EARTH SYSTEM SCIENCE AT 20 ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

DIXON M. BUTLER
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is June 25, 2009. This oral history is being conducted with Dr. Dixon Butler, who serves as a professional staff member on the Commerce Justice Science Subcommittee for the Committee on Appropriations of the United States House of Representatives. Included as part of his responsibilities are the National Science Foundation and climate change. This interview is being conducted in Washington D.C. as part of the Earth System Science at 20 Oral History Project, a project to gather experiences from those who have been involved in various efforts in the launch and evolution of Earth System Science. Interviewer is Rebecca Wright assisted by Jennifer Ross-Nazzal. Thank you so much for coming out to meet with us this morning.

BUTLER: Glad to.

WRIGHT: I know that your schedule's very full. We'd like for you to share with us, if you could, how your interest in Earth science led you to where you are today.

BUTLER: How'd I get there, yes.

WRIGHT: How'd you get here?

BUTLER: Well, growing up basically I was really good in math and pretty good in science, so I kind of got directed that way. Plus, that sort of fit my interest. You meet scientists and it was clear they were inevitably going to be scientists, and I never was quite that way, at least to myself; other people probably thought I was.

I went to college, [Harvard University, Cambridge, Massachusetts] majored in physics and chemistry, not quite sure between the two but fairly early on gravitated toward physics. Wanted to be a particle physicist. Got married halfway through college, which was extremely unusual where I went to school. I think I was the only person in my class who got married before junior year and finished in four years. People would take off, but with the pressures of the Vietnam War, most of the married students that graduated with me had taken off and came back either after serving or to avoid serving.

So trying to be responsible, even though there really wasn't a compelling financial need, but trying to act responsibly as this married 20-year-old, I went to my undergraduate advisor and asked if he could employ me. I got a 12-hour a week job working for his high energy physics research group at the Cambridge Electron Accelerator. This group was full of people, who with one exception, ultimately took seven and eight years to graduate with their PhDs. They took my wife and I under their wings to some extent, even socially after a while, inspired us to become very interested in gourmet cooking because this was the city of Julia Child, Cambridge, Mass [Massachusetts].

But one of them, and the one who probably has had the most successful career as a particle physicist, I'm not comfortable with the term Dutch Uncle, but he kind of had a nice counseling conversation with me near the end of the first semester of my last year. At this point, I was taking graduate courses in physics, and I was going to get a master's as well as a bachelor's degree at commencement. He basically counseled me out of high energy physics. He said, "Look, with your grades you're probably not going to get there. But let's say you do. You're inevitably going to have to be an experimentalist, because you really need A's to be a theorist in particle physics. The way this field goes, you're going to be a family man, and with the requirements of doing experimental high energy physics you're going to have to leave where you are and go be at the accelerator four, six, eight weeks at a time, leaving your family. You're not going to want to do that. Even if you are going to want to do this," and this was actually high praise in some ways, he said, "To really get ahead in this field you're probably going to have to stab some folks in the back, and you'll never do that."

So with that counseling and at my wife's urging, having taken one course that first semester in stellar structure and evolution at the observatory, which was really a course in the astronomy department, and that got me interested. I began casting about. I also didn't do very well that semester. I had one C. I had to get to a flat B average for the year, or I didn't get the master's degree. I started grade shopping, quite frankly, for courses where I could get an A and I dropped the course where I'd gotten a C. But it had a follow-on second semester at the observatory, where the material was taught in the context of cosmic ray physics.

There was also a course—I still remember it. I was reading the course catalogue and there was a course entitled Upper Atmospheres of Planets, and I laughed. I literally laughed even reading the title, because the idea that there was an upper atmosphere just seemed very bizarre to me. So I signed up for that course, and as I was thumbing through other courses—and I had a physics education very heavy on quantum mechanics—I found a course that said Quantum Mechanics of Atoms and Molecules with Special Applications to Upper Atmospheres of Planets, so I signed up for that course as well.

To make a somewhat lengthy story short, the course in atoms and molecules with special applications to upper atmospheres of physics was taught by Michael [B.] McElroy. It was his first course. He was a brand new 33-year-old full professor. I was there for his first lecture. He had bright red hair in those days. He was very young in appearance, even for being 33. Normally, classes began at seven minutes after the hour. He was absentmindedly sitting in the front row, but none of us knew who he was. We'd never seen him before. He was new, right, never taught before, whatever. So it gets to be 10 minutes after the hour, and one of the wise guy graduate students, a real graduate student, in the back row stood up and said, "Would the real Professor McElroy please stand up?" Mike McElroy stood up and began to lecture. It was just amazing. Turns out the course in upper atmospheres of planets has a good anecdote as well. It was taught by Alex [Alexander] Dalgarno who, back at Queens [University], Belfast [Northern Ireland] was Mike McElroy's thesis advisor for his doctorate.

So I had these two, and it's worth an oral history at NASA to put a couple of stories in. One is I show up for this class, and I recognize one guy who is another senior, and there's another guy I don't recognize, and then there's a bunch of guys I really don't recognize. Dalgarno lectures, and most of the way through the lecture one of them asks a question. I can't understand what he's saying, and he's at a level of understanding of this material that I just can't imagine. This goes on for a few lectures, and what I'm really hearing is a thick Irish brogue. It turns out, fortunately, there were only three of us who were really students in the class. All of Dalgarno's post-docs were auditing the class.

At this point, NASA's Grand Tour [*Voyager* Spacecraft] of the outer planets was on the drawing board, and the time scales for getting particularly the mission to Neptune seemed pretty lengthy. They were longer than I was old! Dalgarno talked about these missions, and he basically said—and he was fairly pessimistic and fairly politically liberal—and given the fears of nuclear war, he said, "It will arrive in Neptune in," something like the year 2000 or 2000-something. "Will any of us be here to get the data?" I recounted this story to McElroy, and he said, "You tell Alex I'll be here," which was a fascinating interaction.

In any case, I went off to graduate school at Rice [University, Houston, Texas], which was a place that took me and took me on the same basis as everybody else, gave a full ride. I was just a regular graduate student. Went in the physics department, but with some influence from McElroy, I had decided after three weeks in Dalgarno and McElroy's classes [at Harvard] to leave my desires for particle physics and be an atmospheric physicist, actually technically an aeronomer, which is what the upper atmosphere people were known as. You still hear that term, but rarely. I used to use it for fun.

Sorry, I've got to backtrack one. McElroy had no graduate students, and he was brand new and a little over-impressed with the students. So even though I wasn't any great shakes, since I was interested in his field, he was interested in taking me on as a graduate student. He tried to get me admitted to graduate school there, and actually sort of partially succeeded. But one of the things he did is he asked me to work for him the next summer. That was fine. My wife and I didn't know what we were doing yet, and so I agreed to work for him over the summer. What I did is I took a model that he had developed before he'd gotten to the university, that he'd developed of the ionosphere of Mars—the model was even called MarsIon—that he'd used to participate in various NASA Mars missions. I converted it to work on the ionospheres of the four gas giant outer planets: Jupiter, Saturn, Uranus, and Neptune. Primarily focusing on Jupiter, on which I'd actually done my paper in Dalgarno's class, my term paper—because there was no final in that class—but my research term paper had been ionosphere of Jupiter. So I basically converted his Mars model to a Jupiter model.

Then I went off to graduate school at Rice, and was shopping around for a thesis advisor, because I had a course work master's in physics walking in the door. A number of people were interesting, but there was a fellow named Paul [A.] Cloutier who wasn't in the physics department, he was in the space science department. Rice's space science department was the first space science department in the world and no longer exists, interestingly enough. But it had been carved out of the physics department. Cloutier was its first PhD product who actually ended up staying and getting tenure in the department, but he had no joint appointment with the physics department. He was one of the few that didn't. I chatted with McElroy and got some advice, and he thought Cloutier was a really sharp guy, who was even younger than McElroy, and so I signed on as Cloutier's graduate student.

Cloutier was an experimentalist, but he wanted a theorist—really a computer modeler is what he wanted—because for his work on Venus, he wanted a model of the Venus ionosphere so he could study solar wind interactions and calculate solar wind interactions with Venus. So at that point, I had the very unusual situation that I was going to do an ionospheric modeling degree, both master's and PhD but primarily PhD, with a thesis advisor who didn't do that, in a department where no one did that, in a university where no one did that. It was very odd. I don't recommend it. I don't think anybody should be allowed to do that, but they let me get away with it. The way I really pulled it off was I had the ionospheric model from McElroy from when I was an undergraduate. I'm really good at computer coding. I converted it to do the Venus ionosphere, ran a bunch of calculations, and came up with a thesis. Pushed very hard to get out of Rice in less than four years. I had a leg up because I'd done all this extra course work. But wrote my thesis, my doctoral dissertation on "The Ionosphere of Venus", a modest title.

The morning I defended my PhD, the *Journal of Geophysical Research* shows up, and there is a paper from [a group at] University of Michigan [Ann Arbor, Michigan] that basically has all my results in it. A few of my more nuanced, smaller-importance results were original with me and weren't in their paper. But since nobody at Rice was really plugged into that part of the field, there was no one telling the guys at Michigan, "Wait a minute, I've got a doctoral student going to do this," or telling me, "You're going to have to do something else." So I went ahead, defended, they passed me. I stayed for a post-doc with Cloutier, but one member of my committee—and I tell this more because I think it's important because this one man took a real interest in me in that period of time, which was really, in many ways, career-enabling. His name was Bob [Robert D.] Rundel. He was a junior faculty member doing atomic and molecular physics in the space science department at Rice. I took his course, and he was one of three members of my PhD committee.

At Rice, the system in space science was you actually had one-on-one oral examinations by the members of your committee, and then defended your master's thesis, and that basically gave you candidacy for the doctorate. My first oral had been with Rundel. He asked me questions. I did really well answering them, sparkled, and he wrote it up that afternoon. His write-up glowed enough that my advisor, who I was going to have my exam with the next day, was so proud that his student had done so well and it looked really good that he basically, when I went into the oral with him, just played cat and mouse intellectual games with me. Because he was an intuitionist, and he was just spectacular at asking you questions where you needed the intuition to cut through and get to the right answer, and I couldn't do them all! But I did some. He passed me. The last exam, I muddled through, and the guy let me go.

Rundel, a few weeks before my thesis defense, had me come down to the [NASA] Johnson Space Center [Houston, Texas]. He had not gotten tenure at Rice, although he had an adjunct position so he could be on my committee. He went to Johnson Space Center and joined the Environmental Effects Project Office of the Space Shuttle, whose job was to write the environmental impact statement for the Shuttle. This is 1975. In this group is a man from the University of Michigan, who'd been a post-doc there, named Rich [Richard S.] Stolarski. He, together with Ralph [J.] Cicerone, had written a paper worrying about chlorine in the atmosphere from volcanoes and ozone. It was not the [Mario J.] Molina and [F. Sherwood] Rowland paper ["Stratospheric Sink for Chlorofluoromethanes: Chlorine Atomcatalysed Destruction of Ozone," 1974], but they had speculated about methyl chloride coming perhaps from volcanoes. It turns out it mostly comes from evaporation of sea salt and just gets aerosolized that way, at least I believe I'm right on that.

In any case, Stolarski had done ionospheric modeling. He knows how to do it numerically better than McElroy had, and there were some flaws in my doctoral calculations because I'd just used this stuff, and I didn't get the kind of in-depth understanding one should have of it. He ripped me end from end. Rundel invited me down to Johnson, had me present my work. Stolarski just tore me limb from limb, then they all took me out to lunch and told me, "You'll do fine, you'll pass, no question." I passed. A couple of days after, I'm sitting in my thesis advisor's office, and Rundel walks in and says, "Look, as a group we'd like to put $5,000 towards Dixon's post-doc," which, post-doc was going to pay me, I think, $11,100 a year, so a $5,000 contribution in those days was a big contribution. "We'd like to pay $5,000 towards

Dixon's post-doc here, provided he comes down to Johnson two days a week and interacts with our group. Oh, and by the way, Dixon, I still live up here, so," (I lived near Rice), "I'll pick you up." We were down to one car. "I'll pick you up, and we'll commute together, and I'll give you a ride down to Johnson and bring you home."

This was wonderful. First of all, he and I had these wonderful commutes with me with a much more experienced scientist. A number of different things I learned from him. First of all, I wrote my thesis up. *Geophysical Research* turned it down, because clearly it had been preempted by others. Stolarski helped me rewrite it to try and get it published, got turned down a second time. But he and I undertook to correct the calculations I'd done that weren't done right in my thesis. We did that, we published it, it was our first paper published together. It's on photoelectrons of the Venus ionosphere.

He and Rundel had developed a toy model, what climate modelers would call a toy model. It was a model of a stratosphere that basically mimicked other models better than it mimicked the stratosphere, but it ran quickly. You could run it on a calculator. As a matter of fact, I think they were actually running it on a programmable calculator. Because ozone depletion was their big worry from the Space Shuttle, because there's plenty of chlorine effluent. If we'd gone to 500 Shuttle launches a year, which was a fantasy of NASA's at the time, the ozone depletion would have caused regulatory problems; as it is, it blows a hole through the ozone every time it launches, but it's a small hole and it closes back in quickly, and so nobody gets UV [Ultraviolet] exposure except for maybe a few birds flying around the Shuttle site. So that's about it.

At any rate, I did a post-doc. The end of this group's work is in sight. The leader of the group is a man named Bob [Robert D.] Hudson. He and Stolarski get offers to come to the

[NASA] Goddard Space Flight Center [Greenbelt, Maryland] and form a stratospheric research group there in the [Laboratory for Atmospheres]. It was Nelson Spencer's laboratory. New branch [called the Stratospheric Physics Branch]; Bob would be the branch chief that gets Stolarski, and there was going to be room for a few more people. They offered Bob Rundel the chance to come with them. What Hudson had in mind was Rundel had built two different labs in his life. He'd built up big atomic and molecular physics apparatus, basically for more senior people. Hudson wanted him to do that again at Goddard. Rundel said no.

I had applied for a post-doc with [Richard T.] Dick Hartle at Goddard Space Flight Center, an NRC [National Research Council] post-doc to work on the ionosphere of Venus. Harry [A.] Taylor was the Deputy Lab Chief of the Laboratory for Planetary Atmospheres. He had an instrument going to Venus. I think Spencer had an instrument on Pioneer Venus as well. Hartle was kind of their lead theorist. So I applied to work for him. Basically, a deal was cut. My memory of the deal or what I was told was, "Look, we'll arrange for you to get the post-doc, provided once you get to Goddard, you come work on the stratosphere." Well, for me this was like, "Don't throw me in the briar patch." I dearly wanted to switch and do something more socially relevant. This is 1976, come on, think about the times. I was of a generation that had, to some degree, protested the war in Vietnam. There were a lot of things, including at this point I'm looking for some place to move. I've got two interracially adopted children. I'm in a world where social commitment means something to me, so switching to make my science talents work on a socially relevant problem of ozone depletion sounds great. I'd love to do it. Rundel says no, so they offer me the post-doc.

Basically Bob [Rundel] kind of transformed me, brought me to Johnson, got me involved with Stolarski. I think half my research publications—which aren't that many, maybe 14 real ones—half of them have Stolarski as either the lead author or a coauthor of mine. Several of them were with Bob Rundel.

We get to Goddard. We arrive in May. In August of that year, there was going to be a conference at Utah State [University, Logan, Utah]. Stolarski wanted to give a paper there, bring me along as a post-doc because I got, I guess under those fellowships, you got one or two trips paid for. It's an interesting stratosphere conference. I was never at another one exactly like this. But we have to get ready. So before I get to the substance of that conference, I get handed Rundel's calculator-running model, which is sort of in Fortran or close enough to Fortran. We need to run it, and we need to run it a few thousand times to do a Monte Carlo process to calculate the uncertainties in stratospheric ozone depletion predictions based on uncertainties in chemical reaction rates.

I'm a really good programmer. At this point, I'm amazing, quite honestly. I'm not an amazing physicist, but I could do that. The key thing I could do is I caught my own mistakes as I was typing, key-punching, and I was pretty good at logic, like really good, so I didn't tend to make very many logical errors in my program. And I had Rundel's stuff to start with. I converted his program. A lot of people got one-day turnaround on models through the major computer at Goddard, because frankly you handed your deck of cards to the card reader gal and she ran them in and you waited for your results. If you made few mistakes, chances are instead of waiting for one turnaround a day you made real progress every day. So we were ready.

In August, we went off to this meeting, my introduction to the field. It was a memorable meeting for a lot of reasons, but the key one for me was one of the Consumer Product Safety Commissioners, of whom I believe there are five, there may only have been three, shows up, plays volleyball with us. This is a federal regulator! Because there's a consumer product content, aerosol cans, right? Dixie Lee Ray comes, former chairman of the Atomic Energy Commission, former governor of the state of Washington. She comes and talks. She didn't go play volleyball with us.

But this is pretty intense. The stratosphere community basically was moving at a pace which did not allow time to just work off of publications, and this is before all that email. You couldn't Twitter your results to your colleagues, right? So conferences were critically important. New chemical results and insights tended to break multiple times a year, and if you were in the loop you were in the loop, and if you weren't, Heaven help you, you really couldn't get in. The field was fairly small, probably 200 people maximum—and that's in the world, and this meeting's got 75 of them. Typical meetings would have about half of them. So I got to go to this one with Stolarski, began to learn the people, saw the insights. But you kind of published—it wasn't an afterthought, but it was sort of documenting the past. The pace was all set at these meetings, the personal interactions; things were really dynamic. I got to meet Ralph Cicerone, because he and Rich Stolarski had published together. I got to meet a variety of other people. It was quite some meeting. In any case, I was into the stratosphere.

Now, I have to back up and tell one other story because it's going to be important for talking about Earth System Science, and I used to cite it as an example of an experience in part of my anecdotal pieces of my pitch when I was talking about the Earth Observing System [EOS] to people and the vision for the Earth Observing System. I can't remember which year it is, but I think it's 1975. So I'm still at Rice. In those days, the spring meeting of the American Geophysical Union [AGU] is always held in Washington [D.C.] at the Woodley Park Sheraton, which has changed hands and names multiple times since then. It's a convention hotel. It also happens to have one ballroom that's big enough to hold an inaugural ball, which D.C. fills up. I think the last inauguration with [President Barack] Obama, so many people came they may have had fourteen of them. But typically, in [President Jimmy] Carter's day, I think there were six or seven. This was one of the ballrooms where you had inaugural balls. At any rate, big place.

There's a union session, which is kind of a "y'all come," cuts all across all the many disciplines that are in the American Geophysical Union. It's about ozone depletion. There are five invited speakers each given about a half an hour. I don't remember exactly who the five were, but the most striking talk—because most of the people working on the stratosphere are aeronomers like me. They are people who have come down to the stratosphere from doing modeling of the ionosphere, typically the ionosphere of other planets, a few of them the ionosphere of Earth. Modeling the ionosphere of Earth is much harder and requires much more sophistication than modeling the ionosphere of a planet where we have a paucity of data. This is sort of always true. What you have to know to do things on the Earth are beyond where you have to be to do interesting insights to a planet. That's changed some, but you can imagine it's true, because heck, we walk around and take initial measurements here. If you want to do something more, you've got to do a lot.

In any case, one of the five speakers is a man named Constant [C.] Delwiche, known as Connie as I later would learn. We've got these people in the audience like me who've come down to the stratosphere, a region that does not touch the surface of the Earth, typically never gets below about eight kilometers above the surface. Here's Constant Delwiche, who is this soil scientist, and he is discussing in his paper nitrification and denitrification by soil microbes, and your mind is blown. Because first of all, who knew from soil microbes, but nitrous oxide, N_2O, laughing gas, is a major source of nitrogen oxides in the stratosphere because it tends to have a long atmospheric lifetime. Unlike the smog gases, the other oxides of nitrogen, nitrous oxide still has the bond between the two nitrogen atoms, which is a hard bond to break. So all of a sudden, you're dealing with soils. You're into a whole world, and that really is the hint of what's coming. That really is the moment when you can see, if you look back—you couldn't see it then, unless you were an incredible prophet beyond my abilities—but you could see the seeds of Earth system science, because the stratosphere naturally is being controlled by nitrogen oxides from N_2O, from soil nitrification and denitrification. Stolarski and Cicerone were probably right that natural sources of chlorine from volcanoes or from sea salt probably put the natural abundance of chlorine in the stratosphere.

Well, you've got geophysics, you've got soil science, you're dealing with crops, you're actually also dealing with physical oceanography processes, maybe even a little chemical oceanography. That's all there in the stratosphere, but of course the signal's being overwhelmed by what human beings are doing with chlorofluorocarbons 11 and 12, the chlorofluoromethanes. Dupont's brand name is Freon, which is why the proper name is chlorofluorocarbons. I store that away.

Anyway, in the fullness of time I get to Goddard, trying to publish like a son of a gun because I can work hard. I can't make myself more brilliant, but you get to Washington [D.C]—and I think this happens to a lot of people in Washington—you get here, and you realize that you're really smart, but so is everybody else. The only thing you can do to compete—well, it's not the only thing; you can build political or personal connections, not something I was into doing, or you can work. Most people, whether they build the connections or not, concentrate on working enormously hard. So I'm going like crazy trying to publish and publish a lot of papers, et cetera. My goal is six a year. Over time, Rich Stolarski's the idea guy, and I'm the hard work guy, but he's the senior guy, I'm the junior guy. I need him to get the papers out. I get frustrated because I can't get him to do enough, in my opinion. That doesn't mean he wasn't right, or that these ideas weren't good enough to actually cut a paper every time. But I get frustrated. I also look around, with the haughtiness of youth and the naiveté, I look around and say, "I don't see any better managers than I think I could be between me and the [NASA] Administrator, including the administrator." That judgment was probably incorrect, as time has proven, but it was my judgment.

We also, Hudson, Stolarski, and I, having come together from Houston, plus another post-doc is hired out of the University of Maryland [College Park, Maryland] where he had been a post-doc already, PhD from University of Wisconsin [Madison, Wisconsin], a fellow named Bill [William S.] Heaps, still works at Goddard I believe. Heaps and I become officemates. Stolarski, Heaps, and I are going to lunch every day, sometimes with a few other people, but often not. With Hudson, we become sort of this little corps. We're probably kind of obnoxious. I mean, not actively obnoxious, but we're a tight group. We have our own opinions about the people who were already at Goddard who begin to be in Hudson's branch, and those opinions are not always maybe what they should be, or maybe underestimate some people.

One of those people is a fellow named George [B.] Newton. He had gone on detail to NASA Headquarters [Washington, D.C.] to the Space Science Office, and at the end of his detail in November of 1978, he gets hired into Headquarters. He's a GS-14 [General Schedule, pay scale], maybe he gets hired a little earlier than that, he gets to be a GS-15, and I hear he's moving into the Senior Executive Service. My mind is blown. I'm watching Dick Hartle, who I do think quite a bit of as a scientist, who is a branch head in this lab and a terminal GS-14 who I don't think has a prayer of ever being promoted. I've gotten hired as a 12, I've gotten promoted to a 13, and I'm ambitious. I also wanted to have more influence over things, and I sort of look at myself scientifically, and I'm a yeomen scientist. Every field needs a majority of yeomen, just like an army needs foot soldiers. You've got to have people who can do the intellectual work, that fill in the [gaps], but I'm not going to be a brilliant leader. I'm just not, in my own assessment.

The science boom that ended with Sputnik [Soviet Union satellite] basically ran out of steam the year I got my undergraduate degree. There was a cartoon I love to tell people about in *The New Yorker* that year that has a very nice, attractive looking young man as a waiter in a fancy French restaurant. In those days, fancy and French restaurant were synonymous. He's saying, "Hi, I'm," so and so and so and so, "the fourth, Harvard Magna Cum Laude in physics, and I can recommend the duck without qualification." It was like that! The jobs had vanished. The chances to becoming a tenured faculty member or even getting on the tenured track in this field appeared nonexistent. So what do you do? I also feel like I'm playing with half a deck, that I've got skills I'm not using. The other piece of good news is I've learned to write. I also had applied for the AGU Congressional Science Fellowship. I get turned down. I learn to write better. Publishing helps. Word processing helps, which is new. But computer-based word processing is how I learned to be a good writer.

The opportunity comes up in late '78 to go on detail to NASA Headquarters from our lab, and I volunteer [to succeed George Newton]. It's supposed to be bad duty that you have to be dragooned into doing. I volunteer to go to Headquarters. It's a two-day a week, because Goddard is in the same town. Goddard people can go part-time detail to Headquarters. It's two-days a week. I go, and Headquarters is in transition.

Here, I have to back up a little. Shelby [G. Tilford] may have told you about this, others may have told you about this, but there is the CIAP [Climatic Impact Assessment] Program because in 1971, Harold [S.] Johnson, at [University of California] Berkeley, had suggested that the Supersonic Transport [SST] flying at 20 kilometers in the stratosphere, having nitrogen oxides come out the tailpipe, that they may catalyze ozone destruction, the chemistry lays out. This triggers the Department of Transportation, specifically the Federal Aviation Administration, forming the CIAP program. They get a block of money, like $25 million. It's a lot of money. But money begins to rain on the stratosphere community at that moment, and the stratosphere community all of a sudden has this source of money that needs results to deal with the regulatory question, and they suck all these people in. So all of a sudden, this stratosphere community is meeting and meeting again, they're writing the CIAP reports, and for three years they're like on this trip.

It ends in 1975. First of all, the United States has made the decision not to build the SST. Where's the program going to go? But the Shuttle has called NASA to have expertise in ozone depletion. There's even a little program being run by a [NASA] JPL [Jet Propulsion Laboratory, Pasadena, California] detailee at NASA Headquarters—and his name is gone from me, but other people will remember it—and he's going to start this stratosphere program for which Goddard is forming this group in 1976 to go after the money to do that. NASA takes over the lead role.

There is a Space Act amendment adding Title IV [Upper Atmospheric Research Purpose and Policy] to the Space Act, which basically gives NASA the lead job of the stratosphere. Funding ensues. There's a program in Space Science started to do this because it's viewed as part of solar terrestrial. So there's solar physics, there's upper atmosphere and magnetospheric solar wind stuff. There's all that in a division of Space Science. At this point, it's one of the periods of time where Space Science and Earth Science and other things are separated. There's an Office of Applications, there's an office of Space Science. Applications has a program in stratospheric air quality, kind of a more from the ground up kind of thing about pollution to go with its tropospheric air quality programs and its programs in severe storms, weather, and climate. By this point, NASA's being run for the only time by a scientist [Robert A. Frosch], an oceanographer, and he says, "We need an oceanography program!" He says, "Go make me an oceanography program." There's so many threads here that come together, it's just hard to keep them straight even in my own head.

So for me, I go to Headquarters. Over in the office of Space Science, a man named Shelby Tilford has come from the Naval Research Lab [Washington D.C.], I think on a detail or a loan or something, to run the Solar Physics program. Before he knows it, he's taken over the Upper Atmosphere Research Program, and has maybe a $13 million budget, if memory serves. There's this other $6 million budget over in Applications. The numbers may not be precise, they may be numbers from later budget years because I know those numbers better. Just at the moment when I'm ready to come as a detailee in November of 1978, Dr. Tilford, in a pre-agreed deal, is going to become a branch chief over in the Applications Office of NASA Headquarters, in the Environmental Observations Division headed by Ron Greenwood, with Bill Bishop as his Deputy. I come on detail to Shelby.

Somehow, I've gotten the clue that I want to go do the stratosphere because that's my field and the stratosphere's going to be done there. So Shelby's hired to do the branch over there in Applications, but the deal has been cut to take the money from Space Science and move his money with him. So instead of going to Space Science, I go to Applications. When I get there, things are new and kind of transitional. It's the old NASA Headquarters [building]. For whatever reason, we don't have quite enough room. They don't have a desk for me, or they don't have an office for me, so they give me a desk in the corner of Shelby Tilford's office. As I used to say, two months later I had a new career and a new life. I mean, I didn't say it that way, but what I had was a mentor.

So Shelby and I hit it off, started doing things. The Upper Atmosphere Research Satellite [UARS] was in pre-advanced planning, but no new start yet. I help with the announcement of opportunity for UARS, I literally pull together the review panel, and for the first time there's going to be a set of theory and data analysis investigators on the science team, specifically. I'm not sure there wasn't something analogous on some planetary missions before this, but this is new stuff. What happens is we have a bunch of experimental instrument proposals and kind of a separate category of proposals for theory and data analysis, and I actually chair the review panel for theory and data analysis because I'm technically qualified, right? Since I was helping with issuing the thing, I couldn't be on any proposal or play a role.

I'm still interested on going to the [Capitol] Hill, so I apply again. This time I apply to the AGU and the American Physical Society [APS] and maybe even the AAAS [The American Association for the Advancement of Science] Congressional Science Fellowship Programs. I'm at Goddard chairing the review panel the morning I have to drive into Washington to the hotel across the street [from the Sheraton Park mentioned earlier], the Americana Hotel, to go to the APS meeting to go to my interview. So I chair in the morning, you know, it's a multi-day review. I guess we did three days. The experimental proposals I think were reviewed over the course of four. I remember they were a day longer. So I turn over leadership of the review to somebody else for a couple hours. Drive into town. I'm the last of four candidates to be interviewed. This year, I get interviews with both AGU and APS, but the AGU interview is later.

I go to the interview. My written paper is about ozone depletion, and I've had to write a paper, a short policy memo like I was briefing the congressman or the senator on ozone depletion and what needs to happen. I've done that, and then I have to do an oral briefing, because the chair of the panel, who is Millie [Mildred S.] Dresselhaus who ultimately is president of the American Physical Society, maybe the first woman president, amazingly impressive woman, she's chairing. A variety of people are there, one of whom, although I didn't know it at the time, is one of the two current American Physical Society Congressional Science Fellows, a fellow named Fred [Frederick M.] Bernthal, who goes on to be deputy at NSF [National Science Foundation], a Nuclear Regulatory Commissioner, whatever. For a long time, Fred was the most successful Congressional Science Fellow probably ever, because he went on and was legislative director for the majority leader of the United States Senate, Howard [H.] Baker, because that's where he happened to spend his fellowship year. Anyway, and he went on and had these other big jobs.

I go to the interview. It goes pretty well, you know, but I can't stay and have lunch with the other candidates from the group because I've got to go back to Goddard and chair a review panel, which I do. The review goes well. After we finish, we sit in for a day with the experimentalists; UARS selection happens. I get a call from the woman who runs the Physical Society's Fellowship Program saying—and you've got to understand here, my hair is on my shoulders, my beard is scraggly, and my hair looks really awful. It's not that I kept it unkempt. I washed it. I wear three-piece suits to NASA Headquarters with this hippie look, because I'm not a hippie. I've never used drugs, I mean, never came close, I don't drink, I don't smoke. I'm this guy who looks like his generation and doesn't behave like his generation. I usher at church in a three-piece suit so people won't look askance at me.

In any case, she calls, and basically this woman Mary [L.] Shoaf, who worked at Princeton [University, New Jersey] Plasma Physics Lab, says, "Alright, we'd like to offer you the fellowship. However, some of the people on the selection committee are current or former science fellows and work on the Hill, and they said that I must tell you that with shorter hair and a somewhat more trimmed beard, the committee would feel more comfortable using you with the public." Obviously, she'd hemmed and hawed to get those words out. She was obviously petrified to ask me. At any rate, I said, "I'll cut my hair!" It turns out that [the fellowship call] happens just before my third child and first born child is born. So as my wife says, I'm in pictures in the delivery room on Friday with long hair and a scraggly look, and I come to pick her up—I'm there over the weekend, but I come back on Monday—because after I'm there on Friday, I go get my haircut.

Per the advice of a friend, I walk into a hair cuttery—because he had a beard but worked on a fellowship at the White House. I walk in, and I say, "Make me look like a Congressional staffer." The guy makes me look like a Congressional staffer. I'm passing the reflection in plate glass windows leaving it, and the first thing is I'm a big, heavy guy. At this point, I probably weight 245, and I'm six foot one and a half, so I'm carrying a lot of weight. I had been most of my life at that point. I look in the reflection and I say, "Holy cow," and then I say, "You really ought to lose a little weight to go with the haircut." That doesn't completely happen or doesn't really happen at all, but in any case, I go to the Hill.

I have to backtrack one other thing. There's another really odd, loose thread that matters. Being interracially adoptive parents, we had joined a group in Houston before we adopted the first time called the Council on Adoptable Children, which is basically a group mostly of parents who either have or want to adopt hard-to-place children, could be health problems, could be interracial adoptions, foreign adoptions, whatever. Because interracial adoptions are pretty new still, and the foreign adoptions are still building up, and most adoptions are people who can't have a child and want a kid that looks like them, and there's all that kind of thing going on. There are a lot of nonwhite babies in the United States growing up in foster care that isn't exactly ideal. In some cases, it's wonderful, but generally it's not.

Anyway, we join the Council on Adoptable Children there. We get to Washington and we join Council on Adoptable Children. I'm not sure there were more than two chapters in the world, but there happened to be one in Washington. In that is a fellow named Tom [Thomas H.] Moss, one of the original American Physical Society Congressional Science Fellows. Tom, I don't know what help he was to me, but he kind of helped mentor me in applying, and maybe is the reason I applied at APS where I was a member, not just AGU. He's in this group. We even start a self-help group. They meet at our house.

One of the other things is an article comes about that most of the fathers in couples that adopt these children are scientists or ministers or in the sort of high-end social work kind of stuff. We look around the room and, you know, there are a lot of PhDs, and almost all of them are in the sciences. There's somebody's who's not a minister but he's into counseling of that thing. It freaks you out. But at any rate, that connection helped me, because Tom, I think, played a role in really opening my eyes and getting me to go do the fellowship. So that's that little aside. It's sort of funny the way life works.

Any rate, I go off to the Hill. I still have an enormously high opinion of myself. I go around interviewing. Although this isn't very relevant to Earth System Science, it is important for getting at what I learned. Most Congressional Science Fellows are gravitating to [U.S. Representative] George [E.] Brown and the Science Committee in the House. I guess I say this with some trepidation, but for most of its history the Science Committee in the House is the second least powerful committee in the House of Representatives. Sad but true. I don't immediately pick up on that, but I see everybody running there. The fellowship program's been on for six or seven years. Their staff [the House Science Committee], you've gone from having one or two or three PhD scientists in the entire Hill staff, they've got subcommittees with a staff of seven, five of whom are PhD scientists. They don't need more science fellows, although a lot of my science fellow classmates go there. I interview on the Senate; the Senate's not my place. You might think, as haughty as I was in my opinion of myself, I would like the Senate, which tends to be more that way. But it just wasn't. I was very off-put. I have a bunch of interviews with different members of the House. Some members of the House even met with us individually. I guess it's Mo Udall [U.S. Representative Morris K. Udall]—actually met with the fellows in his interior committee room in the Longworth Building. It's pretty amazing.

We got a lot of attention from folks, including a man in his sixth term named [U.S. Representative] David [R.] Obey, who has chaired the panel that rewrote the ethics rules of the House and is a pretty amazing guy. But he has us in his office, but as often happens, he's off voting on the floor. So all the fellows come in, and they sit around his office in the Rayburn [House Office] Building, and we're on all the seats that are chairs or couches except his behind his desk, and we're all over cushions on the floor, just on the floor, and everything. All we hear is the outer office—because from these offices, there's two entrances to the hallway. One is from the member's office through a little corridor with a bathroom and whatever, just for the member, straight out into the hall, which is how they get around the constituents waiting in the outer office where the receptionist is if they have to run and go do something or just need to get out of there or get in. We hear that outer door slam, and the first words I hear him say are, "Goddamn this place!" He walks in, sits down in his chair, and realizes he has an audience.

He explains why he's upset. He has just had the experience—and he is the poorest member of the House of Representatives. He has no per diems, he gets no honoraria for speeches, he has no inherited money, he has no outside board positions. He is Mr. Ethics, you know, but it's not because he's given them up. It's he's never had them. He's got a wife, he's got two kids. He's representing the northwestern 40 percent of Wisconsin. Any rate, Obey explains he's just been in a vote on the floor about the pay raise for members of Congress, and he describes a situation in which members have voted against it and are standing out of sight of the cameras—because C-Span has started but it's only in the House—standing in the cloak room where they can still be seen and applauding for everybody who votes for it. The hypocrisy of this, and not being willing to take the political risk to do what you know is right, because you know to live where Congressmen have to live, they're going to need a larger salary. You can get into a lot of arguments about this. Obey's a liberal populist and a budget hawk—well, he's not so much of a budget hawk then, but he cares about budgets—and it's just got him upset. He is a man, to this day, deeply invested in the House as an institution, and partly, you also have to realize for this tape, he in effect is my boss. My boss is a staff director, but the staff director serves him as the Committee Chairman. So some people would argue you have to take that with a grain of salt.

But any rate, Obey's one of the people I interview with. I finally get down, I've got three candidates: Obey, [U.S. Representative] Al [Albert A.] Gore, and [U.S. Representative] Andy [Gene Andrew] Maguire, in that order I think, but I'm not sure. I'm wrestling with it, and Al Gore is a sophomore Congressman and feels kind of like an old shoe. I didn't quite realize it at the time, but my uncle and his father were college roommates. My father's family is from Murfreesboro, Tennessee, which is the key city, small though it is, in his congressional district.

Any rate, I go to a man who is retiring from the Hill where he's been staff director of the Science Committee staff to come run the fellowship program for the AAAS, which is the overarching fellowship program. I sit down with him. He says, "Well, what have you got in mind?" I describe my three choices, and he basically says to me, "Andy Maguire: sharp, good guy, smart man, PhD from Harvard in government; however, his name as a cosponsor will cost you 25 votes on the floor of the House," because Maguire—a flaw I obviously still somewhat share—but he would let people understand he knew he was really smart, and that offended people in the House in those days. It's far less true today, but it certainly probably offends people anyway. He then says, "Al Gore," he says, "Al Gore: good young member, sophomore, he will get his act together," future tense. "If Dave Obey will have you, go there."

I go back, I go in. A fellow named Scott Lilly is the legislative director, not the AA [Chief of Staff] who's supposed to be the staff boss, but in Obey's office the AA is this wonderful person with a great relationship with Obey, but the legislative director Scott Lilly is kind of Obey's alter ego. I go in, I say, "If you guys are interested, I'm interested." They say, "You're hired." So I spend my fellowship year with David Obey. Knowing that it's one of the questions you guys gave me in advance, what did I learn from that? I learned coalition politics. Mr. Obey was a good framer of issues in those days, where in the House it's a [Democratic] majority, but it's been a majority so long that it doesn't hang together very well. There's not a lot of party discipline when you've been in power since the early '50s. The Democratic Party's a big tent, so there's not a lot of agreement on many issues, other than who should be speaker.

Mr. Obey's on the budget committee. He wants to propose an alternative budget resolution. The budget is just beginning to be taken seriously. There's a process, we've got big deficits, nobody knows how to control them. Budget resolution comes up. He wants to propose an alternative first budget resolution. Well, we don't have OMB [Office of Management and Budget], but we do have six legislative assistants including two fellows on his staff, and we basically go pull together an alternative. We do it at the margins. We basically come up with roughly a billion dollar change. It's a bigger percentage of the budget than it would be today. It would be like a three billion dollar change in today's budget, I think, correcting for late Carter administration inflation and the passage of time. I pick up the small business and tax part of it, which is how we're going to pay for it, because you have to have revenue to offset the increases we're going to do in spending. The spending is pockets of this and that.

Then I go with Scott Lilly, who's pulled in the lobbyists and the ground troops for all these organizations. We have everybody from the asphalt paving people and the tobacco lobbyists, maybe the health end of those because it'd be very odd for Obey to be lobbying with the tobacco smoking people. But the antismoking lobbyists, the healthcare people, asphalt pavers, all kinds of stuff. There's a huge number of troops, 70 people, maybe, or more in the room. Scott tells them what our budget resolution is and sends them out to work. This is a coalition. We come within 12 votes in the House of Representatives of passing an alternative over the budget resolution from the Democratic Budget Committee. Obey, as a Democratic member of the Budget Committee, he's also taking on his chairman, which he'd actually started doing from the minute he got on the Appropriations Committee.

In any case, it's an amazing experience, but what I learned is the potential of coalition politics. I always felt that they helped me in shaping the Earth Observing System, and then I came to believe they didn't work very well, and I've now swung back to the vision that it really did help. It was another seed for what was happening.

So I get to the end of the fellowship, and I'm a GS-13, I'm looking to move to a GS-14, 15, I want to jump to a 15. On the Hill, you can see careers that kind of seem to move in ways that are not restricted by the normal laws of seniority and succession—not member careers generally in those days, seniority really ruled. But staffers can move up a lot. So I'm trying to get ahead. I have six interviews at EPA [Environmental Protection Agency]. This is the end of the Carter administration. We didn't know it was the end, but we knew it was the end of the first term. I have an interview with the Senate Commerce Committee, and Mr. Obey's running for chair of the Budget Committee, which is a caucus election in the Democratic Caucus. The election of [1980] happens, and Mr. Carter has conspired to lose.

So Mr. Reagan [President Ronald Reagan] is coming into office, which is important—well, for me it's a really amazingly, given that I was certainly a Carter supporter, probably my favorite president maybe of all time. I probably may like Barack Obama more, but politically and personally, he's my kind of guy. He's a pro-civil rights, deeply religious southerner. Hello! So in any case, Reagan wins. Anne [M.] Gorsuch [Buford] takes over EPA. There's no way I'm working for her. I am an environmentalist at this point. I'm a social do-gooder. We lose the Senate, Democrats lose the Senate. Well, there goes the job with the Senate Commerce Committee.

Then there's a caucus election in the Democratic caucus. Well, the way Budget Committee elections work, three people were running: a congressman named [James R.] Jones from Oklahoma, Mr. Obey, and [U.S. Representative] Paul [M.] Simon—not the musician, but Paul Simon from Illinois, who subsequently is a Presidential candidate in another election.

Simon's a great guy by the way. But he's running. We know Simon's going to lose. Basically, you've got a ballot. Unless somebody gets a majority on the first ballot, what happens? First ballot, whoever comes in last is dropped off, you go to the next ballot. There are only three people. The second ballot should be the last ballot. Two people left, right? You vote.

So indeed there's the first ballot: 100 for Jones, 100 for Obey, 35 for Simon. He's out. There's some Democratic freshmen. Even though we didn't do all that well in the election, there are a bunch of Democratic freshmen coming in. The class of '94 in the Congress that was elected in Watergate, 105 Democratic freshmen, is beginning to rollout and beginning to lose in some cases. But Democrats still control a majority. Second ballot, if I remember correctly, it's 117 to 117 tie. Freshmen in those days don't have cell phones, and they don't have office space yet. There's no way to get them. They vote, it's the last vote of the day in the caucus meeting, they're off. It's December. Holy cow. We have to have another ballot.

Next ballot, who's there has changed, and Mr. Obey loses 120 to 115 to Mr. Jones. He can't hire me under the Budget Committee staff because he's not the chairman, and I go back to NASA Headquarters. I go back to Shelby Tilford and say, "Take me back!" What's interesting, in a move of unbelievable ego back in June of my fellowship year, Shelby has offered me the chance to come run the stratosphere program in his branch, and I turn him down because I think I can do even better elsewhere. Never was anybody wronger. When I don't do that, I run into Bob [Robert] Watson in the hall at some point during this period of time, and Watson says, "I think you did a great thing coming to Headquarters," and Watson is a real presence in the field already. He is not the best laboratory photo-chemist, but he's the one who knows every reaction rate, what it means, how to assess the uncertainties of the measurements, and I've been interacting with him because he's been the lead on providing the uncertainties in reaction rates for the work I've done with Stolarski to put the error bars on predictions of ozone depletion. So we've worked on ozone assessment reports under Bob Hudson, who's leading them at that point.

In any case, he says he thinks it's a great idea. By the time I'm coming back in December, Bob Watson is there firmly in charge of the stratosphere program working for Shelby. So I come back, they say, "Okay, you'll come on detail from Goddard full time, and we'll hire you. You're going to be the advance planner for the division." So I'm working for Greenwood. Bishop is off on a three-month long or something management thing for budding young managers. The division has a branch under Shelby doing all the atmospheric stuff and climate, and a branch under Stan Wilson who was competitively chosen over Jim [James] Baker, from what I'm told, to come in and form the oceans branch. I show up as advanced planner.

Here, you need a little background. Shelby's got the Upper Atmosphere Research Satellite, and it hasn't gotten a new start yet. Stan's got TOPEX [Ocean Topography Experiment], and he's had to come in and recover from the 99-day SeaSat [Ocean Dynamics Satellite] demonstration. [SeaSat failed on orbit.] Another piece of the puzzle, which is really more important in retrospect than I thought it was at the time: President Reagan takes office. This is another piece of why Reagan is so magical in all this. He takes office, and he wants to act like he's doing something, because Carter has literally pulled his budget back from submitting to the Congress in 1980. He submits his request to the Congress. The economy starts to go bad. The projected deficit goes from $15 billion to $30 billion, numbers that are almost below notice today in the concept of the whole budget of the United States government. He withdraws his request to rework it and cut $15 billion back so he's back at the original $15. He took balanced budget seriously.

Reagan comes in and he says, "Look, we've got to cut some things," and he says that to the Defense Department. There is a plan with Tony [Anthony J.] Calio firmly in the lead for NASA—and Tony Calio is head of Applications at NASA—NASA, NOAA [National Oceanic and Atmospheric Administration], and the Defense Department, specifically the Navy, to do the National Ocean Satellite System, NOSS. Big satellite, big bus, lots of instruments, big instruments. It's not lots by eventual EOS planning standards, but a goodly number because SEASAT has proven what you can do from satellites for oceans. We're going to go build this system and help the Navy and the civilians and everybody together. This should have been a red light for convergence in the [President Bill] Clinton Administration, but everybody had forgotten, or else they didn't think it was relevant.

Reagan takes office, and at the beginnings of the largest peacetime buildup of military spending in the history of the world, probably even adjusted for inflation given that it was peacetime, they cancel. The Defense Department cancels NOSS! The Navy walks away. There's no way NASA and NOAA can do this without them. This thing's going to be a $1 billion-plus kind of mission. It's big. I mean, this is bigger than Space Telescope, budget-wise, this is huge. So Stan all of a sudden has got a program with pieces on the floor. He has this mission called TOPEX, the Ocean Topography Experiment, and he's looking for a new start for TOPEX. Tilford and Wilson are competing. But I get sent as the new advance planner over to work with Wilson, which was a miracle.

So I'm Shelby's protégé, I'm working for Ron Greenwood. For maybe the first six weeks or so, I'm going to lunch with Tilford and Greenwood, not with Wilson. He's got his coterie of oceanographers in his branch, and they don't eat with the atmospheres people. So I'm going off with my boss and my mentor to lunches and one thing and another, and I won't say all the things that were said. But they send me over to work with Stan Wilson.

Stan teaches me basically the five-pointed strategy for ocean observations from space, which is we've got to have a visible infrared imager so we can get everything from ocean color to sea surface temperature in the thermal infrared, and the aerosol corrections part of that to get to ocean color, which is a really tough measurement. We need passive microwave, because a lot of the ocean is covered with clouds, and you can't see through the clouds with an infrared, so you've got to have a microwave to get sea surface temperature at cruder resolution through the clouds. We need a down-looking radar for surface topography, the TOPEX mission, for surface topography to make a two-centimeter accuracy measurement between the satellite and the ocean surface. The radar naturally—depending on the wind speed and the unevenness of the water—the radar naturally averages over a three to eight-kilometer circle, but that's just perfect, and just radar shot after radar shot, you see the height of the ocean.

Just to put it in the record here, everybody's used to, by now, looking at weather maps with highs and lows. Well, the ocean's a fluid. It has not totally dissimilar circulation, and highs and lows manifest themselves as the top of the ocean. So at the surface, you see the highs and lows, if you can very precisely measure the height. Actually, the atmosphere goes up and down as well like that. But there's no firm boundary in the atmosphere really to see, subtle things but not the firm one. So that's how an ocean downward looking radar—you need a side-looking radar because as wind blows over water, it creates very small little rills, and if you have the right frequency of radar, its reflection looking sideways will be directly related to the number of those rills. Those numbers, little rills.

If you watch really strong wind blow across water, you can even see this. But it happens, even moderately light winds, you can pick it up. So if you've got a wind fetch of at least 50 kilometers, you've got enough signal, and the strength of the wind is measurable. But if you do it at multiple directions, you also get the vector wind, you get the direction of the wind, which is also important. You can't tell whether it's coming at you or going away from you, but usually you can figure that out from the weather, and you get the wind speed. Then you also need passive microwave for looking at sea ice, and then you need synthetic aperture radar for the ice guns.

I go off and start working on a synthetic aperture radar mission with the Canadians. Before I've gotten there, there's been a plan under a previous guy [George Esenucin] trying to do advance planning previously to do an ICESat [not the current Ice, Cloud, and land Elevation Satellite] mission, a big platform with a synthetic aperture radar to study the polar regions. It hadn't gotten anywhere. I go off and look at this plan, and we call it FIREX [Free-Flying Imaging Radar Experiment]. Ultimately the Canadians call it RADARSAT. But I hang out with the polar oceanographers when I'm not going to lunch with other people. I have the worst year of my career, but I do learn those things.

At the end of the year, NASA's been operating with an acting administrator, acting deputy administrator. The acting deputy has been Tony Calio, who was head of Applications, so Ron Greenwood, has been acting as deputy to Calio's deputy Sam [Samuel W.] Keller. Bill Bishop has become my boss. We get along fine personally. We don't get along professionally very well. I'm not good at what he wants done, and what he wants done is not what I want to do. So you know, that's not a great fit, but I sort of tough it out. But I also begin to not be very good at work. I'm not very effective in this regime. But I learn.

At the end of the year, end of 1981, finally the pieces fall in place. Jim [James M.] Beggs, a closet environmentalist, becomes administrator of NASA. Tony Calio leaves for NOAA. A whole slew of other little things begin to change, including Ron Greenwood decides that to educate his four children, he'd better go to the private sector and make more money than he could make as a senior executive. Shelby becomes the division director of the Environmental Observations Division. Bishop goes off to be the deputy of the Life Sciences Division.

NASA brings Applications and Space Science back together in one piece, and Burt [Burton I.] Edelson comes in. Another important thing about Burt Edelson is he is Jim Beggs' college roommate from the Naval Academy. So Edelson comes in. He's been at the Intelsat [Ltd.] labs [laboratories] running them. Interesting man. Reorganization underneath him, we're now the Office of Space Science and Applications, one united office again. Pitt [G.] Thome has been running sort of the land-related counterpart, I guess it was called the Resource Observations Division, counterpart to Environmental Observations. He, I think, really wanted to retire. He's too young. They put him in a job staffing, and it may be that they didn't want him as a division director. I don't know the details of that. But he ends up on Burt Edelson's staff with a job that they can then abolish after a year, and then he can get early retirement.

They put Jesse [W. Moore]—he's the man who was unfortunately the one who made the decision to launch the [Space Shuttle] *Challenger* [STS 51-L] that day, a decision which for every flight until the Challenger accident had been made by Jim Beggs personally. Jim Beggs had been squeezed out due to the machinations of the Attorney General, and so he wasn't there. Jess was head of Manned Spaceflight at that point. He makes the decision. Poof. You know, needless to say his career was over.

But at this point, he's in charge of what they formed, but they formed it by taking Planetary and the terrestrial part of the Earth and putting them in a division together. They carve up Solar Terrestrial, and they put Solar over with the astrophysics guys, and they put the Interplanetary Medium, the solar wind, all that stuff, magnetosphere, experimentally over those guys. Actually, I'm sorry, that's not true. They put the magnetosphere part with us, and they bring the Solar Terrestrial Theory Program to us, which is a new program that George Newton—my former colleague from Goddard who's blossomed into a wonderful manager and senior executive at NASA Headquarters—Newton has been the initial program manager, but he loses the program as it comes over to be in Shelby's division with us. I'm the only one [on the division staff] who's ever had a plasma physics course, thanks to going to Rice and being made to take it by the space science department. So they put me in charge. It's a wonderful little program.

But the other piece of this you need to understand, it's sort of like atmosphere is in Climate, including the stratosphere, but all the way up to the mesosphere and ionosphere and stuff is all in Shelby's division. Jess' division has got planetary, but it's still got a little Landsat [Land Remote Sensing Satellite] stuff, and what gets called Renewable and Nonrenewable Resource Observations as its two branches for that stuff, and then there's planetary. Ecology is off in the Life Sciences Division, which is a small division and has kind of what you now would think of as life sciences at NASA, namely the indoor sports stuff, things relating to astronaut health, living things that go on the Shuttle or whatever. But no Shuttle is launched yet. So what is going to become Earth System Science is cut between three divisions at NASA Headquarters.

Okay, now I'm losing my thread. Any rate, that's the strategy. For me personally, what happens is I'm given the Solar Terrestrial Theory Program to run. I really have fallen apart.

Unbeknownst to me, they've stopped trying to hire me away from Goddard because I'm doing a really mediocre job. All of a sudden, I get my sea legs back. I decide to hit the road and go meet the investigators. There are only 14 research groups. I basically go meet 12 of them. I put together a series of trips, meet them. The selection's already been done, but this is an elite program. This is important for NASA history, not for Earth System Science history. Well, maybe it is. No, it is important for Earth System Science history.

What has happened in the space physics community is it's been dominated by the experimentalists, [James A.] Van Allen being perhaps the archetype, but he's off in academia. But you tend to have, like, Nelson Spencer, Laboratory for Planetary Atmospheres, he's the PI [Principal Investigator]. They have house pet theorists like Dick Hartle who don't get ahead very far. They get kind of terminated at GS-13s and 14s, they don't get the big action, and they don't get lots of money, and they don't get a group, and they don't have much in the way of grant money. As mission after mission is flying to observe the ionosphere and various things, the data's building up. It may or may not get analyzed, and it may or may not get put in the archive, but there's no construct.

The National Academy somehow gets together and basically realizes that this is not the right way to happen, and the National Academy of Sciences has recommended forming the Solar Terrestrial Theory Program, which will be designed to give grants only to theorists. That's the program. Newton's run the selection, picked the people. They're in their first year. I get handed it to manage, my first program to manage. Three million bucks spread across only 14 groups. This is serious money. The largest funding is going to a PI who is still a professor at the University of Maryland, [K.] Dennis Papadopoulos. He's getting $360,000 a year in grant money from me. My second largest grant is University of New Hampshire [UNH, Durham, New Hampshire].

But I still remember, I go for my site visit to UNH. They have like a hotel thing on campus where they put people up and maybe they use that to train students. I don't know what they're doing. PI is a fellow named Lennard [A.] Fisk. Len Fisk has bailed from Goddard where he was the pet theorist in one of the other laboratories, specifically [headed by the name's evaporating but he] ends up eventually as Chief Scientist of the agency and does terrific things. But Len has run away from Goddard. He's an associate professor at the University of New Hampshire. He wins a Solar Terrestrial Theory Program grant. He's getting $300,000 a year. This is a lot of money at the University of New Hampshire. He gets tenure. He knows how to handle people.

He says, "Okay, before you meet the group the next day," because I insisted on meeting the whole group, all the post-docs, the graduate students, I wanted to see the people. He says, "Let me meet you for a drink in the basement bar," of this thing I'm staying in. So I get there; I'm driving a rental car or, I guess, taking a bus up from Boston [Massachusetts]. I get there, he meets me, we go down there. There's nobody else in the place. He orders a beer. I explain that I don't drink, and I order a Virgin Mary [non-alcoholic beverage]. He sips that beer in the wake of seeing that I'm not going to drink anything alcoholic. He sips that beer for the rest of the night. We have a nice conversation, he explains the way his group works. We hit it off pretty reasonably. His group's doing great work, but he and I click, and as a manager, I realized at least in two cases, the only two that ever mattered, I tended to find somebody out there in the science community—and here I really, desperately needed—who becomes my confidant:

somebody I can bounce ideas off of, somebody who will give me the pulse of the community back, and who I can just get some wisdom from. I pick Len Fisk. What luck!

Anyway, so I pick Len Fisk to do the Solar Terrestrial Theory Program, and that's going to be important for EOS. Now to come back, I'd forgotten about it, but my career almost ended with UNISPACE [United Nation's Conference on the Exploration and Peaceful Uses of Outer Space], UNISPACE '82. Beggs comes in. He realizes that maybe there's big time to be made in doing this environment stuff. Politically looking back now, from the light of what's happened in the Republican Party and concerns about climate change and everything else, it's hard to believe. But in the Reagan administration, they came in the door, and they hated what the Carter people had done, and specifically the man who then under Carter had done this big report, the Global 2000 Report. Carter had had that pulled together. The man who headed it ultimately went to New York to head the UN [United Nations] Environmental Program, a long time, major caliber environmental leader. They came in and hated the 2000 Report. Basically Greenwood and Tilford pivoted on a dime, got rid of anything like the Operational Satellite Improvement Program, which they already didn't like, and packaged the Environmental Observations Program as doing science. This was the science of the atmosphere and climate, science of the ocean. Those other guys could do resources, whatever. That's what we were, we were science. We were not applied. We weren't going to go out and mess up anybody's economics. So we got to do it.

Jim Beggs gets this pulled together to go off, NASA pulls it together, and as the advanced planner, my job is to pull together beautiful pictures that will help show the need. So I've got, like, dust, Sahelian dust rolling into a town in Africa and literally burying a town. You know, you see half the town there, and half the town is under a sand dune. I've got pictures of the Dust

Bowl, just dry American West, and I've got other crap going on in the environment. I've got your environmental, fundraising, do-gooder, alarmist pictures that I've gotten from other federal agencies, and a package of them explaining what each is. We don't have satellite data to put in. They had to go in a folder, and somebody at JPL was working with me to pull together a folder, and he's a speech writer or something, and he writes two quotes or something, two passages. It's like a music folder. It's got a big flap at the bottom to hold these pictures in, each one of which has some part of our story on it.

Then there was going to be our written one-pager explaining what we were doing, what we wanted to do. Oh my God, the words were not very good. Stupidly—you'd think I'd know better being a Hill staffer—I didn't run them by the Administrator. I didn't run them by anybody. The Administrator takes one look at them and says, "I don't understand what these words say." So with Hudson's help, because he's then working on detail, kind of-sort of working for me, not really, certainly working for Shelby, he helps me. We go through, we slice the folders on the edge—and these are beautiful folders, and it's too late to get them reprinted. Copies of these folders have already been sent, but we've got enough still at home. They've already been sent to Vienna [Austria] for the meeting. We slice the edges off, the flaps fold down, we slice the [offending words] off the flaps, we fold up with only about a centimeter and a half flap on the bottom to hold the pictures, put them together, ship them airmail to Vienna. Beggs goes to Vienna. Other countries don't like it, so in this UNISPACE conference of the United Nations it doesn't get a great following. Get home, and every agency wants our head. Wants our head. So you've got to start on an interagency basis, and that begins to happen.

Now, Burt Edelson is worried—and unfortunately, he's passed away, so we can't go ask him—but from what I was told and infer, Burt was worried about enough band, enough spaces in the geostationary arc for the growth of communication satellites. So his view is, "Well, maybe we need bigger communications satellites." Well, to get those affordable, we've got to get to a bigger satellite bus. Somewhere in his mind, he must have gotten from that to who can justify a big satellite bus to do something else, and he hits upon Earth science. So he basically says to Pitt Thome, who's sitting somewhat without duties on his staff, "Pitt, go pull a few people together. I want you take a look at what you can do for Earth science with a big platform and polar orbit."

There are studies that have happened of platforms in orbit that are built by taking five Shuttle bays up and bolting them together—sound familiar? Although they were pretty amazing, and we [planned them]. It was like bolting five Spacelabs together. I was sitting in discussions in Headquarters of other advance planners who weren't going to really get anywhere doing that. Pitt goes and forms a group, and it's an interesting group. First of all, there are ten people on it, every one of whom is either a program manager at Headquarters or a manager at a field center. One of them—and I can't remember all the names—one of them is Dave [David] Atlas, who's the Laboratory for Atmospheres Chief at Goddard. Pitt himself. The man who was heading the Earth science Lab at what became [NASA] Stennis [Space Center, Mississippi] down in Bay St. Louis. There's me. There's a young guy from Life Sciences to bring us the ecology. There's a fellow, one of my colleagues from the Solid Earth people, although Pitt's mostly covering that. There's Anne [B.] Kahle from JPL and somebody else from JPL. In the engineering directorate at Goddard, there is a man who runs, I think, a division that does systems engineering of satellites or has that kind of technical expertise to loan out to all the flight projects. I'm sure I'm leaving somebody out.

We form a group. We have a meeting. We have another meeting, which is at the Jet Propulsion Laboratory, and we kind of each have to bring our perspective. I'm supposed to bring the atmospheres and oceans perspective. Pitt and others are bringing the land perspective. Actually, Pitt and the man from Stennis are bringing that, or from what became Stennis. They also had this data system thing. They're really hopped up on the data system. I don't get it, but they're hopped up on the data system. I really don't get it, which is embarrassing as a modeler and data analysis person. We've got a variety of skills, and Atlas is big into radars and weather and all that stuff. So we all talk about our stuff, and there's geology and geophysics talked about, and land resources and ecosystems and everything. It's not clear how we are ever going to gel a mission out of this. I mean, it's really not clear. We have our meeting, and we go back to our hotels for the evening or whatever—we probably went out to dinner, and we got back to the hotel.

We're in the hotel, and I'm up in my hotel room, and magic happens. I would say inspiration happens. I'm sort of thinking about, "Well, what connects these people?" Stretching it a little bit, I say, "Look, it's water." This is the water planet. The fact that we've got water in all these phases, whether it's the arctic or whatever, it's rainfall, the heat transport happens heavily through the condensation, evaporation of water. We've got the oceans, even volcanoes are heavily influenced by water, and water shapes the geology you can see. It's a water-dominated planet. So water hangs us together.

Then based on what we'd been talking about, I say, "Gee, what would we fly as a payload to do water?" I come up with a six-instrument payload. It's got a weather radar. It's got a big passive microwave. I'm pretty sure it's got an optical imager of moderate resolution, like one kilometer kind of resolution imager. I'm pretty sure it's got a temperature and moisture sounder of the atmosphere. I've actually forgotten the other two.

I walk in the next day with some trepidation, because remember, I'm pretty junior on this group. I'm one of the junior Headquarters—well, I'm not even really much of a program manager yet, although I have picked up some additional duties of running the Stratosphere Theory and Data Analysis Program, but I'm not working in the branch. I'm still up in the front office part of the division. In any case, I walk in, I pitch the idea, and much to my surprise, instead of laughing at me or giving me a hard time or giving me a PhD-caliber examination on, "What do you mean?" they say, "Where's the rest of it?" They send me back that night, and I walk in the next day with the other—and I think my memory is 19 instruments total. I come back in with two more satellites, each on a large platform, each with multiple instruments. Not unlike the upper atmosphere research satellites. It's not hard to stretch; UARS had ten instruments if I remember correctly. So there's UARS with ten instruments, so yes, we put a bunch of instruments. It may have been 24 instruments actually. Mark [R.] Abbott, in a talk at this 20th anniversary meeting, I think used the number 24, so maybe it was 24 instruments.

At any rate, I come back in, so there's my wet payload, my water payload, and then there's the chemistry payload, and then there's the everything else payload, whatever, probably the ecosystems payload with a high resolution land imager and probably another wider field of view imager for ocean color. Okay, it's now just a matter of producing view graphs and talking about the data system. Well, I'm the executive secretary of this group. That's my job. I'm Pitt's exec secretary, which in some ways designates you as kind of the junior guy, right, the go-for guy. So I start helping him. We pull together the view graphs. For some reason, maybe a family vacation, I can't be there when it's briefed to Mr. Beggs. But Burt takes Pitt in behind the green doors in the old NASA Headquarters to brief [NASA] Administrator Beggs, and I'm told—and unfortunately this is secondhand—but I'm told that basically Beggs looks across at his former roommate and says, "Stop trying to undermine my Space Station!" Then he says, "But you can keep doing this. But I don't want to read about it in *Av [Aviation] Week*." Holy cow. Well, you didn't have to tell Burt any more than that. He knew he had permission to go.

About this time, shortly thereafter, Landsat-4 launches. What has caused the weird organizational structure under Burt is Shelby Tilford was a laboratory photochemist who'd done one balloon payload not all that successfully as a researcher. He's not viewed as knowing from instrumentation. The guys over in Planetary, they're very strong on the instrument side of things. Landsat and Thematic Mapper, its chief new instrument, have had big problems, cost overruns. I seem to remember numbers like $80 million cost overruns, which in those days was a big—it's still a big deal. That's a lot of money! That'd be like having $150, $160, $200 million cost overrun today. So they don't want that being managed by Tilford because they don't think he can be good enough on that kind of score. So they've kept this funny [organization], and that's why these guys are over there. The minute Landsat-4 launches, gets on orbit, Thematic Mapper cuts on and works, the stress is gone. The need for that is over. There's a reorganization, and order ensues.

We keep Solar Terrestrial for a while, but we get the land guys. The renewable and nonrenewable resources guys come to our division, we become the Earth Science Division: Planetary Division, Astrophysics Division, Life Science Division, and we even wrestle the terrestrial ecology work, which is a really small part of the Life Sciences Division, we get that away from them. Not immediately, but you know, this kind of begins to take shape as a reorganization. I don't remember the timing. You'll have to look at real sources for that rather than oral sources. So the seeds are beginning to be sown. We've had UNISPACE, we've had the System Z study, we've got permission to go forward, and I've got three million bucks to run the study, plus there's $8 million going for a JPL Shuttle instrument that in essence is the high resolution imaging spectrometer, which was clearly going to be one of the instruments in System Z.

Well, I go to Shelby. Ray [J.] Arnold becomes Shelby's deputy. I still remember, the System Z study's kind of over and I go to him—and this must be 1982—and I go to lunch with Ray Arnold and Shelby, and I say to him, "Look, I'd really like to keep working on this." Well, Pitt gets to retire, and I'm put in charge, and the pieces come to our division. I go around to the program managers, my sort of colleagues, somewhat seniors, and say to them, "Okay, guys," and I'm going to use a little foul language here, but basically my rule was, "I need somebody from your community where we're going to pull people together from across all of Earth science." The other conclusion I have is the study for System Z had been done completely in-house, and I knew it would have no acceptance in the outside scientific community as an in-house NASA study. We'd kind of been to that movie with UNISPACE. So I said, "Look, we're going to start over, and we're going to only use people from the outside. We're not going to use insiders, nobody from the NASA Center." I think that was actually true.

Now, I was given a project office at Goddard. Since JPL wanted a piece of the action, at least was supposed to want a piece of the action, the project office got a spacecraft extra help from JPL, so a sort of sub-project office at JPL through a project manager at Goddard, Chuck McKenzie, doing the study. Marty [Martin J.] Donahoe at Goddard comes, he's going to be the lead instrument guy. Goddard hires a young guy away from the Nuclear Navy to be the spacecraft systems or the overall systems engineer in the project, Chris [Christopher Scolese] gets hired into NASA to do this job. Boy, am I getting troops. It's just amazing.

I get given Dick Hartle. Poor Dick. He's still at Goddard, he's still a branch head, but he gets assigned—even though he's not really an Earth scientist in that sense, he's more in the space physics world—he gets assigned as the project scientist. I'm nominally the program scientist, but I'm in charge. I get Alex [Alexander J.] Tuyahov, a colleague, as a program manager. So I'm a program scientist, program manager, got a project manager, a project scientist, all of this stuff. I've got troops. They were really more than troops, but they let me act like I was leading, and for things I needed to lead on, I did.

We started over. I went around to my colleagues in Headquarters and said, "Look, I need to know who to go to because I don't know your fields. But I want somebody who can be recognized as speaking for the field by the leadership of the field." I basically said, "No assholes." I had to have people who could work with other people, so don't give me any personality problems. That's a nicer way to put it. They were good. So we got a group, and we did not yet have the life science part from the ecology part. They were still off in the Life Sciences Division and didn't come for quite a while, so I hadn't gone to them.

So we form a group, and we go have our initial meeting. I call all these people, recruit them, we pay them a consulting fee, they come to the meeting. We start talking, guys make viewgraph presentations. But the first thing the group says is, "Where's the ecosystems people? They're not here." So we had that meeting, and then I go to this young manager who was on the System Z study with me, and who actually left NASA and went into his in-laws' business or something, it was very odd. But I go to him and I say, "You know, who?" He gives me ecologists. Constant Delwiche, of the earlier talk. Paul [J.] Zinke, who turns out to be a long-term, close personal friend of Constant Delwiche, who's a professor of forestry at Berkeley, and is kind of the professor of forestry. He's kind of the most prominent forester, certainly in the top five of the world, if not number one. I get Constant Delwiche, very prominent soil scientist who also knows soils—I mean, they both kind of know soils and forestry, one's more soil, at any rate—from UC Davis [University of California, Davis, California]. I get Berrien Moore [III] from the University of New Hampshire, because he does kind of ecosystem modeling. Holy cow.

We go to our second meeting in Easton, Maryland. It's summer. It's warm. It's the only time I've ever fallen asleep in the chair chairing a meeting, but I do. After lunch, warm. Oh God, really warm. I also bought my first straw hat. Having worked on ozone depletion and being relatively fair, keeping the UV rays off of one, I bought my first straw hat to go out in the sun at that meeting, walking around the block at lunch. It became kind of a signature for me to have my hat. We went to Easton, Maryland. We spent, I think, four days at this meeting with this group sitting around the table. The first thing we had to do, it's like new college roommates. New folks at college have to tell each other about their childhoods. We had to tell each other about our science.

In the process of this explanation, which gets a little tedious—I actually fell asleep while Paul Zinke was talking. Mark Abbott's one of the people. Mark's talking about the California current. He, at this point, works at Scripps [Institution of Oceanography, University of California, San Diego] and JPL and commutes between them, so he was sort of JPL, but we counted him as Scripps. The current flowing south along the California coast. He talks about upwelling, and bringing nutrients up, and upwelling colder water nutrients, and that's where you get the algal blooms, and so you can tell, wherever there's a promontory out into the current from the shape of the coast, you get this upwelling. Zinke says, "And there's a different forest." It creates a microclimate, because there's always colder sea breeze and mists, and for about ten kilometers [inland] that persists and gives you a different kind of forest. For that matter, it's been going on for long enough that you have a different soil. Connection. That's exciting for these individual scientists. I mean, that intellectual ferment, the chance to hear new stuff. They're not, by and large, old and jaded. Only one of us is really old, and he is from England. He's our only foreigner, and he's the man who runs the Scott Polar Research Institute at [University of] Cambridge [Cambridge, United Kingdom]. Holy cow. I mean, so he's enormously prominent, he's kind of at retirement age, but he comes and does this. He's on our group.

Any rate, magic ensues, they build connections, and we start down the path. Zinke agrees that he wants us to come to California, and he proposes a place called the Granlibakken Lodge at Lake Tahoe [Tahoe City, California]. It fits within the government per diem, bless my contractors. They figure out a way. It fits the per diem. We go there. Amazingly, I still remember playing tennis with Berrien Moore, which is kind of weird to think back on. We go there. Our systems guys have been showing us all this stuff that came out of the System Z study.

I forgot to mention one thing. Let me hark back to it. Alex Tuyahov is told by Burt Edelson to do a parallel effort, which we were calling System Omega, to take what we come up with in System Z and sell it to the Army because they have to operate in the environment, trying to get the Defense Department interested as well. That never happens, but Alex has the classified tickets from having worked Landsat type things which can be used for strategic observations, and having come from that world in some way—and worth an interview. Definitely he and Dick Hartle are certainly worth an interview in the history of all of this.

So that's going on. To go back, we go to Granlibakken. We're being briefed on all this stuff. I'm trying to keep this System Z stuff quiet. I have not told them kind of our concept.

We've not taken my vision from the hotel room in Pasadena [California] and plopped it on the table. We're talking about science, we're talking about large platforms, we give them drawings. Maybe we don't even give them the drawings yet, because this may be actually our third meeting. They are pretty frustrated, and five of them get a case of beer and go sit in the hot tub that evening, and I'm off in my room, not being a beer drinker or a hot tub sitter. I'm off pretty much probably being by myself or whatever. But Ray [Raymond E.] Arvidson from Wash U [Washington University] in St. Louis, [Missouri], who's a primary planetary guy but is one of the people in the group, is in there, and he's a data system guru, among other things. They basically come up with the concept for the Earth Observing System. It's still called System Z.

They come back in, in the morning, and I start the meeting and I start to talk, and Arvidson cuts me off. So okay, I'm, other than Mark, probably the most junior person in the room even though I'm in the chair, and I don't act like I'm the most junior person in the room. Arvidson goes to the board and lays it out, lays out their vision, and I recognize my vision in their vision. The details are not the same. EOS never embraces a weather radar, doesn't come in our payload layout, it doesn't end up in the AO [Announcement of Opportunity]. But most of the other instrumentations—synthetic aperture radar, a bunch of instruments to look at the stratosphere, the whole suite of oceans things that Stan had taught me were needed, moderate and high resolution imaging spectrometers in the visible near infrared, thermal infrared stuff. The whole nine yards, lasers, the variety of lasers. All of that lays itself out. We don't know exactly what the instruments are going to be at that point, quite honestly. It's somewhat early for that. But we have an idea.

Then we begin actually not so much dealing with all these drawings of spacecraft. We begin to articulate a mission concept, and I think if you look back at the first report, volume one and its appendix about that, it's really explaining need and overarching requirements, but it's got a vision. The vision has this cloverleaf design showing the interlink of ocean, cryosphere, solid Earth, land, and atmosphere. I mean, literally each one in a different color with the solid Earth I think in the middle as brown and maybe the green. I've forgotten the logo, but it's a distinctive logo. I think Dick Hartle basically mostly came up with it.

We publish our reports. I deal with the writing. Dick Hartle deals with the visuals. It's slick. It's on enamelized paper. It weighs a lot for the size it is. We articulate, though, five key principles, and they all are rooted in the uniqueness of studying the Earth as opposed to doing other firms of science. We can't do the traditional scientific method. We certainly can't do what biologists are required to do, because we cannot run controls. We have no other planet to hold as a control while we do an experiment. We're in the midst of the experiment. It's not a completely controlled experiment. As I used to say, Mother Nature is not easy in giving up her secrets. All we can do is sit and watch and compare different times, different places. But we don't get any real control case, so we can't do science the way every physicist and biologist in the National Academy thinks you've got to do science, which is an important point, because Earth System Science has to convince the old establishment to move aside.

You'll often notice from the people who are on the EOS Committee, it is not very much of the old establishment. We tended to get younger, junior people. I mean, Mark Abbott's in his 20s. He's already a comer but, you know. Most of the people on the committee are under 40, and as I explain there's only one really senior—well, Zinke's pretty senior, Connie Delwiche is pretty senior, the man from England is pretty senior.

Any rate, rooted in this, that you've only got one object to study, that you can't do the controls, and that you have to build up using both time and space, and what it comes down to is number one priority in Earth science is take today's data today, because you cannot come back and take today's data tomorrow. You may infer it from ice cores and sediment cores and all kinds of things. You may try and calculate it. But you cannot take the actual data unless you take it now. So even if you just put the data in a warehouse, the most imperative thing is to go get the data.

The second thing is, as a priority, you'd better manage the data. You'd better hang onto it, and you'd better make sure that it's intercomparable over time and space. Calibration, you've got to understand how this measurement relates to that measurement over your time and space variables. I may not remember them all, but it basically is when you've got that firmly in place, the observations, the data system, you need a robust program to go research the data. There were a few other things, but I've forgotten the five points, and that's kind of embarrassing. But that's sort of the essence, and that flavor has to come up.

All right, parallel track: Shelby in his wisdom, or whoever in their wisdom, in the wake of UNISPACE has decided that they need an Earth System Science Committee. It really is an interagency thing. The man—and I wish I remembered his name—who's head of the U.S. Geological Survey [USGS] gets put in charge of the interagency coordinating group that in effect eventually becomes the U.S. Global Change Research Program [USGCRP]. He's in charge. He's nearing retirement. He's a geologist, but he's a gardener, and he treats me in a kind of avuncular way, which I liked. It was nice. I didn't need the advice, but it was nice to get it.

Shelby's there, and as I said at the conference, this is a set of agency leaders who believe in the mission above the agency's agenda, above the agency's future budget. It's like Bob [Robert W.] Corell and I think, more or less, Mike [Michael] Hall, and the man from USGS and

Shelby. They basically realize they need a science committee, and they go to Francis [P.] Bretherton to chair it, and it gets pulled together. It purposefully has John [A.] Dutton, who's on my study committee, and Berrien Moore from my study committee on it, to make sure there's some crosspollination, and they're also the two most politically important people on my committee in the politics of science. John Dutton has been president of the American Meteorological Society, he was dean at Penn State [Pennsylvania State University, University Parks, Pennsylvania], which was and is one of the five preeminent meteorology departments in the United States. Berrien is a political player. Berrien knows things like Jamie [James Barrett] Reston personally. All of a sudden, you find—you remember?—one year in *Time Magazine*, Man of the Year is the planet on the cover. Berrien made that happen. He has contacts I just couldn't believe in those days.

Any rate, they form, Francis leads, I have to interact with them some. Ray Arnold is made—I'm a little jealous of this, personally, at the time; I've got my hands full though—Ray Arnold is made the key NASA person to go staff them. But in this case, you're really staffing them, whereas in the EOS study, I'm in the chair. So I shouldn't have been envious, but I was. The other thing is I'm doing EOS half-time, about a sixth of my time running the Solar Terrestrial Theory Program, and a third of my time running the Stratosphere Theory and Data Analysis Program. So I'm managing about $10 million. $3 million in Solar Terrestrial, $3.5 million. $6.5 million on Stratosphere, and this EOS thing. The interesting thing, of course, is Alex Tuyahov and the project managers are taking care of the money. I didn't have to think about the money much. I just had to say, "We need this, we need that, we need the other."

We publish our initial report, by the way, and then we go to a new committee. The man from England says, "Look, this is a young person's game. We need people who really will still be intellectually active when the things fly," not unlike what Dalgarno had said to me many years before, about the Grand Tour of the outer planets. So he drops out and is replaced by Seelye Martin [University of Washington, Seattle, WA]. I know Willy Weeks from the FIREX RADARSAT study. He is the preeminent polar scientist in the United States, and one of the two or three or five in the world. He won't come on this committee, but he sends his branch head, who he nominally works for, who's less prominent. He comes on the committee, so I've got my polar guys. You know, there's some rollover, but Seelye becomes very important to have because although it's about a 20-person committee, there kind of begins to be a nested inner five that ultimately does more of the writing: Berrien, myself, Mark Abbott, Seelye, to some extent John [C.] Gille from NCAR [National Center for Atmospheric Research] who's also involved in the Upper Atmosphere Research Satellite. I may be being unfair to some people.

In any case, we realize, I think we're about to just go provide the vision for geostationary observations of the Earth to companion with the Polar Orbiting Earth Observing System we've come up with. It's funny, Berrien plays some sort of role, but we go off and have our first post-report meeting in Dallas, [Texas], like in the North Dallas Forty kind of area of town—the best hotel we ever got to stay in. We pretty much realize, we talk about geostationary, and that's nixed, and we go back to refining the Earth Observing System. Working groups are formed for the data system. Bob Chase chairs the data system group for us. He's now left NASA Headquarters. He'd been a Headquarters colleague. He's now gone to Woods Hole [Oceanographic Institute, Woods Hole, Massachusetts], he's running the Buoy Group at Woods Hole. He takes on the data system. Actually, initially Ray Arvidson chairs the data system, but at some point Ray gets overwhelmed between his duties to Planetary and his duties to EOS, he backs out, and the data system responsibilities are picked up by Bob Chase to chair that report, which is the first of our volume two reports that comes out. It's got a black cover, very sleek, same logo. Because volume one had a white cover, so the first part of volume two ends up with a black cover.

At any rate, we form that group, and then we start forming instrument groups. So there ends up being a group about MODIS [Moderate-resolution Imaging Spectrometer], a group about HIRIS [High Resolution Imaging Spectrometer], a group about LAWS [Laser Atmospheric Wind Sounder] , a group about one thing and another. So there's a whole bunch of working groups to refine what are sort of being thought of as facility instruments. The group keeps meeting. Now we're really wrestling with satellites, their size, a platform, how big is it, different payloads, different payload choices, and things begin to get real.

WRIGHT: Now, do you want to make this a stopping point, so when we pick up again we can talk about the working groups and how the working groups and the new reports came in?

BUTLER: That's probably a great thing. We've got probably three more hours to go based on how we're going.

WRIGHT: Okay, well, we know we're not doing it today, so why don't we stop.

BUTLER: Go ahead, that's great.

WRIGHT: Okay.

[End of interview]