cause mutations. It's hard to quickly study cancer because it grows slowly, but mutations happen very quickly. So he said, let's find a bacteria which we can put in a petri dish, and if we add the chemical, it will mutate. And we can see the mutation occur. There are lots of ways to do that. Like a mutation that causes the bacteria to starve, that's easy to see because they all die unless they mutate.. So using bacteria and their mutation rate when you add a toxic substance, he was able to test substances for cancer in humans, and it was about an 80% overlap between mutation in this bacteria and cancer in a human. It was really quite an achievement.

And then he went over the edge and misused his own invention to draw an unsubstantiated conclusion, which is that pesticides aren't half as bad for you as organic food. Eating organic apples will kill you, he'd say; eating pesticide-sprayed apples will keep you healthy. And he went on and on about this, and the Ames test sort of suggested some basis for him saying this, but he went way beyond what you could logically conclude and ignoring all kinds of aspects of how we've evolved with the natural carcinogens that are in natural foods, but we haven't evolved with Dow Chemical's pesticides.

Anyway, we invited him to give an ERG colloquium, and one of our students, a particularly sharp PhD student named Andrew Cohen, who was very interested in aquatic ecosystems, read up on the paper, and I urged him to come to this talk because I knew he was smart, critical, and articulate.

And at the end of Bruce Ames talk in the question period, Andy just let him have it. He demolished the entire talk. Left Bruce without any defense for his arguments; it was a totally brilliant performance, and it was well attended. People knew that there was likely to be this confrontation. I was going to do it myself. But then I thought, no, Andy is so good. Let's, let's give him a chance. You know, when I was young, I got to do some things that I'm glad people let me do. So Andy did this, and he did a better job than I would have done in demolishing Bruce Ames. So that was really fun.

I made a note about another colloquium that was interesting. We invited another person who we had a lot of skepticism about, His name is David Keith, and he's a proponent of geoengineering the climate, of building machines that can pull carbon dioxide out of the atmosphere and/or putting shiny stuff up into the upper atmosphere to reflect sunlight, darken the earth enough to cool it, to compensate for global warming.

ABG: What could possibly go wrong?

JH: What could possibly go wrong? This is something that we used to call spherically senseless — no matter how you look at it, it makes no sense from any direction. But anyway, he had made this argument that even if we completely switched tomorrow to solar energy, no fossil fuel burning, the current CO_2 would still stay there in the atmosphere, and the planet would not cool and would be too hot. We'd continue to have dangerous hurricanes and heat waves and droughts, and the glaciers would keep melting. We'd keep losing the ice cap and sea level would keep rising.

And it was a completely bogus argument. If we were to zero out emissions, natural carbon sinks — the ocean, forests — already take out each year half of the carbon we emit. We emit about 10 billion tons of carbon as we burn fossil fuel each year. At the end of the year, only 5 billion tons are up in the air. The other half, 5 billion tons, are removed from the air by natural processes and go into the oceans and into soil and forests. But those sinks, he said, will disappear the minute we stop burning fossil fuel. And that's wrong. It's just completely wrong. He doesn't understand that this year's sink is not driven by this year's emissions. It 3s driven by the cumulative excessive amount of carbon already in the atmosphere. So it will keep drawing down, and will go from the current 420 parts per million to 410, to 400, and we'll end up down around 340, maybe which is where we were around 1980. So nature will heal the atmosphere slowly but surely when we stop adding more carbon to it. And he just wouldn't accept that argument, even though it's widely accepted and it's true. And so he defended the necessity for doing geoengineering on the basis of the bogus argument that otherwise will be stuck at 420 parts per million for decade after decade.

And so we invited him to give a talk to tell us what he thought about geoengineering, and then we lammed into him on this point. Thus my most memorable colloquia were ones where we had folks presenting misleading information, and then an ERGie, with their better understanding of how nature works, confronting them.

ABG: Did such folks stop accepting invitations?

JH: Probably not. I was invited to Harvard to give a talk about four years ago, and he showed up, and we talked at the reception, and he didn't bring up the infamous ERG event. But anyway, those were some of the fun colloquia that I remember, when things got confrontational. And that's very educational for students.

ERG core courses note

ABG: You mentioned developing 102 and 202 and how those courses came about. Do you have anything else to add about those core courses?

JH: It was really emerging [out] of what Holdren brought, and it was actually Holdren who came up with the designation of ER 102 as "Quantitative Aspects of Global Environmental Problems." It's a bit of a mouthful, but it was the technical name of the course. And rather than him joining in and doing Physics 80 with me, I brought what I did in Physics 80 into the course he created. So that's really the origin of the 102 course but I brought an equal share of new material. So it became a hybrid with the climate science and ecology that I brought to it along with what he brought. ER 202 was strictly my baby.

A strange night with Jerry Brown; seizing the opportunity to pitch policy

ABG: While we're on the Holdren topic, the next thing on my list is that very surprising and super entertaining story about getting a call from the governor saying, Would you please come be the vice chair of the his new CEC [California Energy Commission]. Apparently they were sitting there in the office, and Holdren was saying, well, I won't do it, but see if you can get Harte to do it. Do you remember that incident?

JH: Very well. I was living in Buena Vista Street, and my girlfriend at the time and I were just going to bed. It was, if I recall correctly, quite late at night, and I get this call.. Jerry Brown kept weird hours — he was definitely a night owl. And they said, Can you come to Sacramento to interview for a position with the Energy Commission. And I said, Yeah, tomorrow morning, I'll drive up. And he said, No, no, no — now.

ABG: And it was Brown himself? It wasn't an assistant?

JH: I can't remember for sure. John would remember. And so I get in the car and I drive to Sacramento. You know, roads are empty at that time, we're talking midnight, one in the morning, and I get there, and I go in a back door to the capitol building. They told me to go in around the back, and there'll be an unlocked door, and you go up three flights, and there'll be a room, and you'll meet Jerry Brown there.

So I walk into this room and there are 20 or 30 people sitting on the floor sort of cross-legged, a few people in chairs, but there weren't enough chairs. And then Jerry Brown, and this guy who appeared to be a guru. I assumed he was Jerry's mystic associate or something. . I sat there, cross-legged of course, and I listen to Jerry going on about philosophy of governance; it was certainly not an interview for a particular position. Holdren's also sitting there. There's some opportunity for Q and A, so I ask some question or other. It was completely chaotic and weird. I had no idea... this was not a job interview. It was "Get to know Jerry Brown by listening to him expound about all kinds of things." And then he'd call on people in the audience to interact. He didn't call on me, as I recall.

Around three in the morning maybe, he called it quits. I chatted with him very briefly and then John asked if I could give him a ride home. So he and I drove back to the Bay Area at four in the morning. And we both agreed we really didn't want to do this. It was just too wacky, and I didn't want to give up research and teaching.

But I recently found something I had completely forgotten about. I went through a couple of file drawers of stuff from the ’70s, and I found a two- or three-page letter I wrote to Jerry Brown shortly after. And it was saying that I don't want to do it, but I want to propose ideas for sensible things California can do, unlike the proposal to build the New Melones Dam, which was a terrible idea. We're doing things that are harmful for the environment, I wrote. Let me propose to you a list of.smart things … and I presented about a dozen projects that the state could do that would be good for the environment, that would create jobs, that would be good for the economy. Win-win projects, like restoration of San Francisco Bay, restoration of oyster industry on the bay, which requires clean water, groundwater restoration which would be good for everybody., And I gave a lot of thought to it.. Later I heard back and some of them were of interest to them. Unfortunately the New Melones Dam, still got built. The Stanislaus River was dammed against our wishes.

Diane Feinstein in her prime

When I was thinking about that, it reminded me of something else that was very interesting that happened around that time. Both Dick Norgaard and I were invited to Sacramento to testify about why the New Melones Dam was a bad thing, why we didn't need it for power or drinking water or flood control, and why it would do an enormous amount of harm to recreation, to wildlife, fisheries, everything. So I went with a bunch of prepared comments and gave a talk to a committee of the State Senate, or maybe it was the assembly, but a legislative committee. Shortly thereafter, I got invited by the San Francisco Board of Supervisors. The board invited me to talk about this — just me, a closed hearing, not with other people. So I went to San Francisco and talked to the board.

ABG: What year was this?

JH: I think mid or late ’70s. Dianne Feinstein was on the board, and also Quentin Kopp.. He was the one conservative, the one Republican-type guy on the board. I gave my little presentation: Why San Francisco shouldn't be asking for this — because the pressure to build the dam came partly from the city — and how they could easily meet their water needs for a century without it. Kopp said, What nerve you have. You live on the other side of the Bay, and you're coming over to our side of the Bay to tell us how we should get our water and how we should run our city. I was dumbfounded, and before I could think of a good reply, Dianne Feinstein said, she called him by his first name, and she said, You know Quentin, Professor Harte, lives on the same side of the Bay as the dam. It was great. I was full of admiration for her after that.

ERG students’ rigor

ABG: Let's circle back to something that you talked about: developing the preliminary exam. Basically, ERG has been on the defensive, both academically and to retain its independence.

JH: I wouldn't give me credit for developing it, but I played a role in making sure it was rigorous and administering it to students. And one of the things that I realized it was useful for was conveying to faculty, especially in the hard sciences, in other departments, that ERG was as rigorous, as tough, as demanding as their programs. Because there's a tendency for people to think, if it's interdisciplinary, it's fluffy. There's no substance; breadth, but no depth. And our prelim was one vehicle — there were others too, which I'll mention — but it was one important vehicle for conveying to other faculty that ERG students really know what they're talking about when it comes to the sciences; that we train them in engineering and biology and physics and economics as rigorously as you would expect from students at the same stage in other departments. And sometimes we even found faculty had trouble answering some of our questions in their own field, on our prelim. Not often, but occasionally. So it was a vehicle for convincing the faculty at Berkeley, other departments, that ERG was serious, that we weren't just doing lightweight, unscientific work.

ABG: You said there were other ways that you demonstrated that...

JH: Our students were the best vehicle because they would go and... We urged our students always to have faculty on their committee, their research advising committee, from other departments. They would have to go and talk to faculty in other departments; they would take courses from other departments and they would consistently impress faculty in other departments with how smart they were. And so faculty from other fields looked forward to having ERG students join them — to mentor them, to take their courses, and just chat at office hours. The students became their own best salespeople.

Keeping ERG independent: A preemptive committee

ABG: You also mentioned a committee that you developed later to defend ERG's need to be independent.

JH: One of the things we had always had to fight against was ERG either just being completely dissolved or merged into some other unit on campus. And we did not want that. We would lose the character of ERG   We connect with every department through our affiliated faculty. And there's no one area where we would be at home. It's not engineering, because only a quarter of what we do is engineering; not biology and ecology, because that's not all we do; not economics, that is not all we do. So we need to have the kind of independent structure that we've enjoyed so far.
But there was resistance, because we would be a small unit reporting to a very high-up administrator. We didn't have a dean. We had a vice provost or a provost that we report to. And for someone that high up in the administration, we're sort of an incidental pain in the neck. And we don't have an advocate. A dean advocates for the departments in the college that the dean is dean of. We didn't have an advocate at that level. So, that was an argument that they would make: If you become part of the College of Engineering, you'll have a dean, and he'll do better for you than the provost can, because the provost has too many other important things to do, to bother about you.

So to keep our independence, John Holdren would fire off these periodic letters when we were under threat. And when I became chair — I was chair from ’96 to ’98 — for those two years, and the threat built up, especially when John left, some people thought, you know, John was very important figure in ERG, critical to ERG at the beginning and throughout. So if he leaves, maybe that's the end of ERG. So we had to deal with that.

My idea as chair was, even though we weren't due for a new external review, to call one, specifically to focus on this issue. Every 10 years department get external reviews, in theory, it doesn't always work at 10-year basis. So we put together a document which I can show you. You may have seen it. It was written in ’96 and it's a document about the future of ERG and why we should be independent. [Harte-Options for the Future of ERG-1996] We had the university organize for us an external review. I recommended some possible names of people to invite — objective and smart. I knew that if we have a good committee and we do a good job presenting the case, we'll win. And we did. The committee unambiguously supported our maintaining our independence. And we did that for another decade or more, but then at some point, I don't know how much you've gone into with other faculty to the reasons why we finally merged.

ABG: Not at all.

JH: Not at all — I'm surprised.

ABG: Well, Holdren was gone by then and Dick and I just didn't get to it.

Why ERG finally merged with a college: CNR

JH: Well, everybody will have a different take on it, yeah, so I'll just give you my take. But I'm not speaking for ERG now at all. I'm speaking just for how I viewed the merger, and it was the practical issue of having an advocate, combined with the fact that we were going to lose our space in Barrows Hall, and we had nowhere good to go, except possibly if we merged with CNR, we could get decent space In Giannini Hall or one of the other CNR buildings. That, combined with the fact that I had an enormous amount of confidence in the dean, led me to think that we ought to do this.

[Side discussion sorting out who was dean at the time of the merger.]

JH: But the idea of having an advocate and having assured space, I thought, was pretty compelling. You should definitely get other people's views about why we acquiesced. It may have been they at some point said, You have no choice. It doesn't matter who you bring in from the outside, or how long a letter you write.

[Side discussion recalling a Fall 2011 Breakthroughs news item welcoming ERG to CNR.]

JH: Both **Gilless** and **Ackerly** were good. I'm especially pleased with David. But Keith was a good dean, and there were deans before him who I would have worried about at the time. I think at the time I felt comfortable with Keith, and then I felt extra comfortable with David.

ABG: During my period at CNR, so much of the work was truly interdisciplinary and cooperative with professors from different departments. Could it have been that it was felt, at that point, that they really understood the importance of...

How ERG is different and why that's important

JH: Different from ERG, though. I don't think there was a substantive change in interdisciplinarity between, say, 2005 and 2011. I think CNR has slowly, over time, become more interdisciplinary, but not like ERG. ERG still is quite different. Some of the differences might seem minor, but they're important. Like, when we admit students, it's not to work with a given professor; it's to become an ERGie, and then you can decide over the next year or two what you want to do, who you want to work with. In ESPM, you get admitted into a lab group, which narrows the focus of students, inevitably.

ABG: How does that make ERG students special?

JH: Because they can figure out what they really want to do. They're not channeled, before they even get to ERG, into a program, into a research slot. By leaving it open, it can be influenced by the courses they take when they get to Berkeley, by the people they talk to, by the opportunities they discover. See, if you're an applicant from some other university, paticularly in another country, you don't really know what's going on at Berkeley, and so in most Departments you have to pick a faculty member and say, I want to apply to your group. And if you want me, please let the admissions committee know, and then the admissions committee will decide if I get in. At ERG, you just apply to ERG, not to a group, not to a mentor. So that's a big difference. Another thing: Everybody in ESPM is working with within the research domain of one of the active research groups, and this is pretty much true across the whole campus. At ERG, we've had many students who come in and do something completely different from anything currently going on. So ERG grows in its scope, in the domain of what it studies over time as the students bring in new interests, rather than continuing to pursue the same interest decade after decade.

ABG: So it's really driven by students intellectual curiosity.

JH: Students drive the direction of ERG... to a great extent, not completely. Sometimes faculty interests, like climate in the 1980s, result in new directions. But often it's students who then take that and say, I'm really interested in climate and international relations. And that brings in a new area of research, and it ties in with somebody from political science, like the late Professor Ernie Haas or someone. We grow and expand organically, rather than stay in channels. So that distinguishes us not just from the rest of CNR, but from the whole campus. That's a unique thing about ERG, and so far, we've been able to maintain that within CNR; we stand out within CNR, just like we stood out within the university before (and still do), so it hasn't harmed us.

ABG: Just to ask an obvious question, but to get it on the record: Why is that a good thing to stand out?

JH: Because it opens up new investigations, new insights. It leads to whole new programs, new efforts, things like you mentioned here, Globstab, and other things. These would not happen in an ordinary department. These happen when faculty want to expand the breadth of their knowledge and interest and create new areas. Think about a topic like biophysics. Now there are biophysics journals, there are departments of biophysics all around the world. Didn't used to be; used to be there were physicists and there were biologists, and then some people in physics and/or biology got interested in each other and created a new subdiscipline. It happens outside of ERG, but in ERG, it really happens. We're a factory for generating these kinds of new directions, and our students play a big role in that.

[side time-keeping exchange]

Joining ERG's core faculty

ABG: I want to make sure that we understand how you came to join the core faculty and how that impacted ERG and impacted you.

JH: I was very involved with ERG before, in that early era — by 1980, '81, '82, I already had a large cohort of students from ERG. So by the early '80s, I was up to my elbows in ERG, and had pretty much disengaged from the lab. By the early 1980s I didn't want to work at the lab; I really wanted to be on campus, and ERG seemed like the right program. Holdren really wanted me to be a faculty member and I was acting like a faculty member. ERG stands on four legs: environmental science, energy and engineering, economics, and social science other than economics, including policy. And the only person who was serving as a faculty member deeply into climate and ecological environmental research was me. So as one of the four pillar topics of ERG, it made sense. John used to refer to it as de-adjuncting me because I was an adjunct professor. So let's get rid of the word adjunct. And they got through a 50% appointment, and then I got my other 50% from ESPM, which was not ESPM at the time, it was plant and soil biology, I think. And that's been true until I retired, that I was 50% ERG, 50% ESPM.

ABG: And that was within the College of Natural Resources?

JH: Yeah. So it wasn't like a big deal to become a professor. I was acting like a professor, and they just deleted adjunct and I was a professor.

Faculty collaborations

ABG: You already mentioned these student names that you have here. So did you want to from here talk about your faculty collaborations that were meaningful?

JH: There's also a difference between ERG and say, ESPM. In ESPM, you have the ecosystem sciences division within the department, and then you have the social science group — policy and social science. They don't really collaborate much. They're separate. It could be different departments within ESPM. One of the nice things about ERG was the opportunity to work with other faculty — to meet with them, to do research, to co-teach — from completely different disciplines. So the first one of these efforts was something John Holdren and I did back in the '70s, late '70s, which was looking at what we called integrated assessment and how to think about the whole complex problem of the impacts of, let's say, a policy decision on society. How do you go from some policy decision like, We will deploy the supersonic transport, to all of the repercussions. And it's a huge problem, because there are multiple ways in which any policy action can impact society. We saw that in the Everglades work. Who would have thought you'd lose your drinking water supply if you build an airport? So how do you develop ways of thinking about policy evaluation that don't let critical issues fall between the cracks? How do you comprehensively, systematically think about evaluating policy? How do you do policy analysis systematically and comprehensively? So we, with a bunch of students, developed and wrote a bunch of papers about ways to do a better job of thinking comprehensively about policy. That was one collaboration. I wouldn't say it was hugely influential. It was good for our own thinking to work through this stuff.

But then there was Globstab and that was much more fun. It was a group from political science, economics, and ERG. Me from ecology, Dick Norgaard, economics, and Gene Rochlin, political science. We were the three from ERG and then Ernie Haas and others from political science, like Todd LaPorte, and these were all people interested in what everybody else was doing, so it was a great group to collaborate with. We were looking at how insights about stability and resilience that came out of ecology could be applicable to international stability and international relations. I had done a lot of theoretical work on looking at what contributes to the stability of an ecosystem. Why are some ecosystems fragile and others robust? And since ecosystems involve the interaction of a lot of pieces, we can think of the world stage as a huge ecosystem of nations, and what kinds of structures and institutions and relationships would make international relations more stable, less prone to outbreak of war. We would have dinner once a month at the Faculty Club, and we would just talk about this problem. It went on for several years. Led by Gene we tried to actually write a book, but we just couldn't... the discussions were so wide-ranging and different from month to month that it was hard to put it into the structure of a book. But it was fascinating, educational event for all of us.

Another thing like this was one I did with Tony Fisher, who was an ERG professor in Economics. Later, he wanted to join the Economics department and leave ERG, and we swapped him for Dick Norgaard, like baseball teams swapping players. And the topic of this project I did with Tony Fisher was how to incorporate uncertainty into economics, and it was something we had thought a lot about in ecology and environmental science. When you don't know things, how do you let that uncertainty or ignorance influence the policies you create? And so for a couple of years, a bunch of students and Tony and I, and a guy named Michael Hanemann, who was an economics prof here. He later moved to Arizona. But we would meet, talk and learned a lot from each other.

ABG: Did that group have a name?

JH: It probably had a name. I just don't remember. And after that Dick Norgaard and I put together a group to work on a project which led to a publication in the Proceedings of the National Academy of Sciences on the idea of the ecological footprint, or "eco-debt" of nations. How much do nations owe other nations because of their destruction of the global ecosystem?

ABG: That's still a big topic.

JH: Yeah, it's a huge topic. And it was fun. We had about six students working on that with us, one of whom, who was a postdoc of mine, was the first author of the paper, and a bunch of students were involved, and Dick and I. You can tell immediately that's a very interdisciplinary topic, and it brought together a whole lot of interesting people with interesting ideas. So those are some examples, and they don't happen in ordinary departments. I used to say that it's these events that put the "uni" into university.

Reflections on ERG students and being a part of ERG

ABG: That brings me to a broad-based question of how ERG or ERG students, or all of it — being in such a unique place — how that has impacted your thinking? Your systems thinking, your specific thinking... Are there any reflections in general about what it's meant to your career to be as part of something so unique?

JH: It's a question that reminds me a little bit of trying to ask, Can you reflect on how being married has affected your life? You know, it's huge. My whole career is really based around me helping to define ERG and ERG helping to define me. It's a total feedback system. Hard sometimes to even differentiate what I did for ERG and what ERG did for me. It's an organic whole. It's a unique program. There's still nothing like it, as far as I know anywhere in the world that has produced such amazing students.

Something interesting that I never would have guessed when I was a graduate student... when, I knew I wanted to be a professor — it seemed like a good life, and I wanted to do research, and I wanted to do physics then and then later I wanted to do ecology and environmental science. But I never would have guessed that I would end up training students who went on to such a breadth of careers. About half a dozen of my students are full professors at some university, some of them in traditional departments, some of them in interdisciplinary departments that aren't quite like ERG but are getting close. But then one of my students, Deborah Jensen, went on, immediately after PhD, to become vice president in Washington, D.C., of the Nature Conservancy. Another student is now the director or president of the World Resources Institute. Another student Jennifer Dunne, is vice president of the Santa Fe Institute for the study of complexity. The students have gone on to administrative positions that I never would have thought my students would do. I didn't train them to be good administrators, but the academic training they got helped them become really effective administrators. So the broadened intellectual horizons make you a better administrator. And that was something that came out of ERG and very few departments have produced such a large percentage of students who went on to major top administrative positions. Andy Cohen, who was the one who confronted Bruce Ames, was elected to the EBMUD board, the East Bay Municipal Utility District. He ran for office and won several cycles of election as a water board member. These are not things that happen often in other departments, so it's one of the interesting little features of ERG that I didn't expect, but I'm really proud of. I think it's great.

ABG: Is there anything I didn't ask you about that you want to get on the record? Anything else you're most proud of when you think of back to ERG in all the decades?

JH: I think if you ask any ERG professor, they'll say something about students when you ask them, What are you most proud of? It's the students that we've all mentored and the careers they've gone on to have just vastly multiplied our effectiveness in making the world better.

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