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

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INTERVIEWED BY JENNIFER ROSS-NAZZAL
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ROSS-NAZZAL: Today is March 26th, 2010. This oral history is being conducted with Dr. Dixon Butler in Washington, DC. The interviewer is Jennifer Ross-Nazzal. This is the second interview with Dr. Butler as part of the Earth System Science at 20 Oral History Project, a project to gather experiences from those who've been involved in various efforts in the launch and evolution of Earth System Science. Dr. Butler serves as a professional staff member on the Commerce, Justice, Science Subcommittee for the Committee on Appropriations for the United States House of Representatives. Included as part of his responsibilities are the National Science Foundation [NSF] and climate change. Thank you for meeting with me again this morning. When we met in June of 2009 you shared with us how you first became involved in Earth System Science and how that interest led to a number of positions which led to working with NASA. We left off with your brief discussion of working groups and instrument groups. I thought we would pick up there this morning.

BUTLER: Great. I should add that since we last spoke, to my responsibilities has been added responsibility for the National Aeronautics and Space Administration, so I'm now responsible for NASA, NSF, Office of Science and Technology Policy, and climate change. When last we spoke, and reading back over the end of the transcript, I noted that I mentioned Bob Chase chairing the data panel. So let me go back and set the stage a little just to get things going. I believe we covered the first report basically laying out the Earth Observing System [EOS], largely as a set of five principles.

We made a feint at moving on to paying attention to geostationary, and then were pulled back from that. I don't know if I made it clear last time, but Berrien Moore [III] seemed to be the one that almost [single-handedly reined in our work to continue focusing on low-Earth orbit]. He belonged to both the Bretherton Committee and our committee. There was a three-person overlap between the Earth System Science Committee, the Bretherton Committee, and the EOS Science and Mission Requirements Working Group: Berrien Moore, John [A.] Dutton, who is a former president of the American Meteorological Society and at the time a dean at Penn State [Pennsylvania State University], and Paul [J.] Zinke. Paul Zinke had actually been added to the Bretherton Committee to add one more person from the EOS Working Group.

As we started into our volume 2 report, we began to really engage—and I think I said this last time—with the substantive issues of a mission. Take these principles, and now what is actually the mission you plan to fly? Or at least what are its science objectives, some sense of its scope? We had to get into far more specifics. We had to deal with a whole lot of issues. We had briefings on orbital mechanics and got comfortable with Sun-synchronous orbits and when they weren't appropriate and what measurements. A scope of measurements began to come out, which had actually been there before, but now it became more specific.

What fairly quickly happened was a recognition that we needed a working group on the data system, that that was going to be a critical aspect. That's exactly the same point of view that the leaders of the original System Z study [had] when I was its executive secretary. Pitt [G.] Thome, and the man who was on that group from what is now [NASA] Stennis [Space Center], from down in Bay Saint Louis [Mississippi] came in with the perception of how critical the data system was going to be. Frankly, I was blind to that, but that was there from the very beginning. They were wise. When we got to this point in formulating the Earth Observing System that impression was really clearly there.

Ray Arvidson, who was primarily involved in planetary science, was on the EOS committee. He was one of the main data system mavens within the space science community, and he agreed to chair the data system panel. Fairly quickly into that, unfortunately, the amount that Ray Arvidson was taking on between his considerable planetary community responsibilities, this EOS responsibility, and being a professor at Washington University in Saint Louis [Missouri] got to be just too much. He backed out of his EOS activities, and we understood completely. We felt a sense of loss but only because he was so good. That's when Bob Chase, who I referred to last time, stepped in and took over chairing that committee. I believe we published our volume 2 report—the main volume, not the instrument or data system-specific volumes—in about 1984.

The first of our working groups to report out was the one on the data system. They came out just one year later with the data system volume. We formed panels, working groups if you will, on a number of instruments. MODIS [Moderate-Resolution Imaging Spectrometer] and HIRIS [High Resolution Imaging Spectrometer] I mentioned last time. Synthetic aperture radar. There was certainly one on the Laser Atmospheric Wind Sounder [LAWS]. We ended up with groups of scientists led by other people, typically chaired by somebody. In several cases they were chaired by people at NASA Headquarters [Washington, DC]; sometimes they were chaired by people on the outside. In any case it quickly became clear that we had more scientists involved in planning the Earth Observing System than many NASA missions had involved during their execution phases. We were well over 100 scientists actively engaged in the planning of the Earth Observing System. Life was going along and we were making progress. We, in our thought, were shooting for something like a 1992 launch, and we were shooting for a 1988 new start. At the time we also had in mind two large polar platforms.

It also should be noted that a significant amount of my work during this period was participating in the planning of the Space Station [Freedom, later the International Space Station]. I don't know if I talked about that, but I had a nice experience in retrospect. There was a moment in the planning of the Space Station—and I'm going back a little earlier. Jim [James M.] Beggs had the contractors and the tiger teams and we were doing a requirements study. For whatever reason it was broadened to what did users need [in low-Earth orbit], and it didn't matter whether the users actually needed the astronauts or whether it was human spaceflight. What counted was what did you need in Earth orbit.

We thought we needed a large polar platform, preferably two large polar platforms, and two platforms got put into the Space Station Program. I was always having to fend off other science disciplines from trying to take them over. Eventually I did that and we had a wonderful rapport. I particularly developed a real rapport with Peggy [Margaret G.] Finarelli who was negotiating the International Space Station agreements on behalf of the United States, and had the delegated authority from the State Department to be the [US] negotiator as a NASA person. We got so Peggy and actually Lyn [D.] Wigbels, who I later worked with very closely at GLOBE [Global Learning and Observations to Benefit the Environment]—both of them—we had a good habit here, and Peggy would actually call me to check on what the issues were with respect to the platform and the Earth Observing System.

We began the international work as well, basically after we were off to the races. Ultimately I think we got one polar platform, and then there was going to be a platform that was going to co-orbit with the Station. Ultimately that idea went away, which is actually a shame. It would have been a very good idea for microgravity research. I think would have been more exciting because you could have had a lab over there where you could have achieved 10^{-6} g [gravity] requirements, which was what the microgravity community said they needed. An astronaut coughing in the Space Station rings at 10^{-5} g's through the structure. Those requirements have never been met by Space Station as far as I know.

Actually NASA is being asked a question for the record this year about that specific requirement and whether it is met at all by substabilizing some little part of the Station. My vision was we would see "Film at 11" with the astronaut going to work over there and coming back untethered with his or her backpack and changing out the samples. They would be typically 30-day runs. It seemed it'd be pretty dramatic and it would also have gotten a lot of public attention, but I digress.

Because of this element of the Space Station program, namely the polar platform, at the same time we formed an international working group, which I think I called IEOS, the International Earth Observing System group. It was the Space Station partners, which at that time did not include Russia. It was the United States [NASA], Japan, with what's now JAXA [Japan Aerospace Exploration Agency], the Canadians [Canadian Space Agency (CSA)] and the European Space Agency [ESA]. Then each of us brought with us our meteorology agencies. We had NOAA [National Oceanic and Atmospheric Administration], and EUMETSAT [European Organization for the Exploitation of Meteorological Satellites], which was brand-new and had never done anything so its secretary-general would actually come. The founding head of EUMETSAT would actually come to our meetings. We had Atmospheric Environment Service Canada. We had the Japanese Meteorological Agency, but in addition to the space agency—

which technically in Japan at the time was not a government agency, it was like this captive private entity. And remember, this was when Japan's economy was just roaring. Japan was a tiger in the 1980s, everybody was awed by them. The main government entity behind that was MITI, the Ministry of International Trade and Industry. They were at the table as well.

It made for wonderful times. Basically each of the four partners hosted a meeting every year, so I started traveling like mad. I would be going to Japan, Canada and Europe each once a year just for this purpose, plus we'd be hosting a meeting in the United States. Wonderful colleagues. A number of different ones. Just to throw out a few names, probably my favorite, and a man who came to the Earth System Science at 20 meeting, was Tasuku Tanaka. He was just wonderful, and I'll tell an anecdote or two in a little bit. Guy Duchossois and Bob [Burkhard] Pfeiffer represented the European Space Agency. Bob Pfeiffer had been the project manager for Spacelab, the European module for astronauts to work in the bay of the [Space] Shuttle. Even taller than me, [a] wonderful man [who] passed on unfortunately more than 15 years ago. Bob worked at ESTEC [European Space Research and Technology Centre**]** in Noordwijk in the Netherlands. Guy Duchossois was from ESA Headquarters. Various Canadian counterparts. Also the Japanese representatives changed. One of them was even taller than me, which is amazing when you're dealing with Japan, but it was a wonderful group of people. Particularly the Europeans made real commonality.

The first meeting was just the ESA Earth observations [person], namely Guy—they had not added Bob Pfeiffer in yet—and myself. We met in what amounted to almost a closet size little suboffice right beside Shelby [G.] Tilford's office at NASA Headquarters in the old building, in FOB [Federal Office Building]-10B on the second floor. It was just pitiful; no windows, tiny tiny place. We had our first meeting there, and then we produced minutes. We decided to bring in the other groups, the other two partners. We decided to bring in our meteorology agency counterparts, and pretty soon we had something going on. In the early days I didn't have support staff.

I remember we hosted a meeting; it must have been the second or third meeting at the Jet Propulsion Laboratory [(JPL) Pasadena, California] or somewhere in California. I didn't [get] jet lag, I mean I didn't time-phase change. I just stayed on East Coast time in my body. So we met, we went out to dinner, got back to the hotel relatively early. For me it was 11:00 at night. I went to sleep, but of course I woke up. I'm pretty much a morning person and an early riser.

Here is something I think is really important. I had a Hewlett-Packard portable computer. It weighed about eight pounds, and had only a black-and-white screen. I also had a portable dot matrix printer and a portable external disk drive. There were not USB [universal serial bus] ports, but there were other things I could use to hook it onto the computer. I traveled with these and did email. We had written the EOS report, and my composition of the EOS report had been written on this portable computer. Well, you wake up early in the morning, I've got the minutes from the day before to prepare. So I prepared the minutes of the previous day, printed them out, got them copied, and when we started meeting in the morning, like at 9:00 in the morning, a copy of the previous day's minutes [was] at the seat of everyone at the table. That's a pretty good way to run a meeting, and that became a precedent that was expected.

It's important I think to mention it as much as anything because Stan [W. Stanley] Wilson's visionary adaptation for the oceans community of email and networking—in a very modern sense he hired a private contractor, which was some people who had been in the oceans community, and they set up basically a limited network connection. There were portable computers, and we had email, and Stan graciously extended it to the EOS planning people. It eventually of course spread and spread and spread. And with EOS money to help pay the bill, the oceanography program was paying for this service. That really was instrumental in pulling the EOS working groups together and having reports come to life, and it also played into this international group and its written record.

There was this really incredible and effective woman, who was really an international affairs specialist, named Lisa [R.] Shaffer working at NOAA. I don't know exactly what was going on, but she came to leave NOAA and we hired her into the support contract for Earth science at NASA Headquarters. Then Shelby gave me that support contract to give me arms and legs to get things done. Not exclusively, but I got a lot of people, of whom Lisa was certainly one. Martha [E.] Maiden, who to this day is responsible for EOSDIS [Earth Observing System Data Information System], came through that path. A number of people came to Headquarters actually out of that group. [Also, Peter W. Backlund.] Lisa rejoined the civil service—ultimately moved up the line into a senior executive position at NASA Headquarters in international affairs, and for a while was my deputy. Wonderful things happened, but Lisa took on the job of being my right-hand person and took over all those minutes and notes and had wonderful people. I remember Mary [L.] Blazek helping her. So a good group came together. These international meetings became a pretty big deal. They became quite formal.

So I've got multiple balls in the air here. I've got this international group, we've got all these studies going on domestically, then of course one is doing other things like [managing] two research programs. We also thought we were going to have a serviceable polar platform. The overall group didn't finish its report until '87, but we began looking at polar platforms, we were looking at instruments. One of the polar platforms was clearly going to be dominated by the synthetic aperture radar [SAR]. JPL kept trying to oversell what that synthetic aperture radar should be. They wanted something that was not only a trifrequency quadpolarization at least in two of the frequencies, which would have been just at the limit and was ultimately done as a Shuttle instrument, but they also wanted that to have squint mode, which complicated the mounting to the platform and made the whole thing just go crazy. They never backed off, and as a consequence ultimately they lost. Their platform was canceled. Synthetic aperture radar was canceled.

Internationally—I pretty much built the case. Everybody wanted to fly their own SAR, so I kept us out of it after that. Europe had a SAR and Canada had a SAR—which was done in RADARSAT [pair of remote sensing satellites] in partnership with NASA providing the launching, getting access to data—the Japanese flew a SAR. It was enough SARs. A variety of other things happened, but we began pulling together these instruments and these platforms and orbits. We settled on some things. We came to settle on 705-kilometer Sun-synchronous orbit.

Let me try and pick out some themes going through this time period, but recognize they're going in parallel. You've got the Bretherton Committee. The members of the Bretherton Committee are far more august scientists of great reputation than most of the people on the EOS group. We're a "young Turks" kind of group. I myself am young. A number of the members of the committee had not yet made their reputations. Many of them went on to full professorships and deanships, which they've now already finished. Mark [R.] Abbott for instance was probably the youngest of us, and he's now on the National Science Board, which is a pretty august position. He's dean at Oregon State [University, Corvallis].

So there's the really august Bretherton Committee. They're wrestling with some big problems and ultimately produced their report. There are two things I guess I will say there. One of them is that they were real good at an intellectual construct about the science, [but] they had no way to get to a substantive mission plan. Nice overarching philosophical—brilliantly done in many ways, and made Earth System Science. But the EOS had started sooner. We were where the rubber met the road. We were basically putting some flesh on this idea; some actual, what would become hardware and a data system, and a set of activities, and a set of ultimately intellectual investigations that would realize real progress in Earth System Science. One thing we got from the Bretherton Committee as they wrestled and they dealt with multiple agencies was that on these polar platforms we should do both research and operations. In other words, we should capture and subsume the NOAA operational mission. That had a lot of effects, and maybe this is as good a time as any to run through them.

NOAA was flying imager and sounder typically. We were beginning to move toward an ozone sensor. There's a whole suite of things though that you should observe beyond just the atmospheric sounder and a cloud imager, but it was relatively early days. They also flew two other things. One was a data relay, a data collection system, namely where like a buoy could pop to the service and dump its data up to a NOAA satellite as it came over. We didn't have all these [commercial] communication satellites sitting there to do that. It wasn't like "ET phone home" the way it is now. A successor to that system is still part of the operational payload of NOAA.

They had search and rescue. That continues to this day. Today even, NOAA in its "what's NOAA done this week" will have ,"Oh, we rescued somebody." Somebody triggered their search and rescue beacon, and they got rescued. There certainly are other solutions today in my opinion, but that still goes on and it still saves lives. A lot of those lives are Americans, even though the US probably invests the least money, but we do fly them on our NOAA birds [aircraft].

The reason I mention those two particular instruments is because they are enormously hard in terms of satellite requirements. Radio frequency interference problems with the data collection system are a nightmare for satellite systems engineers. They just make a mess. Also you don't need them with the other things. They have no reason to fly [with the instruments]. It is a flight of convenience. You're flying somebody for convenience and they're a pain; you should look for other solutions. I actually did that. I went on a visit to Brazil, talking about the Earth Observing System. I became impressed with the fact that they were ready to build their own satellite. It seemed to me maybe we should do something with them.

I came back proposing that we let them build the satellite to carry the data collection system and the search and rescue system. Oh boy, had I put my foot in it. People, largely a group led by people at NOAA, just came unglued. "The United States is the beneficiary, now we won't be doing anything since all we do is fly these instruments for this multination system." I got pretty hardly squashed. So that went away, but I would submit to you that is an idea that its time may have come. Not that the Brazilians will do it, but that NOAA will be looking for an independent free flier for those two systems. I don't think they're going to go on the Joint Polar Satellite System, which is the successor to NPOESS [National Polar-orbiting Operational Environmental Satellite System**]** for the afternoon orbit that NOAA is going to do.

Bretherton Committee had us do that, and several other things happened from that. One is we tried to accommodate the NOAA instruments in our polar platforms. We tried to upgrade them as well. Then there was the issue of do we fly the upgrade and the reliable next to each other? A key thing that happened as we were moving toward getting an EOS new start. I mentioned that the SAR had gone away, but we still had a large polar platform.

Bill [William F.] Townsend, subsequently in many high-level positions like Deputy Associate Administrator for Earth Science and Deputy Center Director at [NASA] Goddard [Space Flight Center, Greenbelt, Maryland], a wonderful individual, was the program manager for the TOPEX/Poseidon [Ocean Topography Experiment] mission. Ironically the successors to TOPEX/Poseidon are called Jason, which of course works with Jason and the Argonauts, but it also works with Bill Townsend's son's name Jason.

The only time in my experience at NASA Headquarters when you did a new start presentation on a science mission to the Administrator, that it was not done by the program scientist, was TOPEX/Poseidon. As the engineer, Bill Townsend knew the science well enough that he could stand up and defend that to the Administrator and did the briefing for its 1982 new start. Impressive. I always liked Bill, I still do. My hat remains off to him for that, but that I think really sets an ideal. If you can be a program manager at NASA Headquarters and understand the science of what you're trying to achieve well enough—not that you're a scientist, but that you understand their requirements so well that you can actually articulate them and make a public case for them, that's pretty good. You got to understand all the hardware stuff, but that's just pretty amazing.

Bill and I tended to work together. He said to me, "Dixon, there's too much risk in this mission." He cited several things. What I'll talk about right now was this business of having the operations and the research on the same bird. We all knew that if you've got research and operations flying and there's a contest for resources, you must give the resources to the operation. You cannot save the resources for science, so the research has the potential to be undermined by being flown on the same bird as operational instruments. It's just a fact of life, and everybody would make the decision that way. There were also risks. There were risks from the multiagency part of this, although NASA builds stuff for NOAA—that's about to actually be reestablished completely, that NOAA hires NASA to do its satellite procurements and get things developed and launched and then [NOAA] takes over operations.

We needed to get NOAA off, and we did manage to do it. My memory is in roughly 1988 or 1989 the opportunity came to pull the operational instruments back off and let NOAA go its own way. That was good, and that happened in the Earth Observing System. But because we had thought so much about the operations, and also because the operational data are critical to Earth System Science—they are the long-term continuous datasets in many cases, preserved because they're flown by an agency whose priority is continuous observation, as opposed to new technology and interesting research and all this breakthrough stuff.

NASA had abandoned the Operational Satellite Improvement Program in the late '70s. It had been a key part of the Office of Applications. In environmental observations it had been a key thing. In those days NOAA would come over and have these meetings once a month with us, and they clearly were ruling the roost. Very maturely, but very aggressively presenting things. Both Shelby Tilford and Ron Greenwood did not like this. They basically put an end to the Operational Satellite Improvement Program and it went away. We stopped improving NOAA satellites nominally.

I understood but I didn't agree. I didn't disagree in public ever, but it just rankled—because I came to really believe in this operational mission as well. As we began to design the Earth Observing System we were clearly looking at improvements. We looked at an AIRS [Atmospheric Infrared Sounder], which was supposed to be the next generation operational sounder to fly in polar orbit for NOAA after we flew it and showed [it could do the job]. One thing and another. We basically took on improvements to the AVHRR [Advanced Very High

Resolution Radiometer], [i.e.], MODIS, which of course broadened the science enormously, but would have been great as an operational instrument. We took on those instruments. We took on things like TOPEX/Poseidon. It showed that you could do radar altimetry to get at circulation in the ocean and sea level to an incredible precision. And you need to keep making those measurements; it's not something you do once and go away or do for five years and go away.

We always had an EOS altimeter. Stan Wilson had taught me as an advance planner the five instruments we need for oceanography, and the Earth Observing System had them all. Scatterometry for sea surface winds, passive microwave imagery, the synthetic aperture radar for sea ice, and you needed the ocean color and also the infrared sea surface temperature from a thermal infrared imager. MODIS embraced all of that. Pretty amazing. I said what happened to the SAR, but basically the deal with the Canadians got us a sea ice SAR.

NOAA wasn't going to [take] over the scatterometer. Still has not gotten the money to begin to develop an operational scatterometer 20 years later, but we put scatterometry in the Earth Observing System to make sure we got to fly it. We put in the altimeter, we had the passive microwave. We had it all. We made sure that we were doing the next generation operational instruments. It's pretty amazing.

The other thing we did in formulating EOS is we came up with basically wanting a 15-year dataset. The reason was because once you launched, you needed several years of a dataset to get the algorithms right, and then to show that it was worth flying it. At that point in time it was taking 10 years to get a new mission launched from the time you started the studies till the time you got launched. So you needed a 15-year mission because by the time you could make the case to start the studies to get to the launch you needed to be up for 15 years to have 5 years to make your case and 10 years to build a successor mission. That ultimately came down to EOS being designed with three copies of every satellite and three copies of every instrument.

But there's another thing that does. Let's say after five years the government wakes up, the science community wakes up, the operational community wakes up, and you understand that this is an instrument that is now ready to go operationally. What have you got? You've got copies. You've got data continuity. You've got time to build the subsequent copies into the budget of NOAA or, for that matter in the case of land observations, the US Geological Survey.

You can build into their budget, which is a big deal, because putting satellites into their budgets rocks the boat. To pick an example, the 2011 budget for NOAA has an increase of over $800 million over last year, and that's like going from $4.6 billion to [over] $5.4 billion. It's a huge increase. It is almost entirely caused by the need to fund the satellites. It's the growth in satellite costs that drive that big increase. It has the potential at any given moment to swamp the agency. NOAA is used to it. US Geological Survey, believe me, is very apprehensive about having to take on an operational Landsat [Earth observing satellite program] responsibility, which would be $250 million extra that they would need every year, and with a cost overrun could turn into $500 million, which is a very big deal for them. Strategy was you'd have the time to get ready, and if it was ready to go operational you could transfer it to this basically recognized permanent need for the observations.

Well, didn't work out exactly like that for several reasons. One of them was that when we got to 1998—and this is past the time I was actually involved, but I've been told this on good authority—Administrator Dan [Daniel S.] Goldin didn't want the permanent lien of having to keep paying for the Earth Observing System's flight hardware through all three copies. Jim [D. James] Baker, as Administrator of NOAA and Under Secretary of Commerce for Oceans and

Atmosphere, wanted to take on the climate role so they cut a deal and dropped the second and third copies of all the Earth Observing System satellites and instruments.

It was a monumentally stupid mistake. It has jeopardized the Earth Observing Systems of the United States. To be fair, I think Jim Baker thought he would have a chance to get the money for NOAA to do this job, but Al [Albert A.] Gore didn't win [2000 presidential election]. The chance was never there, and NOAA never was able to build the resource in its budget at that time. So what it meant was the first copies of the Earth Observing System instruments and satellites were all we had.

The good news is they mostly have all continued to work well beyond their five-year design lifetimes. In some cases, I think at least the first, the Terra satellite and its instruments are at the ten-year point and still working more or less fine. The Aqua satellite is well beyond its five years. NOAA uses MODIS for operational imaging.

If there were a failure in the NOAA polar orbiting satellites that are up today, they'd only have MODIS. For ocean color they do only have MODIS. It just goes on and on. QuikSCAT [Quick Scatterometer] was basically the successor to our first effort of taking the EOS scatterometer and flying it in partnership with the Japanese. The Japanese satellite didn't last very long, we clearly knew we needed scatterometer data; a QuikSCAT was put together and flown again. That QuikSCAT worked until just a few months ago. Now despite the direction from the Congress for them to start a study or propose to start a study in the 2011 budget, the administration didn't do that, although they did almost everything else you could hope for [with respect to Earth observations].

Scatterometer is probably about to become an operational instrument, just 20 years later. NOAA's 2010 budget has in it the money for the altimeter study to begin. TOPEX/Poseidon was always done in partnership with the French. Now that's going to become done in partnership with EUMETSAT in Europe, [and] Jason-3 will be an operational altimeter for NOAA.

That vision is working out now, just after some high-risk in-between stuff, and I think will carry forward actually. It's really quite powerful. And it's also true that if you go back to the 1980s and the EOS planning, NASA could fully embrace the climate objective in Earth System Science. We know that not just climate change, but global environmental change was the concern. In some cases we didn't even have a decent baseline, particularly in terrestrial ecology. We knew we needed this comprehensive set of observations, so we could think about it in those terms. NOAA was much more having to think about weather—operational, forecast-type products. It could be some ocean-related products, but they were still having to think in those terms. We could be the visionary future.

Now what you see is NOAA reaching finally for what Jim Baker wanted in 1998, namely the climate mission. Doesn't quite have it. They just reorganized themselves to form a NOAA Climate Service. They're embracing climate as something that is a legitimate responsibility and that they will need to deliver climate products, not just weather products and ocean products, so things have actually come in a big circle. That's the operational theme coming through this. And again the Bretherton Committee's vision was sound. Jim Baker's aspirations were sound, but the implementation in both cases, a lot of problems.

Now to come to the EOS more particularly. We kept meeting, we had all these instrument panels. The instrument panels would report into the overall Science and Mission Requirements Working Group. It was an amazingly heady time. As I said before, we had so many people working on it. Again, we were shooting for a 1988 new start and a 1992 launch.

Then, as I was sitting at NASA Headquarters on the seventh floor of the old building in a meeting with NOAA, the [Space Shuttle] *Challenger* [STS 5l-L] accident happened. God. Somebody just stuck their head in the meeting room door and said the *Challenger* has blown up. I kept talking, because it just was such a bolt out of the blue that my mind just couldn't even internalize it and my mouth just kept running. After about 45 seconds I realized, "Oh my God." The NOAA guys quietly looked at one another and—it's kind of a death in the family. They got out because they weren't NASA. We just walked over to the Administrator's auditorium and stuck our heads in. What a sadness.

I like to say I didn't have a gray hair in my beard until—and six months later there was gray all over my beard because what this did to the Earth Observing System. We had a sense of imperative, because we could see that environmental change was happening, and we knew we needed to go understand it better, and we knew we needed to get our hands around it, and we knew that eventually people were going to need to make informed policy decisions based on what we were doing. People in the Earth Observing System, on all our working groups, all the people at NASA—we woke up and knew we were engaged in trying to save the Earth. It's a little bit of hubris, but it was very motivated. It provided a really nice selfless arc.

All of a sudden we were faced with multiyear delays. I was already working on a solicitation for the science investigations for the Earth Observing System. Oh boy, that got put on hold. We basically walked into what turned out to be a three-year delay, at least initially. What was supposed to be a 1988 new start became a 1991 new start. We did get serious study money in fiscal years '89 and '90 to get ready for that new start. The Earth Observing System was so big, you couldn't just squeeze the money out of the base research program to pay for the Phase A, Phase B work. So we got money, serious money.

During the earlier study phase—I should mention that HIRIS was basically an instrument intended to fly in the Shuttle bay and had an $8 million a year budget. Alex [Alexander J.] Tuyahov and I went to Shelby and tried to get control of that budget because now HIRIS was really just an EOS instrument. Shelby wouldn't do that, but Shelby did give us about $3 million. We ran the EOS studies on about $3 million a year until fiscal year '89. We probably also got a lot of collateral civil service time out of scientists [and engineers] in the project at Goddard, etc. Had to pay for the time at JPL though.

Challenger really forced a lot of extra time. We put that time to pretty good use. We refined the instrument concepts. Jeff [Jeffrey D.] Rosendhal was the chief scientist of the Office of Space Science and Applications, and he put me on the steering committee which reviewed AOs [Administrative Operations] that came out from the different divisions. In those days they were done one at a time. You had to go and pass muster and defend it to this broad group that had the planetary guys and the astrophysics and the heliospheric physics guys and the indoor sports guys in microgravity and life sciences people. You had to walk in and defend.

I got to be on the group that had to be defended to, so I learned the process. I think I reworked that announcement of opportunity and spent two years on the final version. At the time it was the largest scientific solicitation the agency had ever undertaken, so it's worth saying something about. When it came out, it came with what Berrien Moore called the cube, because you got the reports of all the working groups that had come out for each of the instruments, the data system, volume 1, volume 1 annex, volume 2, and then all the volume 2 instrument and data system pieces. It was a stack of enamelized paper that I think was six inches apart. Unbeknownst to me, but people told me about it, several universities began using it as a textbook or a virtual textbook to teach remote sensing. It was incredible stuff. It was the state of the art, written down on paper with beautiful, enamel paper with lots of color pictures. It was pretty incredible.

That formed the basis on which we solicited. Decisions were made that some of the instruments would be facility instruments. MODIS was going to be a facility instrument, HIRIS was to be a facility instrument. There were several others. The Laser Atmospheric Wind Sounder was going to be a facility instrument. [Synthetic Aperture Radar was another.] Then our international negotiations had gotten us to have a Japanese passive microwave, so we made that a facility instrument. There was a Japanese imager [ASTER (Advanced Spaceborne Thermal Emission and Reflection Radiometer)]. Ultimately since HIRIS got canceled, that became the only one, so we solicited for a US team to work with the Japanese. We didn't solicit on behalf of Japan, they solicited their own team. I mentioned the international working group. One of its real accomplishments was we coordinated our solicitations.

ESA went after building its polar platforms. Japan thought we were going slow—they jumped the gun ahead of us and went forward with ADEOS [Advanced Earth Observing Satellite]. I always viewed it as meaning advanced EOS because they went forward first. They picked up the scatterometer from us. We solicited the scatterometer team I believe. They did a passive microwave, and they provided us with this imager which had visible, near infrared and thermal infrared bands. Not as many as we wanted in HIRIS by far, but more than Landsat and a bit sharper spatial resolution than Landsat. It was an interesting concept. Three separate optics and detector sets. One common set of electronics, including power. It flew on Terra. It's on there to this day and I believe still cranking out the data.

In the solicitation we had facility instruments for which we were selecting science teams and science team leader. We were soliciting other instruments, and we solicited what were basically theory and modeling, data analysis type investigations. The so-called interdisciplinary investigations. That's worth a specific discussion. In roughly 1980, the National Academy of Sciences recognized that a bad situation had happened in space physics. Not so much solar physics, but in the rest of solar-terrestrial. There were lots of missions that had flown and acquired data, but the field was totally dominated by the instrument principal investigators for these missions. They had a pet theorist in the group typically, but there was never enough emphasis on the theory to build the constructs to intellectually put all this together. And the hardware guys kept wanting to fly hardware. Like what was happening in oceanography at the same time, everybody wanted to go to sea and get the data, but nobody wanted to spend a lot of time on land analyzing. And that was happening here in space physics. The data had to be put in the data center, but by the time it got there it was usually way late. So the National Academy of Sciences proposed that a Solar-Terrestrial Theory Program be initiated to give significant funding grants for theoretical research, modeling, data analysis type research in solar-terrestrial physics.

As I think I mentioned before, I was given the Solar-Terrestrial Theory Program to manage. Len [Lennard A.] Fisk was the second best-funded investigator in the program. Len and I understood that. It became the model for the EOS interdisciplinary investigations, because we recognized to knit the global environmental problem together we were going to need these discipline-crossing, integrated [studies] really well funded. When I ran the Solar-Terrestrial Theory Program, we had $3 million, maybe $3.5 million spread across 14 groups. The largest one got $360,000 a year. Len got $300,000. Some of them only got $150,000, which is barely enough to do this kind of work. But we were staring at bigger funding levels than that in individual groups, so we went out and included them.

Here, just to complete the thought about the interdisciplinary investigations—that was my area. I was the manager in the stratosphere for theory and data analysis. I was a person who did modeling. That was my home, but what I didn't understand is how popular this would be. Ultimately we ran multiple panels to select these investigations. I thought we could pick ten US-funded ones and maybe five internationally funded ones and have fifteen [total]. When it came time to do the selection, basically the entirety of the Earth observations capability at NASA Headquarters—all the scientists who were there managing, whether they were coming in as two-year rotators from a university or permanent staff—they all came in. We had a meeting that I chaired to decide what we were going to recommend based on all the reviews we'd gotten from the peers. What was going to be the selection for the Earth Observing System?

In the instruments I had a sense of the scope and maybe there'd be a few solar-terrestrial instruments that we'd include, but we knew what instruments to fly. We knew what the science teams were, because we'd actually specified. So we knew how to get science teams; we knew we were going to have one science team per facility instrument. But my colleagues pressed really hard for more interdisciplinary investigations. More than I thought we could sell, but it was just overwhelming. There was no way I was going to prevail.

So at the end of the day we picked not ten but twenty completely US-funded interdisciplinary investigations, nine international interdisciplinary investigations—and for two of the international investigations we picked US co-PIs [principal investigators] and US science teams that were proposed in concert with them. In some of these cases we put groups together. So in effect we accepted more than 29 proposals. That was also true in instruments to some extent, some instrument proposals. Particularly, the HIRDLS **[**High Resolution Dynamics Limb Sounder] instrument ended up with a European PI and John [C.] Gille as a US co-PI. That was a way of partly offloading onto the Brits [British] the bill for building the instrument, but we both contributed.

It was amazingly complicated. I remember I spent eight weeks at the Greenbelt [Maryland] Marriott [hotel]. There was a woman who was relatively far along in her first pregnancy who was one of the support contractors. She would actually sleep four nights a week in the room with the Xerox machine. We ran the Xerox machine into the ground, because in those days it was not electronically transmitted, this was paper. Notebooks. I think the instrument people had to review—I can't remember—but it was a heavy burden. At the end of the eight weeks I was told that the Xerox service people came out to service the machine, took one look at it and said you've done 750,000 copies in eight weeks, we're taking this machine to the scrap heap. It wasn't worth trying to fix it. It wasn't worth trying to keep it, but it worked like a charm for those eight weeks. And this woman would literally sleep there, I believe Monday through Thursday nights, and if she woke up in the middle of the night throw another Xerox run in. Because notebooks, they were so well organized. The first time I ever really heard of database management was they had this bright young man who was a database guru and he did a database. That was the only way we kept it straight.

We had review panels, often two at a time running, to do all of the facility instruments. We had to do the instrument proposals, we had to do the facility instrument teams, the interdisciplinary—it just went on and on. I think if memory serves there were 14 review panels. Never more than two at a time. In that eight weeks I almost vanished from NASA Headquarters. It was psychologically a really stressful time to me. I was upset about a lot of stuff at work, and I just melted off into that.

I would have to say it worked. It was a pretty phenomenal thing. Jeff Rosendhal had made sure I'd learned my lessons. We'd built them into the text with this wonderful support from the support contractor. They won a group achievement award, several of the people won individual awards—we were so pleased with them it was just incredible. My peers across Space Science and Applications on that oversight steering committee bought it. They bought the selection. I think I had to get up twice; I had to defend it and then I had to go back and refine the defense for certain things—but we bought it. We selected the instruments, we selected all those interdisciplinary investigations, we selected all these teams.

And then the meeting was over. Everybody just left. It was an incredible feeling of loneliness. It was really appalling. It was like oh, great, the most incredible solicitation for science in the history of the space program, and everybody just walked out of the room. We didn't go have a party. Nobody patted me on the back. It was just done. "Next." It still blows my mind to think about that. Actually a lot of things began to feel that way over the coming half dozen years.

We talked about the NOAA theme—actually some wonderful side stories in there. One of the things Bill Townsend said we needed to simplify was we had a large polar platform at Goddard and a large polar platform at JPL. We were going into the new start reviews for the Earth Observing System. Bill quite rightly thought that this was not a very good workable management structure for a project because you had basically two potentially competing project offices, one at Goddard and one at JPL. And the data system [was] at Goddard.

Barbara [A.] Mikulski was elected as the senator for Maryland, which she is to this day. She was the first candidate supported by what has become a well-known big thing, namely EMILY's List [Early Money Is Like Yeast, political action committee]. Its money basically allowed her to beat the congressman from Montgomery County and get the nomination for the Senate. She's been the senator ever since and has overseen NASA budget much of the time since.

She went to the Jet Propulsion Laboratory for a visit. Even though he was former Air Force Chief of Staff—[Lew Allen, Jr.] was the director—he forgot what you do when a senator comes to visit your Center. Namely that's job one, that's job only. He sent a second-echelon person to show the senator around. I was told Mikulski was not happy, was pretty much insulted and correctly so.

That enabled us to drop the JPL platform. I mean it provided a political space to let us do it; we knew we needed to do it anyway. JPL had been screwing around with the SAR. Why did HIRIS get canceled basically? Because JPL's project manager had done a crappy job. They had all the money in the world, but they got fascinated with the technology just like they did on the SAR. "Oh, I can make the technology better." The better became the enemy of the good, and the good lost. No HIRIS on EOS and no SAR on US EOS stuff. That's a lesson that's worth having in the history of NASA. You can really screw up, and JPL did. That and getting the operational stuff offloaded really reduced the number of the risk items. [Bill] and I together were able to check them off and get them done.

There's some other little sidebars of interest in here. The Japanese Advanced Microwave [Scanning] Radiometer [AMSR]. We're doing this stuff internationally. We're coordinating our selections and we're coordinating our plans and what we plan to fly, but quite honestly there's a fair amount of redundancy or quasiredundancy. We relied on the passive microwave of Japan. Japan gave us what ultimately was the substitute for HIRIS, we gave them the scatterometer. The Japanese relationship worked really well. Europe was more into "we're going to fly our own." Nobody was willing to back off flying a SAR except us, we fell into it. So if you look at the European counterparts to the Earth Observing System you will see a lot of similar scientific objectives, but generally the gold standard for most of the instruments are the US instruments that have flown. The other instruments, the data is good, the data is useful. In some cases it's quite creative, but that's how it is.

Italy, particularly a colonel in the Italian military who was a key representative to the European Space Agency, really wanted them to provide the passive microwave. We were having our international meeting in Japan. The Japanese were proposing a push broom passive microwave, meaning it was a fixed, big antenna [that] just totally dominated every drawing of the satellite platform. It dominated the drawing, almost as much as a SAR did the other platform drawings. I think it was going to be four meters from side to side—it's a big thing, AMSR.

Tom [Thomas T.] Wilheit and other passive microwave experts in the United States came to me and said, "Dixon, you've got to convince the Japanese to go to a rotating antenna that produces a conical scan." You can never calibrate a push broom passive microwave adequately for this purpose. Conical scan allows you some of the time to look at a calibration source. What a mess—because calibration is a critical issue. You go back to the 1980s, what the operational payloads lacked was calibration. All they had to do was give you a good weather set of data or good ocean set of data for an immediate forecast. They weren't building, at least [not] consciously, 30-year records, 50-year records, 100-year records.

NASA knew that, as we had for ozone depletion and ozone monitoring, we needed everything done so you had an intercomparable record that would extend indefinitely. You needed overlap of instruments in orbit. You needed calibration sources on board, preferably other calibration against natural targets of various kinds. To this day there are maneuvers that the Earth Observing System does so that MODIS can see the Moon. They basically do as much twisting of the satellite around its normal position as they can so they can get a look and take data on something that's pretty undisturbed, namely the lunar surface. Gives you a nice bright source to look at. I believe they [also] look at White Sands, New Mexico, which is just unbelievable if the clouds aren't there. Just as bright a white as there is on the surface of the land.

I go back to Japan with this fixed idea. I knew that in Japan you have to provide time, but I forgot. I pressed them real hard to switch their design and they couldn't do it. We had a wonderful meeting, [but] at the end of the day I couldn't get anywhere. Went back into the overall group meeting the next day and said well, we can't promise to fly the Japanese passive microwave. And the Europeans reported that back. All of a sudden I had a European offer, to keep Italy happy, of a passive microwave.

But Marty [Martin J.] Donohoe was the instrument manager for EOS during the study phase. Kept working with the Japanese, kept them in the loop. Japanese didn't go away. They didn't go away at all, they just waited patiently. Eventually the Italian offer fell apart. Japanese offer was there and they'd switched to a conical-scanning passive microwave. In the plans of NOAA just now coming together, the Japanese AMSR is the passive microwave operational instrument of the future. That will be what NOAA relies on. It already does for passive microwave, but that will take over from the passive microwave they've been relying upon from the Defense Department. They'll keep using the Defense data, too, as long as it's there, but they'll use this Japanese instrument as the primary microwave instrument for feeding the civilian operational data stream.

What I needed to do was give the Japanese to the next meeting to give me an answer, but by stupidly asking for an answer right then, I got the only answer they could give me that night, which was no. Whereas if I'd given them to the next meeting, another three months, the answer would have been, of course, yes, because they went and changed their design. They understood; they liked our advice, they really recognized us as being way ahead of them in remote sensing instrumentation and remote sensing satellites. And they were really determined to get in the game but they knew to listen and they respected what was said. At any rate it all worked out, but thank goodness Italy didn't provide two passive microwaves. They provided one to the Europeans to fly but not one to fly with us. Boy, that was an amazing thing.

There did come a time—a sad thing happened for me. Up through the selection Dick [Richard E.] Hartle had been project scientist at Goddard, and he and Alex Tuyahov pretty much functioned like silent partners. There was no way I could have done this without them. I'd actually come to Goddard to be Dick's postdoc [postdoctoral researcher], with the agreement that I wouldn't actually work on the research I would have done with Dick, and went and did other stuff, namely stratospheric ozone depletion modeling. Now Dick was my right-hand guy. It was just amazing. It was a gracious, selfless, wonderful contribution. It wasn't his field, and yet he got immersed in it.

It came time to have a project scientist once we had a new start, and [Shelby Tilford] gave the job to Jerry [Gerald A.] Soffen. Admittedly Jerry Soffen had been a Headquarters division director, very senior guy. He had been the project scientist for the Viking Mars mission at [NASA] Langley Research Center [Hampton, Virginia]; an interesting man. Dick got to be the deputy project scientist, but they didn't give him the top job. I, in retrospect, have always felt guilty about that I somehow didn't have the ability to go to Shelby and make that different, but that was what happened.

We got to our first EOS Investigators Group meeting, and I began to realize that I was beginning to lose some of the control of what was going on. They'd set up the room in a way that I thought was completely wrong. They'd set up the room with the interdisciplinary investigators, basically the 29 principal investigators plus the two extra American co-principal investigators, around a U-shaped table. The instrument people were back like they were going to just sit in the audience. It was as if they were the board judging the Earth Observing System. As the interdisciplinary investigators, they were portrayed as the successors to the planning committee in making the science recommendations and judgments. It ended up working that way, but boy, I thought that was just totally wrong. I felt that the decisions needed to involve the instrument guys, that they were all PIs. The team leaders of the facility instrument teams also represented those teams. We needed them. They were all equal, and it never felt quite that way ever again.

What quickly happened is we had to descope the mission some. We had selected too much stuff, the prices were getting out of hand. When we got our new start the 10-year projected budget was $17 billion. We had in the selection some pretty good instrument ideas come out of the blue. One of them was MISR [Multiangle Imaging SpectroRadiometer]. MISR basically went after the angle at which sunlight is reflected off the surface of the Earth and vegetation and other things. It had nine identical imaging instruments, each one a strip of detectors, all flying in a push broom. So think of detectors across the satellite orbit, but nine different angles, each with its own hard-mounted optics. One's pointed let's say 60 degrees in advance and then 45 and then 30, and there's another one in there, one pure nadir—then the same angles repeated going backwards. So as you fly over—and it's some time because you're looking out ahead pretty far and behind pretty far. Even at seven kilometers a second over the ground, you're seeing roughly 3,000 kilometers apart. It takes a while to go by 3,000 kilometers.

There are little changes in the clouds, but you get a close to instant intercomparable reading at all these different angles. Of course the Earth rotates slightly, so what the front one sees isn't exactly what the rear one sees unless you've staggered the angles just right of each camera. I don't remember if they did that or not. In any case this instrument came out. JPL principal investigator as I remember, really lovely instrument proposal. Bob [Robert E.] Murphy, to his credit, who had run the land group before we'd reorganized Headquarters, basically advocated for it and we picked it. It became the MISR instrument. And I think it's still working. Nobody has ever come forward to say we needed to continue to do that, interestingly enough. It was a pretty visionary thing.

I think we got proposals to do laser ranging to the surface that we ultimately dropped, but that has turned into other missions. We had the Laser Atmospheric Wind Sounder and ultimately that just blew up in our faces. Technologically we weren't ready to do it. The price estimate for the instrument was $200 million for the first copy and growing, and it got lopped off. It also had a significant impact in power demands on the satellite itself. To this day we have not flown a laser wind scanner—20 years later, which is a real negative reflection on US technological development in the interim. We have flown laser rangers, things that just basically bounce a signal and look at what comes back, but this was going to have to look at the Doppler shift in the reflection. It would reflect off of aerosols, little particles floating presumably with the wind. So the Doppler shift in the light reflection from those particles would tell you the instantaneous wind speed and you would also range so you would get the wind speed in a whole stack, at least through only slightly dusty air. You would get a stack maybe even all the way to the surface of wind velocities. That went away.

We also picked an instrument that flies on the Aura satellite called TES [Tropospheric Emission Spectrometer]. It was both a nadir- and a limb-looking instrument, but it was using Fourier transform spectroscopy to get at chemical composition. Looking down and looking sideways at the limb of the Earth. Limb stuff had been done a lot; nadir stuff had been done only a little. We also picked an instrument MOPITT [Measurements of Pollution in the Troposphere], Canadian-led instrument. There was actually a group that I'd offered to Joe [R. Joseph] McNeal to make what became MOPITT, a nadir-looking methane and carbon monoxide monitoring instrument. There was a group at Langley Research Center developing it for the Shuttle and flew it on the Shuttle. At the end of the day Joe said to me, "No, I think it should be a PI instrument, I think it should be proposed." We deferred to his judgment. The Langley guys didn't win, and we picked the Canadians.

The Langley guys wrote a good proposal, but at some point we wanted the Canadians involved and the Canadians had written at least as good a proposal. It gave the Canadians an opportunity to pay part of this bill for the world. So it became the MOPITT instrument, and the Langley guys were on the science team. We had a nadir looker for carbon monoxide and methane, but the TES instrument was supposed to get a lot more stuff. I'm not sure how much has really come of it, but this was a knock your socks off technological thing.

We had originally two MODISs. We had a MODIS down-looking and a MODIS that could look forward [or aft] called MODIS-Tilt. The key problem being when Sun hits the water at certain angles you get a Sun glint where the water acts like a mirror. When it acts like a mirror you don't see into the water at all. You're pretty much blinded by the glint, just as you would be on a bright day out in a boat. So it was designed to not look straight down so it would avoid that glint off the surface of the sea. The tilting instrument was intended to be visible and near infrared only with 64 spectral bands. They were 10-nanometer-wide bands, and they were continuous across 640 nanometers of spectrum starting at 400 nanometers and going just beyond one micron.

HIRIS was intended to be 250 some odd spectral bands from 400 nanometers to 2.2 microns, again 10-nanometer spectral bands. HIRIS got canceled. In the down-scoping, the Center Director at Goddard—MODIS was a Goddard facility instrument—had to recommend that he had to basically drop MODIS-Tilt because other people were having their instruments killed; Goddard needed to have something lose. So he dropped MODIS-Tilt. We lost the HIRIS spectrometer, [and] we lost the MODIS-Tilt spectrometer. Going to actual spectroscopy of the Earth was a big breakthrough intended for the Earth Observing System. With the exception of the TES instrument it didn't happen. Most of it has yet to happen. Continuous spectral measurement was part of our vision, and it's a part of the vision that really has not been realized.

It's much more doable now than it was then, but we did get a HIRIS version flying on an aircraft. With all that money they did develop an AVIRIS [Airborne Visible/Infrared Imaging Spectrometer] instrument to fly on an aircraft that has flown and flown and flown again, and shown what you can do with what is now known as hyperspectral data. That's continuous spectrum type of approach.

Things tended to happen in very intense ways. The big descoping story, and it's also a reshaping story, comes after we'd gotten our new start. Dan Goldin is made Administrator starting on April Fools' Day of the last year of the [President] George H. W. Bush administration [April 1, 1992]. At this point Len Fisk and Peggy Finarelli have been at war with the Space

Council and the Vice President's Office because they kept trying to interfere in NASA and mess up the science program. They've been very successful at basically keeping Vice President [J. Danforth] Quayle's guys out of there. Vice President Quayle's guys also have heavy Defense Department ties and military contractor ties.

It comes time to replace the Administrator of NASA. I was told that Dan Goldin was the 20th choice, but he becomes Administrator of NASA. He's a pretty dynamic guy. He came in from TRW [Space and Technology Group]. There's a couple other things going on here. Before he gets there the descoping happens. We've gotten our new start, $17 billion run-out. People are saying it's a little too much—it's a lot too much, we need to descope it.

Even though he was a plasma physicist, [Edward A. Frieman] was head of Scripps Institution of Oceanography at the University of California, San Diego. He's really a political master and a real power, and he's put in charge of a committee to look at alternatives for implementing EOS as if what we were doing wasn't very good. A variety of people are put on his committee—including a guy who looks like he's a hatchet man, maybe from Lawrence Livermore [National Laboratory, Livermore, California]—coming out of that DOE [Department of Energy] but bombology types. All these people have got crazy ideas.

Several things are going on at the same time. EOS Data and Information System [EOSDIS] is out for competitive contract. It's an $800 million contract. It's a huge, huge procurement. This is a procurement of a size that is two and a half times bigger than the old budget used to be for all of Earth science in a year. Big deal, a lot of eggs are in the basket. And I'm responsible for overseeing it, which is not a wise thing. TRW and Hughes [Electronics Corporation] are the primary competitors. I think General Electric [Company] is a third competitor. Fairly early on they don't get picked, but two parallel shoot-off Phase B studies are funded, TRW and Hughes. Each of them goes and grabs people from the science teams.

All of a sudden I've got Berrien Moore on one science team, I got Mark Abbott on the TRW science team. All the guys who are EOS PIs now and were on the planning committee and part of the inner set: Seelye Martin, Mark Abbott, Berrien Moore, John Gille somewhat, John [M.] Melack quietly, and myself [and Dick Hartle]—that was the inner core of getting it written. I'm seeing these people and all of a sudden I got all these competing contractors for a contract that's happening on my watch, even though Goddard is running the actual procurement. Oh my goodness. I'm getting torn six different ways. It's not a great situation, although the people behaved moderately well.

Dan Goldin at this point is the vice president of TRW, and the bid is in his part of TRW. I'll say something out of school, but might as well say it; I don't think anybody can come back and do anything about it anymore. Dan Goldin has also got some ideas for how he thinks EOS satellites [and] platforms should be developed. What he has in mind is to put a clamshell platform in the nose cone of a Titan IV [rocket] and when it gets on orbit not only do you deploy the antennas and the solar panels, the whole body of the satellite unfolds into a long optical bench—because optical instruments, we need a very precise ability to steer them together, know exactly where they're pointing, know how their optics are all aligned, etc. It'll do that deployment on orbit.

Well, that's an interesting intellectual solution, but it jeopardizes the mission. You've added another single-point failure mode to a mission which already can fail if the antennas don't come out right and the solar panels. And this is one that nobody's ever done. He keeps touting this and touting this and touting this. Frankly, Len Fisk and Shelby viewed it as a threat in this environment. I am told that they communicated pretty aggressively to TRW to put Dan in a box and get him to shut up. They pointed out to them that they were bidding on a significant contract [and] this wasn't really helping the cause. TRW people came to me. I basically said if it's a good idea we'll do it, if it's a bad idea it'll fall of its own weight. It's one of the magical times when I really fortunately said the right thing, and I really stand by what I said. This did not create a lot of love for the Earth Observing System or Len or Shelby with Dan Goldin.

We then went to this Frieman committee meeting, this big meeting of their committee at La Jolla [San Diego, California]. It was a pretty humbling experience for me. Here I am, I've been the visionary architect leader planning the Earth Observing System—the panel is listening to Bob [Robert T.] Watson. I think that's partly because at this point there is a power art going on out in the science community which frankly I wasn't a part of, but Bob very much was. He had clout. Bob was getting to articulate why we should do the mission at all and what the real requirements were. Even though we'd worked side by side, we were both Shelby's proteges, I must admit I felt a little shocked at that, a little put out.

Marty Donohoe was the instrument guy. Chris [Christopher J.] Scolese was the systems engineer. Remember, this was still a large polar platform with a whole lot of instruments on it. It was down to just one large polar platform. Jim Baker quite honestly had asked a magical oversight question. Jim Baker was not yet head of NOAA. He was head of JOI, the Joint Oceanographic Institutions. In a meeting, people started talking about launch failure risk. I just remember looking eye to eye [at Jim Baker]. We had what was called these simultaneity requirements.

Back when I first came back to NASA Headquarters, I was going to be an advance planner. Frank [T.] Martin—who at the time was head of the Astrophysics Division, and had led the planning of the [Hubble] Space Telescope—I, for some reason, was in his office. It couldn't have been but 25 or 30 minutes. Somebody else was in the office too, and in that 25 minutes Frank laid out how to plan a mission at NASA. He taught me how to be an advance planner in that 25 minutes. Not sure I remember everything he said, but I remembered enough of it to do a lot of it. The Earth Observing System also reflected something I learned as a science fellow, which I may have said in the earlier interview, namely coalition politics. Didn't work so well for us in EOS, but it was certainly a lesson I tried to apply in bringing all the groups together.

I realized we were at terrible risk for a single launch failure, which would take out a whole EOS platform. Billions of dollars of instruments in one swoop. It's an awful big gamble, and I began to be very nervous about it. Fairly soon after that we were at this Frieman committee meeting. They were listening to all these ideas about how to do EOS more cheaply. Dan Goldin's idea is not one of them, by the way. He has been quieted by even higher management at TRW. Frankly, the committee pretty quickly realizes that none of these ideas have any magic in them. There's no brilliant military community DOE weapons lab fried crap that'll do anything to change the game—other than they all recommend going to a Sun-synchronous orbit down at 400 and some-odd kilometers. Well, what happens as you go from 705 kilometers to 400, which was the next polar 16-day repeat cycle Sun-synchronous orbit, you lose how much width on the ground you can cover. You lose footprint. You're in a narrower swath and you don't get as much data. You don't get enough data to work with NOAA and potentially be operational or feed the operational system. You don't meet the coverage requirements.

[But] you save money. You can build instruments more cheaply if you launch them to lower orbit. The models are such that it's not just a linear change in orbit. It's more than the cube of the orbital altitude in the cost of the instrument, in the cost models we were using then. So you could save money, but nobody said let's fly more of them—let's build yet more copies, and fly them at a lower orbit. Nobody came to that. I'm not sure why, but they didn't.

We ultimately got this, but at the same time they were looking for how to squeeze money down. Nobody knew. We were in budget freefall from a 10-year $17 billion run-out to what's it going to be. Barbara Mikulski is now in a position of power overseeing NASA as an appropriator, a position she's still in. I assume Kevin Kelly, who was her staff director for the subcommittee on the Appropriations Committee staff in the Senate—but the word comes back and she picks the number $11 billion. All of a sudden we're going to be an $11 billion 10-year run-out. We just all agreed.

That's when, by the way, [$2 billion of] the Laser Atmospheric Wind Sounder has been cut, just goodbye. But it was pretty clear that we needed to solve multiple problems. Chris Scolese and Marty Donohoe put their heads together, with my kibitzing as usual on the science type requirements. And overnight we came back in and they showed the Frieman committee the three-satellite Earth Observing System. And we have it.

So what seemed like a merciless outside attack to descope the mission that had us all upset was a major blessing. We had no business trying to build LAWS. It would have been a waste of the public's money. It was nowhere near ready. We got the scope under control. We were able to drop certain solar-terrestrial instruments that we'd put on the platforms to make nice with other parts of Space Science and Applications at NASA Headquarters. We got to get rid of that, got to get rid of LAWS. NOAA had gone overboard already before the new start. We got honed down. We went to these three platforms, plus money for an altimeter. After

TOPEX/Poseidon they needed a follow-on altimeter. Jason-1 is the EOS altimeter, so we took the money from EOS to pay for it.

Commercialization ideas were revealed to be the total foolishness they had always been. Total waste of the public's money and time and jeopardized our land observing system. Commercial pricing for the data kept the data from being used for decades. That data now flows out at incredible rates into the user community because it's now free. It's not even worth running the charging system for marginal cost of providing it. The public paid for this, and they're recouping. The commercial recoup wasn't going to pay for the satellite or the next satellite. So all of a sudden NASA had to find $500 million to fund a Landsat? That money came from the Earth Observing System. We just put Landsat into the Earth Observing System because we needed it.

We took our run-out. $11 billion, even as it got squeezed to about $7.25 billion. Some of that was because we paid for the altimeter. We had a scatterometer in EOS that flew with the Japanese. We got the Japanese instrument instead of HIRIS. All of these things worked together to give you what ultimately became the payloads, which work pretty well. Work amazingly well actually. Chris and Marty pulled that off overnight in La Jolla at this meeting.

We thought we were done at $11 billion. Subsequently Dan Goldin became Administrator. He couldn't do anything about EOSDIS because he had a conflict. For a year he couldn't even ask about EOSDIS, but he brought in his military buddies and asked all kinds of questions. We had to go and have a red team and a blue team exercise to descope EOS further. We kept falling down and getting descoped. Then the Science Investigators Working Group would be called in and they'd have to go with whatever the painful change was and look at it.

Several things struck me specifically about descoping the data system that were just awful. I still remember to this day, since I was responsible for EOSDIS at NASA Headquarters, Berrien Moore one year when we got our budget through OMB [Office of Management and Budget] as proposed—Berrien Moore congratulated me on the EOSDIS number. I didn't even view that as an achievement. My concern was not the data system but the whole mission. I wasn't in there fighting for my piece of it. It really was a jangle. I don't think that was understood by people ever, particularly not on the outside. I still remember that moment.

The other thing took me a long time to think back on, but if you'll remember in 1991 we didn't have the World Wide Web. The Internet was there, [but] we didn't have the World Wide Web. While we were picking these contractors we were going to have some period of time. Len Fisk said to us we need something to show for the data system. Data systems available for Earth science data need improvement now. You need something to show for this large investment because the data system was treated like it was the Lord. "Oh, we can't cut the data system shop. NASA has always screwed up the data system in the past. We're going to put serious money in the data system."

We did. EOSDIS had a lot of money and a huge raft of requirements, which ranged from the traditional "got to control the satellites" all the way to—got to be an incredibly creative distributor of data that is not optimized for queries that are space-specific or time-specific but allows both, and delivers huge volumes of data. The whole system is a stretch; 15 megabits per second continuous from space from a science satellite was unheard of then. It was a weird world. Len Fisk said we need Version 0. I think it got called Version 0 EOSDIS. So between 1991 and 1994 the team at Goddard pulled together Version 0 and built it. During that period of time the Web came into existence, and unbelievable things happened.

One of the unfortunate things that happened is the science users in the Investigators Working Group and elsewhere began to really get spoiled by what you could all of a sudden do. They could see how you could do more of that, but this was all in the data delivery database access end of the problem. It was a big problem in data processing. Because of bad history that went back in the solar-terrestrial world of PIs—and I mentioned before—monopolizing their data and not releasing it until after they'd published everything good in it for anybody else to look at, we had a rule that the data had to go into the public domain immediately. Because we were overly suspicious of our instrument PIs wanting to pull these old shenanigans of hanging on to their data till they've milked it for everything good, we knew we couldn't do that with this size investment, we insisted that they deliver algorithms to EOS Data and Information System DAACs, distributed active archive sites. The processing would happen at the DAAC with software delivered by the science team.

Oh my goodness. It didn't work out very well, but it certainly meant the investigators didn't really embrace the processing portion of the system and fight for it. We knew the algorithms would need improvement. We sized it so that if you decided you needed to reprocess because you've got an algorithm that was better than what you'd been using, and it was good enough to create a longer-term dataset, you would have to have the processing power to start using that new algorithm for the data flow that was coming in, plus to go back and process all the old data. If it was a year's data you should be able to process a year's data in six months while processing continuously what you needed. So you needed three times the processing power of just moving forward. And you needed redundancy, so it became six. It became a 6x size processing system. FLOPS [Floating Point Operations Per Second] weren't free. They were pretty reasonable but they weren't free. They cost a lot more then than they do now.

When we came to descope EOSDIS, everybody knew you couldn't descope the command and control part of it. That was bread-and-butter NASA stuff. You got to run the satellites, you got to get the data to the ground, but the only thing Goddard could give a decent cost to incrementally was the processing capacity. They couldn't put a price on all these unbelievably ballooned requirements for the database to be able to support all these different forms of access and all these user services and do everything in the marginal cost, billing system, the whole bit. All of those requirements were there driving cost, but nobody could quantify the cost. You couldn't say this requirement reveals this many lines of code, that's this much time. They were very much in a 1980s military, government data system, MITRE [Corporation] as the main brain supporting defense and other systems, the IRS [Internal Revenue Service] system, the air traffic control system for FAA [Federal Aviation Administration], both of which by the way failed. Totally failed. They were both MITRE-done. There was another one, and they all just went to hell in a handbasket.

But that hadn't happened when we were picking EOSDIS contractors and scoping things, so we were using their model, which had real cred [credibility] with the Department of Defense and NSA [National Security Agency] and people who'd dealt with a lot of data before. Not as much as we were going to deal with, but a lot. We were using their model, and the Web made their model obsolete. Silicon Valley had made their model garbage.

What happened in the descopes is—all the scientists could understand they could save money when we had to save money on the data system, not just on the instrument side, not just on the science investigation side, was capacity. So the capacity got eroded horribly. As a result a lot of EOS MODIS data wasn't available as a consistent dataset for quite a while because they lost the ability to do the reprocessing. I'm not sure they even launched it. It was no longer my responsibility. You ought to ask somebody if at launch they actually had enough processing capacity to keep up with the data. It's just amazing, but the scientists were a part of doing this. So nobody could say NASA cut the data system and didn't invest enough in the data system. At the end of the day it worked out, but it didn't work out as fast as it should have.

I didn't realize until it was too late how the Silicon Valley model would work. What happens is you just said, "Oh, I need a word processing program." You go buy [Microsoft] Word, or in those days WordPerfect. You needed this kind of program, you'd go buy one. There were things like that in the science community. The rule in the government data system developments were that a programmer could develop one documented line of code per day. So it was basically $100 a line a code. You need 1 million lines of code? It's $100 million. That was just the view, that was how you costed data systems.

I know of almost nothing else in the world where individual productivity varies more from person to person than in developing computer code. I was really good at it. The really great guys are more than 100 times as productive. I'm sure some of them can do hundreds of lines of code in a day. The man who invented relational database systems I think did it in a month. He built the whole first whole database management system in a month. By himself. Went off on a mountaintop and came back with it. That's the Silicon Valley model. Bright people working as many hours a day as they can possibly stay up. They're a small, tight, super hyper-energetic group who are just brilliant at cranking out code. All of a sudden you've got a product.

The cost structure is multiple products come to market, the market chooses, your company, one team, gets rich. All of a sudden they're worth $10 million in the early 1990s. Everybody else, they're not bankrupt. They go work for the next startup. That one is bankrupt, they write it off, and they go on to the next startup. Sooner or later they hit and they become multimillionaires as well. That's the model, but it's done with this intense hyperproductive set of folks.

We've just issued 1980s vintage $100-a-line-of-code, one-line-of-code-per-programmer-per-day procurement ultimately to Hughes, not TRW. Oh boy, have we made a mistake. Because the expectations from the science community were driven by what they saw Silicon Valley doing, not any realistic perception of what could be done in a government model.

We got great attention. CAZ Zraket, Charles A. Zraket, who was the former CEO of MITRE, I think had stepped down by that point. He chaired a National Academy review of EOSDIS. They gave us great recommendations. The Goddard team could kill me because at the end of it I took their recommendations and I turned to Goddard and said implement every recommendation they gave us. I didn't stand up and oppose a single one of them. Interestingly enough Tom [Thomas R.] Karl, who's a major player still running the [NOAA] National Climatic Data Center, was on that panel. CAZ was wonderful to me. My hat is off to him. But he'd come from the MITRE model, so they didn't criticize that model. They didn't tell us [to] write it off.

The good news was we spent these three years developing System 0, and System 0 became the cornerstone of what became EOSDIS, and made a big difference I believe. Ultimately TRW lost. They lost, I think it's fair to say, because they strayed into proprietary software, which didn't work very well. We wanted a system that we knew would have to grow and evolve and [be] flexible where you could change in and out software. And we wanted the software to be such that people would understand the software. So public domain. I think that may have been a key point, but I digress.

We're '96 launch, we got a '91 new start. Then we had some tensions. We began flirting with launch delays. The EOSDIS ultimately screws up, and not on any of the hard stuff. The contractor blew it on the satellite control part, the part that's been done for generations. They were trying to do some new things. They screwed it all up, and took the blame for the three-year launch slip where Terra ultimately launched in 1999 instead of 1996. In retrospect—since I think I'm pretty good at acknowledging my mistakes, at least as I perceive them—we should have done on the flight side what we did on the data system side. We should have developed MODIS-Nadir as a stand-alone instrument precursor flight, much like the Japanese going with ADEOS, which did launch in '96. We should have scrambled a MODIS-Nadir instrument and flown it. Maybe even a somewhat descoped one, but we should have gotten MODIS in the sky as a precursor.

That is not unrelated to a later decision. The one wise thing that happened out of this horrible decision to drop the second and third copies of the EOS instruments and go to an operational system for climate now, which then didn't happen, was NASA put in the NPOESS Preparatory Program. It's been delayed so much that it won't be a precursor. It will have to be operationally ready on day one, but it's still being developed on NASA's budget, NASA's control. Except for one instrument it's under good control. It'll make it.

We should have followed that model with just a MODIS earlier and flown a MODIS as a Version 0 EOS. Frankly, I was a bit afraid if we did that—since I believed about 60 percent of the science of EOS would come from just the MODIS instrument—that we'd lose the money to develop the whole system, but I don't think that would have happened. I think there's a lot of wisdom in doing things that way. I would say also hard lesson, but goes back to that multiple copies—today I believe very strongly if you're developing an instrument or satellite at NASA that you think the instruments or the satellite are going to have operational potential, you need to build two copies so you've got enough time to deal with the procurements that NOAA or the US Geological Survey will need to buy it on an operational basis without a data gap. An extra little burden on the research community, but it's worth it because when you're buying one, buying the second one is 30 percent less usually to buy that second copy if you buy it at the same time.

This is another cautionary tale. The EOS new start puts the Earth science activity at NASA Headquarters on a path to a real dollar buying power that is more than three times its historical maximum. You take the biggest pre-EOS year in the history of the agency and adjust the dollars for inflation, EOS was carrying by itself almost the Earth science budget at NASA to more than triple that buying power. That's big stuff. We all viewed manned spaceflight as the big leagues so one of the mistakes we made is we tried to behave like manned spaceflight.

Manned spaceflight had lots of support contractors at NASA Headquarters. So we went from my 20 support contractor people to 100. What a mess, just a mess. It was just a waste of money. The people worked hard. They did stuff, but nutty, nutty way to do it. Ultimately we had to get rid of them all except for a very small group that had been included there from the very beginning, that we'd had well before we'd even gotten up to the 20 people helping me—which was the group that handled all that peer review process for us. They did a wonderful job of that.

Now that job is actually a specific group at the Stennis Space Flight Center doing it for all of NASA. It's interesting, but Earth science had it very much that way. Frankly we did that really well, and so that grew to be a group that processed everything for the whole Space Science and Applications area. Now it's done agencywide that way in a different place. That worked really well, but this [large support contractor] was just a waste of talent and money.

We were on our way to becoming really big-time. Clearly we were going to be a really big division under Len Fisk with Shelby in charge. We had the Earth System Science recommendations. Several things go on. One of them is I'm feeling put-upon because I'd expected Shelby to leave the government and go to an outside job. He didn't get picked for the outside job. Here he's got this protege who's clearly chomping at the bit to take his place and the interaction didn't really work all that well psychologically for me when he decided to stay. It felt to me like I was being pushed back down in the system some.

It's also true at this point Bob Watson is becoming a bigger and bigger deal in the world, and justifiably so. He's playing an enormous leadership role in stratospheric ozone depletion. Even though he's not even a US citizen yet, he's the clearly lead person for all the science in the negotiations of things like the Montreal Protocol [on Substances That Deplete the Ozone Layer]. So he's going with the US State Department delegation and really informing them. He's leading the assessments, and he's doing a brilliant job of it.

But as the competing protege of Shelby, it didn't always feel so warm. I assumed Bob had some greater thing but I would take Shelby's place running Earth science at NASA, which is what I wanted to do. Then I got pushed back down. That's one of the reasons I hid away for eight weeks from NASA Headquarters doing the review process. It just was protective cocooning, and I was not very happy at Headquarters.

Shelby at this point had Wes [Wesley T.] Huntress in as his deputy from JPL. They came up with a scheme for how they wanted to reorganize in light of EOS' new start. They decided because of Earth System Science that they would reorganize into basically two science divisions, not one. One of them was really just a science branch. The other branch was going to do science plus be responsible for EOSDIS plus be responsible for all the data systems and operations of all operating satellites in Earth science.

Both of them knew the stratosphere program. In the stratosphere program it had worked brilliantly to have one person manage—namely Bob Watson—the field measurements and the laboratory measurements, and somebody else manage the modeling and data analysis—me. Worked like a perfect dichotomy. They decided to carry that dichotomy up the line so Watson would head a branch that would do all the field measurements and laboratory measurements, and then the theory and data analysis of course would also be responsible for the data systems and the operations. You begin to see a data to model theme here.

It works in the stratosphere as an intellectual discipline. It does not work really well in oceanography or terrestrial ecosystems. We've got a division that's structured researchwise with a land group, an oceans group and an atmosphere and climates group. So we've got three different—we were going to have flight systems and my group and Watson's group. I was going to be science, data and information systems.

I think part of the structure was motivated by Tilford and Huntress saying we got to have a job for Dixon, and I don't think it'll work very well to ask Dixon to just work for Watson. So we got to have a group for Dixon, we got to have a group for Bob [Watson]. Wes was close to Shelby, not a protégé because he'd grown up on his own, but he was close to Bob. He was close to me. Even some social interactions—much more with Bob than with me in the old days because they'd both been at JPL.

The point is there were lots of warm ties here, and that can get you in trouble. So they basically came up with this structure that required choosing, having to break all the oceans, atmosphere, land groups up to try and put the theory and data analysis with me and the measurements with Bob. The managers are people who tend to be in everything from the measurements through to the modeling. They're not split up like this in that way.

Everything's run differently. Stan Wilson loses a branch. Bob Murphy is heading one of these branches, he loses his branch. John [S.] Theon is going to lose his branch too, although John Theon is well protected ultimately. In this mess, basically people start trying to game the system. Bob Murphy and the land people assume that it's better to be with Bob Watson because he's going to be stronger. So they come in to Shelby and describe their work in such a way that it all goes to Bob.

I realize I'm about to get nobody, so I conspire with the oceanographers. They describe their work so I get almost all the oceanography stuff which is heavily dependent on satellites. We weren't big launchers of [oceanographic research] cruises. That was NSF and NOAA—still is NSF and NOAA, and Navy in those days so it was a little easier. Stan Wilson was robbing the research budget to pay for the data systems for oceanography like mad. He really understood the data thing.

Watching your colleagues basically get pulled apart like this and behave badly—I really was so angry at them at the time, but in retrospect they were being placed by management in a totally unviable, irresponsible choice. It was a dumb choice, frankly. They should have brought in a data system expert to run the data systems. They should have asked me to be Bob's deputy, and I should have had the humility to do it. It would have worked out perfectly because I would have stayed Bob's deputy until he went to OSTP [Office of Science and Technology Policy]. Then I would have been in charge of science. I could have kept my fingers into EOS better. And I wouldn't have been responsible for the data system, which would have been nicer too.

But as it was we did this. The other thing that happened is they told me I had to stop being EOS program scientist, and Stan Wilson got to be EOS program scientist. Stan didn't like EOS. Oceanography at that point was pretty light on believing in big-time modeling. Here are all these interdisciplinary investigations which Len Fisk and I are the ones that really understand what they're supposed to do and how it really should work, but the program scientist should really have control of their budgets. I wouldn't let go of it, so I didn't really let Stan control that budget. That created enormous bitterness as you can well imagine, and real problems.

Stan did begin to exercise some power over it when it came time and we had to descope. He and Ming-Ying Wei, who he hired to be his helper in doing this, ultimately did lead the way to the descopes, because I wasn't sworn in to get rid of any of them. We basically created some real problems. Most of them got healed in the long run. Some of the people are still even at NASA Headquarters. But it wasn't good, it really wasn't good, as a set of things interpersonally. Here we were on this wonderful idealistic—and yet we crapped up our relationships in our team at NASA Headquarters.

It was good for oceanography by the way because I used the data system money not for EOSDIS but for operating things. I said wait a minute, you shouldn't have to take that on the research budget, that's a data system responsibility. I put in the money, and that freed up a lot of money for the oceanography research to grow. So for oceanography it worked out well. I'm not sure they cared or appreciated or said anything, but in substance it was working out well.

There's a cautionary tale there: you can't take the paradigms of one area of research and just arbitrarily managerially apply them to the others. It may not be right. In this case it certainly wasn't. It lasted about three years and ultimately then the scientists all went to Bob and to an integrated science division. Again there was harmony, a reasonable science operation, but it was hard in those intervening years.

I was trying to go back to some of the interpersonal things. Let me just go on and tell the rest of one of those tales. We get to the last April of George Herbert Walker Bush's presidency, and Dan Goldin comes in as the new NASA Administrator. He's very much been put in that job by a NASA man who's been pushed out of the agency over to the [National] Space Council, but who's still a NASA employee. He ultimately is viewed by Goldin, at least I was always told, as the person who really got him the job. Dan Goldin loved NASA. He loved being the Administrator. He didn't say he loved the employees. He came in kicking butt—wasn't so bad.

To his credit, he broke the cost model spiral. We were only using historical data in the cost model. Nobody ever underruns a government program and they often overrun. Overruns got built in, so everything got more and more and more expensive. It never got reined back in. If you went to do the same thing again, it cost a lot, and if you went to do something new it cost a lot plus a contingency. It was just taking us out of affordability. Goldin basically said we're going to use technology not to improve, not necessarily to do new things, but to control the cost of the things we do. It broke the price spiral for a time. After he left by the way the price spiral resumed. I'm in sticker shock today at what instruments and satellites are costing at NASA.

He came through the door with three [or four] people he was upset with. Len Fisk and Peggy Finarelli, who'd been playing such an effective job at keeping the Space Council at bay, and Shelby and Len because of EOSDIS. He couldn't exactly fire Len, but basically created a situation in which Len left and went to the University of Michigan [Ann Arbor] in a pretty rapid thing. The Office of Space Science and Applications was broken up into three parts: one headed officially by Wes Huntress, one headed by Shelby on an acting basis with Bill Townsend as his deputy official, and the indoor sports people [microgravity and life sciences]—I don't know who headed them, but they were a much smaller activity even then.

Len has to run away, and Shelby didn't get put in officially; he got put in on an acting basis. Peggy got ousted from the head of all of international affairs and she was Associate Administrator [for Policy Coordination and International Relations]. So she had all the public affairs people, all the international relations people. It was really good. Peggy went and worked in a big advance study about the future of the agency—one more of those exercises—for a time. Then she leaves and goes to be in charge of international at the GLOBE program.

Dan Goldin disestablishes the Reston [Virginia] Space Station [Freedom] office. Lyn Wigbels, who's working there, goes to GLOBE. GLOBE is important; they end up being my colleagues at GLOBE. The Reston people have to start looking for jobs. At this point I've now got a division in an office, and I've only got ten people. People think I need more people so I get ten people out of the Reston office come to work for me. Actually somebody who had been like their lead comptroller, for some reason I'd given him a positive impression and he counseled a lot of those people to come work for me. It was amazing. One thing miraculous is help showed up at my door time and time again. In this case not all of it was first-rate help but some of it was just wonderful. Only one person within that set of people was actually really weak, I won't use any names. It was just amazing. All of a sudden I had to manage 20 people.

And when I stopped having to manage 20 people it was a relief—I will have to say that—because I felt their careers were all depending on me. [It] wasn't managing them day to day, or [that] I didn't know how to treat them well or I felt uncomfortable treating them well. It was when then things were falling apart, we were being forced to downsize, get rid of all that external contractor, and try and shrink the Headquarters management team to do Earth science a lot of them just had to go find job elsewhere again. So coming to work for me didn't play out very well. But in that process Shelby got put in, Len and Peggy got ousted from their associate administrator positions. And it was not good. It really wasn't good, and it certainly wasn't deserved. Then ultimately Shelby got ousted as well.

Charlie [Charles F.] Kennel came in. Thank goodness Charlie's second wife is a psychiatrist because he had a group that was really a group psychiatry problem—we were Shelby's people. Bless his heart, Charlie Kennel came in and said in a public meeting to Shelby that [he] was honoring Shelby before he left. [Charlie] came in and said look, no one has ever organized such an incredible science program for the federal government before. It made a lot of difference. It made Charlie a good guy. Charlie came in the door liking me because of some interactions in the Solar-Terrestrial Theory Program.

I will make one other comment. When Dan Goldin's period of exclusion from paying attention to EOSDIS ended, he wanted to take a look at EOSDIS hoping to downsize it because he wanted to spend money on new technology, particularly new flight technology. That's not a bad inclination by the way; it's a needed thing at NASA. Maybe it'll get to happen under the President's [Barack H. Obama] new direction to the agency.

Goldin was no fan of EOSDIS. He called in all of his defense community, intelligence community buddies who he knew were really experts and they came and looked at EOSDIS and basically told him leave them alone, they're fine. We passed muster. The reason was we were following what they'd done, which wasn't working for them either, but they didn't have any bright ideas. Certainly none of us understood at that point this Silicon Valley model.

I'm still not sure how you would do a government procurement that gets at the Silicon Valley model. The closest I've seen are these ideas about offering prizes, like $10 million prize for the first one that can do something. It's sort of a quasi way of getting at that kind of government funding—somehow the winner of a contest that engages people to go work like crazy to do something in a much more productive way than paying a contractor to go meet some requirements.

That, alas, is probably enough for today. We should have [another interview] because I think GLOBE is relevant to Earth System Science, and we should talk a little more through that. I should also tell you my departures from various things.

ROSS-NAZZAL: Absolutely, I look forward to it.

BUTLER: Great. Thank you.

ROSS-NAZZAL: Thank you.

[End of interview]