NASA HEADQUARTERS ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

LENNARD A. FISK
INTERVIEWED BY REBECCA WRIGHT
ANN ARBOR, MICHIGAN – SEPTEMBER 8, 2010

WRIGHT: Today is September 8th, 2010. This interview is being conducted with Dr. Lennard Fisk in Ann Arbor, Michigan for the NASA Headquarters Oral History Project. Interviewer is Rebecca Wright. Thanks again for finding time in your busy schedule. As you mentioned, today is the first day of class, so I know you have lots going on. We'd like for you to start today by sharing with us about when you first went to [NASA] Goddard [Space Flight Center, Greenbelt, Maryland]. I think it was September 1969. Tell us about this opportunity and your first assignments there.

FISK: I always like to joke that I went to work for NASA essentially on the day of the Moon landing, in the sense that it had been the previous July [Apollo 11]. It was all downhill from that point on. But let me back up just a little bit. I'm a Sputnik kid. I was in high school. In 1957 I was 14. Sputnik [satellite] made me want to be a scientist. That was the event.

I did an undergraduate degree at Cornell [University, Ithaca, New York] in physics, and started Cornell in graduate school in space science. My thesis adviser was a fellow named [Sir William] Ian Axford, who just died recently. He died last March. He was a very famous scientist in the field, one of the early pioneer theorists. Halfway through my graduate career he decided to move. He couldn't stand the Ithaca winters where Cornell is. So he went to the University of California, San Diego, and I went out with him, joined him at that point, and finished my degree from there.

Then it came time to get a postdoc [postdoctoral fellowship]. I looked at a number of opportunities at that point, and Goddard was the one that I chose, and it was a very good choice, because this was in the height of the space science program associated with Apollo and NASA in its heydays. So there was a lot of activity. Frank [B.] McDonald, who was the director of the lab at that point was one of the really influential people in NASA in space science, so the lab was quite successful.

I'm a theoretician, it's primarily an experimental lab, so I was basically what we refer to as a house theoretician, you have a few theoreticians around, but not too many in an experimental group at that time. So there were basically two of us. Reuven Ramaty was the other one. He was a very very good solar gamma ray type physicist. Reuven died some years ago prematurely, early in his life. But he and I were the two theoreticians, and I embarked upon my research career at that point.

WRIGHT: Well, talk about some of those first assignments.

FISK: Well, the good news about a postdoc position in general—and for Goddard at the time—was that you were given a great deal of freedom to do the research that you wanted to do. That was the expectation. A postdoc position is not a permanent civil service position. In fact the program was run by the National Research Council, which is part of the [United States] National Academies. They ran the postdoctoral appointment at all NASA centers and other government labs as well. It was a prestigious award. You applied for it as you would a fellowship. So I applied for this and was successful, and went to Goddard.

Basically as is often the case when you're a theoretician or even any graduate student, you tend to keep doing your thesis after you leave. So I was an expert on cosmic ray propagation. It's called cosmic ray modulation. What happens to a cosmic ray when it comes into the solar system and fights its way upstream against the expanding atmosphere of the Sun, and how does the intensity change, all those good things.

So that was the research that I pursued. I was valuable enough at the end of that postdoc that I was hired on to the civil service at that point, 1971. Now you need to appreciate the environment at that time in NASA, because NASA was imploding at that point. Apollo came to an end in '72, but NASA was on its way down even before the Moon landing, if you just look at the budget profile for NASA at that time. I think if I remember the numbers correctly there were 30,000 or 35,000 NASA employees at the time of the peak of the Apollo [Program], and by the time the program ended, they were down to 20,000 or so. Even though Goddard was not part of the Apollo, it was part of NASA. So Goddard was in the middle of a RIF [reduction in force] at almost all times. The thought of getting hired in the middle of a RIF was some question.

I did look at other positions, because I wasn't certain as to whether or not I could get onto the civil service, and had job offers from [Johns Hopkins University] Applied Physics Lab [Laurel, Maryland] and other places like that. But fortunately Frank McDonald was a master of working the system. So there was a very narrow window when the hiring freeze was lifted, and I was suddenly trotted through that narrow window with a couple other people he wanted to hire in 1971. But for many years thereafter I had the lowest seniority at Goddard, and was always wondering whether I would be a RIF candidate if the situation arose.

I am indebted to Frank in a number of ways for that reason. Of course for being hired, but also for another reason. I got my PhD quite young. I was under 26. This was the middle of the Vietnam War. I was married, but no children or anything. So I was never sure exactly what my draft status was going to be when I finished my degree when I was still not 26, and I had been on student deferment.

Frank McDonald wrote a letter to my draft board in Elizabeth, New Jersey where I grew up, which I still have, which said the Apollo astronauts are going to die unless I predict solar flares for them. Elizabeth, New Jersey is a working class town in New Jersey, and they didn't really have a lot of trouble filling their draft quotas. It wasn't as if that was too much of a difficulty. But I always had this image of this draft board saying yes sir. I never heard from them. I never heard from them again ever. So who knows whether or not I would have been drafted. But at any rate I'm indebted to that protection that Frank provided.

WRIGHT: How did your interest in studying the Sun and its powers start?

FISK: The space part has always been my interest, because anyone in the '60s would have been interested I think. It was the field to be in. It was actually a reasoned discussion on my part. I could have been an astrophysicist, a real live astrophysicist, but I didn't like the lack of data. Astrophysical theories were almost like parlor games, where you say I've got one data point and I have this wonderfully complicated theories—we always refer to these as inverse pyramid theories where there's a lot of things built on just a small point of information.

Whereas when you work in the solar system—where there were a lot of measurements being made—that was more satisfying to me. The theories would have to actually explain numerous observations to be successful. They could be tested I guess is the way to say it.

But probably what happens more than anything else in graduate school—and this is true today—is when you choose an adviser to work with, you work on what he's working on. You don't ask him to do something that he's not personally involved in. Axford was one of the major pioneers in this general field of energetic particles. It was energetic particle research. In our cases—in my case and in Axford's case—it was in the magnetosphere of the Earth, in the solar wind and so on. So you just gravitate to that. He gave me the thesis topic to work on. That's the field you ended up in.

I didn't regret it, it was a good choice, but it was just that things were placed in front of you as opportunities, and you basically just chose it. Most students don't know what to work on until they are confronted with some opportunities and then they say okay I'll take that or not take that. In my case I took it, and it was a good choice.

I by the way just two weeks ago was out at the University of California delivering a eulogy at Axford's memorial service. He's ten years older than I am. I consider that young to die. But it wasn't terrible. I was having flashbacks for what it was like to be his student. The style of research that he gave to me. There is a sort of laying on of hands that takes place, that runs through this field. Probably any science field for that matter.

Axford's thesis adviser was a fellow named [Sir Michael James] Lighthill who was probably the most famous applied mathematician in England. Axford was a New Zealander but he was educated in England. So that was a style of research that came to Axford. I don't know where Lighthill got it. I can only go back so many generations. Then Axford's style of research came to me, and then I try and impart it to my students and so forth, and it is a style where you don't tend to use a lot of computers—which today are more in vogue. You look at some new phenomenon, you create an explanation of it through your imagination, you find analytic solutions and approximations, formulas that describe it, and then you hope somebody looks at data and says it worked or didn't work.

You also tend to look for problems which are not understood, and they're new phenomena. If you say how do you have a successful career in theoretical space physics, the best way to do it is get two or three problems in the course of your life which are preferably new phenomena for which there is no explanation, and you produce the explanation. It's even better if everybody tells you you're wrong when you first propose it, and after that you are proved to be correct. If you have a couple of those in your life, you're a famous guy at the end of it. Now high stakes, if you're wrong, and you turn out to be wrong, you may not be famous at the end of it. But that's the gamble that you try and take.

You look for things that are unknown. I can think of three or four such events that I've had in my career, and that's why I have a certain reputation now after all these years as a scientist based on the fact that I've been able to have these ideal events. Something new, something you explain, something everybody tells you you're wrong, and you turn out after a while to be correct.

WRIGHT: Well, would you like to talk about those now?

FISK: Sure. One of them was a Goddard experience. In the early '70s, '72, '73, there was a new phenomenon discovered in the data which was labeled anomalous cosmic rays. Cosmic rays are particles that come from the galaxy normally. Galactic cosmic rays. Or solar particles. They come in all the usual flavors. Hydrogen, helium, nitrogen, oxygen, neon. All the elements. In the proportions that they're supposed to be in. In an energy range at the low end of galactic cosmic rays and at the high end of solar cosmic rays, there was a new component that was discovered that only had helium, nitrogen and oxygen, and nothing else. So it was labeled anomalous.

It puzzled people. It was interesting, because your explanation depended on what you thought you were doing in this field. If you're a galactic cosmic ray person, you say I'm measuring these particles that come from the galaxy, and so they are telling me about objects in the universe. Stars and other things. So if I see this component which has unusual composition, it must have come from an unusual object in the galaxy. So the cosmic ray physicists were all excited. Their field was being made. They had discovered this new thing.

Some very famous people, like Willy [William A.] Fowler at Caltech [California Institute of Technology, Pasadena], dreamed up an explanation that—I've forgotten exactly the details. But there were brown dwarf stars colliding with each other or something and giving out anomalous cosmic rays. So everyone was happy. I didn't think that explanation worked. There are some technical reasons about how cosmic rays behave in solar wind. You could not produce the spectra that people were seeing and so on.

But that was a downer for some to suggest that their explanation didn't work, and it got even worse. In the office next to me was this Reuven Ramaty, and he had a friend, a colleague who was visiting from Israel named [Ben Zion] Kozlovsky. One afternoon we were sitting there talking about this, and all the thoughts galvanized on the idea that these particles came from interstellar neutral gas, which comes into the solar system unaffected by the solar wind, because it's neutral. Gets ionized when you're close to the Sun. We proposed that it got accelerated in the solar system someplace. If you do that you get exactly the right composition. Because the interstellar neutral gas had the same composition as this anomalous component.

So we were a downer for everybody because not only was this not from some exotic object in the galaxy, this was trash from the solar system, it was just stuff that was made locally. So Frank McDonald, who is one of my really close friends these days, he's quite a bit older than I am, and we'll come back to some things that he did good for me even after this background, he didn't like this theory at all, because he was one of those conventional cosmic ray physicists. I found an interesting object out there, don't tell me it isn't an interesting object.

So my life became very unpleasant at Goddard. My colleagues Ramaty and Kozlovsky—well, Kozlovsky went home to Israel and Ramaty ran for cover. I decided this was my moment. In the field by the way it's called the Parker moment, because of the most famous physicist of our time in this field, Gene [Eugene N.] Parker, he's the man who predicted the solar wind. He was a young man at the University of Chicago [Illinois], a young faculty member, in the '50s. This was in the '50s. He predicted that the solar wind would be a supersonic flow. He was untenured. There was another person at Chicago who said no, it won't be supersonic, it's going to be subsonic, it's going to be the solar breeze, not the solar wind. The other guy was big and famous, and he tried to keep Parker from getting tenure, but Parker had someone who came and rescued him, and he survived, but no one believed his theory, and then in 1962 with the first Mariner 2 spacecraft that went into interplanetary space, they discovered the supersonic solar wind, Parker is famous, the other guy was never talked about again. So that's the Parker moment.

So I viewed this as my Parker moment. I had a theory. I was convinced I was right. Everybody was telling me I was wrong. It did turn out to be absolutely correct as the explanation. But it made life very unpleasant for me at Goddard, and I left, I went to the University of New Hampshire [UNH, Durham].

It actually took 20 years to confirm the theory. Just since we are not obligated to be chronological here, there's an interesting final ending to this. The theory became more and more accepted as time went along, but there was no smoking gun measurement that said this is the right theory. The first satellite that made that smoking gun measurement was SAMPEX [Solar, Anomalous, and Magnetospheric Particle Explorer], which was the first mission I chose as [NASA] Associate Administrator [for Space Science]. Now I chose it for other than to confirm or deny my theory. But it pleased me that the first mission I got to choose, it was a Small Explorer mission, and it was the first mission that as Associate Administrator I got to select, and it flew and made the measurements. It was not being strongly debated anymore after that time, because there was so much circumstantial evidence by that point that the theory was correct. But this mission nailed it once and for all. So this goes full circle.

WRIGHT: It had to be a rewarding moment at the time.

FISK: Right.

WRIGHT: That's great.

FISK: Now you've got me moved. I'm on my way to New Hampshire.

WRIGHT: You are. During that time though before you left you actually spent just a little bit of time in Caltech. Did you make association with JPL [Jet Propulsion Laboratory, Pasadena, California] there at the time?

FISK: No. This was just an interesting opportunity. You can get a part-time appointment at Caltech, and I was looking just for the exposure to new things. It's interesting. It's actually where I wrote the anomalous component paper. This is early 1974. We were finishing writing the paper and negotiating with the referees to get it published, because that was hard to do, because the theory was considered controversial. There is a funny story that goes with the publishing of that paper, because the referee was a person named Bill [William R.] Webber, and Bill is a wonderful scientist and a wonderful person, but a bit of a—scatterbrain is too strong a word. But disorganized, let me put it this way. So he wrote this review of my paper that was very favorable and said this should be published in *Phys Rev Letters*. The only difficulty was we had submitted it to the *Astrophysical Journal Letters*. He wrote the wrong journal. So the editor looked at this and said, "Well, not my problem." So we lost a couple months trying to convince the editor that it was favorable for his journal. It was just one of those flukes.

WRIGHT: So close.

FISK: So close. But we spent—my wife and I and our two small children at that point—we spent four months in Pasadena as something to do. I wouldn't say it was the best experience I ever had, because it was the middle of the gasoline crisis in 1974, so we couldn't drive anyplace. If you live in California, that is not the most convenient situation—so all the thoughts we had about seeing California and doing thing, we were limited. The person that I was invited to join at Caltech, come visit him, just before I got there was denied tenure and left. I was out there on my own as a result. Ed [Edward C.] Stone and Robbie [Rochus] Vogt were all part of that group.

Ed was off of course worrying about Voyager as head of the Voyager mission. Robbie Vogt was ill at the time. He was a major professor at Caltech. So I was really very lonely at that time. Got a lot of work done, but none of the things that you want to do on a sabbatical, learning, meeting new people, was really realized. So I don't regret that I did it, but it really had no particular impact on my career.

WRIGHT: Tell us about how you were able to move over to the University of New Hampshire.

FISK: I needed to leave Goddard. That was the thing. I'd concluded that my career was being stymied there because I was at odds with the system. I was going to be forever a GS [General Schedule]-13 or 14, whatever. Maybe I think I might have still been a GS-13. Other people were going to get promoted around me. It was unpleasant. So New Hampshire has a very good space group in space science. Bill Webber was at New Hampshire at the time. Jack [John A.] Lockwood and others.

They first of all had me interview for a job just to go up as I think maybe it was an assistant professorship at that point. The chairman of the department there was a guy who believed all people should be paid exactly the same amount of money—and if I was going to enter as an assistant professor I was going to make a salary that I hadn't seen since I was a postdoc or something. So I said thank you but that's not for me. My friends in New Hampshire said, "Well, look, why don't you come here on an IPA, intergovernmental personnel assignment, for a year." Frank was happy to get rid of me anyway. So we moved up in calendar '76. We moved our family, rented our house in Maryland where we were living, and joined the faculty. I was just a visiting professor essentially up there.

I got to know the dean, I got to know the system and so forth. Suddenly the powers that be decided I was worth something more than just an assistant professor. So when I came back from New Hampshire—I had to come back for six months in the first half of ’77. When you’re on an IPA you can’t just leave. Then they made a decent offer as an associate professor with a decent enough salary. I took it untenured, because I hadn’t taught enough to satisfy them. So I then went on in New Hampshire after that.

WRIGHT: Ttalk about that program there, because the Institute of Earth, Oceans and Space certainly has a reputation that is well known.

FISK: The New Hampshire experience was really a great one for me. New Hampshire is one of those great places. First of all it gave me an open field to run. There were no constraints. One of the things that I was an advocate of within NASA was a program called the Solar-Terrestrial Theory Program. There was a growing recognition finally that theorists were really important in the space program. They weren’t just house theoreticians, you shouldn’t just have one or two to cause you trouble, you should actually have some critical mass theorists that are able to do real theory—the subject was growing in importance, and the requirements for explanation were more than just a few folks proposing cartoons of things. You needed critical mass folks doing calculation and so forth. So the Solar-Terrestrial Theory Program was established. I was in the first round of grants. It was big numbers at the time. It was like $300,000 a year for theorists. So we were funded finally.

I was able to build a group of theorists, recruited around the country for people to come and join me. So that got me off and running on the science side, on the professorial side at UNH.

I was tenured and promoted to be professor and so forth. In the early ’80s I got on the slippery slope of administration where you just go further and further into it. I had what could only be described as an unplanned meteoritic rise in administration. I was starting to get bored just doing the science. I got asked—I have to check the dates exactly, but I think it’s 1982—to be the director of research of the university. It was a dean’s level position. It was my first foray into any kind of administration at all of any serious size. So I did that. I apparently was a hit, and at the end of the first year of doing this thing the administration of the university more or less quit. The president went off to be the president of Brandeis [University, Waltham, Massachusetts]. She had a vice president for finance who no one liked. So he took one look at his cover being removed, and he ran somewhere else. The academic vice president became the acting president, and looked around and said who’s going to run the place.

He asked me to be the vice president for finance of the University of New Hampshire, finance/administration. I remember saying to him there’s only two things wrong with this. I don’t want to do it, and I’m not competent. He said oh, you’re only going to be there for a year. You just got to help me out. You’re my friend. As soon as we settle who the president is going to be, we’ll recruit for the vice president, you’ll get out of it. So okay.

Then I made a mistake I guess. I looked at doing this thing for a year, and I said I don’t want to be bored here, I don’t want to be just a caretaker. So I assembled all the direct reports that I had—there were people I’d never met, like the guy who runs the police department, the transportation system, the finance side, the materials side and so on. It was the infrastructure of the university. I said look, I’m going to be here for a year, let’s make a list of the ten problems you always wanted to have solved, and see if we can solve them.

So we made this long list. The buses never make any money. This doesn't do this, that or the other thing. We solved them in that one-year period.

WRIGHT: Goodness.

FISK: So at the end of it, the acting president gets the job as president, and he says look, Len, you're part of the reason I got to be the president, because we're running the place so well. You can't leave me now and so on. I said well, look, this is really drifting away from being a scientist. I want to be the vice president for research and finance—of which there is none other in the country. This is the fox in the chicken coop kind of position. So basically he says fine. You do that. So I become the vice president for research and finance.

Well, concurrent with all that activity, there was a cabal of ambitious faculty members, Berrien Moore [III] was the key, and a fellow named Robert [W.] Corell, who were recognizing that the future of Earth science was in Earth System Science. This was just the recognition. This is the same event that also led eventually to Mission to Planet Earth and the Earth Observing System and all the big Earth science activities we did. But it started in the early '80s where the leaders of the Earth science community were recognizing that the way to study the Earth is as an integrated system. You just don't look at the oceans, you look at the ocean-atmosphere coupling, the land surface, the biosphere, the cryosphere, it's all merged together, and that's where the future of the science is.

So Berrien and Bob Corell and I conspired to create the University of New Hampshire as a place that would be known for this science. It was part of the strategic thinking of the university, because when you're a university like New Hampshire, a state university, but you're in the shadow of MIT [Massachusetts Institute of Technlogy, Cambridge] and Harvard [University, Cambridge, Massachusetts] and all the other constraints, you say what are you known for. You can't be like [University of] Michigan [Ann Arbor] here where you're sort of known for everything. Here we say to yourself we're supposed to be number one in everything here, that's our mantra. In New Hampshire you say let me find some niches where I can be world-renowned and put some emphasis on those. Then the rest of the university will benefit from the fact that there are some things that we are particularly well known for.

When we looked at our faculty and we looked at the environment that we were in, namely Earth System Science is developing, we have faculty that can successfully play in that area, and let's put together this Institute for the Earth, Oceans and Space to essentially accomplish this goal of giving the university something to be known for nationally and playing into this broader national agenda—or worldwide agenda—of emerging Earth System Science.

It succeeded for a couple reasons. It succeeded because it had drivers like Moore and Corell behind it. But it also succeeded because I was sitting in the power place in New Hampshire and I could make it happen. Otherwise we would have been selling it to somebody else within the university. I was the person who had the research portfolio and the financial portfolio and the support of the president. So that made an environment where we could make it happen, and it did happen.

Berrien remained the director up until I think two years ago. Corell left pretty early to be the assistant director of the [National Science Foundation]. I left in '87 to be the Associate Administrator at NASA. So we got it started, all three of us got it started, and Berrien is the one who really built it. Frankly he built it beyond even my expectations of what was going to be possible. He really did a superb job in making it the institute that it is today. But we had laid the foundations for his being able to do so. But he's the one who really carried it to success.

WRIGHT: What kind of risks were you taking? Because now you were talking about interdisciplinary studies with scientists that very much liked to be very proprietary in their work.

FISK: I think we weren't taking a risk in the broader Earth science community, because this was a national, international movement that was happening. There were certainly scientists out there that were never going to leave their discipline. But the community leaders were clearly on this new trend. The risk at UNH was a different type of risk. EOS as it's known, the Institute for Earth, Oceans and Space, was something they had never had before. On the UNH faculty, there are ambitious people that are national players, and there are other people there that really their attitude is I'm at UNH, I can't do very much, and I have this big teaching load, and any big research institution where they don't have to teach as many students is the enemy, you got all the support, we get all the work. A lot of faculty rivalries. In a place like Michigan here, such institutes are a dime a dozen, we've got hundreds of them around here. Nobody thinks twice about it. But at UNH this was and is today still the exception to the place.

So there was a great deal of animosity on the part of the broader faculty, outside the people who were determined to do this, that had to be mowed down I guess is the polite way to say it. It was a gentle sale, you don't get to run over faculty, but I think my position of authority was essential, because in the absence of that the difficult parts of the faculty would have found a way to stop it somehow. Resolutions in the faculty senate. Appeals to the trustees. Who knows? But that was a risk in the startup of this thing.

There were a couple good events that happened. They got an earmarked building where their congressman, Senator [Warren B.] Rudman to be exact, gave some earmarked money for a really nice new facility. That added to the jealousy because the institute suddenly had this wonderful new facility. It's a lovely building. But there were many things that came together to make this kind of thing happen, and it was successful.

Very recently I served on a visiting committee for EOS in New Hampshire. I may still be on it but it doesn't seem to meet very often. But I was surprised at some of the animosities that 25, 30 years later are still there. They're still there. A university like New Hampshire will still have the people that are perceived to not have—the haves and the have-nots. Rather than take advantage of all the good things that EOS could provide even to everybody, there's still a fair amount of resentment built into the system.

WRIGHT: Talk about the response that you get from not only graduate students but new faculty members moving in on this new whole concept of how to study the Earth.

FISK: In the community of future thinkers, this has never been a hard sell. They found this exciting. In some ways it's obvious when you think about it. The Earth is a coupled system. You're talking about the global warming problem. Well, how much heat is absorbed in the ocean? It's not just an atmospheric heating problem. Changes in land use pattern are going to affect the climate. So anyone who thinks at all says this is a coupled system and therefore needs to be studied as a coupled system. There are people for whom that was outside their comfort zone. But it's not a scientifically defensible position. So they can say well I don't want to play, but it’s hard for them to say that this is not the right way to do it, just because of the nature of how the Earth works.

The famous event in this field was the Bretherton committee of the early 1980s. Francis [P.] Bretherton—I think he’s deceased now, I’m not sure—had this committee that met in the early ’80s to essentially launch in some ways Earth System Science. I was on the Bretherton committee. In some ways I was there—this was when I was at UNH—as the token solar-terrestrial physicist on this. I think my own community viewed that I was supposed to defend them against whatever the Earth System Science Committee was going to do. As it turned out, I became interested in the subject as a result of that too, although it’s not my own research.

But Francis was an amazing chair. People measure sound volume in Brethertons. He’s a very loud speaker. So at some point you say, “Francis, you’re up to two Brethertons.” Also he produced something called the Bretherton Wiring Diagram, which shows how all the systems are interactive. If you ever ask was there a report of the Bretherton committee, I don’t believe there ever was one. At least I have never found it, and I was on the committee. But the Bretherton Wiring Diagram has lived forever. It is the document that basically showed how all the systems are wired together, and also it’s a diagram that basically sold the Mission to Planet Earth and the Earth Observing System when I was Associate Administrator, because all I had to do was take that Bretherton Wiring Diagram and overlay the missions that we wanted to do it, color-coded, this mission addresses these processes, this mission addresses these processes, and show that we had created a program that was going to study the system as a whole. So it was a one-stop shopping diagram that showed how the Earth worked and how NASA’s missions to address this were going to satisfy the science that needed to be done by the Bretherton Wiring Diagram. It was a very clever diagram as a result.

WRIGHT: Speaking of your time as Associate Administrator and the Bretherton report, how were your projects at University of New Hampshire impacted or supported by NASA funding? You had mentioned you had gotten a grant when you first had started there. But was there other grant funding that helped?

FISK: Yes. For anyone involved in space of any kind, it's mainly NASA. NSF provides a small amount of money. So we were all funded by NASA. In my case more the solar wind interplanetary type physics. But the Earth scientists also. The Earth Science program has other funding sources available to it. NOAA [National Oceanic and Atmospheric Administration], the USGS [United States Geological Survey] and so on. But the big bucks have always been in NASA. NASA has the largest by dollar Earth Science program in the world. Part of that is just the cost of space. But if you want to briefly leap ahead to the Earth Observing System, the concept was under development before I became Associate Administrator. The person who was leading that was Shelby [G.] Tilford, who was the division director for Earth science. Shelby was mustering his community up to be able to do this and designing this very large program that was incredibly expensive. It was off scale by anything else that was going on in the space science area.

But there was a wonderful window of opportunity in I want to say '89. It would have been '89, when the first [President George H. W.] Bush administration was under pressure to show that they loved the environment. Being Republicans, they didn't want to regulate anything, and they wanted very much to study it. We literally almost got a call that said give us an Earth science program, and we don’t care how big it is. So that window allowed us to take up everything that Shelby wanted and more so.

The run-out cost for OMB’s [Office of Management and Budget] official run-out for the Earth Observing System, was $50 billion. It’s over 30 years. It’s a long program. But it had everything anyone could possibly want. It had all the missions that we needed. It had a huge interdisciplinary program in order to connect the research and the ground-based activities as well. It had the very large data system, the EOSDIS [Earth Observing System Data Information System], built into it to handle all the data that was supposed to come back. We were selling this program in effect as a major policy shift in NASA, because it was comparable to Space Station, when you compare the numbers at the time. The agency and OMB bought it briefly, but it was outside of NASA’s comfort zone to have this in effect rival to human spaceflight. It was outside OMB’s eventual understanding of what the government should spend on these sorts of things. So the program began to be downsized almost immediately after the new start.

It got painfully downsized in the mid ’90s after I left to the point that to be blunt the nation does not have a climate monitoring system today that it requires. There’s no debate about that. We had put in place in the late ’80s a system that had it been executed would have ensured that whatever policy decisions we need to make, they would have been based on sound science, because we would have made the observations. It was destroyed in the mid ’90s. Even though the [President Barack] Obama administration has added money back to Earth science, all that did was really just stop the bleeding. It doesn’t recreate the program that is necessary to make the climate measurements that the country is going to depend on.

WRIGHT: Let me take you back to the time in May ’87 when you decided to take the Associate Administrator job. I guess my first question would be what prompted you to do that. It was a time that NASA was recovering from [Space Shuttle] *Challenger* [STS 51-L accident] and just changed Administrator, and about ready to change Administrators again.

FISK: Right. It’s an interesting story. Space has been part of my life ever since I started, still is. So I cared a lot about NASA, and the fact that it was in trouble after *Challenger* was a motivation for me to help ride to the rescue. But the actual mechanics of that was earlier in I guess it was ’86. I was approached by the National Center for Atmospheric Research out in Boulder, Colorado, NCAR, to be their director. I had been in an administrative job at UNH, and most of that job, despite the fact that I had the title of research, was really being a financial vice president. Chief financial officer of the university. So I’m drifting further and further away from my own science and from the space program. So there was a certain dissatisfaction on my side. But we were having a good time. I was enjoying it. We were building buildings and changing the university and starting the institute and so on. So it wasn’t as if I was really unhappy, but I was worried about whether I was going to end up just out of space business forever and into this financial world, which gets dull after a while.

NCAR came looking, and I thought I want to know how saleable I am. I hadn’t recruited for anything; I hadn’t looked for any other jobs. I thought let me find out. I said there’s no way these guys are going to hire me, because I’m not an atmospheric scientist, I’m space. There is a solar portion of NCAR called the High Altitude Observatory, which was one of the nation’s better solar physics research programs at the time. But still I thought there’s no way in creation they’re going to hire me. But I’d never had a job interview. I figured I’d learn what it was like.

So I go out there and I have this job interview and thought well that was fun, they won't call me. I discovered I made the next round. So somewhere along the line I had to get some references. I said okay, well, I want to see what happens, I'm going to play this thing through. So I got some references from people. I kept making round after round after round and finally there were just two of us left. I'm thinking holy moley, what have I done, what happens if they offer me the job. My wife and I had just built a house. My wife was the mayor of the town. We had settled in. My kids are growing up there. Everything's fine and dandy. I'm thinking what have I done.

So finally at the last minute they choose the atmospheric scientist. A fellow named Rick [Richard A.] Anthes, who's now the president of UCAR [University Corporation for Atmospheric Research], which is the parent organization. I remember when the guy called me up to break the bad news to me I said, "Oh, thank you." I kept thanking them for not choosing me. The guy was absolutely confused. That ended that chapter. But I had sent a message out to people that I was movable, because there were references and things like that. So when [James C.] Fletcher became the Administrator he did something that the agency didn't do under the [Space Shuttle] *Columbia* [STS-107] accident. He cleaned house. Fletcher went through and got rid of all sorts of Associate Administrators and to some extent he brought in his friends from the first Fletcher administration. But the NASA Associate Administrator for Space Science was not involved in the *Challenger* accident obviously. But they had an Associate Administrator named Burt [Burton I.] Edelson.

I never could decide whether he was really clever or the clown he appeared to be. He was one of these people that the community just felt—he wasn't a scientist. He was a person who came from the Comsat [commercial satellite] side. Burt couldn't relate to the science community. He was always cheerful, but he gave me always the impression I can't be that smart because I'm always this cheerful. He was even oblivious to insults, that sort of thing. So the science community had been complaining about him for a long time. His one claim to fame, he was Jim [James M.] Beggs's college roommate. So there was a connection there. But Beggs apparently—I didn't see this. I wasn't at Headquarters at the time. He used to abuse him. He'd come and make a presentation on the space science program. Beggs would just ream him out. So science did not consider it was well led under Burt.

I think he got caught up in this let's clean house and start all over again. So Frank McDonald was the Chief Scientist at that point under Beggs, and he was still there under Fletcher. Frank led the search committee, and Frank called me up and asked me whether I would be interested in the job. That one I couldn't turn down. I cared too much about the space agency. It was a trauma for my family, as you might imagine. I have a very tolerant wife. She goes where we go. But we literally had built a house that we had spent a year designing and we'd lived in it less than a year when I got on my power trip to Washington.

I have three sons. Two sons, they think it was the best thing that ever happened to them. The third son not so true. I think as a family thing on the whole it was a good thing to do. But it was nonetheless a family sacrifice for me to go off and do this. But that's how I got to Washington. I went down and interviewed with Fletcher. He was a strange man at that point. I don't know what he was like in his first time through as Administrator. I thought he was pretty good but there were times when I thought he was gaga too, that sort of thing. But maybe that was a style. He had some good people around him. Dale [D.] Myers as Deputy and so on.

I also think he was amazed at what he got in me, because as far as he's concerned, he was doing this to appease the science community. You guys wanted to get rid of Burt, and so we'll hire a scientist. But I don't think it ever occurred to him that I was a sitting vice president, and so his expectations for me were just this fuzzy-minded scientist who had never run anything. Well, NASA is bigger than the University of New Hampshire. But my theory of management is any time the budget is bigger than my checkbook it's a different kind of administration. So the fact that it was a $130 million budget at the University of New Hampshire, and I had a couple-billion-dollar budget at NASA, still those are just zeros at the end. The management techniques, the personnel of running an organization, they are the same. I made mistakes at UNH just in terms of things that would not work—personnel mistakes and other things. Because I had come to this from no experience in administration.

But by the time I got to NASA I had learned from those mistakes and I didn't have to repeat them. So there was no startup for me to be the NASA Associate Administrator, as it would have been if I'd come cold out of the science world. I think Fletcher was amazed that I could do this stuff, just as he would have expected an administrator to. I never knew whether he wanted to go back to the science community and say you made me do this and look what you got sort of thing. That was my experience.

WRIGHT: Did he have expectations? Or did he state his expectations for you of what he wanted from the science program?

FISK: No. This was true almost throughout my time, and all my predecessors. Basically the science program at NASA ran completely separate from the agency. You had to sell things to the Administrator and to OMB if you wanted a major new program or something like that. But there was no top-down direction. You were the CEO [chief executive officer] of science. That was true of my predecessors as well. That was the expectation that I went into the job with. It was true.

That changed primarily under [Daniel S.] Goldin where so much of the direction came from on high. So the strength of the science community, the science program of NASA, has been the foundation of this very large external community that believes it is an integral part of the program—it plans things. It sets the goals. In some ways the community thinks that NASA works for them. For the rest of the agency, the external community, the aerospace industry and all those things, they obviously work for NASA. I think many NASA Administrators have not understood the science community, but so long as it caused them no harm and brought them good news they would leave it alone to do its thing. I think that's perfectly acceptable given the different cultures between the science community and the agency versus the aerospace industry and the agency.

[Michael D.] Griffin is the one that also got that so wrong. He looked at the science community and said well you're just a bunch of subcontractors, I'll turn you on and off. That caused so much of the friction between NASA and the science community. Administrators who understand this difference and let it run successfully are really in some ways the best Administrators.

WRIGHT: As you mentioned there are always some programs. If you could talk about some of the things that you inherited and then in the midst of this you didn't really inherit but you got another Administrator who happened to be a former astronaut. So tell us how that worked when the programs and how you defined what was in existence and which ones you wanted to move forward.

FISK: There's a couple dimensions to this. Obviously the Associate Administrators are SES [Senior Executive Service], they're not political appointees. When I went to work in '87, which was what, one year before the election, under the [President Ronald] Reagan years, and I said to Fletcher, "Look, there is going to be an administration change by definition. I'm moving my family to Washington. I don't want to discover that I'm out of work the next day." He said, "Well that never happens. Absolutely never happens. Only the political appointees change." Nothing. He was absolutely right. We went right smoothly through that administration change. Dick [Richard H.] Truly is a dear friend. When he got the position he checked with me to make sure that I would stay. So it was like please Len stay. I thought I'm glad you think that way rather than please Len go. So that was an absolutely smooth transition.

When Goldin came in of course that was the end of my being Associate Administrator. That's an interesting set of stories which you should get at some point. But even he had to work to make me go away. Or anyone else. There wasn't any summarily dismissing the Associate Administrators at a change of administration. In my judgment Mike Griffin set a terrible precedent, because he did that wholesale. The rules are at the end of 120 days a new Administrator can transfer SESs, before that time he can't, and make their life miserable so they go somewhere else. It was never done before in NASA until Griffin. Then he came in and he—now there are some people that I agree with him would have been removed—they were good people to have do something else. But this wholesale 50 people are going to disappear. It set the mindset. People worried about that when Charlie [Charles F.] Bolden [Jr.] came in, and he hadn't done it to my knowledge. But it should be only if the individual is really not performing, but then it should be part of just a natural evolution. Not the fact that now you got a new boss, you have to leave. The question is are you doing your job, should you go off and do something else different. It's very individual. But this wholesale removal—it makes them political appointees basically, the SES are made political appointees.

It keeps people from taking the jobs as a result. If Fletcher had told me that was the Griffin system, I would not have gone to Washington. That would have been a foolish thing to do, the risk would have been too high.

WRIGHT: Tell me some of the challenges that you had to tackle when you first came in there.

FISK: The biggest single challenge of course was the backlog. There was a huge stack of missions that hadn't launched, because the development of the Shuttle to start with, and then the *Challenger* accident. It cost space science—I believe my number is still accurate—about $2 billion to have those missions sitting on the ground long after their projected launch date. Hubble [Space Telescope] cost $5 million a month just to watch. Of course Galileo was at the Cape [Canaveral, Florida] at the time of *Challenger*. Ulysses was at the Cape at the time of *Challenger*, and it had to be sent home, to Europe, and JPL in Galileo's case. There was just mission after mission that was just stacked up.

Now on the positive side, the nation and the administration responded very positively to NASA after *Challenger*. The NASA budget doubled after the *Challenger* accident into the early '90s. Space science actually had—formula funding. Space science got 20 percent of whatever NASA got. I'm glad you're going to reassemble this in some way. But in the early '80s when Space Station was being sold, Beggs was looking for support from the science community. The chair of the Space Studies Board at that time, the big academy committee, was Tom [Thomas

M.] Donahue. Tom is deceased now, but he was on the faculty here at Michigan, and my endowed chair position is named for Tom. Tom was still alive when I was here. He actually recruited me here to Michigan. But Tom was the chair of the Space Studies Board. He was leading the charge of the science community against the Space Station.

Frank McDonald was the Chief Scientist, and Frank brokered a deal between Donahue and Beggs in which Beggs—and I have a copy of this letter—Beggs sent Tom Donahue a letter which in effect says we'll give space science 20 percent of the NASA budget if you just keep quiet in effect. He wasn't looking for support, they were looking for silence. I can find the letter someday if you'd like to see it.

It's inconceivable to me that that happened. I find that whole event just an amazing piece of NASA history. But the consequence is that 20 percent formula lasted for easily more than a decade, where the expectation was that the space science budget, the budget for OSSA, the Office of Space Science and Applications, would be 20 percent of the overall NASA budget. You have to be very careful with the accounting when you think about that. Because someone in NASA will tell you that space science today is 30 percent of the NASA budget or some number like that. We didn't pay for launch vehicles back then, and we didn't pay for tracking, and we didn't pay for civil service manpower. This was back in the days of RPM [Retro-Propulsion Module] and the launch vehicles were done by Code M at that time, and tracking was done by Code T and so on, Code R, whatever it was. So the money was a more significant number. If you had all the other things, you're probably I would guess in excess of what the number is today, but I wouldn't be sure of that. Someone would have to reconstruct.

So if the NASA budget doubled as a result of getting to work more seriously on the Space Station and recovery from the *Challenger* accident, the space science budget doubled in the time that I was there as well. We were able to put in new starts and many other things. We had a very good time with new initiatives as well as the just keeping track of all the assets that were ready to launch but were grounded because of the delays.

I was a very successful Associate Administrator. I'm not sure it was my doing. But the environment made it possible to be very successful in selling things because we had this envelope in which to work.

WRIGHT: I'm going to use the word in the midst of the time, the issue with the Hubble happened. Tell us about your background or involvement with the Hubble telescope project prior to that or to what extent that was, and then about the situation where you were basically in the midst of trying to sort this out.

FISK: First of all when I went to NASA Headquarters, I'd had no involvement with the Hubble Space Telescope prior to that. I'm not an astronomer. I'm a space plasma physicist. So I watched it from afar. It had a lot of development problems. It cost a lot more than anybody said it was going to do, and it had some management problems along the way. So by the time I got there, there was actually a separate division that ran just the Hubble Space Telescope. We didn't keep that when the development was finally over, just because it made no sense. Merged it back into astrophysics. But it was done by the time I got there. It was in storage at Lockheed.

We had reviews as you would imagine. Preflight reviews and so forth. Of course the glories of the mirror were advertised in the course of these reviews, and one of the questions I would always like to have asked but never did was—in the middle of one of those reviews, "How do you know this mirror really is as good as you say it is?" But I didn't say that. I didn't ask. We just merrily went on that the problems were solved. The mirror was never even in debate. It was a diffraction-limited mirror. So it was ground according to spec, everyone said. In fact in reality it was ground to a smoothness that was unprecedented—not unprecedented in the DoD [Department of Defense] world, but it was unprecedented as far as anyone knew in the open world. So this was going to be the greatest mirror that had ever been flown anywhere.

So we marched merrily along. Of course by the time we finally figured out when we were going to get a launch slot to get this thing up, we hyped this thing. First of all, you wanted to justify all the money that had been spent. It was a huge amount of money. So we hyped this thing. I had interviews after interviews. I sat down with senators and congressmen, because they wanted to be seen with this thing. [Senator] Barbara [A.] Mikulski and I would have a little TV show together, TV clips talking about this thing. Advertising its virtues. I had all these wonderful sound bites I can't reconstruct now. Like at last we're going to be like the little kid who needs glasses, and finally gets his glasses, and he can see what the teacher is writing on the blackboard. Something like that, some wonderful sound bites for this thing.

So we hyped Hubble gloriously. As a sign of the NASA attitudes at that time, in the press conference for the launch of Hubble Space Telescope, NASA wanted to do it as it normally did, which was just advertise what the astronauts were going to do. I said you got to be kidding, the astronauts are going to dump this thing off, this is the biggest science mission ever—so we hijacked the NASA press conference to advertise the glories of the Hubble Space Telescope.

The launch campaign was full of all sorts of guests and things like that. We even had Harvey Hubble, who was the descendant of Edwin [P.] Hubble. Funny story with that. Harvey Hubble owned a hardware store now. So Harvey Hubble thought this was an opportunity to hawk his hardware activities at the launch. So we had to get Harvey's hardware off the page while we were launching the Hubble Space Telescope. There's these wonderful little glasses and things. I've still have goblets with the Hubble Space Telescope on the side. It was a big deal.

So the thing gets launched, and I move on to other things that I was responsible for. I hear about the fact that the first light, the focus is a little out of whack and stuff. But didn't think too much about it. This was just after I had sold—well, I had sold three new starts by that point. I had sold AXAF [Advanced X-ray Astrophysics Facility], I'd sold CRAF [Comet Rendezvous Asteroid Flyby]/Cassini, and I had sold the big Mission to Planet Earth. I had been in Europe finalizing the arrangements on Mission to Planet Earth with the Europeans. You have these remember where you are kind of experiences. I remember I was flying home from Europe, and I'm thinking Fisk, you've got this job knocked, you got the Hubble Space Telescope up, you just sold three new starts, everything is wonderful, you are terrific.

It was over a weekend. Monday morning. Not sure I could reproduce the date, but I think it was in June sometime. We launched in April if I remember correctly. June, I come into my conference room Monday morning, and there were these sad-faced people sitting in my conference room from [NASA] Marshall [Space Flight Center, Huntsville, Alabama], who was responsible for the Hubble Space Telescope.

They said we have spherical aberration. Now I'm a plasma physicist. I don't know what spherical aberration is. But I could figure out pretty quick it wasn't a good thing. They were convinced and correctly that the mirror was distorted. It was as smooth as it was supposed to be, it was just ground to the wrong shape. Something was wrong.

I remember saying to them, "Space science just had its *Challenger* accident." That's the impact of this thing. I don't remember if it was then or later but I remember saying that we were going to get measured on how we respond, not what happened. We put together a review panel almost immediately under Lew Allen [Jr.]. We got everybody that we thought was important. We weren't playing, we wanted to get the best people, and Lew was the ideal chair of this thing. They went to PerkinElmer [Inc.]. I guess it wasn't PerkinElmer at that point, it was Hughes Danbury by that point. But it was the PerkinElmer facility in Danbury [Connecticut] that built the mirror.

We had a real stroke of luck, which was the PerkinElmer people had fallen on hard times after they had done the Hubble mirror. Their business base had eroded for whatever reason. So the device which was used to set the shape of the mirror—called a null corrector—was still sitting there in the corner of the lab 20 years later. You could look at it, and you could see that there had been an extra spacer placed into—it's a mechanical device. It shoots light around and it bounces off mirrors and lenses and stuff. Then you get what's called an interferogram, which when you got the mirror to the correct shape, the lines are all straight on the interferogram. So the mirror was built to the shape that the null corrector said it should have been, but there was this extra spacer that was put in.

Apparently when they reconstructed back in the late '70s I guess, maybe early '80s at the latest, this was just a routine piece of equipment. They had a workman building this thing. They didn't have a lot of QA[quality assurance], it was GSE, ground support equipment, and they didn't have QA guys looking at the thing. The guy said oh this bar doesn't fit quite right, I'll just put a washer in. It was really that simple. No material deviation kind of report or any of those things was ever filed, and the company actually believed that they had—this null corrector was in itself a new design. They were convinced it was the best one that could have been built. There were actually a number of measurements of the mirror that were made along the way that showed the spherical aberration, and they went through other null correctors, other devices, and they said oh, this one is the best one, so we're going to believe it. This is self-delusion that took place.

But the beauty of knowing exactly what was wrong made it possible to build a correction which was very precise. Otherwise you would have been guessing. You didn't have the mirror in front of you, and you would have had to guess as to what the correction would be. It really literally is a pair of eyeglasses that were built in the COSTAR [Corrective Optics Space Telescope Axial Replacement] instrument that was placed by Shuttle astronauts and put into Hubble. COSTAR essentially is in the optical train of Hubble and with a corrective lens that's like your eyeglasses it refocuses the light the way it was supposed to be. All subsequent instruments have built the correction into the instruments themselves. But COSTAR worked for the instruments that were up there.

The agency and space science took a terrible beating. The cover of *Newsweek*, the $1.6 billion blunder. My kids were at home at that point. They were still in high school. They were living this with me a bit, as you would imagine, and they went to see this movie—it was one of these Leslie Nielsen movies. I've forgotten which one it was. But they went to see this as a diversion. There's a scene in that movie of the blues bar, and it turns out that on the wall of the blues bar are all these various tragedies, like the Hindenburg and something else, the sinking of the *Titanic*, and then there's a picture of the Hubble Space Telescope.

WRIGHT: Ouch.

FISK: Ouch. So my kids come home. God, we can't even escape this thing going to the movies. A lot of hearings, a lot of posturing congressmen, senators, wanting to have somebody to yell at.

I was the guy who was seen with it last. But we never deviated from saying—the beauty of Hubble was the astronauts could fix it. That's the thing that scares me about the James Webb Space Telescope. It's out at L2. It cannot be fixed. They've got a very complicated instrument. You really worry about whether or not it's going to work the first time.

In the case of Hubble, it was designed to be serviced. We just said okay, we're going to service it. Stop yelling. The agency did very well on this. Joe [Joseph H.] Rothenberg got his claim to fame at Goddard by being the person who led the recovery team, the COSTAR and the repair mission.

Before we discovered what was wrong we had all these wonderful solutions to solve the problem, because we put a general call out. Anybody got any ideas how to fix this thing? That was before Lew Allen had completed his discoveries. We had one which was called the toilet bowl solution, because the distortion of the mirror is on the outside, and so the idea was to put an annulus around the cover. It would reduce the amount of collecting area, but the focus would have been good. Then there was one which I put an immediate end to. They were going to have an astronaut crawl down the barrel of the Hubble Space Telescope and change the secondary mirror, it's a Cassegrain, so the secondary mirror is in the middle. I thought that's all I need, is an astronaut dying on television in real time stuck in the mouth of the Hubble Space Telescope, that's more than I could think of as problems to deal with. So that one went away. But the easy solution was the COSTAR.

Then I had to raid money from other programs to fund it, because of course it wasn't in anybody's budget. Associate administrators were given a lot more freedom then than they are now to raid. Today there's an awful lot of constraints by Congress and OMB that wouldn't have let that happen. I borrowed here and borrowed there so we could make it happen. The actual repair happened after I left NASA. But all the pieces were put in place. I do think space science's reputation was restored by the way it handled the problem rather than simply roll over and be embarrassed by it. We systematically fixed it.

WRIGHT: Now while all of these solutions were being analyzed and your team was coming up with the solution you were still in the business of space science. Were you attempting to sell new projects? Then also tell us about some of the methods that you used to sell some of those new projects.

FISK: There were two intervals. After *Challenger* and up until the early '90s you have the agency that is well supported, the funding, the budget is doubling, and we're doing just fine. In the early '90s, '91, 2, 3, it's a very different environment. You remember the first [Norman R.] Augustine report, which was on the future of NASA. The budget assumptions that they were working under were 10 percent growth to the agency every year. That was an OMB-sanctioned schedule. Well, then you get into the early '90s. George Bush, read my lips, no new taxes, economy is going bad, and suddenly all the planning assumptions that we had turn over into a flat budget. NASA has got the embarrassment of the Hubble Space Telescope and hydrogen leaks on the Shuttle all at the same time.

So the agency is in trouble. The budget doesn't support it. So the environment for selling anything big just ends, poof. On top of that all flight programs start small and grow. So when we sold AXAF, CRAF/Cassini, EOS, it was under the assumption of a continuously growing space science budget, which was what we had been allowed and told to plan to. It wasn't that we made this thing up. But when it didn't materialize suddenly you have a different world—you can't afford what you even sold, least of all think of anything new.

That's where I lost CRAF. I got asked to choose one or the other. I chose Cassini. There are a lot of I think good reasons for that. But it made the CRAF people kind of mad. Many of them were here in Michigan by the way. That was before I came here. They remind me occasionally of that. We downsized even Cassini and AXAF. They became smaller missions than the ones we sold. The turndown on the Mission to Planet Earth and EOS to a more modest program began in earnest at that point. So when we talked about selling new things, we only sold small things after that. Enhanced the Explorer program, things that kept some vitality in the community. We sold the big things basically when it was possible to sell the big things, and we didn't sell big things when it wasn't. It was as simple as that.

WRIGHT: Part of the EOS that you had mentioned earlier—goes back to one of the original things that you talked—one of its segments was data and being able to share data. Would you describe why you had such interest in that part of the EOS and its value?

FISK: Well, of course Earth Science program produces an enormous amount of data. You're looking very close. The basic philosophy on EOS and Mission to Planet Earth Observing System, which was the core of the Mission to Planet Earth, was to err on the side of completeness. The global climate problem is basically a feedback problem. You know you're putting carbon dioxide, greenhouse gases, into the atmosphere, you know you're doing that. The question is what's the response of the atmosphere, and is there any mitigating effect or negative amplification that takes place. Because if all you did was put greenhouse gases in the atmosphere, you know that the Earth is going to get warmer. There's a 20-degree greenhouse effect today just because natural things in the atmosphere. You add to the greenhouse gases, it's going to get warmer. But the question is what does that do to clouds, do they reflect things, more things, less or fewer, sunlight, how much is absorbed in the oceans, do the phytoplankton grow or do they die, there are all these feedback things that take place, many of which at that time and I think to a large extent today are not well understood.

So whenever you study a problem that has feedback mechanisms in it you want to make sure you don't miss the dominant one. You may find a feedback mechanism, but if there's something else going on over here that you weren't studying that was more important, you would miss the result that you want.

So we had a lot of instruments covering a lot of different processes, erring on the side of completeness. We were going to produce a lot of data. The difficulty at the time was that people did not believe that we could handle the data. For simple things like data storage. Of course there were all these sound bites. The EOS mission is going to produce three Libraries of Congress a day or something like that. How are you going to handle this data? All this stuff. These were legitimate questions to ask.

So the data system was constructed to essentially answer those questions and to deliver the science and tell the world how we were going to handle the science. Now a couple things happened I guess. One was the video industry took off completely separate from NASA and created all the storage media we could ever possibly want. Every now and then NASA thinks it drives technology development. This is a case where NASA drove nothing except saying oh gee thanks guys for creating all these capabilities out there in terms of video games and other things that just solved the problem. Nobody even mentions that problem anymore.

There's also a lot more use of the Internet these days than was back there, even at the time. So there are lots of things that I think the EOSDIS system may have gotten wrong—I've lost track of it but I wouldn't be surprised if it was too rigid for the flexible environment that ultimately resulted out there. Maybe they've adjusted it to be such. But mainly it was recognition that we were going to have a lot of data, and there were some big technical challenges to handling all that data—perceived to be technical challenges at the time—and that EOSDIS had to address it if this was worth doing. So the EOSDIS was created to do that.

It also had some wonderful political consequences because they had these Distributed Active Archive Centers which are called DAACs and it was possible to put a DAAC in various political constituencies. You too could have a DAAC, Mr. Gore [former Vice President Albert A. Gore]. I used to joke that I was going to put the DAACs on railroad cars and as the political landscape changed I was just going to drive them off to the next political district of substance. There was a certain spreading of the wealth of the EOS program that gave it a political base too that was useful.

WRIGHT: It also had a global attraction to it. Talk about that for a few minutes.

FISK: Well, the Earth Science program even today of course, is a global program, because in many ways other nations actually care more about global climate change than we do. You live in the Netherlands, you want to know what the sea levels are. There are certain islands which are saying blub blub, I'm going under, if you guys don't do something. It's very strong in Europe, and they have a major satellite program. Japanese as well. So it really made sense to integrate these programs in such a way that—this is a global problem, it should be solved with the technologically advanced nations of the world all contributing what they can to the solution.

So the Earth Science program of NASA has like 150 or so international agreements at any one time. Just sharing of data and resources and so forth. There are today various coordinating bodies. I believe I'm accurate on this. Things like CEOS, the Committee on Earth Observing Satellites, which is an international body which tries to coordinate the planning that goes on in various nations to make sure that the Earth Observing System will work for us.

When the US withdraws from playing its part in the way that it should, you lose your position of leadership in those kinds of organizations. But it was always part of the planning on the Earth Observing System. We didn't try and do something that somebody else was going to do and vice versa. That's what needs to happen. Because many nations, almost even emerging space nations, one of the first things they do is Earth observing. Because it's the stuff that their society recognizes is most relevant to the society. Much more so than space science for example. That's a luxury of an advanced nation.

WRIGHT: Did you find a hesitancy among the scientists about sharing data?

FISK: Not in the US. In Europe there was a move afoot at the time—I can't tell you I know what it is today—that they wanted to sell the data. But the US has always freely exchanged data. Earth science was among the best. We had some science disciplines in space science in which you get a PI [principal investigator] who says I worked my life building this thing, and I'm going to get all the good stuff out of it before anybody else sees it. But that attitude died 20 years ago. People have a legal obligation under NASA contracts to get their data out and share it almost immediately, and very few people fight that at all. There's a few old curmudgeons left over from the old years. But the rest of the world is good. We are very good in this country in that regard.

WRIGHT: Can you remember some of your first international interactions and how different that was for you from what you had done in your career?

FISK: It was not that different. I've always maintained strong contacts in Europe. I got my political spurs selling the Ulysses mission, the international solar polar mission. It started when I was at Goddard. NASA Headquarters asked me to represent, be part of a delegation that met with the Europeans at ESTEC [European Space Research and Technology Centre], which is their facility in the Netherlands, to pursue what was then called the solar polar mission, this mission to go over the solar poles. That was the meeting where we decided there'd be a US spacecraft and an American spacecraft and they'd go out to Jupiter together and one go north, one go south. Great mission.

I took on the responsibility of being the person to sell this in the United States, in a political campaign. That's the way I learned how to lobby Congress. It was one of those things that you don't instinctively know. I didn't even know the difference between authorization and appropriations committees—I remember I called [James A.] Van Allen to get him involved and told him I was running this campaign and I wanted him to see if he could exert some influence on it. I've forgotten what exactly I asked him. He said well is it the authorizers or the appropriators you want me to influence. I realized I didn't know. So I said okay, I want to learn here before I even mobilize the community. But we did all the war charts where you have the list of congressional districts and scientists who can influence things. It was all this very professional, semiprofessional done by a bunch of amateurs, political campaign to get the new start for Ulysses.

It was done in coordination with a similar campaign which was taking place in Europe to sell their half of the mission. Axford, my thesis adviser, had moved to Germany by that point, and was leading the European side of that. Johannes Geiss who was from Switzerland—I was part of an experiment for that mission that he was a PI on—was leading it. So there was this international coordination. Because you have to sell these new starts simultaneously. So that was taking place.

Being a working scientist, you travel all over the world. Our science meetings are in Japan and they're in Australia and so on. So you travel all the time, and you know the players. So I happen to believe that space science should be conducted internationally. I have no issues there except positive ones. So I just continued what I'm doing. And I had the contacts left over from my normal business prior to becoming Associate Administrator. It was no change in life that took place.

WRIGHT: But what a great start, to be able to work with your former adviser and people who could help support your effort. I was looking at the time. Is this a good place for us to stop and pick up tomorrow? Or do you want to finish a little bit more about being --

FISK: It's entirely up to you. Is there a short subject? I make very few subjects short.

WRIGHT: Well, could be. You can decide on this one. I know as part of when you were there and as part of Administrator Truly's goals you put together a strategic plan called Vision 21, and you were a participant contributing to that plan. But of course it was cut short later in '92. So maybe if you could, you could start on what some of your goals and objectives were to put in there.

FISK: There's more to that story. I'll tell you the story. This is going to sound somewhat pompous but I believe it is an accurate statement, that I introduced strategic planning to NASA. It goes back to Fletcher. When I arrived as the Associate Administrator, I said, "I need a strategic plan, I don't know how to run something without one, I don't know whether I succeed if I do something. So I'm going to develop a strategic plan for space science."

Fletcher said, "Oh, you're nuts. You never do that. Don't tell them what you want to do. They'll only give you the lowest thing on your list. Just keep it all close to yourself."

I said, "I don't know how to do that. That doesn't make any sense to me. I've done administrative things before. The university had a strategic plan. That's the only way I know how to do it." So I ignored the advice or direction or whatever it was, and developed a strategic plan. They're behind you there. You can see them. They go back into essentially almost the second year I was Associate Administrator, I'm guessing '88. It was very successful, because we sat down with the community and we drew up priority lists of things that were going to be done.

But more important, we had what I refer to as decision rules. You can't predict the budget. So we would say if the budget is this, this is what we're going to do. If we have to make a decision at a lower level—essentially this is how I as Associate Administrator am going to make decisions. Everybody could see what the queue was. So the effect was that everybody supported all missions, because they understood that if they were second on the list, the only way they were going to get their mission was to get the first guy through.

So the community ended up uniting behind the plan. Industry loved it, because they could say okay where am I going to make my investments, I'm not pursuing this big portfolio of things which may or may not happen, I know the order in which I'm going to put my investment. Congress liked it. Maybe even Fletcher liked it at the end. I don't know. But it was a very successful activity which continued every year that I was Associate Administrator.

I have forgotten the '92 plan, but I'm assuming that was basically the agency trying to do something similar. But we had done it in space science from the get-go. I was just reminded last week. I'm on one of these decadal surveys for my own discipline. Dick [Richard T.] Fisher, who's the division director, was talking about the beauty of that strategic plan of 20 years ago. Now I'm sure it's because I was sitting there. But nonetheless, it is fondly remembered as the way that planning should be done. Because it creates a community cohesion which is very hard to achieve.

One of the biggest problems of anybody who takes those jobs when they're selling a program in Congress is a bunch of other people in the community that come in from somewhere else and say I want to be put in the budget ahead of this and use their political connections to do so. Then you get the chaos we have today. The plan at least at the time did not have that chaos associated with it.

WRIGHT: Why don't we stop today? We'll pick up what happened to the plan tomorrow.

FISK: All right. It's a plan.

[End of interview]