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

ERIC BARRON
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is July 1, 2010. This oral history with Dr. Eric Barron is being conducted in Tallahassee, Florida, for the NASA Headquarters Earth System Science at 20 Oral History Project. The interview is part of a series to gather experiences from those who were significantly involved in the efforts to launch and foster the concept of Earth System Science. Interviewer is Rebecca Wright, assisted by Jennifer Ross-Nazzal. Thanks for taking time out of your schedule to talk with us. We'd like you to start by giving us a brief history of how you got into your field of expertise.

BARRON: I actually wanted to be a geologist when I was in the third grade, and decided somewhere in high school that I wanted to combine geology and oceanography. I went to my undergraduate program at Florida State purely because it had geology, and if I did well I could take graduate classes in oceanography while I was an undergraduate. Then I went off to graduate school in oceanography at the University of Miami, so I had a deliberate path. In third grade, I thought I wanted to be a geology professor.

WRIGHT: You were actually involved in Earth System Science as early as 1986.

BARRON: Even earlier. But in a very direct way by 1986.

WRIGHT: Would you share with us how you got there and how you brought [the concept of Earth System Science] forward?

BARRON: I suppose this is a strange thing, but I was sitting there and I had a fellowship in graduate school from Texaco, and Texaco let me take any class I wanted. They paid for my books, they paid for my tuition; there was never a time during my graduate career where I didn't keep taking classes. So I kept exploring, and I kept changing my mind as a consequence, what it is that I really I wanted to study. First I thought it was organisms and then I thought it would be geophysics and then I decided it should be climate in Earth history.

The whole notion of plate tectonics had come of age while I was a graduate student. What was going to happen following plate tectonics? Well, my thought was that the ocean and atmospheric circulation would change as the continents moved, and nobody was thinking about that. So I decided I could be a pioneer in that particular area.

Since Texaco was paying for it, I started to take classes that were in climate, and I had a physical oceanography class. I ended up taking a dynamic meteorology class, but I was a geologist. Most geologists don't take dynamic meteorology class. I put on my committee a physical oceanographer, an atmospheric scientist, and a couple of people who worked in geophysics. The meteorology professor said, "I want you to apply to the National Center for Atmospheric Research [Boulder, Colorado] for a summer fellowship in supercomputing. There's six of them at NCAR." NCAR is the name for National Center for Atmospheric Research. "There's six of them, and you should apply. You spend half the day learning how to use advanced supercomputers, vector-based machines, and you spend half the day working on a scientific problem."

Well, I thought this was really hysterical in some ways, and truly I rolled my eyes after I left the room, because there was no way a geologist was going to go to the National Center for Atmospheric Research on one of six coveted summer fellowships. But he was on my committee so I applied.

Lo and behold, I get one of these fellowships, and I go to NCAR to spend the summer. The person next to me, who's now a professor at MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts], was doing this complex simulation of thunderstorm development, and somebody else was doing some simulation efforts of planetary waves. Here was this crazy geologist. I started to work on an ice age atmospheric circulation problem.

I finally got up enough nerve to ask the people in charge of the program, "How is it that I got here? That you decided on a geologist?" My knowledge base was very different. It didn't match up with everybody else that was there. There was no one at the entire institution paying attention to Earth history.

She said, "Well, Cray—" Seymour Cray, Cray [Computer Corporation] supercomputers, had given them the six fellowships but had told them that they had to accept one oddball. So for all I know I was the only oddball that even applied and they breathed a sigh of relief and said, "Oh, thank goodness, there's an oddball we can accept and follow through on this."

I brought my maps of the way the Earth looked 50 million years ago and 100 million years ago, and I went around the institution showing people those maps, and talking about whether or not we could do a climate simulation for 100 million years ago, 50 million years ago. I found people there like Warren Washington who were very interested in the problem and what it might mean for doing climate simulation. So they invited me back.

This was a crossover between geosciences and atmospheric sciences and ocean sciences, because I couldn't do my simulations without having this background that crossed three different disciplines.

They invited me back each summer. I worked on my dissertation there. They gave me a postdoc [post-doctoral], and they gave me a job as a Scientist I and then a Scientist II. Wonderful institution that provided great opportunity, changed my life.

Then it turned out that there was a committee at that very same time, quite famous, creating the Earth System Science Report, and focusing on these same topics. A number of institutions decided that this approach to Earth System Science was about to take off. I'm a young guy, I'm 35 years old, I'm not that far out of my PhD, but Penn State [Pennsylvania State University, University Park, Pennsylvania] decided that this had a lot of merit, and that they were going to create an Earth System Science Center. It was the first Earth System Science Center to come up, and they were going to connect their departments in atmospheric sciences with ocean sciences, with geosciences, with geography.

I looked at an advertisement that they had in journals like Eos for a Director of the Earth System Science Center. I read it; I said this is a great idea, its time has come, we've got to promote these intersections between these different disciplines if we're going to get anywhere, I'm going to look forward to watching them do this. But I wasn't going to apply, I was just too young in my mind.

I got a phone call from a Harvard [University, Cambridge, Massachusetts] professor, Dick Holland, who was spending the year at Penn State helping them get it organized and seek a Director. He said, "Eric, I want you to come up and give a talk at Penn State."

I said, "Dick, I'd love to come give a talk at Penn State."

He said, "To be truthful, we want to look you over for possibly being the Director of the brand-new Earth System Science Center. We're going to hire a group of about ten faculty. Got a new model. We're going to promote this intersection between all these disciplines. We want you to come do this."

So I interviewed. I had a great time. I called my wife up on the phone and I said, "Look, these people know what they're doing, they've got a vision for the future, it's smart. I just don't think they're going to offer this job to me, but as soon as they offer the job and get a Director I'm going to call up the Director and say hey, I would love to be one of these ten faculty hires." But they turned around and offered me the job as Director. Again, I said, "Well, you took a bit of a risk there, because I'm really five and a half years out of my doctorate, and I'm sure you had a lot of very experienced people."

The dean, John Dutton said, "We had a lot of experienced people who were experienced in one discipline. We didn't have people that we felt could communicate with more than one discipline. You have gone from geology to oceanography to atmospheric sciences and climate, and we could tell you were comfortable talking with people in the departments. The biggest job we see is, how do you get all these departments talking to each other?" So that started the Earth System Science Center at Penn State, which just blossomed into a wonderful entity and grew beyond Earth System Science to a broader institute and a large number of people participating and many more than ten positions that were hired eventually at Penn State.

WRIGHT: Did you become involved with the [Bretherton] Committee's work or when NASA was pulling this together?

BARRON: The Bretherton Committee's Report came out, and I keep the document to this day, maybe for its historical significance. I still have one that's still in its plastic never opened because I just figure that someday I'm going to want to go back and look at it and think about it. That spurred all sorts of different activities and focal points, including changing how advice was given to the federal government, particularly in the National Academy of Sciences. What you saw emerging was a Climate Research Committee out of the National Academy of Sciences, the Board on Atmospheric Sciences and Climate, the Committee on Global Change Research. The Committee on Global Change Research had many different manifestations. The name changed several times during its history, but you had those three groups, and then some others, that all began to interact within the same arena.

Very shortly after I became Director, I got asked to chair the Climate Research Committee, which made me a member of the Board on Atmospheric Sciences and Climate, and also made me a member of the Committee on Global Change Research. I view those committees as the ones that stepped in following the Bretherton Report specifically to look and evaluate many different programs, so I had responsibility as chair of the committee for looking at all the world climate research efforts.

The Committee on Global Change Research had a focal point on all the international geosphere-biosphere projects and the US Global Change Research Program. So we really jumped from the Earth System Science Report to all of these different manifestations of projects within the federal government and internationally. They began to take off from that vision.

I don't think I actually would have been so involved in those except I became Director of the Earth System Science Center [at Penn State]. Because I became Director of the Earth System Science Center, people viewed this as a commitment to an area, as being from a place that was actually doing these activities, and even though I was a relative youngster, Penn State must have hired me for some reason. I ended up getting very involved in those activities and from there very involved in NASA Earth System Science activities.

WRIGHT: In 1992 you took a committee membership under the [NASA] Earth Science and Applications Advisory Committee and stayed on that committee for a number of years.

BARRON: I was actually chair for a short period of time. During Shelby [G.] Tilford's last year as the Associate Administrator for Earth Science, I was the chair of that particular committee. Actually I was on it for a period of time and then left and came back on it again when [Charles F.] Charlie Kennel was the Associate Administrator.

WRIGHT: You had the opportunity to be there as the [NASA] Administrators were changing and during the [Daniel S.] Dan Goldin restructuring effort. Can you tell us about your involvement or what input or influence you had on some of the decisions he made regarding Earth Science?

BARRON: I would say that there are a lot of different elements to this. When I was Director of the Earth System Science Center, I wrote a proposal and became PI [principal investigator] on one of the interdisciplinary programs for the Earth Observing System [EOS]. The one that I worked on, I think it was called climate and hydrology, but I was on this working group. Then I got elected as the chair of the Science Executive Committee for EOS, and I held that position for quite a while. So whenever we were having a discussion about the Earth Observing System, almost all of the scientific questions and how they matched up with instruments and launch schedules came to the Science Executive Committee. Most people don't realize that every six weeks we met in the Chicago airport from all across the country and I was chairing these meetings, so that was truly interesting.

I was coming at the NASA issues from a viewpoint of being involved in the National Academy committees because I was chair of those sets of committees for a string of about 13 years, different committees, different terms, what they called ESSAAC, [Earth System Science and Applications Advisory Committee], the advisory committee, and the Science Executive Committee for EOS. I would say that this is a period for which what was going on at NASA was under constant review, and constantly changing parameters. We began with a vision where there's one rocket sent up into space and we needed to design many instruments to fit on it. The reason why we focused on a mega constellation was because we were taking advantage of the launch vehicle, and because we wanted to measure things simultaneously. We also planned on making copies one right after another, so that we would have a continuity of measurement in five year increments, always eyes in space looking back at the Earth for all the things you want to measure - a long-term, robust, continuous, consistent effort.

Many things happened through this time period, one of which was we couldn't afford everything that everybody wanted to do. Of course this always happens. I would say there was repeated scientific scrubbing plans and many discussions among ourselves about what we could launch and what we couldn't launch and what would fall off the table. I would say this was a lot of scientific stress that we were imposing on ourselves as a community. So that's one element.

Another element was that the budget projections also began to change. We began with this picture of growth that this would continue and our effort would continue to expand, and NASA would attract young scientists one right after another, and even though they had these existing interdisciplinary teams and specific teams focused on instruments and combination of measurements, it would remain a land of opportunity for many many scientists to write proposals.

What occurred was the budget started to become much more conservative, and we lost the opportunity to add new people. NASA's idea was that they would give the scientists five, ten years to work on this problem because they knew it was a big problem, and it wasn't going to be the one-year grant or the three-year grant, the typical way things would go. They would tell a whole team of scientists, "We're going to give you a decade to work on this project." I actually think that it was a brilliant, much needed strategy, because that is the type of problem that they had. You still had accountability but you had to have people committed for careers to work on this.

When the budgets got tight, the rest of the community started to say, "Why do these people have a free ride for ten years and we can't even get aboard? We didn't know that when the first call for proposals came that that was it, that if we didn't get on the train then we weren't going to be able to get on the train." It was never intended that no one else could get on the train.

We began to see the long-term approach lose political and scientific support, from the viewpoint of why we weren't going to have new instruments come online, why do we have to just make copies, we're going to not have technological evolution, we're not going to be able to get different scientists on board, we shouldn't have these long-term projects, we should go back to commitments that are three years or five years or two years or whatever else. The budget was clamping down, and I would say this is the same time we have this transition to the Goldin era of NASA where we had cheaper, better, faster and a focus on technology, because the budgets were getting carried away, and NASA had too much on its plate.

We went from this big vision - long-term, continuous. Well, other launch vehicles also became options. We didn't have them at the beginning; we couldn't design anything different than what was actually designed. I didn't do the design myself. I was focused on the science. We really watched that evolution occur where the vision began, I think, to stumble because the long-term commitment was harder to come by.

Now lots got done. Wonderful instruments got launched, but today we struggle mightily with how to not go backwards. There's every sign from the last five years of budgeting that what we have in space will be less than what we had if you go back a decade.

But it was a grand plan and a grand scheme, and the objectives are still good ones. I don't think they're ever going to go away. Not for decades.

WRIGHT: The years that you served on those various committees, how did they assist each other in furthering the concepts of Earth System Science? Then yet again, were there times where different committees had maybe just a little bit of a different goal that might have caused non-advancement of the goals?

BARRON: Well, there's no doubt about it that every time the budget gets tight you then move from working to do things synergistically and try to combine efforts and take advantage of it to one where you become a little bit more competitive. So there's no doubt that we became more competitive because you start to think about what's going to fall off.

In my mind the investment in the synergy of the observations also lost ground because of this same reason. I've got my instrument, and I want to do my measurements, and I want to do them the best that I can. Here's somebody else with an instrument, and that's what they want to do. Then, there is the vision that by combining these measurements we can finally solve a problem, like understanding clouds. Go back to 1968, and you can read articles about the biggest problem in trying to simulate future climate is related to clouds, and you can go a year ago and read, "The biggest problem that we have in solving our ability to simulate future climates is clouds." That's because they're very complicated and you have to have multiple measurements going on at the same time.

But then consider - I'm trying to get my own piece done, but there isn't enough to go around? I'm barely getting my own piece done. We didn't have the capability to bring the other pieces into the fold? So we really lost two things. We lost the strategy for continuous measurement, which was part of the linchpins of EOS because we're struggling mightily to get things back up there before we lose datasets—and I don't care what dataset it is. Over and over again. Whether it's Earth radiation observations, whether it is ocean color, whether it's—every single measurement you're struggling to keep them going.

That's one thing we lost, and the other thing we lost is the dream of really doing all those synergistic observations. A lot is happening, I'm not saying a lot is not happening. But, we didn't get to realize the dream was when the dream was within our grasp. It's interesting. Such great science, great things. That little bit of delta in the amount of money, and we would have gotten a lot of cream that we didn't get.

WRIGHT: It's so interesting for us to hear how you happened to be basically in the right place at the right time that all this was starting to evolve, and you had set yourself in motion to be there.

BARRON: It seems like luck to me. I think the door opened and I walked through it, even though I had a perfectly nice environment. But still the door opened so I decided to walk through it, and lots of fun things happened because of it.

WRIGHT: During those times, especially when you began as Director of that first center, and building that staff, did you find resistance or hesitancy from the "old school" of thought which said you needed to stay within your discipline?

BARRON: Yes. But it was actually set up in a very clever way. I give a lot of credit to people like John [A.] Dutton at Penn State who thought about it and had multiple purposes in what he was thinking about. I think his view was that some of his departments had aged and he needed to revitalize them. He didn't want to give these faculty positions to a department that had aged, because they might go and just do what they had been doing, and the Earth System Science Center concept was there at the same time. Perhaps because of that, the plan that we worked out was that the money for the faculty positions would sit outside the departments and I would work with the department to hire somebody new. If the department liked the person and I liked the person then we would hire the person and I would actually transfer the money out of my budget into the department's budget. There was an enormous amount of incentive for a department to work with me because they could get a new faculty hire, and from my viewpoint and I think from the dean's viewpoint that faculty hire was unlikely to be like any other faculty hire that they would have done by themselves, because we were out after a different kind of purpose.

The other thing is that we learned a lesson from the environmental focus that occurred 10 and 20 years earlier, because the universities that created environment programs, they tended to fall flat, because the faculty were viewed as too shallow, too spread out among different disciplines, and they had trouble getting tenure within the typical silos of the university. But we said from the beginning that the department wants to hire this person, the department has already agreed that they want this person on board. If the person left, I got the money back to start over again. This was like this little engine that you put in the middle of a structure of departments that could help motivate moving in new directions without being threatening to the departments.

I also told the departments that if those faculty brought in research dollars, which they had to do, I would let the department count it. The department could do a regular teaching program. I said, "I'm not going to count this on the Center ledger. Instead, I'm going to energize this, I'm going to facilitate this." Then I asked the dean, "Can we double-count? I'll tell you what my impact is, but when it comes to the university's statistics and evaluating the departments, the departments get to count all their faculty, including the ones that are mine that I transferred the money over." That became an economic benefit to the departments.

The people we brought in were exceptional. They were put up for Presidential Young Investigator Awards. They got all sorts of grants and contracts. We had two [David and Lucile] Packard [Fellowship] awardees. Here's this little program and I think we had our third Packard nominee. Only the top 50 universities in the country can nominate someone, and they can only nominate two. Here's this huge university, and in three years I had three nominees. It's because even the people that were attracted to come into that environment realized it was different. The money came from an Earth System Science Center [because] we want you to focus on the frontiers. But I have a home in a traditional department. This was an experiment, so you got people that liked to think of themselves really as part of that cutting edge.

Sometimes there was resistance. We interviewed in my view a hotshot scientist doing a postdoc and he worked on the ice caps and ice cores and modeling ice sheets. He had a lot of papers even though he was very young and I really wanted to hire him. The Geosciences Department said no, that he was too far afield, that ice didn't have a home anywhere, and that he wouldn't be able to attract students, and so he would feel uncomfortable and he would have no one to partner with. The young man's name was Richard Alley, who is now one of the most renowned glaciologists in the world, and a member of the National Academy of Sciences, and he has a whole team at Penn State. He would not have been hired by that department if it wasn't for the Earth System Science Center, and initially the department said no.

I went to the department and said, "Come on, he's free. I will give you all the money for him. I will pay for the startup. He's free. Take a chance." So they turned around and voted yes, and look what happened. Now this to me is this dream of what you want for Earth System Science, to have all these disciplines together. He brought in seismics coming from one scientist and chemistry coming from a different direction, and all of a sudden ice cores become this tremendous and exciting laboratory. That's what made the Earth System Science Center a lot of fun, because you could bring in really interesting different people that were working on problems that everybody knew were important. So it was good.

There was some resistance, but a model that helped you overcome it. I said, "Departments, you take all the credit. I'll provide the money, you provide the tenure-track line." Good model.

WRIGHT: Somewhere in there you managed to get your doctorate degree.

BARRON: I had my doctorate before going off to do all that, 1980.

WRIGHT: I was curious. I notice you got a prize for having the most creative dissertation.

BARRON: I did. But see, this was crossing two disciplines. The fact that I was working in two different departments in the geology side and the atmospheric sciences, physical oceanography side got me an award for the most creative dissertation. Same sort of idea, isn't it? Just because early on I got the opportunity to experiment and go take classes that were out of the realm of the main stream of classes in my field.

At the end of my graduate school career I was still taking classes, because they were free. Even when I went off to NCAR as a postdoc I took a class at the University of Colorado. I was still trying to get the background in more than one discipline, because that's not how I was trained at the beginning. I wish I'd been clever enough to think of it at the beginning. I could have been really well trained.

WRIGHT: It seems from everything you said so far that multidisciplinary and interdisciplinary are keys to making this whole concept benefit the whole—

BARRON: I will tell you flat out that I would never ever have had the career I've had if I hadn't crossed two disciplines. Never would have happened. I can't imagine I'd be a university president [Florida State University]. I became dean because more than one department saw how I operated the Earth System Science Center. They realized I was out there to promote more than one department, it wasn't just me standing there thinking that my one department was the great department. I wanted all my partners to be great. Everybody liked the way that worked. They said for a long time, "You're going to be dean, you're going to be dean."

I don't know, I like my research career, I got a lot of grants and contracts, I got great students, they're going off, taking faculty positions, this is a pretty good life. But I became dean. I had a wonderful experience in being dean, and you get to go to the next level.

You can imagine what it would be like to have gone to NCAR as the oddball, and then 28 years later to go back as a Director [2008] to run the whole place. It is an interesting world.

WRIGHT: When you went back, [NCAR] had some budget issues,

BARRON: There are a lot of budget issues. There are budget issues everywhere. We basically watched the Earth Observing System and the difficulties with that budget actually transform the project. At Penn State as a dean, we were having to give back money because this was a time where states started to believe less in the public good of education and their contributions began to decline and university tuitions started to go up. I would say that for that period of time, that last eight years or so was a time where I don't know of any institution that wasn't struggling with its resources, because the public dollar was declining. Not as easy to come by.

NCAR was in that mode, but I think NCAR was a special situation. The topics were so important, and you see the climate issues all emerging, and their significance, or the importance of severe weather. The importance of improving our ability to predict weather and climate and observe them. So you had this anticipation that, "oh yeah, this was a bad budget but next year will be better. Next year has got to be better. Look how important this is to society. I think people started to not solve the budget problems as they came along because they thought the next year would be better, so the problems began to build up until you had to do something about it.

I walked into the NCAR job and within three weeks I was told that I had to cut about eight to ten percent of the budget. It wasn't my most fun time, but we came out of it quite well in my opinion.

WRIGHT: Sounds like you have a lot of experience talking to people about budget declines. I know that all your years working with NASA on its committees there's a number of times that you've addressed [US] Congress. Could you share with us those experiences of trying to talk to subcommittees about the fact that budgets need to be adjusted?

BARRON: Well, we're probably not the most famous group for being good communicators. I think that if you look, once we launch something or do something we never want to give it up. We always want more. I do believe the strategies that we had were to go in and talk about the new things we wanted and what we wanted more, and you could sense Congress through that period of time going yes, and [asking] what are you going to give up. We never wanted to give up anything.

I do think it became very important to start to move to a mode of what the impact of the science was going to be, what the value of it was going to be. You start to also realize that topics like climate became politically very sensitive and a political controversy that is quite frankly much greater than the scientific evidence would allow; I always note that a conversation among scientists is very different if it was outside of the press. It also became a lot more important to talk about how it is that you gain value across society not just from this one issue but from many many different issues.

I think part of this dream for how Earth System Science works is that you have put enough information at your fingertips from many different sources and create an ability to predict the future that is applicable to any problem. You have an atmospheric chemistry problem, you have the simulation of the atmosphere done well enough that you can add the chemical reactions and come up to a solution to that problem, or realize that you need this observation, and if you have it coupled with these tools you can go out there and solve it.

My view was the notion of it wasn't just we can combine these disciplines and have a whole Earth view. I thought the beauty of it was that we would have the tools to be able to predict the future to solve all sorts of different problems from society, whether it's what's going to happen because of a volcanic eruption, what happens because of some new chemistry that's a fumigant for pests, what might happen because X happens to the ocean, or an oil spill and where will it go. It's phenomenal to think that we struggle to decide where that oil is going to go, and do we don't have a clue what's going to happen when a hurricane hits and interacts with the oil spill, and the answer is no we don't know. We didn't quite get that far, but I thought that was really what the beauty of the whole Earth system effort.

That would mean that we can now promote economic vitality because we have a much better sense of how storms will evolve. We can promote safety and protect property much better because we've increased our predictive skills in a different way. We can be better stewards of the environment. But, we haven't quite gotten there.

I think in going and talking to a lot of Congressional committees a part of this was to switch from some specific pieces of science that people didn't understand what you were talking about, and this notion of "we must save the Earth" because of climate change, to one which is we can have a very broad impact on society because we'll develop the observational capability and the predictive capability that we can help solve problems from all sorts of different directions. That was my attitude. As time went on, I felt more and more strongly that that's what we should be striving for.

WRIGHT: Money can always be an issue, but there are other challenges associated with trying to reach some of those objectives. Can you give us some examples from the last 20 years that have been addressed as you try collectively to move toward some of the goals that you just described?

BARRON: Well, I think this notion of your approach to the problem between a very big science and fostering a lot of little science, one of the things that became a challenge was to do both. You might think of that as a money problem but it's not really a money problem. It's a difference in philosophy of how you're working with huge teams or whether or not you work as individuals. I think there was a tremendous challenge in the fact that you got a sense that science for the Earth was being prioritized. There were some areas of science that were starting to drop off the table much more quickly than I think people anticipated.

Again you could call that a money problem if you wanted to, but some of those problems were so big and so comprehensive that people began to say, "And why do we need to know about how the continents moved so accurately? Studying deeper time in Earth history to understand how climate changed, does that actually have any particular value? Volcanic eruptions are infrequent; do we really need to have something that's deployable to catch them when they do happen, when we have all these other issues that are there?"

So you began to prioritize science. There was one National Academy panel that I was on that was called Grand Challenges in the Environment and it was like a Noah's ark of science. Two from each discipline that were coming on board to discuss what the grand challenges were. My view was people started to become very protective about their own area. You would even hear someone say, "Climate has enough money, it's time for us to do this."

In the social sciences there was this constant notion that social sciences were going to emerge as a full partner to the physical sciences. I heard it for a decade. It never happened. Just never happened. There was never the investment, nor did you find enough people who were willing to say, "We absolutely have to know how the physical-chemical-biological system interacts with the human system."

You wade through all the documents through time, and you'll see the same statement, that it will emerge at a time that social systems have the same kind of importance. The language barriers there were huge. They were just huge. It's not just language. The approach to science hugely differs. There were a lot of things there.

The big science, the little science. The whole notion—we had to prioritize. We're so used to going in and saying, "This is what we need, this is the next thing we need, this is the next thing we need," and we moved into this realm of saying, "These problems are more important in studying the Earth than these other problems." You have to do it, and if you don't do it somebody will do it for you. But the scientific community had to step up to do that. A big challenge.

Then some of these things we just didn't manage, like crossing over to the social sciences. Still a big problem. People are getting better at it, but the investment in the human side of the equation is a small fraction of what anybody thought it would be, even 15 years ago. They thought that it would be 100 times bigger than what it is today.

WRIGHT: Internationally you were also working with this. How did that differ from what you were doing with your colleagues within the States?

BARRON: What was interesting was the discussion about whether you could build this great constellation of observations and not do something in the US because the French were going to do it or the European Space Agency was going to do it or Japan was going to do it. There was a huge amount of pressure, I think, to make international agreements as a way to get more, but a huge resistance to letting another nation do a set of observations when we wanted to do them within the US. Arguments constantly were this instrument is more tuned to exactly what we want. We have more control over the data. That instrument might not be as good. We're not involved in the quality control. Very interesting discussion.

Then it had on top of that, quite sadly—could the US be trusted as a partner? We were in this process over that period of time of knocking things off our list because the budgets were tighter and we were moving to cheaper, faster, better. And we weren't in this cheaper, faster, better mode, we were also in this, how much can we measure at one time and how long can we do it [mode]. So things were pried off the list.

Then we'd have an international agreement about it and then we couldn't satisfy the international agreement. So a lot of discussion there. Some good partnerships. TOPEX/Poseidon [satellite] is a perfect example of a great partnership that emerged, so there were good ones. But it was a challenge because there was constant worry that a piece of the puzzle would be lost because some partner wouldn't step up on quality, wouldn't step up on data accessibility, wouldn't step up on the parameters that they had decided that would control the characteristics of the measurement, or that someone would renege on their agreement.

The worry was most of the time US would renege on the agreement because we were losing ground, not gaining ground.

WRIGHT: During this last 20, 25 years, you have seen data being collected in different ways, but it's also been exchanged in different ways. Again interdisciplinary and internationally. How has this worked? Do you feel like it is continually evolving? Or do you feel like it has reached challenges that might need to have [the plan] altered to have this data shared among all these partners?

BARRON: This is also the case of dream versus reality versus we actually did okay even though it wasn't as good as we thought we were going to do. From the beginning there was this notion that the data system should be as robust an investment as the investment in instruments, so this notion of the EOSDIS, Earth Observing System Data and Information System, came into being with really a massive budget.

This was being done in private companies. The scientific community began to get truly depressed that it was not going to be there and that it wasn't going to be what everybody wanted because one of the other keys to this was this notion that NASA was for the select few. People used to say that if you were part of the NASA team, NASA wards, you had access to the data, this was your world, and that there were thousands of scientists who could take advantage of NASA data if it was accessible and they could trust it for what they wanted to use. This was another key to Earth System Science, that "my" data [was available so] you could go grab, [and] believe that you had something that was good, and understand how it was developed. You could reach into this data and information system and pull it out knowing it had gone through all these steps and quality control.

This was a huge lofty goal, again from people that had an enormous amount of vision. There was a challenge in delivering this, because I guess you could say it was on the leading edge of data system development.

The notion also was that this could cross the world and others could make their datasets accessible. NASA even was helping design a journal called *Earth Interactions*. I was the first editor. It was an electronic journal. The notion was you could put your datasets in there. You could provide access to data in an electronic journal.

This truly was revolutionary thinking that was occurring all at one time, not only this comprehensive set of intersecting observations, but to be able to reach in and trust the data. The scientific community got really upset because we were worried there wouldn't be anything by the time it launched that worked and functioned. We almost went in near revolt. Now you can go get the data. It's not quite what we dreamed. We do have to have different agreements with different countries, and some countries want to charge for things, and we believe in open and free access. Some countries don't want you to have any data over their country because they don't know what that is actually going to mean. There are still a lot of challenges there.

But again the vision was probably a little bit bigger than what the budgets would let us do, especially as they began to go down.

WRIGHT: A few years ago you were part of a NASA Vision Panel for the American Geophysical Union (AGU). What is your process when you start to develop a vision that's going to be affecting an agency or a group?

BARRON: I'd like to have some great credit for myself that I'm sitting there thinking with some level of vision, but, I often think of myself as a great big sponge listening to all sorts of different things that are going on and then seeing how those can come together into something which you can actually take that next step.

I like setting the goal and then saying, "Are there concrete actions that get me to those goals?" I suspect that may also be part of the reason why I was a dean and director of a national lab and then the president of a university, because I like thinking that way. "What's your objective? Can you come up with the concrete actions to get yourself there?"

The vision committee from AGU was perhaps a little bit different because to be quite frank, AGU had a very specific challenge, AGU is a very broad society and mission to Mars has a great deal of appeal. At the same time everybody believed that NASA couldn't afford Space Station, human spaceflight, Earth Observing System, advanced rocket technology. They could not afford everything.

If you added to that Mission to Mars and didn't give NASA any more money—and nobody believed the US Congress was going to provide any more money—a) you were going to fail at all of them, and b) the presidential initiative was going to eat the lunch of something else and everybody believed it was Earth Sciences. So, that says: vision committee. How do you make the case that all the contributions of NASA in Earth Sciences were so fundamental and had such a big impact that you dare not have it degrade any further while the AGU also thought Mission to Mars was a noble objective?

I don't know whether you call that a vision committee or not. I call it walking a tightrope committee.

WRIGHT: You were there actually before the beginning [of Earth System Science]. When you look back over the last couple decades, what do you feel are some of the greatest accomplishments that have come out of this era of seeing the Earth as a system?

BARRON: You know something? I read that question. I didn't have time to look at all of your questions very much just because my life is a very hectic one. But I started to think about it, and I didn't want to go grab one specific thing or five specific things. If I go back to something I said before, this notion of being able to anticipate the future, to be able to do better predictions, my view is almost nothing matches that in terms of an accomplishment, because that's what makes society, people, powerful, is if they can anticipate what's going to happen next. We use it constantly in everything we're doing. We're trying to anticipate what's going to happen next, because if we can anticipate well, you can either make money off of it, or you can protect life and property as I said, or you can be a good environmental steward.

My view is that the development of these predictive models probably tells you more about the evolution of science over the last 20 years than anything else. It's not the "aha" moments, it's how well can we predict a storm. The mere fact that the experimental models nailed the path of [Hurricane] Katrina—it was dead on, days in advance—tells you how powerful the tools are that scientists are developing. The fact that we can actually simulate the record for the last 100 years if we say what the Sun is doing, what aerosols are doing, and what greenhouse gases are doing, and we can simulate that track of 100 years, is an extraordinary development. It is not an "aha" or "look at this great discovery." It is literally hundreds of people working together on parts of something that they are then putting in as teamwork to develop the next version and the next version with advancements, going back and testing it against the observations, and then saying it's not good enough and making the next improvement. So I don't really see that the 20 years has got the great discovery, the "aha" moment. I see it as the things to be most proud of are these very deliberate collaborative things that are yielding tools that could have an enormous amount of impact.

Those models have evolved a lot in those 20 years, and you couldn't do it without all those observations. You just could not do it.

WRIGHT: What is your hope for possibly the next 20 years, if you could have that vision and choose a path?

BARRON: I think the next thing—is maybe a true Earth System Science—because I don't think we got there. I think as you heard me say, we've had the big dreams, we set the stage, we didn't get to realize it in a lot of different ways. I think the thing that's going to change the most is that people are starting to realize that you cannot put every bit of data for the entire globe together and we are not yet ready to build a model of the Earth system that is global that includes all the chemistry, all the biology, all the physics, and everything else. It's too massive, it's too great. Where the intersection actually is of all the pieces tends to be more regional and local, because for a particular region, a river basin, you probably can put all the data from every different discipline at your fingertips. You probably can develop a high-resolution model that includes all the different pieces and be within the computational capability we have.

You can address a specific problem that has impact on humans. You can sit there and say, "What happens if we change this land use pattern, and it changes this flow rate, and this amount of nutrients go into a bay, and then what happens to that bay?" The place you can intersect all these disciplines is actually a smaller more regional domain. In some ways that's truly an Earth system approach, because you put it all together.

My belief is that if you do that over a few areas of the US, people will have—imagine having all the data at people's fingertips, a data and information system that makes it accessible, high-resolution models that match up with that. Then when people want to go off and do process studies, they want to work in that region and contribute to the improvement of those models, because they have so much other information at their fingertips, it helps them do their process study. Then you can connect to society because you're working on a real problem. I think that's the place that you could put it together, and if you put it together well, I think we'll have so much predictive envy and data envy that we will actually grow a bigger and more global picture from what we learn by taking a tractable size in terms of dataset predictive capability that can sit on a modern-day computer, and people, society, will see the value more.

Right now it's very hard for them to see the value in this big global picture. It's very hard to say, "What dataset do you want to give up?" when every dataset is important and you want to add the next biology, or sit there and talk about somebody wanting a genetic database to go with it so that you understand the distribution of life-forms. It becomes so massive that it's hard to imagine that we can actually pull it off. So I think actually how massive the problem is is holding back the integration.

Every time you advance the spatial resolution of a climate model, so you get higher and higher spatial resolution in your prediction, you get rid of parts of the science because it's so hard to get that higher resolution on a computer. Then you slowly add back the biology and chemistry and things like that. You watch that path repeated over and over again through time, and it makes you wonder whether you can actually pull off the supermodel that some people expect. My bet for 20 years—we're going to have a huge focus on regions to specifically solve problems, societal issues, environmental issues. It's all going to be hung on the big models, but they're going to telescope down to regions to help people make decisions. If we get really good at it, it'll make people want to go all the way.

WRIGHT: I remember reading some of the discussion that's been shared of what you have participated in, and you made a comment about the potential of NASA losing its leadership in its field. Talking just now about these models, is that where NASA needs to go, is to leading that? Or are there other organizations that you feel in the future NASA needs to work with to create this?

BARRON: Well, this is another one of the great challenges that occurred through this time period, because it all started in my view when the leadership of four agencies had individuals in charge that could sit down together and map a strategy of how to work together. So the people responsible for the Earth-related enterprise in NASA, NSF [National Science Foundation], DoE [Department of Energy] and NOAA [National Oceanic and Atmospheric Association] I think probably deserve a tremendous amount of credit for having launched what got launched, because they were capable of sitting down together and saying, "This is what we're going to do. We can do common budgets. You do this piece, I'll do this piece," and have it work.

I hate to use this word, but I actually saw in a completely different venue when you had three universities in a state who said, "We need to do this to accomplish something," and every single university in the state said, "Me too, me too, me too, I have to be in there." They ended up with a new law but it was a garbage-pail law, and included everybody, and everybody had their vote and their say. This is how things evolved in the federal government, 16 different groups became involved in setting the agenda for global change in Earth System Science, and they had different views, and they had different things that they wanted to do. It was no longer the powerful collaborative that was coming out of there.

Here's NASA with this massive amount of money going into this area, and here's another agency with this little bit of effort, and they're all at the table talking about it. In my view it became much less successful. A lot of people went back and said, "We have to have one agency that really focuses on these things." I think the key is the big players need to be able to sit down and either work together or have somebody in the White House that's directing them to work together. You need a different kind of collaboration, because we ended up with this garbage pail, and it became much less effective.

So how do you return? I think you do have to have those big players have responsibility for the agenda once again. That includes NASA.

WRIGHT: I know that we were talking about Congress earlier. In May of last year, the American Clean Energy and Security Act was passed. Based on the data that you know has been collected and the discussions that have been held in the last 20 years, how much of that information do you feel influenced the language of that bill to help set forth the goals for the immediate future that that act is addressing for clean energy?

BARRON: Well, certainly all that science laid the background on the importance of the problem. I think it's also true that there are big pieces of a bill like that—like if you start to have to keep track of carbon and its impacts and outcomes, I don't think we're ready to do it. So I think it's also pointed out things that we probably should have been able to do but because we lost the investment—I had a number of staffers calling me up on the phone and saying, "I'd like to say it this way. Can you do that? Can you verify that?"

I'll say, "We've been doing everything we can to protect the instruments that we have, and there's no carbon observing system, it's not there. It was always part of the dream, it's not there. OCO [Orbiting Carbon Observatory] went right into the drink. The rest of it has just been struggling to come forth. We haven't put the observations on the ground. It's been in decline for a period of time."

I thought that those bills should be an opportunity to have dollars that could be reinvested in research and reinvested in regions having a better understanding of what their carbon budgets were and their climates would be and their impacts would be.

WRIGHT: I know our time for this interview is growing short. You have a new position as of January I believe.

BARRON: President [Florida State University, Tallahassee, Florida].

WRIGHT: Yet you have so much of all these other experiences. Are you still offering or will be offering to NASA and to other agencies the life experiences that you've had?

BARRON: I always want to. Whenever NASA calls, I always try to say yes, but I have to be very careful. I have two things that I've been doing right now. One is the NOAA Science Advisory Board. I'm a member and I was a member before I took this job, and I want to do my duty before I step down.

Then the second thing is I have been chairing a National Academy [of Science] committee on what our ocean infrastructure should look like 30 years from now, because it takes a long time to develop that infrastructure. You have to have a vision for what it might be like and what science drivers there might be and therefore what types of ships or submersibles or something else [are needed]. It is a very similar way of thinking as from a lot of these other committees. I need to see that through.

But being the president of a university, there is no free time. So it's really a challenge [to do more]. It's interesting, because I often felt like I could go either way—I could just focus on my discipline and continue to do that and direct something like that. Or I could spend my time and effort focusing on education and the success of faculty members and growing a research agenda. I like both. I haven't been in this job very long but I'm enjoying it a lot. I doubt I'll do more than the two committees that I'm on right now for at least another year and a half until I really understand this job and what I want to do.

WRIGHT: They sound pretty busy as it is. Is there anything else you'd like to add that we might not have covered about challenges? And, Jennifer, did you think of anything?

ROSS-NAZZAL: Can you give an example of what you meant by working with social sciences on issues?

BARRON: One of the fascinating things for a physical scientist is thinking you know something about societal benefit. You would say something like well, this must have huge interest to people, we've got to do this, look at the impact it's going to have on X or Y. Then if you're sitting there with a large group of social scientists you realize that what people know and learn will actually change what their behavior is. So how do you understand how that behavioral change may take place?

One of the interesting things about uncertainties in climate models is, we don't know what the emissions will be in the future. You can have a particular growth rate but you actually don't know how to factor human behavior, at which point they will switch over and say no more Styrofoam cups or something, the way they did with ozone. Changes in human behavior becomes an interesting component of predicting the future.

There are psychological impacts that occur. If there's an adverse health outcome say, because you have a tropical disease delivered by mosquitoes that is moving poleward, humans may actually change what their behavior is and how they do things that could completely mitigate the problem. So you see at the border of Texas and Mexico on one side is a huge concern about dengue fever delivered by a mosquito, aedes, and in Texas they have an occasional outbreak but they can respond. A boundary between two countries is a boundary between an area of serious outbreaks and a region where the disease is not so prevalent. It's because of human behavior, and also economics, and providing alerts so that when there's a risk you know it and you don't go out. All that begins to occur as a change in human behavior. Institutions change and are capable of changing how it is that you set up institutions that serve particular purposes.

This whole notion of sea level change, with all the humans living at the coast, becomes decisions about whether you harden a boundary, whether you move things back, whether you create preserves, whether you create new laws, whether you change insurance policy and you no longer insure somebody who's been knocked down by a hurricane, so you never do it again, and so no one wants to risk putting their house on the beach anymore. You've changed the entire risk of the coast to sea level change because long before sea level actually increases enough storms occur that the insurance agencies change what the rules are.

All of this human behavior has to be matched up to all this knowledge about physical sciences.

ROSS-NAZZAL: Very interesting. Good examples. I had one other question. You talked about budgetary challenges, but I was wondering if you could talk about economics. There seems to be this great divide between growth and development and what people see as—I don't know, making an impact on the environment. There seems to be this great divide between the two. How can you approach it so you can move the two closer together?

BARRON: I really wish I knew the answer to that question. It's fascinating to sit there and take an example where you hear people say over and over again, "We can't do that because you're going to harm the economy. Can't do that, it's going to harm the economy."

Then you look at the other side of it which says, "We have this obligation. We want clean air. All those issues are not particularly important." Well, I grew up in Pittsburgh, Pennsylvania. Pittsburgh, when I was a little kid—I watched the slag being dumped from the steel mills. The pollution was terrible. All sorts of new laws went into effect, and Pittsburgh gets ranked as one of the most livable cities in the country, and there is an economic boom that occurs there.

The "the sky is falling" scenario wasn't true, and the "we can't do this because we're harming the economy" wasn't true. The middle is actually much closer to the truth. You want to drill in the Arctic. Economics, you've got to do this, we've got to go tap this oil. You're going to harm the environment. Who says, "Can you guarantee to me the technologies and the backup systems where you wouldn't harm this, so you can go get the oil? We can do that." The same thing goes with this Gulf [of Mexico] oil spill [2010]. There were in place things that were skipped that would have prevented that from happening. There could have been a relief [oil] well, even though it cost more, set up in advance so that it wouldn't happen. We can do both. I just think people have decided that the object is whether you win, to pull it one way or another.

Neither "the sky is falling" nor the economic arguments, they just don't work. I've never seen them work. It's somewhere in the middle. I don't know what it'll take before people quit playing tug-of-war and instead sit down and say, "How can we solve this problem?"

That's probably gotten worse in the last 20 years, worse than any other issue that I can imagine, that it's a tug-of-war: I'm right or I'm wrong. I just don't know of any right and wrong problems like that. It's an important issue. I don't know how to beat it back. Education would beat it back but even that seems to be a challenge to pull off.

WRIGHT: Thank you.

[End of interview]