EARTH SYSTEM SCIENCE AT 20 ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

DR. BERRIEN MOORE III
INTERVIEWED BY REBECCA WRIGHT
NATIONAL WEATHER CENTER, NORMAN, OKLAHOMA – APRIL 4, 2011

WRIGHT: Today is April 4, 2011. This oral history is being conducted with Dr. Berrien Moore at the National Weather Center in Norman, Oklahoma as part of the Earth System Science at 20 Oral History Project for the NASA Headquarters History Office. Interviewer is Rebecca Wright. Thank you again for taking so much time out of your schedule today.

MOORE: Happy to.

WRIGHT: I'd like to start by asking, how did you first get involved in this field? For nearly thirty years you've been a prominent participant in both the scientific investigation area and policymaking aspects of climate change.

MOORE: Well, I'm a great believer in fortune or luck, and I think I can trace it to one day in the spring of 1976. I had several things happen in my life. One, I had a Fulbright [Award, scholarship for international research] and I was headed to Romania to continue my work in mathematics. We had our child; our daughter was born in February of '76. I was in California lecturing in mathematics when I got a phone call from the University of New Hampshire [Durham], and they asked me to go down to a marine science meeting at Scripps [Institution of Oceanography, La Jolla, California].

I was at [University of California] Berkeley, and probably in classical New Hampshire fashion of "Live free or die," I was already on the West Coast and therefore it was cheaper for me to go down there and put in an appearance. It was, as I recall, March timeframe, and I arrived a little bit late. It was an auditorium filled with people, and I looked around and there was just one seat that I could identify, and so I slipped into that seat. After a while, I had no idea what they were talking about. They were talking about something in oceanography, and I turned to the guy next to me, we just chatted, and I said, "Are you following a lot of this?"

He said, "Well, yes," it was something he knew about. He asked what I was doing there, and I told him I was just covering for the university. I thought it was interesting, but I didn't really understand very much of it. He said, "Where are you?"

I said, "University of New Hampshire."

He said, "Well, I've just moved to the Woods Hole Oceanographic Institution [in Massachusetts]."

I said, "Oh, that's interesting." We talked some more, and I told him I had this Fulbright to go to Romania, but I just wasn't sure I wanted to do it; I was becoming increasingly interested in applied mathematical topics.

He said, "If you ever want to hang out in Woods Hole, I'm sure I could get you comparable to the Fulbright. You could spend a year in Woods Hole on your sabbatical." That person was Bob [Robert A.] Frosch. After getting back to New Hampshire and thinking about it some more, I thought, "I think I'm going to do something different. I feel guilty taking the Fulbright because I'm not really as interested in the mathematics as I once was." I had become very interested in environmental issues and Earth science issues.

So that fall we go to Woods Hole and Jimmy [James E.] Carter's [Jr.] elected president, and in 1977 in April he nominates Bob Frosch to be the NASA Administrator. By that time, I'd become friends with Bob, and he said, "Well, I convinced you to come down to Woods Hole. Maybe you can come down and spend some time at NASA every once in a while. After all, they do Earth science." I said, "Oh, really?" That was the beginning, and I've thought to myself a number of times since then, what if that seat hadn't have been there? But it was, and so that's where it all began.

Another thing that happened very importantly—about the time that Bob was nominated as the NASA Administrator, Bert [R. J.] Bolin, wonderful Swedish scientist, was in Woods Hole to give a series of lectures on the carbon cycle and on the buildup of CO_2 [carbon dioxide] in the atmosphere and on how much was going into the ocean. It was quite mathematical, and I thought, "This is something I could contribute to." So I also got to know Bert Bolin very well, and he became a great mentor of mine all the way up to within a week of when he died. He was just a wonderful mentor. So that's where it began.

WRIGHT: During those next years, tell us how you acquired more knowledge and your enthusiasm grew into this new field.

MOORE: I think I had an opportunity in Woods Hole. There were two things. One was a constant stream of seminars, and there were enough people in the seminar you could sneak in the back and slowly learn. It was difficult going because I didn't know any oceanography or chemistry. The other thing is the library was like L.L. Bean [catalog clothing company]; it was open twenty-four hours a day seven days a week, and so I had a pattern of going to the library and spending about six or seven hours reading journal articles about something that I found interesting. That was just shopping, yet I think that the Bolin lectures on the carbon cycle really gave me some focus, and the situation was that Bob was good enough to suggest that I might stay at Woods Hole.

I talked to the University of New Hampshire, that I wasn't really going to be doing mathematics in the future and so maybe I should just stay at Woods Hole. They then said, "Well, we'd really like you to come back. And also if you've taken a sabbatical you're supposed to come back for a year." I never knew if that was true or not, but they told me it was true. So the dean of the College of Engineering and Physical Sciences and Bob [Robert W.] Corell, who was head of the marine program, convinced me to come back and they gave me $30,000.

Out of that $30,000 I could pay my salary, I could hire a grad [graduate] student, and I could do whatever I wanted to do, but I was going to essentially go on to soft money even though I still had tenure. I thought, well, why not? I hired a young man to work with me. His name is Charlie [Charles J.] Vörösmarty. He eventually became my first Ph.D. student. He's now heading the Environmental Studies and Water Program at City [University] of New York, marvelous scholar.

We started up the Complex Systems Research Center. We termed it Complex Systems because we thought trying to distinguish it from, say, a traditional engineering system. If you think about a TV [television] as a system, if you go in and take out some of the tubes or some of the transistors, the TV won't work anymore, and you wouldn't say it's a system. Whereas in an ecological system, if you went and took out a species, it would just evolve and become another system. We thought that evolutionary capability is complex, how did that work? So we started developing computer models of ecological systems or the carbon cycle or things like that. It was really great fun, it was all starting afresh.

We grew the little Complex Systems Research Center up to about thirty people over the next six or seven years, and then we had an opportunity with the new building that was being developed on campus at New Hampshire to bring the Complex Systems Group together with the Ice Coring Group who did reconstructive paleoclimates. Then we brought in our Space Science Center—there was a very distinguished Space Science Center at New Hampshire—and our Oceans Modeling Group and our Oceans Experimental Group. We brought all those together and formed the Institute for the Study of Earth, Oceans and Space. That was in ’86, and I became director in ’88.

That was another very interesting story, because in my view of the world, Len [Lennard A.] Fisk was going to be the director. I was so sure of that I took a sabbatical, my second sabbatical, in ’85-’86 to go work at the University of Paris [France] on carbon cycle in their Institute for Physics and Chemistry of the Ocean [Laboratoire de Physique et Chimie Marines, Université Pierre et Marie Curie]. That was a great experience. Then, it was either the winter of ’86 or winter of ’87, I got a phone call from Len Fisk. He said that he’d been offered the position of [NASA] Associate Administrator for Earth and Space Science [Space Science and Applications]. It was director for all of science; it included Earth science, space science, material science, life sciences—had all the sciences together. And what did I think he should do? I said, “Len, you’ve really got to take the position. This is a very important time. There are all these large issues on the table, and we really need scientific leadership at NASA. So you’ve got to take that job.”

He said, "I thought you would say that. As a consequence, you've got to come back and head the Institute for Study of Earth, Oceans and Space."

I said, "Oh, no."

He said, "Well, I'm not going to take the other job, then." Len's a pretty tough negotiator.

Yet I'd agreed with the French to stay, and so I spent three weeks in France and three weeks in New Hampshire. That was the time that the way you got back and forth was on TWA [Trans World] Airlines. I was doing that flight all the time, and I would talk with the stewardesses on TWA. We would think of what does TWA stand for, and I suggested it stood for The Worst Airline, and they said, "No, no, it stands for Try Walking Across." We had great fun. It was a wonderful experience to have that time in Paris, but also to begin to see the Institute come into existence at New Hampshire. I served as interim director with Roger [L.] Arnoldy and then I became formal director in '88, and I did that for twenty years.

WRIGHT: Also during the eighties, if I'm correct in my research, you were part of a working group that was looking at the Earth Observing System [EOS]. Can you tell me about the early days of those discussions?

MOORE: Those were marvelous days. Dixon [M.] Butler was heading the working group from the NASA side, and Burt [Burton I.] Edelson was the Associate Administrator. It was called System Z. I think in part it was Burt's idea—maybe he was searching for some way in which he could get more scientific support for Space Station [Freedom]. They had this concept of a co-orbiting platform near Space Station which the astronauts could go out to and work on, add instruments, take them off.

But that [low-Earth] orbit is not ideal to Earth observations. You don't see the high latitudes, for instance. So they then went from the co-orbiting platform to make a copy of the co-orbiting platform and put that in polar orbit. I remember there was once even some idea that they would use some form of [Space] Shuttle servicing or robotic servicing to replicate what the astronauts were going to do, because you're not going to put astronauts in polar orbit because those are not safe regions as you go over the high latitudes. It never really made any sense as to why this was connected with Space Station, but it was; that's the way it got going. Slowly the co-orbiting platform, which is not an ideal orbit, vanished or faded into the ether. We ended up with [International] Space Station in the inclined orbit, and in the polar orbit we had this Earth Observing System.

I remember my wife asking me, "What's the connection? How do you get from one to the other?" I said, "Well, you would leave Space Station, and you would return to Earth and probably end up at Cape [Canaveral, Florida], and then you'd take a bus out to Vandenberg [Air Force Base, California] and you'd get on a rocket at Vandenberg and go up to polar orbit." I mean, it's no connection whatsoever, but Burt had this idea of the co-orbiting Space Station and ended up going to polar orbit.

It became then the Earth Observing System, and we stopped saying System Z. We had a payload panel that was meeting and arguing about what should be the instrumentation. At that time there were two platforms as I recall. There was the morning platform and afternoon platform, which were not unlike the morning weather satellite pose and the afternoon crossing time, so that you would cross the equator at the same time every day, so-called sun synchronous orbit. We then began to think about those, what we might want to put on those platforms. I think there were a couple of free-flyers, also. I think LAWS, the Laser Atmosphere Wind Sounder, was going to go on a free-flyer. That was an instrument probably at least ten years ahead of its time.

It was a very heady period because we really had the "sky's the limit" to think about this, so there was this major effort on what you might like to do from space. But what was missing was there wasn't really a scientific rationale. It was a little bit ad hoc, and that lack of an underpinning scientifically is what led to what we often called the Bretherton Committee, which was a committee that Francis [P.] Bretherton chaired to talk about Earth System Science. That had about a four-year run.

It was very interesting to me to see what later people thought was a brilliant marketing strategy, was really a reflection that scientists tend to take a long time to do things. The committee was supposed to report out in a year, but at the end of the year we didn't have anything to report. Shelby [G. Tilford] said, "Well, you've got to report out something." And so we created a little foldout and it said "Earth System Science," or something like that, "A Preview." That's all we had, we had some ideas.

Another year goes by, and we had a lot of documentation by this time but we still didn't have the report finished, not by a long shot, particularly because Francis just talks forever, and so it was going to take forever to get this thing done. We published about a forty-pager, and we said, "Earth System Science: An Overview." So we first had a preview, then a year later we had overview, and finally we published the so-called Bretherton Document, which was a very thick document.

In fact, it's interesting to me to think back—we all served pro bono on all this, but the one perk was we got a little Radio Shack [electronics retail store] laptop. I think they called it TR-80, and it had sixty characters wide and four lines. That's all you could see. We wrote a monumental document on that four-lines by sixty-characters [machine]. I don't think I could do it now, but we wrote the Bretherton Document.

I think the most interesting aspect of that whole period was when we came up with what was called the Bretherton Diagram, even though Francis didn't have anything to do with it. John [A.] Dutton and I were chairing a meeting of the Modeling Team. John Dutton and I shared an interest in addition to modeling the Earth, which was skiing. So we decided to host this meeting in Jackson Hole, Wyoming. Our idea was we'd get up early in the morning and work early from, say, seven o'clock in the morning through breakfast up to noon, and then we'd ski in the afternoons, and then at five or six o'clock we'd come back and work up until maybe eight or nine, eleven o'clock at night. That way, we'd put in more than a full day's work, and we'd get an afternoon off to ski.

It turned out we really made good progress that way. We were working with the beginnings of the outline of this diagram that describes how all the pieces of the planet work. The top half of the diagram was biogeochemical cycles, the bottom half was the physical system, and partly what linked the two was the hydrologic cycle. We were working on this evolving diagram, and we were using an overhead projector—this was way before PowerPoint [presentation software]—and we were shining the overhead projector on the wall of the room that we were working in at the hotel.

The name of the hotel was the Snow Bunny Lodge. It had already caused JPL [Jet Propulsion Laboratory, Pasadena, California] a little heartburn to have this meeting at the Snow

Bunny Lodge, but what really was going to cause them heartburn is what happened. John [H.] Steele, who was then the director of the Woods Hole Oceanographic Institution, was standing beside the overhead projector adding some equations. John also is a mathematical ecologist. This is one of those things where you saw an accident about to happen, and you just froze, you didn't say anything. John was writing these equations on the transparency paper, and he stepped back and he started looking at it. He saw a mistake in his equation, and rather than walk to the projector, he just forgot what he was doing and he walked to the wall and rewrote the equation on the wall with [permanent] Magic Marker. So now we're standing in the Snow Bunny Lodge and we have to pay to have the wall painted. I remember John Dutton and I saying, "Do you think we could slip this past JPL?"

The Bretherton Diagram began in the Snow Bunny Lodge in Jackson Hole, Wyoming. Francis was not there, but I'm happy that it's called the Bretherton Diagram, because Francis is a great scientist. We began at that meeting to describe exactly how we saw the Earth worked. Now when you look at it, it looks very primitive, but it was the first time we actually really tried to write down basic equations, looking at the physical system, you might say the climatological system, and then at the biogeochemical part, and then the feedbacks between the tip of the water.

I think that all comes together in '86. We made a presentation at [NASA] Headquarters [Washington, DC]. I remember working on it in 1985 at AGU [American Geophysical Union] in San Francisco [California], and we were really pushing to get the thing finished. In fact, I believe it was in the spring of '86 that I was in Paris partly, but Jim [D. James] Baker, who later becomes the new administrator [of NOAA (National Oceanic and Atmospheric Administration)], John Dutton, myself, Francis—we formed a little Kitchen Cabinet saying, "We've got to get this thing finished."

Then we also had the help of a very creative person, Payson [R.] Stevens, who was a graphicist. He had the ability to pull all of this material together with very good graphical imagery and it became really a monumental document. That gave us the underpinning for the Earth Observing System.

Now you had the huge scientific case in the report of the Earth System Science Committee, shorthand it's called the Bretherton Report. That set the stage for guiding the EOS Program. That, of course, is a tough period, too, from about '88 through '92, '93, because the budget just wasn't going to support the full-up program. We went to a smaller spacecraft that was a little more terrestrial-focused, and then the second one was a little more aquatic-focused, and the third one was more chemistry- and atmospheric-focused. So we went away from that morning and afternoon—even though in some sense we still had it because the morning was the terrestrial and in the afternoon we went to the so-called aquatic one.

We gave up several of the instruments. At one time there were going to be two MODIS's, a Moderate Resolution Imaging Spectrometer that was going to look at the land, primarily at atmosphere, and then one that was going to look at the ocean that would look off nadir, not look straight down. We had to give that up, and then the LAWS instrument, the Laser Atmospheric Wind Sounder, went. I think there may have been radar in the early payloads, and then that went.

Slowly there was a budget readjustment. I think, though, that in the end we did remarkably well and the instruments have performed spectacularly well. The amount of science that has come out—the citations would just be endless, may be amongst the most productive of all. It certainly is right up there with Hubble [Space Telescope] in terms of the productivity, across a much wider scope of things, too, because Hubble is more than just astrophysics. Earth

Sciences really hit all areas of Earth Science. I think that you would have to look at that as one of the great success stories. I do regret that we lost the ability to fly the multiple copies, to get long-time series. That was unfortunate, but NASA has trouble with the idea of multiple copies of the same thing. They want to call that *operations*, even though it's not. It's just science that takes a long time to do.

WRIGHT: During this evolution, how were you able to keep up with all of the progress? Was that part of your involvement on the NASA committees, or did you have other groups that you were instrumental in?

MOORE: I think that where I ended up was in two main service areas. One was chairing the NASA Science and Applications Advisory Council Committee, and that was a very demanding job which I'll come back to in a second. Then the second was chairing the EOS Payload Panel, where we were trying to cope with the downscaling of the various missions and evolving it from the a.m./p.m. structure on large spacecraft over time to the Terra/Aqua atmospheric chemistry program, and handle that restructuring that had to take place.

The part that I look back on that I was pleased with was that we were able in the Science and Applications Advisory Committee to come up with one of the first strategic plans for all of science at NASA. It had priorities within planetary, within astrophysics, within Earth Science—which was basically a U.S. program—within the heliospheric, within the life sciences. That strategic plan was consistent with, at least in the astrophysics area, the decadal surveys that were coming out. In the other areas where we were just beginning to have the first decadal surveys or not even have those, we were able to get a community buy-in. This was difficult, because you couldn't do everything, and there had to be compromises across the different areas, say, astrophysics versus heliospheric. I think we ended up doing some fairly clever, some very fundamental creative things.

At the beginning of that strategic planning exercise, heliospheric was essentially approaching their science almost in exactly the same way as the astrophysics, which were the great observatories, even though that really wasn't what the community wanted and resources were not going to be there. So we ended up stepping away from the Solar Observatory, and moving to a multiplicity of smaller missions that really are the hallmark now of the heliospherics. In some ways, heliospheric has been a very robust area of science, it has done extremely well. You have a steady stream of new missions, a variety of scales, probably one of the richest areas of P.I. [principal investigator] involvement.

And I think that reflected, coming out of in the 1991 or '92 timeframe, that we came up with this new strategic vision for the Office of Space Science and Applications. It really set the pattern for what science did at NASA for the next ten, fifteen years. In astrophysics, the hallmark were the four great observatories built around different parts of the spectrum, just looking out at the different, if you will, wavelengths or different regions of the spectrum, and I think that was a very creative thing.

The planetary programs started looking at the outer planets versus the closer-by ones. We started talking about some of the moons that might be interesting to look at and really crafted a strategic vision for what the planetary program could be. Then, as I mentioned, the heliospheric was going to be built around stepping away from the great observatory approach and doing it differently, a more robust approach, and then the Earth Sciences was essentially the Bretherton EOS tradition. In addition to that, a couple of other things were important to get off.

The TOPEX/ Poseidon [Ocean Topography Experiment] mission, where we're going to measure the topography of the ocean at very precise scales, the upper-atmosphere research satellite was the predecessor to what we could see, big platform of what we could do. Those were enormously rich endeavors, but they really just set the stage for the U.S. program.

WRIGHT: Did the multiplicity of the smaller missions coincide with some of the direction that NASA was taking under [Daniel S.] Goldin's leadership [as NASA Administrator]?

MOORE: No, this was pre-Goldin. Dan came in, and I think he liked what he saw coming out of the heliospheric. I was beginning to wind up my tenureship as chair of the committee under Dick [Richard H.] Truly and overlapped Dan, I think, for one or two meetings. I think he came in the spring of 1992, and then by the summer of '92 I'd finished my chairmanship. Certainly through the Payload Committee that I continued to serve on, it was Dan's insistence that we break up that a.m./p.m. into slightly smaller missions. But this was still prior to the so-called "cheaper, quicker, better" era, which I don't think turned out to be very successful. As someone said, two out of three, but not all three.

WRIGHT: I found it interesting when you were talking about when you all get together, that you knew you couldn't do everything, but you were able to compromise. Can you share some of the other thoughts about those compromises?

MOORE: I think it came from people taking a very strong, scientific leadership role, and one that I can point to is Mark [R.] Abbott. We were at a meeting at Scripps with Ed [Edward A.

Frieman]—Dan had formed an outside group to come in and oversee alternative ways of doing business on the EOS Program, and the Payload Committee was there, essentially defending the actions or trying to find ways in which we might do it differently. We saw that there was a very real budget pressure, and in particular I remember Mark Abbott saying, "I have a harder time justifying flying a MODIS that looks straight down and then a MODIS tilt instrument that looks off nadir."

For the ocean you can get more information that way, but you're paying a lot just to get that off-nadir look, namely a whole new instrument. We thought maybe we could switch it back and forth or different things, but in the end, we said what we're going to have to do is trade the perfect for the good and have one MODIS that does both atmospheres, land and ocean, ice. Mark took some real heat from the oceanographic community for putting that idea on the table, but it was absolutely the right idea. Things were going to go, and I think Mark Abbott standing up and saying, "We just have to make some tough decisions," set the tone for making other tough decisions. So it really came from individual people willing to take some heat and look at it honestly and say, "Well, if we've got to do this, we have to do this. Let's do it rationally." In other words, it's not everything we want, but that's just life.

WRIGHT: During this time period, it's still a new concept for all these different type of scientists meeting together for one goal. Was establishing the goals of what you all wanted to work for a difficult challenge to you?

MOORE: There were a number of difficult [challenges]. The Abbott one that I just mentioned is one, but there were lots of tough decisions. I think there was something else happening then, and that was the e-mail. We didn't call it e-mail, it was tele-mail. And it was something called ScienceNet, prior to the Web. The way it worked was there was a company in Cambridge [Massachusetts], and tele-mail was nothing other than an 800-number anywhere in the world that you could call. Not anywhere in the world, but most places in the world. In effect, you were calling a mainframe computer in Cambridge, and the mail was just file-swapping within that computer. There was no Web, there were no servers, none of that. But there was this tele-mail, and it was offering a form of communication that was right at the heart of building a community. I've thought about, "What if we'd not had tele-mail?" I don't think that any of this would have worked. It just would have been too episodic; you couldn't have gotten from one meeting to the next, there wouldn't have been enough connectivity. The space for compromise would have shrunk rather than expanded. It just wouldn't have worked.

Somehow on these little TR-80 Radio Shack computers and tele-mail, we began to form a much broader community that could work together. It is hard to believe now how all of that came together, because the one thing you couldn't really do on the old tele-mail—and that's why when the Web came out and HTML [HyperText Markup Language] was there, we flocked to that like bees on honey—because we could then share pictures. Tele-mail was just text. There was this second-generation breakthrough when all of a sudden now you could push imagery back and forth, and that just revolutionized the whole nature of the world. In the early days it was a really exciting period because you had this daily connection.

I remember being in France, and if I were traveling, I always had to figure out ways I could penetrate the French phone system because they didn't have the jacks. At one stage there was some way you could do it with alligator clips. You would unscrew the mouthpiece and use alligator clips, and you could connect out with your little laptop. It's interesting to think back—I remember the older European scientists never used it, but all the young scientists were using tele-mail like crazy. And of course now it's a whole new world.

WRIGHT: It also seemed like there was a mutual respect system among all of you emerging scientists in these new fields, and that you were willing to put your agendas for your own field aside to work toward a new field that would help each other.

MOORE: I think that there was that, and I think that, in addition, the problems on which we wanted to work, which were gaining attraction, were these large-scale environmental problems, most of which connected up with the climate question. Not all—there was the CFCs [chlorofluorocarbons], how the atmosphere worked. But invariably you found that the problem required multiple disciplines to crack into. Even the relatively narrow ones, like the fluorocarbon ozone hole, involved very different areas of chemistry, so-called heterogeneous chemistry and so forth. But if you looked at other topics like ocean carbon cycle, how much CO_2 are the oceans taking up, well, that involves the physics of the ocean, the chemistry of the ocean, and the biology of the ocean, and all of those play into that topic. So there were these areas where the subject matter itself simply required multiple disciplines to work together.

I think there was another set of things that were taking place, and that is that there were two organizations that were pushing forward. One of those was the WCRP, the World Climate Research Program, that really was laying out the physics of the climate problem at the large scale, at the planetary scale, and within that you would have programs like WOCE, the World Ocean Circulation Experiment. The World Ocean Circulation Experiment gave a lot of the fundamental rationale to the TOPEX mission, and very distinguished people like Carl Wunsch at

MIT [Massachusetts Institute of Technology, Cambridge] was pushing that forward. Then, because that was pretty much the physics system, there was the movement in the mid-eighties to capture more of the biogeochemistry aspect, and that led to the formation of the International Geosphere Biosphere Program, IGBP.

If you think back now to what we talked about with the Bretherton Diagram, the bottom of the diagram was the physics of the system. That's the World Climate Research Program. The top of the diagram was the biology, biogeochemistry. That's the IGBP. The place they intersect and where we used to have some battles was the water cycles. You had a program within the IGBP which dealt with the water cycle, and then you had a program within the World Climate Research Program that dealt with the water cycle.

The IGBP program was called BAHC, Biological Aspects of the Hydrologic Cycle, and the WCRP Program was called GEWEX [Global Energy and Water Cycle Experiment]. Those two often dueled with one another. I was always trying to get those people to stop fighting with one another, let's enjoy both of them. The two IGBP and WCRP began to work together, even in difficult areas like the water cycle. We began every once in a while to have a joint meeting with one another. When I was head of the Scientific Committee of the IGBP, I would go to the WCRP meetings, and vice versa, Larry [W. Lawrence] Gates would come to the IGBP meetings.

I had a vision that over a long period of time we would try to merge those two organizations, but they have a structural difference. The World Climate Research Program is part of the United Nations [UN] and it sits within the climate arena in Geneva [Switzerland] as a UN body, and the IGBP is a standalone organization, so it's hard to put those two different organizations together. But I think they gave, particularly at the international level, a glue to the community and they really set the stage. That was the way in which we orchestrated the scientific work that was done as part of the Earth Observing System.

In fact, the Europeans then went on to fly their large platform and completely reenergized the whole European Earth Science Program. I remember Dixon Butler, as only Dixon could do—we were there to give a briefing of our plans for the payloads of the Earth Observing System, and he was to brief the European Space Agency—Dixon got carried away, and at some stage he bellows out that, "We are going whole hog!" and the Europeans had no idea what he said. So I piped up and I said, "*Tout jambon*," all ham. Well, whole hog is *tout jambon* also.

I don't think Dixon ever slowed down. He was a great enthusiast. He eventually ran afoul of a very difficult part of the EOS Program, which was the Data and Information System. That was an area where I think the convergence of what we needed versus where the industry was—we were just a little ahead of our times so it ended up costing us a hell of a lot of money to create the EOS Data and Information System. Whereas if we'd added another four or five years, then that would have been a piece of cake too. All of these things were evolving and co-evolving, like tele-mail and the Internet, like the international scientific organizations, like the space agencies, and the fact that so much of it converged—I'm not going to quibble about parts of it that proved to be a heavy lift simply because we weren't quite there.

WRIGHT: You mentioned just a second ago about the Europeans and moving toward the same type of concepts. Talk about your workings with the international community and how you've been able to help bring what they do and what the Americans are trying to do to help globally.

MOORE: It was very interesting to me, several aspects of foreign collaboration. At first that was really U.S. and Europe and then U.S. and Japan, but of course now it became much broader than that. I think the Europeans really went to school on us, they really watched what we were doing. I think they were somewhat envious. In the early stages it was really a U.S.-dominated initiative because we got out so fast with the Bretherton effort, with these opportunities to have access to these big spacecraft coming out of the System Z. We had this co-alignment of talent in Washington [DC], Fisk and Shelby at NASA and Mike [J. Michael] Hall over at NOAA [National Oceanic and Atmospheric Administration], and Bob Corell. When I first started migrating out of my mathematics, he was heading geosciences at NSF [National Science Foundation]. There really was an alignment of a lot of forces—scientific, political, otherwise—that gave the U.S. an enormous boost that really took us right up to the year 2000 when we start launching.

The Europeans came along a little bit behind that, but one thing I do observe is, boy, they sure don't back up from it now. I think that Europe really is—they're certainly co-partners, and scientifically there are very, very strong programs throughout Europe. Huge initiative in the UK [United Kingdom] through the National Research Council, the Max Planck entities in Germany have focused on these bigger science problems now, with institutes being formed. The Hamburg Group in Germany is still very strong. The French—in the early eighties, the French were publishing in French journals. It was not an internationally focused effort, the young scientists were not really flowering. Now the French scientists are all over the world, major programs in Earth Systems Science throughout France. They play both sides of the Left Bank/Right Bank [in Paris]. You've got on the Right Bank the French space agency, CNES [Centre National d'Études

Spatiales], and on the Left Bank you've got the European Space Agency. So, if it doesn't sell in one, get on the Metro [rail public transport] and go over to the other one.

I have observed that Europe really has become very, very strong. Likewise Japan, they've taken on these big topics. They have had some setbacks when they lost something, like in the ADEOS [Advanced Earth Observing Satellite] mission, but they really have made huge contributions. The first big Earth-system-oriented super computer, the Earth simulator—the thing's the size of a soccer field, and, yes, it was set up to demonstrate Japanese technological prowess in big computers, but also really went after some big topics.

We really have begun to see an internationalization of the science, and hence the IGBP and the WCRP. Those two big international organizations have much less of U.S. dominance than they did at the beginning, much more international. Programs in Japan and Europe and Russia and China now are really contributing major pieces. We first had a major scientific meeting in China, IGBP, in 1990, and at that stage, if I look at the progress and the Chinese contributions to large-scale earth science from 1990 to now, it is two generations and it's a revolution.

WRIGHT: I thought we might go back and revisit some of the work that you were doing at the institute in New Hampshire and the accomplishments you were making there and, of course, when you decided to leave in 2008.

MOORE: I think one of the nice things that I was able to do—because I was beginning to see clearly by participating in the activity where Earth Science was going, and I was also meeting and working very closely with a number of other senior colleagues who were graduating students—essentially I saw that there was a real opportunity if we could build appointments around research grants as opposed to a traditional academic teaching with some research. So we created a research track at the institute in New Hampshire where a person would have a very, very light teaching load, maybe only one course every other year, but most of their funds would come off research grants and we would tide them through rough periods on the funding. We used that as a vehicle to grow the institute. I think we started with about seventeen positions that were tenure track and two research faculty, and we grew that eventually up to only about twenty-three positions that were tenure track and about forty research faculty.

We were able to create the institute on a much faster time path than traditional academic appointments would allow. If you're only going to grow your program maybe slightly faster than replacement cost—that is, you have to wait for retirement in order to hire a new person, or on a rare occasion you get one extra new person—that's a very slow path. By building it around research faculty and non-tenure-track people, we accelerated the New Hampshire program rapidly at a time when there were expanding research budgets and people saw that this was an important topic. We had to train a whole new generation of students to move into these new areas. It gave us a lot of flexibility.

I think that New Hampshire was not alone. You can see the same thing happening at Penn State [Pennsylvania State University, University Park], where Eric Barron and John Dutton were providing the leadership. Oregon [State University, Corvallis] began to see the same thing with Abbott. But I think in New Hampshire we really got out quick and we were able to have a very robust program. We were graduating eight and ten Ph.D.s a year. They were going all over the world, they were getting good jobs. We were combining the satellite information with the modeling on big topics, and I think that that was a very good period for us. I'm a little bit nostalgic about that, because if I look at where the Earth Science part of that enterprise has gone at New Hampshire, the last two or three years it's gone the other way with a lot of people leaving, which is too bad. But for every time there is a season.

WRIGHT: That's true for you because you left New Hampshire yourself.

MOORE: You know, when you've done something for twenty years, it was time to go. I liked the new president that came in, Mark [W.] Huddleston. We'd had a pretty shaky period before he got there in the 2005 and '06 timeframes where I think the university lost its way, and I really hated not to have a working relationship with him because I would have enjoyed that. But again, I'd been there for twenty years. I kept thinking, "It's going to be difficult to stay around with a new director," and yet you needed a new director. Twenty years is enough. And I couldn't quite figure out exactly how that would work. I saw a wonderful little pillow somewhere in a baby shop that said, "If Mommy says no, ask Grandmommy." I didn't want to be the Grandmommy.

Yet I couldn't figure out where to go. I looked at a couple of possibilities. I was offered the directorship of an institute right outside of Vienna [Austria], an institute I'd known very well, the International Institute for Applied Systems Analysis. It was a modeling systems analysis place that was focused on, I thought, really important, good questions. I felt that would have been something that I would have liked to have done, but it turned out that for family reasons it was not the time to go abroad. Then I considered the opportunity at Woods Hole, to be the director there, but I think that they decided to offer that position to someone else, and I think that they chose the better of the two candidates. So I was a little bit at loose ends.

Then Steve [Stephen W.] Pacala at Princeton [University, New Jersey] came to me with an idea that I had known about, because the community had been talking about this idea, of a think tank on climate that was not only a think tank on climate, but also a think tank on climate communication. Anyone who had been in this business for as long as, say, someone like myself—you see that there's this complete muddying of the waters. Things that we know absolutely for sure about, which there is zero scientific debate, get bollixed up with things about which we're not so sure.

You will find people saying things like, "We don't know that CO_2 is increasing in the atmosphere or if that increase has anything to do fossil fuels." Well, yes, we know precisely how fast CO_2 is going up in the atmosphere. We've made a daily measurement of it since 1957, we have ice-core data before that. We know without any question that it has increased by almost 40 percent since the Industrial Revolution and that that increase is due to human activity, primarily fossil-fuel burning and, secondarily, bad use in agriculture. There's no debate about that. Yet the body politic thinks that that's some big uncertain scientific question.

I had been, for a long time, concerned about the fact that people simply were completely confused about what we know for sure, what we think we know, what we don't know, and all of that gets smeared out and confused. If you look at the debate today, it's worse than ever. I think that maybe I could trace my idea to take on this new post at Princeton [executive director of Climate Central] to 2008, because I was very concerned that there was so much noise in the debate. I have to look back and say, well, I hope *I* didn't do it, but it's just gotten worse.

This is one of those times when I can't look back with any pride whatsoever about having done anything. I can't point to a single aspect of that period from 2008 up to now where I could say, well, we made some progress. It's only become worse, and that's a truly unfortunate thing because the problem's not going away.

CO_2 is a very long-lived gas in the atmosphere, and in order to stabilize the concentration in the atmosphere, which is what we're going to have to do, you have to cut the emissions by something like 80 percent. If you just stabilize the emissions, and we're far from being able to do that, then the atmospheric increase moderates but it keeps right on going up ad infinitum. So you have to make a very huge change in the energy system of the planet, and that's not going to be easy, and for it to get so filled with noise—if you're trying to do a very big task, you need to be as clear about it as possible. So, I didn't make a whole lot of success in terms of changing how Americans think about climate. It's almost as if I made it worse.

WRIGHT: Do you think it's possibility that maybe you did well, but the other side has chosen to hire communicators as well to diminish your progress?

MOORE: Yes, and I think that the scientific community, when we had the e-mail hacks [Climatic Research Unit email controversy of November 2009], I think we should have been clearer there. I think there were some statements that you could find in the e-mail traffic that were less than our best, and maybe we should have said that. Maybe we should have expressed some criticism about that. I think a lot of that was just informal e-mail chatter, and so I could understand it, but I do think that the community might have been a little too tolerant and a little too, "Oh, well, that was said poorly," as opposed to saying, "That was really a dumb thing to say. That's inappropriate." I think we'd have been better if we'd have done that.

Whether or not it would have mitigated the so-called “Climategate,” I don’t know, I don’t know. It got so blown out of proportion. If you ask what would be the effect of looking at the fourth assessment report of the IPCC [Intergovernmental Panel on Climate Change] and went back and corrected every possible thing that might be criticized, you would probably pull out three or four references and maybe change twenty words. Not one fundamental conclusion would be at all different, it was purely superficial.

In fact, there were hearings last week where the House Science Committee Chair, Ralph [M.] Hall, has brought in some people who are the age-old critics, and they’re all saying, “Well, this was a completely cooked-up deal.” Yet the reality is that last year, 2010, was the warmest year on record, even though December was cool. Of the decade, from 2000 to 2010, it was the coolest December in that period, but the year was the warmest year on record. And even though December was cool, it was still the 310th consecutive month that is warmer than that particular monthly average for the twentieth century.

If you took the twentieth-century average temperatures for December, December 2010 was warmer than that. If you took November 2010 and compared it to the Novembers of the twentieth century, November was warmer. So was October, so was September, so was August. In fact, you have to go back 310 months to find a single month which was not warmer. Then after that, you have to go back another couple hundred months to find the next one. So there is no question the planet’s warming. That’s just observations. Arctic Sea ice is at an all-time low, and it’s probably going to continue that way. These are just observations.

Now, there is scientific issue about how fast will the climate change, what the future will hold. There are lots of scientific uncertainties, but the fact that the planet’s warming and the fact that CO_2’s a greenhouse gas and the fact that it’s increasing in the atmosphere and that it increases in the atmosphere due to humans—about those things, there's no debate. Yet people on the [Capitol] Hill right now are now hammering away at NASA, "Oh, well, you can't study the climate." It's just nonsense. It reminds me of the Scopes Monkey Trial [1925 court case on the teaching of the theory of evolution] or something.

It's interesting, too, if you see the language. I think this is something that we really have failed to do as scientists, is to watch the way people say things. "Do you believe in climate change?" This is not a belief-based system. Somehow we've gotten into a situation in the country where, for a sophisticated society that is highly dependent upon science and engineering technology, it is woefully illiterate about science and engineering technology issues.

The Hill—it was interesting when John [E.] Sununu, the senator from New Hampshire, was in. I disagreed with John quite often politically, but at least I could talk to him about scientific and technology issues because he was a graduate of MIT. He'd studied calculus. We could even talk about good and bad calculus books, and I can assure you that is not a normal conversation that you can have with representatives on the Hill. The Hill is illiterate when it comes to science and technology, and that's not good for the country. I think it puts a lot of pressure on NASA to try and explain what it does, because the average person on the Hill has an inadequate science and technology education, woefully inadequate.

WRIGHT: Did you believe, when you were testifying or offering information based on your results of the decadal survey, that you could possibly educate them to move forward?

MOORE: Well, we had a window there. That was actually a very interesting time because the NASA Administrator—these are just my views. Mike [Michael D.] Griffin is a highly intelligent man who said some absolutely stupid things. I thought that was always interesting, how such a highly intelligent person could say such dumb things. We got ourselves involved in a little bit of a debate about the decadal survey where he said, "Well, you've just asked for a bunch of money," and said we had come up with this decline in terms of Earth Science funding at NASA, and we'd created this envelope of what we would want to do if we got back.

Well, we simply observed the fact that we'd lost almost 30 percent funding in terms of real dollars between 2000 and 2007. I think that was just a period at which NASA's Earth Science Program was not being particularly well led and it was not putting forth a persuasive argument. And you had very good scientists in astrophysics and elsewhere, like Ed [Edward J.] Weiler, who can sell ice to an Eskimo. So you had very strong proponents for space science and not good proponents for Earth Science, and that led to this decline. Yet it was accompanied at the same time with an aging Earth-observing fleet, which we had not been able to replace because NASA had stepped away from the subsequent EOS missions, and we lost our way.

So when we did the decadal [survey] and we've put a persuasive case back onto the table, in some ways I think the Administrator's response to that—which was negative, saying that we couldn't afford this, we couldn't afford that, we couldn't do these things because he wanted to go off to the Moon—gave us a real debating arena in which we could put the ideas out there. *The Washington Post* and *The New York Times* and so forth covered it because we had a disagreement, and it was a fairly high-level disagreement.

My colleague, Rick [Richard A.] Anthes, and I were not about to step away from that disagreement because we really believed we had the numbers and we had the logic. If you added to that these large-scale environmental issues that were not going to go away and that were going to require more and more understanding of the planet, and you had these other changes that were happening around the planet, and we had a declining set of observations—we were not on a good path.

I think the Hill got it, and I think they got it because we had some very good staffers on both sides, Republican and Democratic. I think we made a lot of real progress. Unfortunately, I think with recent events we've taken a real step backwards, but this will pass. I think really what happened is that with the frightening events in the economy, the body politic really was frightened, and when people are frightened they become very inward-looking. I think they cease to look clearly at things because they're just so scared, and it makes them reactionary, it makes them illogical. They have reached a point in their lives where they're just scared.

And even when—it's really quite interesting to me, politically—the government felt, at all levels of the government, that we had to essentially pump-prime the economy to keep out of a core meltdown, and now people are not very confident that the president [Barack H. Obama] did the right thing, as if he were responsible somehow for the original core meltdown. It's all so illogical, because you say, "Wait a second, he didn't tee that ball up. He got clobbered with it." Remember, they even wanted to suspend the campaign for a while because the economy was in such a freefall. [John S.] McCain [III] wanted to do that. It's as if we forget these things. If you put the climate question back in there people say, "I've got enough problems with the economy. Don't tell me about the climate." They're just, "I don't want to hear about it."

WRIGHT: Can you share some examples of how not paying attention to future climate issues basically does affect our demise?

MOORE: Yes. I think that, to me, one of the grand challenges at the heart of the science is when we recognize that a lot of our predictive infrastructure has an aspect of stationarity to it. For instance, in the weather forecasting business we continue to refine the models, but we don't go in and say, "Well, yes, but the whole basic initial conditions have changed." In most areas, we're still treating the planet as if it has not been altered in some fundamental way. You hear people say, "If you don't understand the past, you're going to have to repeat the mistakes." Someone said if you want to predict tomorrow's weather, just say it's going to be like today, and better than half the time you'll be right because there is some kind of historical precedent for what happens tomorrow. We all know that.

I liken that to the fact that if the road is fairly straight ahead, I can get by by driving by looking in the rearview mirror, but if I were to try to drive through the Rocky Mountains that way, that would not last very long. So having some prognostic capability, some ability to look out into the future, and understanding how robust those results are, is extremely important as things begin to change.

It's a totally different area of science, has no connection with the climate problem, but imagine where we would be today in Japan [after the 2011 Tōhoku earthquake and tsunami] if we had the capability of more successfully forecasting or predicting earthquake events. Just imagine if there had been the ability to say, "In the next week we are going to have a significant major earthquake," how much better Japan could have been prepared. The loss of life could have been minimal as opposed to something like 20,000 people. You could have had reactors powered down, you could have had people out of the coastal zone, and right there you would have saved a lot of people. But this came without warning. The tsunami happened so fast, the tsunami came ashore over 500 miles an hour. They just didn't have a chance because there was no predictive capability.

The climate issue, that's a very different thing. It's not the same kind of discontinuity, not the same kind of massive event, but we need some understanding. What are going to be the implications of the climate and other issues of an Arctic Ocean that is close to ice-free in the summertime? What are the strategic implications to that: does the United States Navy need to park an aircraft carrier up there? What are going to be the commercial issues: are we going to start shipping fuel through the Arctic Ocean in the summertime? Does the [U.S.] Coast Guard need to be up there? So all of these big questions, we can't even talk about them because everyone gets all irrational about the climate question. It's as if you mention the word and then people will say, "Well, you can't talk about that." In fact, we've seen people saying, "You can't do climate missions."

Then the Administrator [Charles F. Bolden, Jr.], regrettably I think, says, "Oh, NASA doesn't do climate missions. It does Earth Science missions." Well, yes, Charlie, but you're really splitting hairs. You ought not to say that. The CLARREO [Climate Absolute Radiance and Refractivity Observatory] mission, which has now been cancelled, is a climate mission. You're not going to look at Earth's radiation budget for any other real reason than to get a fundamental measurement about climate. In fact, that's what we said in the decadal report.

So the fact that politically people have become so confused on the topic is really quite regrettable, and it bothers me, too, because I think that the beginnings of this problem—quite frankly, in my personal opinion—are traceable to Al [Albert A.] Gore [Jr.]. I believe the vice president, when he became vice president, was so committed to this topic that he, unfortunately, politicized it, because it wasn't originally a political issue. The act that brought the USGCRP, the U.S. Global Change Research Program, into existence was crafted under the [Ronald W.] Reagan presidency and was signed into law by the first [George H.W.] Bush president.

Throughout that whole period the EOS Program was a Reagan-Bush space program. So I think that somehow the vice president almost loved it to death. Somehow it became more political than it was, such that when the second [George W.] Bush presidency came in, then he took the other side. If the Democratic view is climate change was a real issue, well, then the Republican view was going to be it's not a real issue. It had become polarized. And now we're just living in the aftermath of this polarization, and it has an impact because these are fundamental scientific issues and we need to be after them. Yet now we're going to get into a situation where people say, "No, you're not going to go do that, because that's a climate mission." No, it's a science mission. It's as if the science aspect of this has gotten lost.

WRIGHT: What are the real risks, not just to Americans but to the globe, if the current satellite system is allowed to go dark and a replacement is not made in a timely fashion to continue the data that you have collected?

MOORE: I think the risks are many. One is the fact that the scientific enterprise of taking observations and testing hypotheses, making more observations, restructuring the hypotheses—that whole activity is not well understood by the body politic. They think that if you alter the hypothesis that somehow you're "cooking the books," as opposed to that's the way you should do science. The reason you take observations, almost the main reason you take them, is to refute the hypothesis, to find out what's wrong about the way you think about it so that you can make progress.

So you have a situation where people don't understand the scientific approach. Science, and then a topic within that, climate, has become a belief or a political issue, not unlike evolution or other political scientific issues. Observational strategies that are supposed to help clarify this issue become handicapped. Then we begin to actually lose the connection between the whole role of observations, for instance, weather forecasting. Somebody said, "What do we need weather satellites for? I get my weather from the Weather Channel." Yes that's a joke, but it was said and I think it is there in many ways that, well, maybe they're not really needed.

In fact, here we are today in 2011 trying to get a budget for [FY (fiscal year)] 2011 and 2012, which is going to be even more difficult. If you look at some of the elements of that budget, some of elements are to replace the low-Earth-orbiting weather satellites or the geostationary satellites. These are satellite systems that go back for fifty years. We have been in this business for a long time, and we had a certain characteristic in that business over a very long time, which was you did not want to ever be blind. Therefore we went to great efforts to have the ability to launch on failure, to bring up something they would call hot spare, if we did have a launch failure. I think it was the NOAA 13 that failed in low-Earth orbit in the POES system, the Polar Orbiting Environmental Satellites.

There was this fear of having a gap in the observational base, because that's what we use to make weather forecasts. Well, we're perfectly willing right now to proceed ahead where we're almost going to assure a two-year gap, because the funding is not getting in place for the JPSS [Joint Polar Satellite System] Program. Here's something that just five or six years ago would have been unthinkable, and now people are just, "Well, who needs weather forecasts?"

It is so strange that you really feel that somehow I'm in a bad dream and I'm going to wake up and then everything will be back to at least some form of rationality here. But no, we have to balance the budget, we have to do this, and weather satellites, well, who needs those? Well, I can assure you here in Oklahoma you don't want to go into tornado season without having both low-Earth-orbiting geostationary satellites, because people are going to get killed. And our friends to the South, same thing down in Texas. Weather is no stranger out here, it's no stranger in New England. We need to sober up here and start acting like adults again and stop this naiveté. Yes, we've got real budget issues, but those budget issues are made up of many, many elements.

WRIGHT: Is it a strength or a weakness in the overall system to have a number of agencies allegedly doing the same thing? I believe that's a perception by those that you have already categorized.

MOORE: I've thought about that. Could you take a research technology agency like NASA and have it take over the operational weather aspect of NOAA? Maybe this is just my narrow thinking, but I tend to think you're better off keeping those slightly separated, even in two different organizations. The reason for that is that I think that the day-to-day operational demands almost always will cut into the research base, and that when that happens it's regrettable. But, it would be very unfortunate if that research base leapt over to the research agency. For instance, if NOAA's eating into its research base then led to knocking down NASA's research base, then it could really be bad. That's why I've felt like a strength of the U.S. research scientific program is that it has a certain degree of diversity. You've got Department of Energy labs, you've got NOAA labs, you've got the NSF, you've got NASA. You've got things spread around, and there's a cost to it, but I think the cost is more than offset by the benefit of robustness. It just gives you more diversity, and I think that that diversity has many strengths, and this is one of them.

WRIGHT: In May of last year, you came to Norman to be a part of this great facility that's here. What were your goals when you came here?

MOORE: Well, I was very comfortable working Climate Central. I did have some things change that were not as I expected when I went into the effort at Climate Central in Princeton. I had been assured by many people that I would not need to worry about fundraising. I should have gotten that in writing, because after the first, oh, six months, nine months, I had to worry a lot about fundraising. In fact, that became the job. I also felt that maybe this wasn't my strong suit, the whole political communication and so forth, that maybe they needed people who knew more about how to communicate than I did. So I began to think of other things.

Charlie Bolden and Lori [B.] Garver talked to me about coming down to be the chief scientist at NASA, and that discussion was taking place in the March-April timeframe. At the same time, the University of Oklahoma had contacted me. If I think back, as we entered into the early April timeframe of 2010, I had pretty well settled on the fact that I was going to leave Climate Central and probably come down to NASA Headquarters. I like the people, some people I've known a long time. Chris [Christopher J.] Scolese [NASA Associate Administrator], I know him very well. Ed [Edward J.] Weiler [Associate Administrator for the Science Mission Directorate], I've known a long time. Lori Garver, less long, Charlie Bolden, I'm a big admirer of his. I disagree with him about his statements on we don't do climate missions, but I understand why he thought he had to say it.

I was kind of headed in that direction, but Oklahoma kept pressing me so I agreed to come out here with my wife in mid April, and we spent two or three days here. In talking with the president, learning of his vision, seeing what a change he had made in the place, caused me to think back about my experiences at New Hampshire. One thing that I think that our university struggled with at New Hampshire is we had too many turnovers at the top. The most successful presidency was Joan [R.] Leitzel, in that she gave a real body of time, and she was a wonderful person. I think a wonderful person plus a long body of time allowed her to be very successful. We had many times when the president would be there three or four years, and changing presidents every three or four years gets to be pretty chaotic.

David [L.] Boren, the president here, has been here sixteen years. He comes from great experience politically, senator of the United States, governor of the state, family from the political world who knows the state extremely well and had a grand vision. They have essentially moved this university from a mid-tiered state university in the Midwest to now one of a true international rank. More National Merit Scholars are here than any other public institution, fifth in the country in National Merit Scholars, one of the top producers of Rhodes Scholars, second largest ballet school in America, huge program in the arts. In seeing that expanse, particularly in the arts, and in walking around the campus and seeing the students and seeing the nature of the place, I realized that I'd be better off at a university than I would be at NASA.

Coming out here, one thing I want to do is the physician's oath, I want to do no harm. I certainly want to not break anything. It's a huge, powerful School of Meteorology, but I do think that there's a real role for Oklahoma to build out from that very strong meteorological tradition into regional climate studies. That that's where the grand problems are, to really be able to begin to develop an understanding of how climate might evolve at regional scales so that it becomes at scales where people can act.

That, I think, is a very important topic, particularly if we can begin to get an understanding at regional scales of what water will do. Understanding how precipitation might change in this area of the country is really important. You could imagine, say, in Texas or any of these areas—to understand how the hydrologic cycle might change, understanding how extreme events might change, understanding, for instance, heat waves or ice storms, what's the pattern going to be. Could there be changes in the distribution of tornadoes in the country, is that going to shift? These are all big questions, and the physics is very, very complicated in all the connections. So this is an area I want to see if we can't begin to make a contribution.

WRIGHT: I think of the Bretherton Diagram, and on the right-hand side is the human activity impact, and to me that's where you are in a sense. You have moved through the entire diagram and now you've touched all the parts.

MOORE: This is a really, really important area that we see now ever more, the beginning of the real inclusion of the human into the system, as opposed to just treating it as some kind of external forcing term. Because there's going to be a relation, there's going to be connections. A human uses energy, and, in part, the use of energy is to exploit how the human lives in the environment. Well, if the environment begins to change, then the human will begin to probably change energy consumption patterns, which will change the environment again. These are not separate objects anymore. And I think the more we understand of that, then we're going to get at the hard aspects of the problem.

When we talk about things we know for sure—for instance, the increase of CO_2 in the atmosphere is due to fossil-fuel burning, we know that for sure. We also know for sure that the burning of fossil fuels is right at the core of all economic systems on the planet. There may be some very few tribal places where they don't use fossil fuels, but they're very few today in the twenty-first century. And this is not a good thing or a bad thing, it's not an evil thing. It's just reality. When we were children in high school and grammar school, we learned about the Industrial Revolution. Humans replaced animal and human labor with the burning of coal and machines, and that leads us to the fact that today the consumption of fossil energy is deeply interwoven in all human society. So the idea that somehow you're going to change that easily or quickly is as naïve or as incorrect as saying humans don't have anything to do with CO_2 going up in the atmosphere. These are all interconnected things and they are reality.

I do think that on the green side, some people have been a little—not a little, way too optimistic about how easy this might be to fix. "Oh, we'll build a few windmills and we'll have some solar arrays, and we'll go on living happily ever after." Well, no. Right now, wind energy contributes about 2 percent of electric energy on the planet, and there's a lot of wind-generating stations. Look at Oklahoma and Texas. Electric power consumption is increasing at about 2 percent per year. So if we want to keep the problem from not growing, and suppose we have wind, that would mean I would need to double. You would need to rebuild the total current capacity of wind energy on the planet today next year, because you're going to grow 2 percent and we right now contribute about 2 percent. That means if you didn't want to have the fossil-fuel terms grow or the nuclear terms grow, and forget about solar right now, you'd have to double your wind capacity every year just to stay even. That's not going to happen, so we really have a huge problem.

And now we've decided to be noisy about it, so this is the worst of all things to do. You've got a huge problem across clear thinking. It's like driving in very hazardous conditions. Those are the last times you want to talk on your cell phone. Like in New Hampshire, if it's snowing out and it's a dark night and the wind is blowing like crazy and the roads haven't been plowed and you're driving, you'd better pay attention. Same thing today. All those things are happening. We ought to be paying attention and not throwing noise into the system. And we're doing the complete opposite, so it's just really crazy.

WRIGHT: What decisions or events need to happen, do you think, to shape the next twenty years?

MOORE: Well, I think several things. I think, first of all, we'll come back to rationality because the evidence is just so overwhelming, and it's just going to become more and more overwhelming. I think there will be a surprise or two. I think that the most likely aspect for surprises are in the Arctic and probably in the ice part, if we had some massive ice loss from Greenland.

Already to me it's just staggering, if you look at the ice loss in the summertime from the Arctic Ocean—and this is something that we have a very good record, because you have a historic record with the Defense Meteorological Satellite [Program], DMSPs. You've got a passive microwave instrument on there since the late seventies all the way up to the present. You've got a big passive microwave on Aqua, and we're able every day to get maps of the Arctic Ocean, and the passive microwaves allow us to separate frozen from open ocean, and we don't need to worry about clouds. So we have a fundamental satellite record that is extremely robust.

Claire [L.] Parkinson at Goddard [Space Flight Center, Greenbelt, Maryland] has just done monumental work in this area, and we know for a fact that we've had a very large ice loss over the last thirty years. We also know for a fact that not a single one of the climate models even remotely predicted this. So here you have a big change in the climate system, in the Earth system, summertime Arctic Sea ice, a very big change that no model got right. Well, that ought to give you some pause. And, in fact, in this case many times we hear from Congress, "Oh, well, you can't trust these models. They might not get it right." You're right, we didn't get this right. But guess what? It's worse. The reality is worse than what the model said. In fact, you could take all of the models that participated in the last IPCC, look at what's called standard deviation around the whole cluster of models, that still doesn't describe it. You got to go out two standard deviations. In other words, they didn't even get close to predicting the rapidity of the ice loss that our satellites are telling us is occurring. That shows you that models can be wrong by underestimating the changes that we're seeing.

I look at that and I say anyone who thinks about the precautionary principle or anyone who says, hey, does that get your attention? We're underestimating. We're underestimating what the changes are. The models are not showing you how much the change is. As opposed to overestimating the change, now they're underestimating it. And yet we're still fighting over, on the defense side, whether or not to even put a passive microwave on the follow-ons to the DMSP. This is lunacy, like being on a dark road and it's snowing like crazy out and the wind is blowing and there's ice everything, "Why don't we just turn the headlights off?" You know, it's just nuts. It's just nuts.

WRIGHT: You've been in this field since its beginning. What do you believe to be some of the greatest accomplishments that have come through?

MOORE: I think the really big accomplishment was the idea that we ought to look at the Earth as a system, that we really began to accept the fact that the simple ideas we have about cause and effect really don't make any sense when you have a system. What is cause and effect, what is effect and cause? These things are so interwoven, and we really began to seriously grapple with that. We recognize that not just superficially but the absolute connectivity of the Earth's system, and that we then began to actually successfully create Earth system models. That really has led to foundation for our beginning of the understanding of not just the climate problem, but how the Earth works. I think that is a monumental achievement.

The regrettable thing is that we still are not where we should be in terms of paralleling that modeling, understanding piece with a robust observational system, and I worry about that because you need to challenge these models with what the reality is to make progress. That's why some of the missions that we recommended in the decadal I thought were going to be so pivotal—for instance the CLARREO mission which has now gotten parked.

I can't completely be critical of NASA for parking it. All of these missions have run the price right through the roof, and we've lost the ability to do things efficiently. We feel like that you need eight people watching every one person who's about to turn a screw, as if that somehow adds for turning the screw more correctly. You've got all this wasted time, but that's another topic.

I think that if you looked at something like the CLARREO mission, what it had as its core was to use our ability, which we've already done with several instruments, to observe what the Sun is radiating out so we know what kind of the energy [is going] into the Earth system. Then if you could measure the reflected energy, the so-called short wave—some of the Sun's energy bounces off the top of the atmosphere, some of it gets into the atmosphere and comes right back out as short wave. As solar energy in, it bounces off clouds, bounces off of ice. We measure how much short wave energy comes out. Then we measure the thermal, the heat that comes out, and that's the part that the greenhouse gases interrupt. We do that carefully over a long period of time, then we essentially have the basic bookkeeping of energy into the system, energy out in the short wave and energy out in the long wave.

Then we would say to the climate models, "You've got to get this part right. If you don't get this part right, don't talk to us about climate because if you can't get this, we don't trust you." So in this case what you'd really be doing is you would have a litmus test and say, "This is fundamental. You don't get this right, you don't get to play. If you get this right, then we'll begin to listen to you." Then we start moving down into the models and seeing other aspects of where the other litmus tests that we might have, and then you make fundamental scientific progress. Well, we're not going to do that now. We parked that mission. Partly because it ran up in price and partly because it had the "C" word attached to it—and the budget. Yet we're going to have to get back to these things. The problem is not going anywhere, not going anywhere. And it's just going to get worse.

WRIGHT: One of the areas that NASA's looking at in this latest strategy is to branch out with more commercial partners. Do you see the area of Earth System Science branching out with commercial partners?

MOORE: Well, I don't know. I do know this; we've got to find a different way. Because I think that we've gotten into a situation where we've had some failures, and our response to failures I don't necessarily think is logical. That's why I think Dan Goldin had a certain correctness in terms of trying to get cheaper and quicker. I think when he put "better," that was probably where he went astray. We've got to find a more effective way of doing business, and I think this is where I'm in agreement with him, partially. People say we've got to get more effective with our tax dollar expenditures. Whether it's the fact that the three big science centers at Goddard, Langley [Research Center, Hampton, Virginia] and JPL have become too inbred or the preservation of the Center becomes more important than the doing in science, I don't know. But we're clearly in not a sweet space right now.

In some ways, NASA itself—I think big organizations are hard to change. We built up a huge amount of infrastructure, all these Centers, to essentially go to the Moon. And it's been clear to me that ever since then we've been trying to figure out—we've been there and done that, now what do we do with this big machine called NASA? Well, we build the [International] Space Station. Well, why? We're never very clear about that. We've had different reasons and so forth. I have a lot of friends who've been astronauts and have been there, and they found it great, exciting, and I'm delighted for them, but I'm still not clear what it is that this thing is really going to do. Then we want to go back to the Moon [2004 Vision for Space Exploration]. Well, I knew right away that wasn't going to work because the body politic doesn't want to go back to the Moon. It doesn't know what it wants to do, but it knows it doesn't want to go back to the Moon because we've been there, done that, and it was gray and dusty.

That was one reason why in the decadal [survey] we used the word "venture" for the small missions, because we started to think that what would a Bill [William H.] Gates [III, founder of Microsoft] do when he was young, or Steven [P.] Jobs [founder of Apple, Inc.]? What would they do if they had $150 million? How could we be more venturesome?

I still think that that is something that the agency needs to come to grips with, how do we actually change the way we're doing business? I, for instance, wanted to suggest to NASA one time that they ought to have one day a week where you're not allowed to send e-mail or develop PowerPoints, that PowerPoints should be forbidden. Maybe you're only allowed to do PowerPoints one morning every week, and other than that, you can't do them, because we somehow lose focus. The tools become the end, and we're not being venturesome, we're not thinking how to do things more creatively.

When we did Earth System Science we were really thinking far more out of the box than today. I think we ought to be more risk-tolerant. I know that this is a very hard thing because people get blamed if it doesn't work, but we ought to somehow get our minds into a situation that if it fails, well, okay, do it again. Fix it. Right now the way you ensure failure is you engineer to death and don't do anything. CLARREO is the worst of all things. It's just parked and the Destiny mission is parked. We have to find a different way.

It's interesting to me, too, because you take the two failures we've had with orbital [satellites], and in both cases it's almost the same failure. We lose the OCO [Orbiting Carbon Observatory] mission, and then a year later or so we lose the Glory [Earth-observing satellite] mission. It's almost the same [launch] failure. You say, "Well, wait a second." Then that ought to say something about all of this having watchers watch watchers watch watchers. Maybe what you should have done is put three smart people in the room and say, "Let's get to the bottom of this."

You remember in the first [Space Shuttle] *Challenger* [STS 5l-L] accident, where they really went right to the heart of the problem and brought out what failed? It was just thinking clearly, it wasn't taking massive amounts of data. These O-rings, what do they do when it gets cold? Well, they become brittle and break. We need to find a different way of doing business, and the Earth Sciences particularly. With the private sector, well, the private sector has some of the same burdens. We've got to find a way to be more creative.

WRIGHT: I know I could visit with you for the rest of the afternoon, but we're not going to do that so I wanted to ask you two questions. One was if you can think of some other areas that we have not had a chance to discuss that you would like to, and then before we stop I'd like to ask you about your thoughts about your Nobel Peace Prize.

MOORE: There is something that I felt that should be recorded, and that was what happened when we lost the follow-ons to EOS and how did all that occur. Because I was there and I watched it and I couldn't seem to effect a change. It's one of the few times in my life where I could see what I felt would be the end result. It's really quite interesting because you have a convergence of forces that led to bad decisions.

Dan Goldin, it was clear, believed that NASA was an R&D [research and development] engine and that you fueled that R&D by having missions do different things. That's understandable. I can certainly understand that, because that really is a lot of NASA. That really is at its heart. So when you have this need—to understand the Earth requires a long time series, he looked at that and saw a threat to the R&D engine, and these were very real different dynamics.

For instance, [Charles] David Keeling, in 1957 as part of IGY [International Geophysical Year, 1957-1958], starts measuring CO_2 at Mauna Loa in Hawaii every single day. I remember talking to David, he wasn't going to let that instrument change. He wasn't going to change anything. It was going to be a hell of a price to pay to swap out putting some new detector. "No, I'm going to make this measurement. I'm going to know exactly what I'm doing, and I'm going to make it every single day." Even though the Department of Energy after about ten years said, "Oh, we see it's going up. We don't need to do this anymore," Keeling's persistence said, "No, this measurement's too important." It's now called the Keeling Record. It's a hallmark of what it takes to do climate-relevant measurement. The same with Claire Parkinsons's work using microwave to look at sea ice.

You can cite these things over and over again, but Dan wanted out from under the EOS repetition. I thought maybe Dan would redesign the instruments and make them better. Well, that wasn't going to be big enough change for him. He just said, "No, we're going to do something else." So here's Dan Goldin wanting out from under EOS, the out-year missions. Jim Baker now is head of NOAA, and Jim knows the climate question is a huge question, and he wants to position NOAA into becoming the climate agency. I think Jim recognized that that would cost some money, and I think he felt the [William J.] Clinton administration—and we talked about Al Gore was very, very involved in this climate thing—that if he could get into the climate side of the equation, money would flow.

Then this opportunity came along which was called convergence. The military's DMSP program, the low-Earth-orbiting military satellites—there were two, one in the early morning and one in the early afternoon. Then NOAA had one in the mid morning and one in the early afternoon. So the idea became, well, we've got four satellites and only three orbits. We get rid of one, we could eliminate the extra one. The original idea of convergence, which led to a presidential decision directive, was to get rid of the duplication of an extra satellite. In fact, if you read the presidential decision directive, the word "climate" doesn't even appear. It is to make the NOAA-Air Force Weather Observing Satellite Program less redundant, more efficient, save some money, and then go to common ground processing and common instruments and so forth. Very simple idea.

Jim Baker, I think, saw the opportunity to say, "Well, what we're going to do in this convergence is a whole new program, and it's going to take over the NASA climate mandate," which Dan Goldin was willing to toss over because he wanted to go do other things. Now you've got yourself set up for a potential problem, because here come these climate requirements from NASA with nobody over to NOAA and the Air Force, mainly at Jim Baker's insistence. And the Air Force is not paying too much attention because when I last checked—in fact, it's proven out today—the Air Force didn't have any climate requirements.

NOAA takes the climate requirements on board and brings the Air Force along with it. Of course, the Air Force has got all the money, NOAA has very little money, and then the program gets into trouble. Now what begins to happen is the Air Force says, "Climate? I'm not interested in climate. I'm in the weather forecasting business." So the climate starts peeling off. It was just amazing to me to look at how NASA got out of the business, passed it over to NOAA, NOAA embraces them, the Air Force doesn't pay attention, doesn't know whether it should embrace them or not. The vice president is interested so they decide not to push back, and this thing then goes off the tracks in cost overruns and they cut back. I thought, this was doomed for failure.

Now we've had to divorce, so the Air Force is over here doing their thing, NOAA's over here doing its thing. Thank goodness the Europeans are going to fly something in the mid morning because now we're down to two satellites, one NOAA is going to do in the afternoon and one the Air Force is going to do in the early morning. We've gone from a four-satellite system down to a two-satellite system with the Europeans handling the mid morning; both programs grossly over budget, way late. And now in the CR [continuing resolution] situation [FY 2011 budget crisis] probably we're going to have gaps in our coverage. It is just a disaster, and highly thoughtful people somehow didn't see this thing coming. I remember sitting in the meeting watching Baker take on this responsibility and Goldin give it up, and no resources to do it. Crazy, crazy.

We kept this in the decadal. The decadal survey, I fear, has a little bit of the way people read mystery novels. They speed-read through up to the last ten pages and they read that. So the decadal, they just look at the recommended missions, speed-read through everything and say, "Well, what missions did you recommend?" We really tried to describe this particular problem in POES and the disaster and so forth, because I think there are real lessons to be learned here, that these are major programmatic things and we just don't think clearly about them.

I just think we really need somehow to come to grips with what it is we need NASA to do and have a very clear discussion about that. I don't think it's to become a NOAA for Earth because of what we talked about earlier; I think operations and research need that separation. But we clearly haven't figured out what it is that NASA should be doing and how to afford it.

WRIGHT: When we first were talking, we talked about all the scientists and the interaction with NASA to help build that direction, and of course now with the budget issues—who do you think needs to make that direction?

MOORE: I think that we have collectively given up some things and we didn't truly realize what we were giving up. I think that there was a period where the advisory infrastructure of NASA became weaker because people were saying, "I know what to do." Mike Griffin, I think, said that particularly, "Got enough advice."

But the advisory structure really is a two-way street. It's a way in which a group of people become deeply engaged with what NASA's trying to do, and can, as non-federal employees, sit down on the Hill and deeply engage the Hill in what is trying to be done. And in that dialogue you can begin to find areas of convergence or areas that we can figure out ways of doing better. Somehow we've lost some of that connectivity between the scientific community, Congress, and NASA, and now there's a lot of talking past one another. I would think that we've got to find some pathway back in to build that community back into the system.

I think historically I can look back and see where the Earth Science Program became an example of when that dialogue really fundamentally broke down, and therefore there ceased to be a vision. In other words, it was after EOS, then what? When that went away, there was no then what. It wasn't clear where anyone was going, and if you're not clear on where you're going, then I can assure you you're not going to get any money. I think we're in that same situation right now. Not clear what the Manned Space Program should be, it's not clear what the Earth Science Program—because it's gotten all involved in politics. The astrophysicists have got a dinosaur on their back with the James Webb [Space Telescope]. Here, in some ways, good old heliospheric's chugging away, and it's the old NASA P.I. missions.

And I don't fault anyone. It's just like we talked about earlier, about the big breakthroughs. Really at the heart was to recognize the Earth was a system, and the simple linearity of cause and effect is way too narrow a model. NASA is a system, and the simple linearity of, well, this decision led to this, led to this, led to this—no, it's more complicated than that. We need to get back to that dialogue, because I think we could have a dialogue that was more systematic.

WRIGHT: Do you want to share a few minutes about the Nobel Peace Prize [awarded in 2007 to the IPCC and Al Gore] and the impact that it had?

MOORE: Well, clearly we didn't get the Nobel Peace Prize, but we were significant participants in the organization that was honored. I think that it was, on the whole, really a very strong statement, and at least for a period of time the strength of that statement held. I don't have a good way of saying this, but I do fault Gore as the vice president of politicizing the program, and I worry that his actions subsequent to the Nobel did nothing to de-politicize it. He could say, and with some justification, the film [*An*] *Inconvenient Truth* [documentary] were statements, by and large, of factual material put into a more popular medium. But even there, he stretched the truth in some areas. That's a perfectly normal thing for a politician to do, and I have no problem with exaggeration—I mean if I'm telling a joke, quite often I'll exaggerate—but if I'm speaking about a scientific topic, then my DNA [deoxyribonucleic acid (hereditary material)] says, "You're not telling a joke. There is no exaggeration here. Precision of your language is very important."

The vice president doesn't have that same DNA because he's not a scientist. In fact, he's a journalist in terms of his academic training, and he's a politician, and the rules are different and the DNA is different. But he often tried to present it as if he were a scientist, and that's where I think he overstepped. With his receipt of the Nobel it would have been better, but maybe it's an impossibility, if he had then set about de-politicizing this problem. I think that's probably asking the impossible, to ask a politician to de-politicize something, but I do think that then led to a further polarization. So there is some downside to that Nobel.

That's why it was so important in the early days with Reagan, that he embraced the topic. I am told by people who, I think, know—in particular Bob [Robert T.] Watson who used to be with NASA Headquarters and knew the UK political establishment very well—that Margaret [H.] Thatcher [former UK Prime Minister], when asked by Reagan about the climate question, her response was, "Ronnie, take it from me. It's a real problem." And for Reagan, that was all he needed to hear. She didn't say it's a real *political* problem, it's just a real problem. It's reality. Probably walked him through the elementary part of it, and that's all he needed, and therefore we have to take this on.

It's true that Reagan wasn't about to turn off fossil fuels, but nobody else is either, from what we talked about earlier. It's just too big a problem. But you sure need to understand it as a scientific issue. Yes, it touches upon society and political things, but it is a fundamental big scientific problem, and it would have been fine if we'd have gotten back to that. I don't see the pathway back to it now, but we will have to find it.

WRIGHT: And we look forward to seeing the work that you'll be accomplishing from here, and hopefully all of those paths will meet together.

MOORE: I'm learning about Oklahoma, and it's a grand place, and the university here is committed to making a difference on many topics, including the climate topic. I think a lot of other universities are recognizing the grand problems are the ones that we need to go after, be they in health or social issues or scientific issues, and we're going to try and make progress on it.

WRIGHT: Thank you for your time afternoon.

MOORE: Thanks very much. It was great, glad you could come up.

[End of interview]