American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview with Robert Cowen

May 29, 2001

Interviewer: Gene Bierly

TAPE 1, SIDE 1

Bierly: Good morning. This is an interview with Robert C. Cowen. The date is May 29, 2001, and this interview is being held in the John B. Hynes Convention Center on Boylston Street in Boston, Massachusetts. I'm Gene Bierly, a senior scientist at the AGU in Washington, D.C. Bob,

I know that you were born in Concord, New Hampshire, in 1927, but I really don't know anything about your family. I always thought you were a born and bred Bostonian. So perhaps you could give us a little background about your parents, what they did, and a little of your early family life.

Cowen: Well, I really am a born and bred Bostonian because my parents were living in Boston, but went to Concord, New Hampshire, hospital for the birth. My father was born in New Hampshire in a little town called Boscoyne (?) which is about six-seven miles from Concord next door to Penacook. My mother was from Vermont but had lived a long time in New Hampshire so they were New Hampshire parents living in Boston at the time I came along. Except for that brief stay in the hospital, my birth certificate would read Boston.

Bierly: OK. What did your dad do?

Cowen: My dad was a printer for Forbes Lithograph. Now you probably can think back to World War II and after the invasion of Europe, what were they going to do for money? The U.S. government issued a thing called "scrip," if I remember correctly. My father printed scrip.

Bierly: That's interesting. I do remember that scrip, yes. So, did you have any brothers or sisters?

Cowen: No.

Bierly: You were an only child.

Cowen: That's right.

Bierly: That makes for an interesting childhood and makes for an interesting life.

Cowen: It does and perhaps it would be interesting to know that I grew up two blocks down the street from the Hynes Center on Huntington Avenue, right across from where they built Northeastern [University]. I watched them build Northeastern. So I could walk to the Museum of Fine Arts. I could walk to Symphony Hall, I could walk to the great Boston Public Library. I could walk to Latin School and most importantly, I could walk to MIT, so I didn't have any living expenses at MIT.

Bierly: That's wonderful, wonderful. Before we get to MIT are there any schoolteachers or other people in your early life that you feel may have had a very strong influence on you.

Cowen: Well I think the whole Latin School experience did, and the teachers there were all very, very good including one Latin teacher, Mr. Roach, who was a holy terror to the students because he knew every ?trot and students had an attitude toward him, but my attitude changed very markedly one day when I was walking down the hall past his office. The door was open. He was a little guy and he had this tall teacher, and he was chewing him out, and this was important because it shows the atmosphere and outlook and expectations of all of us high school students at that time, and Roach was saying, "This young man is going off to war to defend us, and may never come back. You can **give** him a passing grade." Now that little vignette sort of captures the atmosphere of my youth. All of us who grew up in that era did not have what later became a normal teen age. Our expectations beyond high school were the flag, and some of them were going even before they graduated. I did not go because I was 4F. However, to skip ahead, I later did get a commission although I never served a day in the service. They just gave commissions to everybody graduating in meteorology at MIT.

Bierly: Well, you're right. You had talked to me about this before, and I've been thinking about it since—that just being four or five years older then I am would put you in that mindset because even I and my classmates felt some of that same pressure, too.

Cowen: Yes.

Bierly: Well, OK, so you did go on to MIT. Did you always expect to go to college? Had that been an aspiration—

Cowen: No, no, because as I said, our only expectation was war.

Bierly: So how did you get to MIT?

Cowen: Well, now that's another interesting story and very typical of the kind of career I've had. One day out of the blue, my classmate Jake Shapiro said, "We are in the upper twenty percentile of the Latin School graduating class. We can get into MIT without an exam. Try that today." This was something like late April or early May, so we wandered over to MIT and we are going around the corridors peeking in classrooms and laboratories, and somebody saw us and asked what we were doing, and we told them. They took us down to the registrar and he says, "Well, you know its pretty late to register but here's the material; do it anyway." And about six weeks later, we were at MIT because on a wartime schedule they were running year round, so our first semester was in the summer.

That's how I got to MIT.

Bierly: Good. When you went to MIT, did you decide on meteorology right away, or how did you decide on meteorology?

Cowen: No, I didn't. I had been interested in aircraft and so forth as a kid. It just looked glamorous, so I went into aeronautical engineering and that turned out to be acres of engineers and so forth, so I found out about the meteorology department. There was no Earth, Atmospheric and Planetary Sciences department then. Meteorology, by itself, part of the engineering school, and not the school of science. So I went over to the registration officer, who turned out to be Tom Malone. I believe most people now in our science would know Tom Malone, but in those days, he was a young guy, thin, and looking at me and, "Why do you want to go there?" Well, I thought I would learn about the atmosphere that had something to do with flying, but also it would be a science and it was a small department. So I was in the meteorology department and that was again another rare—not unique—but rare experience at MIT.

Bierly: Two things about that I would like to think about. First of all who was on the faculty at that time, and then secondly, as an undergraduate at least who were some of your classmates or your peers.

Cowen: Yes.

Bierly: With you—

Cowen: I should point out that the meteorology department went on and off about undergraduates as to whether it was better to just get a good solid science

background and then do meteorology or not, but at the time they had an undergraduate program and among the people who were there were J. Murray Mitchell, Jr., who became a famous climatologist, and L. Fowler and Dick Shotlin and several others. The big point though about the department, particularly at that time, was it was small: two floors in a building at MIT, so everybody was together. This is what was rare at MIT. You were there all the time with the faculty, with the graduate students, and with other students who were taking the same courses you were, but it was no "Joe College" atmosphere. I would look at the students on my left and he would [be] a Major, I believe; I would look at the student on my right and he would be a senior civil servant. One of them was from New Zealand, John Gabortise, who became head of their what corresponded to their national science foundation. So we had maybe three or four, maybe six of us typical undergraduates mixed in with these mature and senior people, so there was no Joe College atmosphere. It was all—I wouldn't say overly serious, but it was business like and you learned a hell of a lot that you didn't learn in other courses at MIT where it was a bunch of undergraduates, until the following year. Here again the wartime generation, especially those of us who were just at the end of it, had a totally different college experience than a lot of people have had since then. Again the Joe College atmosphere was toned down because all of these returning vets were there, and in other classes they weren't seeing people who were still serving the way I was, but they were seeing people who had been through it more mature, and I am astounded now to read about suicides at MIT, and so forth, because people feel pressured. These guys had been through hell, and they weren't going to commit suicide over a calculus exam. It just made for a serious get-on-with-it, can-do atmosphere.

Bierly: Well, how about some of the faculty?

Cowen: OK.

Bierly: [Can you talk about] them a little bit?

Cowen: Now you had asked me about Rossby. Rossby had gone by that time, but the Petterssens would come through occasionally and visit, but on the faculty, the first one that comes to mind because he just died was Jim Austin who was doing climatology. I don't know how many meteorologists realize that Jim was the first TV meteorologist in this area, doing things with flip charts not with fancy graphics. He was the graduate registration officer, but he also took some of us in on his projects so I earned a little money as a sophomore. Then Henry Houghton was the head of the department. You had Bernhard Haurwitz for dynamics, you had of course, Herb Riehl, who taught general meteorology but Hurd

Willett…Delbart Kiley did instruments, and the one who had the biggest impact on me, Victor Starrr, and I joined his project.

Bierly: He had just come not too long before.

Cowen: That's right, that's right, and I joined his project as an undergraduate. Now, consider the typical undergraduate experience where you are thrown in with teaching assistants and all of that. Here we were a small group on this project: Bob White had the next desk, Ed Lorenz had the next desk, and there was tradition in the department that at lunchtime there was a bridge game. Victor didn't like bridge, we didn't like bridge, we took off to lunch, and so here is this undergraduate sitting with people like Bob White, Ed Lorenz and Victor Starrr, and they are ranging way beyond meteorology and talking about general science, and Victor is applying dynamics to galactic rotation and so forth. That is not your typical education experience at MIT.

Bierly: Well, it's not the typical education experience anywhere.

Cowen: No.

Bierly: So you were a very lucky person.

Cowen: And I learned some life lessons there from Henry Houghton, which are amusing, but very important. I happened to be a personal friend with Harlow Shapley, the Director of the Harvard Observatory, who was one of what they used to call a physical scientist, and I knew Harry Wexler, the chief scientist with the Weather Bureau. Harry wanted to meet Harlow, and my memory is he wanted to talk about setting up a world meteorological organization and how to go about it. So I said, "OK, I can get you guys together for lunch." Well, I did this and the correspondence I had went through the department secretary. So Uncle Henry calls me in and he gives me the typical lecture: "Why don't you work on getting your degree." Life Lesson number one: If you are doing something outside the box, do it yourself! Don't put the correspondence through the system. And that stood me in good stead forever after. The other life lesson: they accepted me to graduate school and Uncle Henry calls me in and says, "Well, we've arranged a faculty scholarship for you. These things aren't automatic," and he smiled. "We just like to see them happen." And I thought, life lesson number two. We just like to see things happen. That's what you want to do particularly if you're running an operation.

Bierly: Well, I'm assuming from what you've said that you had made a decision early on that you were going to stay with meteorology.

Cowen: Oh, yes, I was in the Ph.D. program.

Bierly: And then working with Victor Starr, obviously you were going to be working in more theoretical aspects rather then forecasting or things like that.

Cowen: Oh, yes, forecasting was not for me. The country, mankind, owes me a debt of gratitude for not becoming a forecaster and misleading them with bad forecasts.

Bierly: OK, so as a scientist then, and a budding scientist starting to work on your Ph.D. program, what did you think–what did you expect out of life as a scientist? What was your thinking into the future?

Cowen: Well, even at that stage, I tended to think broadly and laterally, and Victor was good for that. But you could see from that little vignette about Harry and Harlow. That was thinking way out. So I'm sitting there, we had these old _____ calculators. People haven't seen them, they look like a big electric typewriter and you could add, subtract, multiple, and divide, and Victor was doing momentum flows in the atmosphere so you calculated them at the gridpoints, and I am sitting there doing this and thinking, my Lord, there must be some more to it than this! And so that's where I was. I should say the Ph.D. program was fully funded. _____ agreed to fund it, and I was going to work on the polar motion, the Chandler Wobble, and how that might be related to the shifting of masses in the atmosphere. Something that only now people have begun to really find, and I don't think I would have found it because I was looking at the pressure data on the surface, and they are looking at the bottom of the sea and that's where they are picking up the signal.

Bierly: OK. As a part of your Master's degree, did you have to write a thesis.

Cowen: The thesis was my research proposal.

Bierly: OK.

Cowen: And there is another interesting vignette. If you look in Walter Munk's book about polar motion, you will find the footnotes of that thesis saying its indifferent results, but I told him what was "indifferent" about a research proposal that got fully funded? Yeah.

Bierly: OK, so you were working on a Ph.D. and then obviously something happened, and the Christian Science Monitor position came into existence.

Cowen: I knew the editor, and the paper is published by the Christian Science Church, but they have a person in place who acts as publisher. The manager of the publishing society puts it out. I knew both of them. We

had had a pioneering science editor, Herbert Nichols, who along with the great people from the New York Times and others started the profession in the country, and founded the National Association of Science Writers. He had gone onto other things. He had gone on, in fact, to the Geological Survey, where he acted as Public Information Officer for awhile. So the Monitor was left with no science editor at the time when the whole science journalism game changed because of the atomic bomb, and they needed somebody who could come in and restart the program with what was then a modern scientific background and perspective. So they contacted me and asked if I would like to do it, and I thought—now again, the sense of adventure there and the broad outlook that I was missing doing those clunky calculations, so I did something I'd never done before. I made an appointment with the big man and went up to see the Chairman of MIT who said to me—

Bierly: Whose name was Carl Compton.

Cowen: That's right, that's right, I thought people would know. Anyway Compton said, "MIT turns out enough Ph.D.'s, this is more important." And I did not know how seriously he had taken it at the time. I took his advice but years later I got a letter from Mrs. Compton saying he'd come home that night talking about it. So he at that point had seen it would be good for MIT students and graduates to get into journalism, which a number of them hav done since then.

Bierly: That was very fortuitous that that happened because it totally changed your life and I would wager that it changed a lot of other lives to because of your writing.

Cowen: I don't know about that, but once again it was one of those things that came out of the blue, but somehow I had been prepared for it. I had not wanted to spend a lot of time on my Ph.D. with a doctoral minor. MIT at least at that time let you take advanced standing in courses if you studied on your own and took the final. So I got several undergraduate courses out of the way and took the graduate courses for my minor as an undergraduate in mathematics and physics. So when this happened I could easily turn my program into a Master's degree because I had all the requirements.

Bierly: I see. Well, that's very good. So you've already answered a question that I was thinking about and that is the nature of the Christian Science Monitor itself; as you said, it's published by Christian Science but it's a separate entity.

Cowen: Yes.

Bierly: Within the framework.

Cowen: That's right.

Bierly: Of the church.

Cowen: It's a regular newspaper.

Bierly: Right. So when you first went to work for the Monitor, what position did you have and were there any other scientists on the staff and what were your responsibilities? Can you give us some idea about that?

Cowen: Well, anybody who goes into our Media Fellow Program coming out of graduate school may have the same experience I did. Although they probably would work with a science editor. There were no other scientists on the staff. It was a bunch of humanists, many of whom sort of looked at science in the vein of : "Well, I suppose it makes you more accurate." So I landed in the midst of a bunch of very skilled humanists, politically savvy, some of them with world reputations as journalists to do the science thing. My position was technically that of a reporter, but it's always been the same thing. There is no science-writing slot on the paper. As far as I know there isn't one now, because I don't think there's anything been established I don't know about. So the science writer gets a correspondent's slot or what-not, and which is very nice because it means you can write everybody else's budget when you want to take a trip.

I went in without a job application or an interview. Irwin Cannon, the editor, whom I knew, and Savelle Davis, a national editor whom I did not know, took me down to some fancy restaurant in downtown Boston and we talked about it and then he looked at me and he said, "Do you understand nuclear physics?" And I said, "Oh, we can do that." I didn't tell him that the thing had been so classified you couldn't study it at MIT, but by that time things were out and a quick study could bring you up to speed on it. So I went in to re-establish the science writing and it was, as I say, it was severe culture shock. I was the guy who was not supposed to do good essays in MIT humanities courses, to the point where one of the instructors a year later sent my book review back to me and said, "I didn't think you had it in you," and Tom Malone said, "Why didn't you write a more literate thesis?"

The one thing I brought to it was an instinct for stories. You could feel them, you could see them, you could feel the excitement, and I went back and I took a look at my first two months on the job, thirty-six stories, a first by-line, a first page one by-line, one editorial and one book review. Told the current editor they owed me back pay. They weren't paying me enough for a starter who did that much. But to my mind, it was racing

with your tongue hanging out, trying to learn how to write, trying to show you could play in this very big pool with these very big fish. Not just on the Monitor, but elsewhere and suddenly, you know, all the excitement and adventure I had hoped for was there.

Bierly: Can you remember what the first front page article was?

Cowen: Oh, yes, everybody will remember this one. It was Irving Langmuir and Vince Schaefer and Bernie Vonnegut and silver iodide seeding, and a National Academy of Science meeting. I remember Murray Mitchell and I drove out to it, and they were showing these charts and showing the rainfall lining up with the seeding experiments, and we all know the history of that, but that got my name on page one, that was my first story.

Bierly: Well, good. Were there any other big issues not only in meteorology but other science issues of the first decade in the 1950's that you can recall?

Cowen: Oh, yes, in fact meteorology, and atmospheric science and geophysics were right in the center of a lot of this stuff. The big issues— if you think back to the impact the atomic bomb had on people, the big issues had to do with nuclear physics, secrecy, control in the first part of that decade with the military vying with the civilians, and eventually the decision was made to set up the Atomic Energy Commission, and put the control with the civilians and not the military. Then we begin to declassify and you had your first "Atoms for Peace" conference, and things begin to come out and begin to develop nuclear power. That was your big overriding story. But there were others. Space was beginning to come along, and I did a major series on space in something like 1952 or 1953; you could do that then. The other thing that was coming was a lot of new knowledge of Planet Earth which was funded by Office of Naval Research and by Air Force Geophysics Directorate out here and Hanscom, which had funded the ___ Project, so I knew some of those people. Helmut Landsberg was in charge in the early part of that decade, and I went out there and we did a big series. Helmut, an overall article, and I wrote the rest. That became a brochure for GRD, so that was setting the stage, and as you probably remember, because I think you were getting involved in this stuff, we began planning the International Geophysical Year.

Bierly: That's correct.

Cowen: And people began talking about satellites and you remember the long-playing rocket, which was a satellite and so forth, so that got to be an important story. I am to this day somewhat puzzled at the reaction people had to Sputnik when it went up. Now I lived in Concord, and within a couple of miles in Concord was Fred Durant, the president of the American Rocket Society. I would go to his meetings, they were all

talking about America putting up a satellite, and the Soviet Union putting up a satellite. The officials of the Soviet Union involved, I think it was Vladimir Ryvov himself who was the top guy, would come through Boston. The State Department said you can't go outside of Boston. Fred would sneak him out, he was briefing the CIA, among others, and he would debrief him and then he would tell me, and I knew the satellite was going up and that it was probably going up in the fall, and I wrote about it and other people wrote about it. Sputnik goes up and there is this great hoo-hah and I think, what is the matter? I can understand the general public, but why is the administration acting this way and why do smart people like Chris Craft, Christopher Columbus Craft Jr. who became the great flight controller at NASA why did they not know it? In Chris's new book he says how astounded he was. It just showed tunnel vision there, but not on Eisenhower's part. Lloyd Berkner told me that it was the toughest thing he felt he ever had to do, to break the news to Ike that the Russians had got there first. So Lloyd didn't know the background, nobody else knew the background except a very small group around Eisenhower, and this has only come out in the past five or six years as the historians have gotten into it, and one of them wrote to me and explained that Eisenhower had wanted to have an "open skies" program with aircraft flying over Russia and Russian aircraft flying over the U.S., so we knew what each other was doing and the Russians wouldn't have it. If you could put up satellites in such a way that everybody agreed it was like the high seas, and was available to everybody, you could do this thing.

So people at BU, I think working on all of these high-powered lenses and things. Eisenhower wanted the Russians to get up there first because he knew they would say: "We are doing this for mankind. This is a peaceful arena, anybody can go here," and that's exactly what happened, and then we had the Discoverer series of scientific satellites, which took my friend who worked on it tells me some of the sharpest pictures you've ever taken from space, bar none.

Bierly: Before you get into that, let's go back to this business about Sputnik. Just prior to Sputnik if you recall, and maybe you don't because you were writing and you weren't involved in a research project, there was a great withdrawal of research support by the military, by ONR and by Air Force Cambridge cutting down on research projects. Sputnik went up and boom! people were really throwing money at you.

Cowen: No, I am aware of that; I was right in the middle of it.

Bierly: I don't understand why the military was pulling out.

Cowen: All right, you had two things. You had a feeling that comes up all the time. Now why is the government funding all this fancy science, it's

"welfare" for the science. But you had Charles Wilson who had been head of General Motors come in as Defense Secretary. He wasn't much into research, he was saying, I don't care why the grass is green and research never did anything for General Motors, but he also discovered that the Defense Department was incredibly sloppy in its accounting and paying its bills. He said, "You have got to bring this into line and pay all your bills now," and he drained the cash flow. Now at that point, _____ Radner and others who were running GRD, called me out. Sputnik had just gone on and they called me out, and they said, "Look, this is not really secret only administratively secret, but the budget for research here is part of one big budget, which includes plowing the runways. We plow the runways one more time at Hanscom, and we're dead. The thing is running out. People are leaving, teams are disbanding…"

So I knew about it. I wrote this up in the Monitor. It was when we splashed it all over the front page. Jim Killian said to Irwin Cannon, "I see you know what's going on so that's what was behind it." Sputnik went up and that changed it. As for the Navy later on, not too much later on, they still were worried about the relevance of this to the mission of the Navy. I was doing consulting work with Arthur D. Little at the time as well as working for the Monitor, and Jim Killian said to the Secretary of the Navy that you're killing science, and he said, "Well, give me a reason to do it. So the Navy gave Arthur D. Little a contract to investigate why basic research is relevant to the mission of the Navy, and I got on the contract and we went back and we looked at the roots for a lot of their technology. Went back to Michael Faraday and people like that, and came out with a report that Arthur D. Little can't find now—I don't know where it is—but we made a thorough study of it. It was very interesting and that helped the Navy rationalize its expenditures towards research. So, yeah, I was sort of involved in that stuff.

Bierly: One of the things that was beginning at that same time was the whole experimentation with numerical weather prediction.

Cowen: Yes.

Bierly: And of course that would have been a rationale that would have helped the Navy, too, if you could depend on the forecast.

Cowen: If you could.

Bierly: Now were you involved in much of that in knowing what was going on? Had you gone to Princeton and been involved with any of those people?

Cowen: No, I hadn't gone to Princeton, but of course Phil Thompson and Jule Charny would come up to MIT and so forth. I was over at MIT a lot. I've

maintained one foot in the scientific community, in the atmospheric science community, as you know, right along, so, yes, I knew about it. But at that time we didn't know where it was going to go and so forth so it was not a big deal. I didn't really get to writing about it until the 1960's when Joe Smagorinsky got going.

Bierly: Right OK. One more thing then was starting or at least beginning to start in the late 1950's I believe and that was due to two people that you knew very well. One was Henry Houghton and the other was Tom Malone who was very close there and that is the whole concept that has now grown into UCAR and eventually into NCAR.

Cowen: Oh yes very much.

Bierly: Do you have any thoughts about that?

Cowen: I do indeed, I knew Walt Roberts. I knew Walt Roberts before he made the change from astronomer to atmospheric scientist. I remember at the time we were talking over dinner or something about it, and even though as well as I know that the meteorological community was hidebound, tunnel-visioned and generally needed a kick in the butt, and NCAR was the kick in the butt. It's interesting that Alfred Vagener is remembered for his continental drift, but his big thing was interdisciplinary research and of course that's what NCAR was all about and it was very important in getting an independent research institute that could push interdisciplinary research. Think back: this is one of the things that used to get to me in the meteorology department. We are studying this special case; what are the generalities? We ought to be looking at all the planets, but of course we couldn't do it then. Now we do it, now we have all that, but I was one what—how do we know that this is the way things really work, we've got a special case, and not only was it a special case, but they would cut it off at about the thermosphere or something like that. You couldn't talk about the upper atmosphere, and there were a couple of things there that I would like to talk about, also in that… To get back to the Charlie Wilson era. At that point just before Sputnik, space research was a bad word with the Defense Department. It was frivolous, like searching for extraterrestrial intelligence. So one day I get a call from E. Fradner's office saying could they have their publicity photos back, they want to change "space" research to "upper atmosphere" research, and we were getting into that, and so it was an interesting time when you suddenly went from a drought for science to a glut, and the National Defense Education Act and all of that pushed science, and at this point you've got NCAR coming so it benefits from all of this. There is a dynamic going there which propels atmospheric science forward and if you fast forward a few years to Bob White going down and taking over the Weather Bureau…Bob said to me,

"Well, this is meteorology's last stand!" But, of course, it was the beginning of the great era.

Bierly: One of the things about NCAR of course was always this tension between NCAR itself and the university community it supported, and that tension to one degree or another still goes on in various ways, but did you see much of that? MIT of course was very much involved. If it hadn't been for Henry Houghton, there might never have been an NCAR.

Cowen: Oh, yes, I saw that and I was amused. I figured these people are just going to have to learn their lesson. After a while in science writing you see this all over the place, not just in atmospheric science, you see it everywhere. So they are human beings and so they want to be laboratory researchers and academics and not political, and they are as political as hell about the field, but there's something we missed in the 1950's that was coming and that's weather satellites.

Bierly: That's right.

Cowen: They were coming along fast and you remember Harry Wexler did a theoretical picture of what a satellite might see and it turns out to be absolutely correct, but you have people like Morris Tepper and so forth, beginning to develop this, and meteorologists skeptical and wondering. I remember Jim Austin at one meeting getting up and saying, "What are you going to do with all this fragile data? you get it back from the satellite and by the time you get it out to the users it's gone." Of course, modern communications have taken care of that but they were working through these issues and thinking about it and it was a change of life. Suddenly you were looking down on things, you were seeing the whole thing. It was just a change of perspective.

Bierly: And in addition to that part of it, there was also the international part where Wexler was working with the Russians and trying to come up with what became the World Weather Watch.

Cowen: That's right, I remember World Weather Watch and WWW is now immortalized on the Internet and those were great stories. I also – there was also another life lesson with Harry Wexler, more for government scientists then for budding science writers, but one of the minor issues in atmospheric science was cloud seeding, and it got to be quite an emotional issue for meteorologists. Clinton Anderson, Senator from New Mexico, held hearings on that and I went down for the hearings and Harry and I was talking with Anderson. Harry was bemoaning the fact that there wasn't enough money for regular atmospheric science and Anderson read him a lesson. He said, "The trouble is, you are not up here telling us why we should do it. You come to the hearings, you don't come to me at any

other time. You don't brief me, your scientists don't brief me, what's the matter with you? If you believe this why aren't you here?"

Bierly: A lesson that we can still use today.

Cowen: Yes.

Bierly: It should be preached loudly and clearly to the present scientific community also.

Cowen: Yes, and they can take advantage of our Congressional Fellows program.

Bierly: That's true, that's true. OK, well, we are getting to the 1960's and one of the things that seems to be a little different, but maybe not so is the fact that you did publish a book, wrote a book and published it, called it "The Frontiers of the Sea," the story of oceanographic research. So could you tell us a little bit about how that came about, and some of the repercussions from that book.

Cowen: Well, I knew Dick Winslow of Doubleday and we were talking about books to do and this seemed like a good area to get into, because here again, you were at the beginning of a great renaissance. There was the major United Nations-sponsored international oceanographic meeting in New York, which brought everybody together again, and you began to think of the Challenger days and things like that one. You were starting to look at the whole ocean so it was a beginning and it was a good time to write the book. Murray and I went around and talked to oceanographers here and in England and so forth and wrote the book, but it was also a very prescient perspective people were getting. They were beginning to sense plate tectonics, just beginning to, and they were beginning to sense that the carbon dioxide/global warming thing would be an issue, and, you know, that had leaned sort of dormant since Arrhenius had brought it up in the previous century. I interviewed Columbus Iselin who was the Director of Woods Hole Oceanographic Institution at the time, and he talked about this so I did a major full-page story on it. It must have been something like 1958. The historians at the American Institute of Physics tell me that was the first significant newspaper story pointing up possible global warming in the last century.

Bierly: Is that right? Interesting..

Bierly: Did you have any contact with Roger Revelle, about global warming, because that was one of the things that he was saying were in his geophysical experiment and we don't know what the outcome is really going to be.

Cowen: Oh, yes, I talked – Roger was a friend. You notice he wrote the intro for the book. He is a friend and mentor and we talked about that and talked about what became plate tectonics and so forth, but I remember at that meeting I think it was 1958 they had that oceanographic meeting. We were in the men's room and I had asked him about it and he said, "Well, it's a subject that we no longer have to talk about in the men's room." So he was beginning to sense it.

Bierly: Yes.

Cowen: But we hadn't yet seen the Alenpine stripes in the seafloor.

Bierly: Good, well, all right. We are into the 1960's, there are a lot of things going on. Are there any particular ones that come to your mind that deserve to be mentioned now?

Cowen: Well, good heavens, the 1960's were when my big story took off—space.

Bierly: Right.

Cowen: Space of all kinds, but particularly manned space flight. We went from Mercury to Gemini to actually walking on the moon, and every reporter is entitled to at least one major epical story and that was mine along with a lot of my colleagues. That was the big thing and we continued to have these International "Atoms for Peace" programs every two or three years where the Russians and the Americans and so forth get together. The whole world got together and I would go to those and that was another important thing that was developing.

There were some interesting sidelights on those. I tended to be a little naughty with the KGB because the Russian scientists always had KGB with them, and we would be at dinner or something and I would say to the KGB men, it must be tough on you here with all of these distinguished scientists trying to understand what's going on, keeping them in line. He would look at me and say, "You've seen too many movies." I sat with one of them and I was glad I did because afterwards, I found out he was bullying the Americans and bullying his Russian colleagues, and finally the Russian colleagues got rid of him at one of those meetings.

But one of the things you had to realize in the Soviet Union days, and I'm sure all of our scientist friends know this as a scientist, but also with the Russian reporters, was that you always had to suspect that they might be debriefed by the KGB. So you acted accordingly and I liked that arrangement because I knew exactly where I stood with them, and therefore we could be friends and we could do things and so forth. I don't have a totally negative view of the KGB because if you were on an official

delegation to Russia as I was in 1971, KGB smoothed your way. We went around customs, we went around immigration, we had no problems and when we looked at each other and said we can't do that when the Russian delegation of science writers comes to America.

Bierly: That's very true, very true.

Cowen: But during that era you were constantly meeting them. They were scientists of course were in COSPAR and all that, but they were also in Pugwash.

Bierly: Right.

Cowen: If you remember the Pugwash movement—named for a town in Canada which was working toward world peace and test bans and disarmament. I went to one of their meetings in London and you had some scientists in Russia who were so strong that they felt independent, and Akseimovich was one of them. He had been critical in their nuclear program. Don Brennan of our delegation told me Akseimovich is coming out. After listening to Tom Chif talk and saying, "It's all a lot of wind, and its coming from you and its coming from me, but at these meetings, the Pugwash meeting you could really get together with these people, and I could see our scientists and so forth working with them. And that had some impact on them on—Kennedy and Khruschev and others, as they were thinking about test ban.

Bierly: Going through all of that with Pugwash came – I don't know whether it came earlier but the Bulletin of the Atomic Scientists.

Cowen: That's right that's right.

Bierly: With the clock…..

Cowen: When the clock, which we all watched regularly.

Bierly: Right, right. I think we are getting close to the end of this tape so I think I am going to stop the tape and we can go on to another period.

Cowen: Yes.

END OF TAPE 1, SIDE 1

Interview with Robert Cowen

TAPE 1, SIDE 2

Bierly: This is tape number two from the interview with Bob Cowen on May 29th 2001.

OK, Bob, we are still in the 1960's and there's one more point at least that I think is worth talking about, and that is during this period you began to write a column for the Technology Review, and this is—as you've told me earlier—an MIT alumni publication. So tell me what was your thinking behind this? Why did you think it important for you to contribute to this publication?

Cowen: Well, at that time the MIT administration wanted a prestige publication for MIT, and the alumni association wanted to rationalize all the printed material and other communications we were sending out. We felt that things had gotten out of hand, so there was a major study and I was vice chairman on it. The chairman was Roy Stevens. president of Arthur D. Little. One of the things we said to MIT was, "Look, Technology Review can be your prestige publication." It's got to be professional and we need a professional editor so we went through all of that. We convinced John McTill to become the editor and transform the magazine this way. So having been thoroughly involved in the planning of it, when John asked me to do a column I was eager to do it, and that's how I got started doing the column for Technology Review.

Bierly: And you continued to do that for some years.

Cowen: Oh yes, and I continued to be on their advisory board for, oh, I don't know maybe sixteen years or more until John McTill retired.

Bierly: Well During the decade of the 1970's, although you tell us that there was really not a position on the Monitor called "science writer," you did make a little change at least and become for all practical purposes a senior editor.

Cowen: I was the senior editor.

Bierly: For features.

Cowen: Yes there **is** a slot for senior editor for features. John Hughes, who started on the paper two years after I did, became editor and I became part of his administration. You had several senior editor positions under the managing editor. There was the national editor, the foreign editor, the feature editor and the chief editorial writer. So I had all of the features.

At that time we started a science page, and we had that in addition, and I did a column for that while I was feature editor. So I did that for four years, paid my dues as an executive and went back to science writing.

Now I should explain that on the Monitor in particular, the really good jobs are correspondence jobs. You earn just about as much as any, certainly you earn as much as a senior editor or more. I had people reporting to me who earn more and just about as much as the editor. On the other hand, you are totally free to freelance, to get on radio and television, to do other things, to have a great time reporting, and none of the headaches of the management in Boston. So for a long-term career, I realized I wanted to get back to science writing. I should explain that it became apparent soon after I joined the paper that if I was going to do what I had been asked to do, to revitalize their science writing, I couldn't be restricted to any one department. I wound up in national news for administrative purposes, but I had to work for everybody. So it was made clear that in effect I reported to the managing editor. So I worked for foreign news, national news, features, editorial, advised them and became a hairshirt if they wanted to run stories that were not very sound scientifically. Wrote articles for them and so forth and that's the job I went back to.

Bierly: So did you have an increase in staff since you opened up a science page or a science section of the newspaper; was there any increase in science staff?

Cowen: Yes, there was, we had been up and down on that. In 1969 we moved to London for two years, and I was able to do it because we had four science writers at the time, so it was easy for me to become what I think is still the only American newspaper science writer with a permanent overseas assignment. We consider an assignment to a bureau a permanent overseas assignment.

Bierly: So you actually went to London.

Cowen: Yes.

Bierly: You yourself went to London?

Cowen: Yes, Mary and I lived there for two years. We knew our way around very well because I had gone overseas for several months at a time for years, and arrived in London and I was the only member of the bureau who didn't report to the bureau chief, I reported directly back to the foreign editor, and drew money on my own signature which wowed the accountants. They never had one who drew money on his own signature and accounted for it back at the home office instead of through them. So it

was nice, and I had somewhat that status in Washington when I left London and we were stationed in Washington for about eighteen months. The bureau chief just said to me, "I know you operate in your own right." So its always been that kind of thing where I've had to take the initiative to see to it that broadly across all departments of the paper including editorials, I wrote editorials, we were covered and that's somewhat unique for science writers on this paper.

Bierly: While you were in London, you mean in England, or looking at Europe what struck you over there at that time, and what years were they?

Cowen: That was April 1969 to April 1971 when we moved to Washington. This may sound strange, but true of other expatriates at that period: what struck us was the United States. It looked as though it had gone bonkers, to use a British term. That was the time when you began to get all of the Vietnam riots and so forth. In the sciences, nuclear power went from being good to bad. We would come back occasionally on home leave, everybody was uptight, we didn't know what was going on. Went back to Europe, the Europeans would say, what's the matter with your country? It really from over there looked odd, and that's what stood out more then anything else. You could see Europe as being more calm, and the science was perking along. British science was strong, you began to see the fading of it that came on very strongly after we left as our support was cut back, and all of Europe began to come up much more strongly, perhaps in Germany.

In connection with nuclear power I had a very interesting experience. Shortly after I got back to Washington, I went right back to Europe to cover one of these "Atoms for Peace" conferences. Glen Seborg was leaving as the chairman of the AEC, and Nixon had put in Shlesinger to try to clean up the AEC's image and make it honest, because it had been caught fabricating stories too much. So I'm – they were both in Geneva—so I made an appointment with Shlesinger to talk to him, and I said, "Look, I've just come back from Europe to America, I can't stand it. Why is nuclear power so bad?" He said, "Look, sit down. Now if you say I told you this, I'll deny it, but you've got to take this seriously because there are real bad problems." And then he began to sound like Greenpeace. He briefed me totally on the core meltdown problems and so on, and how I had to come up to speed on that and understand it so when I came back and was covering him at the AEC, he had great credibility.

Bierly: But he felt that if he was briefing you on things like core meltdowns and things like that, these problems were problems that were made by the operators of the plants usually.

Cowen: Oh yes, oh yes, he laid it all out, he knew what was going on and he was in there to try to do something about it.

Bierly: Interesting. Well, in addition to you being overseas and then back in Washington. This was from the meteorological standpoint in the 1970's, a period of great experimentation if you will. By this time, as you had pointed out earlier, Bob White was in Washington and became the head of the Weather Service and transformed it into, at that time I think, first of all, ESSA.

Cowen: Yes, Environmental Sciences Service Administration.

Bierly: Right and at that time we had an experiment called the Barbados Oceanic and Meteorological Experiment (BOMEX), which was a predecessor and a "get ready" for a more global experiment, part of the Global Atmospheric Research Program.

Cowen: Yes, yes.

Bierly: The program that took place in the equatorial Atlantic. So do you have any thoughts about those things, and were you involved in any of them?

Cowen: Well, I was involved in writing about them, yes, very much. I want to put this on the record about Bob White: When he was down there he built up NOAA and so forth. The president of the National Academy of Sciences once said to me that Bob White was the best civil servant in Washington and I think that sums up his career there.

Bierly: Yes. Well I must say that I tend to agree with that, but if we didn't have in the Department of Commerce J. Herbert Holloman at that time.

Cowen: Oh, yes, that's right.

Bierly: —Bob White may never have come to Washington.

Cowen: Yes.

Bierly: And fortunately he was a very forward looking gentleman also.

Cowen: Yes, yes, I remember him and he then wound up at MIT. At one point during that era, it may have been NSF was funding some briefings for science writers through AMS—which was a forward looking thing—and I should put on the record here that the science writers have a great self education program, which grew up in the 1950's, and continues whereby in many fields, the professional societies saw to it that they were working with the National Association of Science Writers, that there were educational sessions that last a day or a week. American Institute of Physics would bring in their top scientists in the field and we would have a

day of briefings or a week of briefings. That metamorphosed into the yearlong yearly briefings by the council for the advancement of science writing, which the science writers set up. We had to set up a separate council to accept money for that and so every year we have a weeklong series of briefings, which will include atmospheric sciences quite often co-sponsored by the university, and we go back for an education. So that's in the background of all of the accomplishments of people like Dave Pearlman and Ben Silon.

Bierly: Good.

Cowen: And as part of that, a lot of atmospheric scientists—through AMS, my memory is in the 1970's were trying some outreach.

Bierly: I don't recall that from the standpoint of the division of atmospheric sciences at the NSF and it probably came from the education part of NSF.

Cowen: Yes, yes.

Bierly: That would be my guess.

Cowen: Yes.

Bierly: Well at any rate by the time we got into 1974 or so, we were in this experiment that was called GATE.

Cowen: Yes.

Bierly: The GARP Atlantic Tropical Experiment. You did a lot of traveling, did you go to Senegal…?

Cowen: No. By that time I was a senior editor.

Bierly: That's true you were…

Cowen: Yes from 1972 to 1976.

Bierly: The thing that struck me personally was that was like a military operation.

Cowen: Yes.

Bierly: With all those ships.

Cowen: Yes.

Bierly: From various countries and it was an international operation.

Cowen: I remember Bob White talking about the thrill of actually seeing the thing work with the satellites going up and the ships out at sea and the planes flying.

Bierly: As a matter of fact that was for meteorological purposes we used a communication satellite that was taking pictures, but it was actually for communications taking pictures over the Atlantic tropical area, and I remember that you could see the cloud patterns, you could see how they moved and I know there were a lot of people who felt that that was going to really revitalize and turn around forecasting.

Cowen: Yes. I kept my counsel on that and took it with a grain of salt. I thought it would improve, but I didn't know about turning around forecasting. That is a naughty problem.

I should give a little anecdote here. During that time as feature editor, I received a strange communication from the Philippine Embassy, saying, "Your airline tickets are in the mail," and with some investigation I found out what it was all about. The Marcos' in the Philippines had decided they wanted to have some of the big world conventions in Manila and they were particularly after the World Bank and things like that. So Imelda had said to him, "Let's have a trial run on science and development and try out the center," and this is what they did. And they brought people from all over the world. I was pretty impressed as a reporter, so we all arrived together at San Francisco at the airport. Went over to Philippine Airlines and there was Ed David, who had been the science advisor to the President, and senior people from the USGS and we are all a little puzzled. So we got to Manila. It turned out they didn't know what to do with the press, and I was just wondering when I heard somebody say, "Well, there's Bob." Well, part of the contingent at MIT when I was a student was a bunch of Filipinos, who had come for an education, and by then they had worked up and this guy was deputy director, I think, of their weather service. I went to school to him. So instead of being a reporter, I signed on as an atmospheric scientist and got in and John Simpson was there, so we had a lot of interesting talks about things. Marcos gave a speech on development and David said to me, "You know, I wish the President would do that." That was pretty darn smart and at one point Marcos suspended the convention, took us all on his yacht out to Corregidor, and showed us where he had been locked up by the Japanese. On that trip John Simpson and I were sitting at a table having lunch with some of these people and exploring various things. It was a strange but interesting interlude, and he was talking to them about the hurricane research he had done and so forth. I was talking about conveying the information to the public, but that was a little vignette that went on during that era.

Bierly: Well, you had to interview a lot of people in your lifetime. How – do you have any remarks about how to get information from people that are tough ones to interview? Easy ones to interview are probably easier to get information to come across..

Cowen: Well, you want to do your homework. Always do your homework, and one of the things that still bugs me about some parts of the press is they will go into a press conference or a scientific interview, and they don't know what they're talking about and will, in a sense, ask an atmospheric scientist, what's atmospheric pressure, you know, just dumb questions, and that's going to turn off people, and if they are a tough interviewer they are going to despise you. So you really want to know what you are doing, and also you want to see if you can establish a relationship so they will work with you.

One really tough interview I had was back in the 1950's, I think it was, in England. I went over to the British Association for Advancement of Science meetings which is like the American Association, and the president of it, Sir Robert Robinson, a Scot, misunderstood the Monitor and he looked at me and said, "Why should I give an interview to a religious newspaper?" and I— well, Cowen is **Mc**Cowen, and I have a bit of Scot in me, even though my middle name is Churchill, I have got a bit of Scot in me. So I looked at him and said, "Sir Robert, you disappoint me, I didn't think a man of your stature would let his prejudice stand in his way." "And he said, "Oh, come see me in the morning tomorrow." And we had a good interview. You had to be tuned into that sort of thing, but gain the respect. I will tell you one thing that really has helped is this ring on my finger which shows the MIT beaver, and I know Monitor editors particularly say, "Well, you found access because of the reputation of the paper." I said, "No I'm sorry, in the scientific community they didn't know who we were, but as soon as they found out I had two MIT degrees, I was in the door, and they said, "Oh, you're one of us.""

Bierly: That's correct.

Cowen: Yes, yes.

Bierly: Well, you pointed out earlier that Mary had gone with you, your wife for the record here, had gone with you on several occasions and particularly with regard to the book on oceanography. But did she travel a lot with you and help…?

Cowen: Oh, yes, she was not going to be left behind on any of these trips. I decided as soon as we were married we were not going to have the kind of marriage where the wife says I don't know what my husband does. So I

took her in on everything I did until she'd had enough of it and that took a number of years. So she came along on interviews, and helped take notes, always as my first editor, and because of that, she saw the Mercury astronauts take off, because in those days I could bring her along as an assistant press. She stood on the _____ power plant, the first big nuclear plant, on top of it, the day before the Queen came and opened it along with me. She interviewed Quinton Hogg, Lord Halshim, Britain's first Minister of Science, and he was one of the politicians with a good memory because the next year I went back without her, and he wanted to know where she was.

Bierly: Good for him.

Cowen: Yes, so yes, she got in on these things. She couldn't go on the trip to Russia in 1971. We had been working through the National Academy in a formal way as the National Association of Science Writers to have an exchange of science writers, and as soon as the Nixon-Brezhnev Accords were signed, the deal went through, and so our group went over there for eighteen days, and their group came over here for eighteen days, and we went around and saw labs, saw a lot of things.

Bierly: Those agreements that were signed in 1972, I think, most of them were actually signed or certainly in the early 1970's.

Cowen: Yes.

Bierly: They were really very, very useful.

Cowen: Yes.

Bierly: Very useful.

Cowen: Yes, they were.

Bierly : Because not only science writing, but there was one on atmospheric sciences and technology that's lasted throughout all of the days of the Cold War and was never stopped.

Cowen: Yes.

Bierly: Something happened useful every year.

Cowen: Yes, yes.

Bierly: OK, well, let's go on into the decade of the 1980's. Was there anything there that comes to mind that you want to think about, that was different? There had to be new science. Science was burgeoning all over the place.

Cowen: Oh, yes, it was burgeoning, but also it wasn't. Back in the late 1960's, _____, who was president then of the National Academy of Sciences, said to me that the scientific community has got to realize that the joy ride is over. They are going to have to work harder for funding and that scenario played out until by the 1980's, you were really having to work, work hard for it. And if you remember when Reagan came in, and then Jay Keyworth came in. I made a date with Jay and we had an interview and he was telling me, he said, "You know, I sit in the White House breakfast room, and the Director of the Bureau of the Budget comes along and says, there's Jay Keyworth with his tin cup." So you had to work at it, but I think from an atmospheric science point of view, as well as from many other sciences point of view, if you think back on it, the 1980's were when the new communications were beginning to cut in, and they developed over the 1980's and 1990's, and this has made an enormous difference. If it happened all at once, you'd say it was a big as the impact of Sputnik. It's just a change of everybody's perspective. Look what we take for granted on the TV news, for the views of the weather, never mind the forecast, which is much better. You started getting the better radar, then the Doppler radar with the NEXRAD program; which put the 1980's and 1990's together in one thing. The satellites improved enormously. We now take the numerical forecast models and have great confidence in them, which we didn't have. It's just so different from what meteorology was fifty years ago, that if you had spelled that all out in 1950, they would have said it was science fiction. That's what stands out to me.

Bierly: OK well I think that that's true and of course the 1970's these experiments, these GARP experiments helped understand the general circulation immensely, so that we were able to do better forecasts. But the in the 1980's you have to remember that the second objective of GARP was the climate objective.

Cowen: Oh, yes.

Bierly: And then climate began to come into its own.

Cowen: That's right.

Bierly: It really all started with the Global Atmospheric Research Program.

Cowen: Yes.

Bierly: And so with the climate came the problems of ozone, and the ozone depletion, and more pollution problems, and the Montreal Protocol and all of those things, which really began to bring the science and the politics together in ways that one could argue are good and bad.

Cowen: They did but look what happened to the meteorological community. Their parochialism really showed through.

Who did the ozone thing? Chemists from outside, yes, Sherwood and Molina, who started pushing the problems of global warming and what did the atmospheric scientists say? "Oh, I don't believe that. Oh, you can't do this. This is just propaganda." It showed a lack of vision, foresight, scientific foresight, scientific insight, and I think the lesson was being rubbed in throughout the whole second half of the last century, but it was really being rubbed in the 1980's and 1990's: that the atmospheric scientists had better get out of their box, or that box was going to be closed up and put in the trash and other scientists were going to take over their field. You could see that in writing the stories.

Bierly: There's no question about that. I agree with you fully. I know that there will be people in the atmospheric sciences community that will not be very happy to hear that. But I think it really is true.

Cowen: They should sit on the other side of the press conferences.

Bierly: Right. The other thing that started too – you have discussed earlier some of the interdisciplinary programs and interdisciplinary itself, and during the latter part of the 1980's I believe that the IGBP came into existence, the International Geosphere Biosphere Program.

Cowen: That's right.

Bierly: That came from Herb Friedman wanting to commemorate the IGY again after about something like forty years, something in that area. And out of that grew the IGBP.

Cowen: And the IGPP and people studying the biosphere interactions, I think, are going to upstage the IPCC, the International Panel on Climate Change, because the presentations they gave at the American Association for Advancement of Science meeting in February in San Francisco showed they had a broader view of what's going on with climate and the earth system than you are getting in the people that are looking just at the climate system. They made the point that they didn't want to criticize what the IPCC is doing, but that it's once again a little too narrow.

Bierly: I'm going to take the interviewers prerogative here in a moment and say, I think you're right, but one has to remember that the IPCC has three parts to it and we hearemostly about the science.

But there is a second working group on adaptation that is bringing in a lot of the interdisciplinary programs such as land use, and across the world, and then there is a third working group that's on mitigation. Given these problems, how does humanity position themselves to live with it, not only to adapt, but to maybe make other provisions, and these are the areas of the future, I believe.

Cowen: This is true, and what I see from a science writer's perspective, is I see all that, I see the other work, I see a need for everything to come together. Somehow this has all got to come together as one big earth system study, and the new sustainability science—to use that difficult to pronounce word—is maybe where it's going to come together. We are just beginning to think about this. Tom Malone's Knowledge Partnerships will play into it. Jane Janco's sustainability science will play into it, and probably those two thrusts need to come together more, but I think our next big revolution and change in perspective will be to see the thing as a whole. The thing being the earth as a geophysical system, a geobiological system, and a human political system. Tom Jordan of MIT once said, how do you factor in the human response when you do a numerical simulation, because if you are simulating the effects of CO^2 on climate, how do you feed in what happens to that CO^2 when people being to respond to the threat? You begin to see some economic models and other things tied together with climate models, but somehow this has to be integrated in a much more dynamic and direct way.

Bierly: Right, and that's exactly where the adaptation worked.and eventually the mitigation. You are absolutely right. In fact, I hope that there are a lot of nations of the world and scientists—in particular, atmospheric scientists—who understand this, and will begin to work on the other aspects of the IPCC. I think there is from what I can see because we have ?start in the part – well they are at the AG U building and they are actually part of the AGU. They are working together with IPCC. There is a tremendous amount of interaction with these things that is going on. And I think that's very good.

Well, there is one other area maybe a couple of other areas, but you retired in 1995.

Cowen: I retired officially.

Bierly: Right.

Cowen: In other words I picked up my pension and then signed a contract.

Bierly: Right. I have been intrigued with some of the titles of things that you have written about, and one that I really love is Why Moon Hits Your Eye Like a Big Pizza Pie.

Cowen: Yeah.

Bierly: And so could you tell us a little bit about—since you retired, you are under contract to the Monitor and you are doing articles at your choosing obviously.

Cowen: That's right. Working with my successor, Pete Spotts, whom I hope is in the press conferences here.

Bierly: Right.

Cowen: And what I've been doing is trying to make sure we continue our broad coverage. Fill in where Pete can't do it, and see if I can't bring more of this larger perspective to some things. I've just done a piece to appear Thursday, only six hundred words, on a little more understanding of how life arose on earth. The people at the Whitehead Institute have actually gotten RNA to duplicate itself to a large extent, showing that it might have been possible to jump-start life with RNA alone, without proteins and so forth, and other people at Carnegie have shown that calcite mineral can actually segregate left-handed and right-handed amino acids, and maybe that's how the biological importance of left-handed amino acids was established.

Now six hundred words. In the 1960's, I started on my own a little experiment. Was it possible to take a scientific development or idea, just one idea, concentrate on it, write it in a small compass. Work on it so that it read very easily, and therefore you had an hors d'oeuvre for people they could pick up, but do it in such a way that the perspective was right, the insights were right. I think of a Toulouse-Lautrec painting of a horse, just a few lines, but it's a horse, just a few words, but its actually a scientific development, and I've perfected that over the years, and so this little six hundred word piece is in that vein. It takes me some times days to figure out what would look to you as though why didn't he do that in fifteen minutes? But to get it just right and get those words just right. So that's the kind of thing I'm trying to do more of.

Bierly: That's wonderful that you're doing that. Are you going to teach a course in that sometime? I'm not a teacher but I've been mentor to interns.

Bierly: Good, because that's a talent to be able to do that and in today's fast lane society, sometimes that's all people have time for, but…

Cowen: That's right, that's right.

Bierly: Get the _____

Cowen: Get the ____ what's happened, but also the flavor of it and the perspective. Well, when I left MIT and started writing, I felt as though coming out of the academic environment into the newspaper environment, I was being asked to take my clothes off in public, because you know the inhibitions about how you say things and so forth had to go. So to try to do that, I did a lot of studying of what other artists did. I read Stanislavsky's "An Actor Prepares," and Agnes DeMille's "Dance to the Piper." I read Maugham and Hemingway on how they learned to be writers and so forth, and went through what amounted to a sort of self-education in that field to try to get to grips with writing. It wasn't until five years later that I felt really comfortable with it. I went back and looked at my file, and I was doing acceptable work, but I didn't feel that comfortable with it and the boss said at that time, ah, turn the corner.

Bierly: Good, good.

Cowen: So that's, you know, you say it's a talent, but you have to work at it.

Bierly: Well, since you have retired one of the things you've felt more free to do is to become involved with some of the scientific societies now that you have no real conflict of interest anymore.

Cowen: That's right

Bierly: So could you tell us a little bit about some of those involvements.

Cowen: Well, as soon as I retired I sent a letter to Fred Spilhaus of AGU and said I am available, and so Fred put me on some of the committees for the Sullivan Award and so on, and eventually I wound up on the public information committee and was chair of the public information committee. I'm now on the development committee. Urge anybody who hears this tape to realize that the American Geophysical Union is a significant organization that really does do some good for humanity by getting out the word on science, and one of the things I would like to reinforce along with many others in AGU is the notion that scientists don't really fulfill their obligations to society until what they know gets out to the general public, and I would urge all of our members to reach out themselves, but also to help the science writers do it; it's important. We talked about the need for all the various aspects of the study of global change to come together, but

one of the big factors in that is cultural differences, and the cultural lag, and even scientists suffer this trying to come together. So the more you can communicate the knowledge, the agreed-upon knowledge, to everybody, the more you can help this transition.

Bierly: Right. OK, well, this is a recording that is going to archived in UCAR, and I think that what you're saying about the AGU is probably also can be applied to the American Meteorological Society, too.

Cowen: It can but it's behind the curve and it needs to come farther forward. They are doing a lot better with their press releases and so on. There is one problem with the AMS, though, that just from the journalistic point of view, is that their big meeting comes so close to AGU, American Astronomical Society, American Association for Advancement of Science, that it's hard for the science writers to get to that meeting and justify the cost, and I think what this says is that not just AMS but every professional and learned society that's really interesting in getting the message out, needs to do some strategic planning. I'm not saying they need to change their meeting, but they need to think of how to get that message out given those constraints. I think now they've gotten some good public information, consultants they probably are thinking about that.

Bierly: Good, OK. Well, another thing that you have received a number of awards in your career. They are listed in your CV; and so I am not going to go over them except to point out that several nights from now, you will receive an award for sustained achievement in scientific journalism from the American Geophysical Union.

Cowen: Yes.

Bierly: Henceforth that award will be known as the Robert C. Cowen Award. How does this make you feel?

Cowen: Well, should I be embalmed?

Bierly: No, definitely not.

Cowen: Mary and I have joked that so many people we knew like Roger Revelle have become medals, awards or buildings. Homer Newell was a building down at Goddard Space Flight Center.

Bierly: But I think the nice one about this one is you are sitting here, you are alive and you know about this award, which is nice.

Cowen: Yes, it was a total surprise, but very nice.

Bierly: OK, is there anything that we've left out – things that you might want to put on the record here?

Cowen: I don't – well, there's another anecdote that might amuse people.

Bierly: OK.

Cowen: At one point during the Eisenhower years back in the 1950's, Lincoln Laboratory thought it might be an interesting communication experiment to put a flood of copper dipoles in orbit around the earth—came to be known as "Needles."

Bierly: I remember that.

Cowen: And that became an international *cause celebre:* the U.S. is mucking up the environment without telling anybody. Well, I was out at Lincoln taking a tour around with the director, Carl Overhage, and I happened to mention that I was about to go to Europe and in the course of it would have an interview with Lord Halshim, the British Science Minister. He said, "Please, take a personal message. Tell Halshim that Eisenhower means it when he says we won't do it again, and we're very sorry, so go see Halshim." And at the end of the interview I said, "Lordship, I have to take my journalist hat off, and give you a private message from Overhage and give it to you." Halshim laughs and he says, "You tell Overhage that Her Majesty's government know you have your military necessities just as we have." And this was in spite of the fact that publicly they had been making a big stink about it.

Bierly: It's great to have friendships like that, an access to people like that is wonderful.

Would you have done anything differently if you had your life to live over again?

Cowen: That you know – that's an unanswerable question.

Bierly: I know it is.

Cowen: Because you have got hindsight.

Bierly: I know it is.

Cowen: Yeah there is one thing if I could take a bit of the hindsight with me and that would be to stop being so anxious about getting something done and

what's the future going to hold, because you do get something done. You do accomplish a task.

Bierly: Right. OK. Well, I think we're pretty much near the end here, as far as I can think of. So I want to tell you that it has been my privilege to interview you this morning. You are a unique guy.

Cowen: Well, it's been a pleasure to have you do it, Gene.

Bierly: I wish that there were more like you in the field because as Carl Thompson told you and he was right, MIT and other institutions provide too many Ph.D.'s, but the public understanding of science is essential and it needs to be worked on and worked on a lot.

Cowen: Yeah.

Bierly: And it's really important because as you pointed out the scientist are doing their thing and it doesn't really get communicated to the users – to the ultimate consumer, and that link has always been weak, and that's our fault as scientists that we haven't pushed into that and that's what we need to do.

Cowen: Yes.

Bierly: And that's what you've been promoting as long as I've known you on a closer level since you became apart of the public information committee at the AGU.

Cowen: Oh, yes, and I think with Harvey Leifert we're doing a great job.

Bierly: A wonderful job as a matter of fact. And these Media Fellows, I love them. It's a great program that the AAAS runs.

Cowen: Yes, and I think we're getting a lot of science writers out of it.

Bierly: That's obvious. Well again thanks very much and we'll close it off.

Cowen: Right, thank you, Gene.

Bierly: You're welcome.

END OF INTERVIEW