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DR. CHARLES F. KENNEL
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is October 21, 2002. This oral history interview with Dr. Charles Kennel is being conducted at the Scripps Institution of Oceanography at the University of California, San Diego for the NASA Headquarters History Office. The interviewer is Sandra Johnson, assisted by Rebecca Wright and Jennifer Ross-Nazzal.

I want to thank you again for taking time out of your schedule to meet with us today.

KENNEL: My pleasure.

JOHNSON: For the past thirty-five years or so you've worked with NASA in a number of capacities. Can you tell us about how you first became involved with NASA?

KENNEL: Sure. My first really good paper was to analyze NASA data on the Earth's radiation belts, and this paper, which I wrote with Harry [E.] Petschek and was published in 1966, we used data from the early NASA satellites, and my and Harry's theoretical understanding of plasma physics to show that plasma processes in space actually were limiting the number of electrically charged particles that could be trapped in the Earth's radiation belt. So I was, from the beginning, a space plasma physicist.

When I came to UCLA [University of California, Los Angeles] in 1967, August 21 of '67 I turned up, part of the recruitment deal was a one-day-a-week consulting job that I had with, I think it was called then, the Space Analysis Group at TRW Systems. At that point, I was working with a gentleman named Fred Scarf, who has passed on, died in 1988. Fred was a co-investigator measuring fluctuating electric fields in space on spacecraft. The one that I remember was OGO 5, which is a very pioneering spacecraft. I got an opportunity to look at the data and reduce it in the light of theory. It was very interesting. But Fred was deeply involved in all of the work that was required to put a spacecraft into orbit and to build the experiment. So from Fred I learned my first part about how space experiments get built, and I actually consulted. You know, I watched them put the stuff together.

Then about three years later, in 1969 or '70, one of my UCLA colleagues came to me. His name was [William M.] Bill Kaula. He's also just recently passed on. Bill came to me and said that he was on something over at JPL [NASA Jet Propulsion Laboratory] called the Science Advisory Group, and this group had been put together chaired by Jim [James A.] Van Allen, my hero. This group had been put together by NASA after [United States] Congress had voted down a program to go to the outer planets called the Grand Tour. The Grand Tour was supposed to go to all the outer planets. It was a single astronomical opportunity that we could do it, and they voted it down.

So, the question was, what to do next, and we spent eighteen months working on a strategy. The strategy committee was staffed by somebody very competent, [Dr. Louis D.] Lou Friedman, who is now the president of The Planetary Society, and it was one of the best-run committees that I've ever been on. Then it was my first one, but then or later. Over that eighteen-month period, we basically arrived at the conception of the Voyager spacecraft, which I still think is NASA's greatest science experiment.

I can remember at the end of the discussion that we had, my job was to do the radiation belt physics for Jupiter. There was a great unknown. The Pioneers [spacecraft], actually, were going to fly into the Jovian radiation belts. Nobody knew the intensity of the radiation. Would they damage the spacecraft? How good were the models? Could you calculate what the risk was? And we did all of that as part of this study. But the real thing was to design the Voyagers.

At the end of the time, we had designed something called Mariner Jupiter-Saturn, and there were to be two spacecraft. It was two big Mariners and they were to go to Jupiter and Saturn. One of the key moments came when people at the end of this said, "What about the Grand Tour? Can't we go onto the other planets?" So, Van Allen asked Friedman, who was then staff director for this study, "Would you go back and tell us what it would cost to send these out to, let us say, Uranus and Neptune? Forget about Pluto."

So a month later—we met, I think, once a month—Friedman came back and said, "Well, sir, the answer to your question was [with the usual] 99 and 44/100ths percent NASA [reliability] standard, it will double the cost of the mission."

So Van Allen then went back and said, "Well, suppose we were to just put on enough consumables on the spacecraft? Suppose we were to do that? What would it cost just to preserve the option to go to these other planets?"

And so a month later, Friedman comes back and he says, "Ten million dollars."

And Van Allen looks around the table and he says, "How many of you have been on a spacecraft that failed before its designed lifetime if it was launched?" And nobody raised his hand. "How many of you have been on a spacecraft that's given useful data, if not 100 percent complete, at twice the lifetime?" Everybody raised his hand. He said, "That does it. We're going to call this Mariner Jupiter-Saturn, and we're going to put on the consumables, and if the spacecraft is still working as we get past Saturn, somebody's going to argue for an extended mission."

And that's exactly what happened, and it was the greatest experiment that I've ever been associated with. I went to three of the four encounters, and all working with Fred Scarf on his experiments. At that point, early on I began to understand from JPL how NASA worked.

So then my next involvement with NASA was about five years later, and I was on the Space Studies Board—then the Space Science Board—and I was chairman of its Committee on Solar and Space Physics. We developed a strategy, and I did a lot of the sort of National Academy type of bureaucratic work for NASA and for the Academy. And that was fine, but the really interesting debate occurred because that was the Space Science Board that approved, on its part, that NASA should go ahead with what's now the Hubble Space Telescope.

There were lots of very big discussions about information policy. The CIA [Central Intelligence Agency] wanted NASA not to fly that experiment, because if they should turn it earthward, it would reveal what the spy-in-the-sky satellites could do. There was a big fight between the astronomy community and NASA over who would manage it, and that fight ended up with the Space Telescope Science Institute being designed as a compromise. And, once again, extremely interesting. And basically my whole research career at UCLA was pretty much involved with NASA space science, not earth science.

I'm going to fast-forward a lot, because you want to know how I got to Mission to Planet Earth, too. So then the career went along on that way. When Fred Scarf died, I ended up taking his place as co-PI [Principal Investigator] on the Soviet Phobos Mars spacecraft, both of which failed. I didn't know tiddly about how to build an instrument, but he had built it and my job was to see it through the experimental phase. And I got to know NASA again with that.

I accompanied Noel Hinners in 1975; [he] took a group of people to Russia to discuss a Shuttle-Salyut mission that was a follow-on to Apollo-Soyuz, and I went on his team. So over the years I've done a lot of work for NASA, all in the National Academy, NASA committee advising sort of way. Never worked for NASA, worked with NASA.

So in about 1990 or '91, Stan Shawhan, who was then head of the Space Physics Division in the Office of Space Science, ran a major study on space physics in the 1990s, and I chaired that study for Stan. I got very rebellious, and I was very upset at that point by the trend towards gigantism in NASA experiments. They were getting huge and very difficult to manage, and the costs were running over. So we actually engineered a plan in which about one-third of all the spacecraft that would be flown would be small spacecraft. This is important, because about two years later I was in this very office here visiting my thesis advisor, who is [Dr. Edward A.] Ed Frieman, and Ed was then the director of Scripps, having moved from plasma physics. I was in this office just visiting him, student to thesis advisor, and he asked me, "What are you up to?"

I said, "Well, you know, I worked on this study with Stan Shawhan."

Stan tragically died, by the way. He had died of a heart attack, but it was basically because he had diabetes. A great tragedy.

So I was talking about that and how this opportunity to implant small spacecraft to NASA was lost. And Ed's sitting there and I'm getting more and more impassioned about this, and he says, "You know, there's a job opening up at NASA."

And I said, "Well, what do you mean?"

And he said, "Well, I have it on good account that [Dr.] Shelby Tilford is on his way out at NASA, and his job is open or will be open shortly. Are you interested?"

I said, "Well—." I had been elected to the National Academy the year before and I was at loose ends. I didn't know what to do with myself anymore, because I had achieved the research goal of my life ten years early or something. So I was looking for something new to do, and I was asking myself the question, "Well, how come all my friends are heads of—all my buddies are now heading up big Space Science institutions? What's wrong with me?" So I said to Ed, "Yes, I guess I'm interested."

I don't know, a month or so later I'm in my office and I'm working on this book, and I literally have finished this book on the word processor and typing in the dedication and all of that, and I get this phone call, and it was about four o'clock in the afternoon. It was seven o'clock in Washington. It was [Daniel S.] Dan Goldin, and he said, "Charlie, I've been talking to Ed Frieman. I want you to come to Washington."

I said, "Well, you mean to run Astrophysics or Space Science?" But eventually [that became] [Dr. Wesley T.] Wes Huntress' [Jr.] job. We talked about that.

He said, "No, no, no. I want you to run Earth Science."

"Earth Science?"

"Yes."

I said, "We're going to have to talk about this." And I said, "I'm coming to [Washington] D.C. in about two weeks."

There was going to be a major National Academy symposium on the changing environment for physics, and I was actually chairing that symposium or chairing a big part of that, at least. In any case, I said I would slip out and go talk to him. It would have to be late-ish in the afternoon. And this was held out at that big conference center out in Virginia. I forget the name of it. It doesn't really matter.

So at four o'clock, the man that I came to know as Charles came in the black car and picked me up, and I walk out of this conference, full attention of everybody. Here's this limousine picking me up, taking me off someplace. Well, it was to see Dan Goldin.

Dan's apartment then was in the Watergate apartment complex, and he wanted to meet me there. So he greets me there, and he's got on blue jeans, a black shirt and cowboy boots, and he's wearing dark glasses, inside. The dark glasses were because he was recovering from a detached retina, and he was at home because he couldn't do anything at all, couldn't even go to work. So there he was, going absolutely crazy, but he looked like a Hollywood producer and not the NASA Administrator. So this was my first true intimate discussion with Dan. We sat down and passed out Coke [Coca-Cola], and about five hours later we'd gone through everything: the need for small spacecraft, what was happening in the Space Science program, how you couple science to technology development, and so forth.

Then at the end of that time, he said, "I want you to eventually be my Associate Administrator."

And I said, "You know, this is a radical step. I'm from outside the field. I've worked in a different field. I don't know. I've been in the Institute for Geophysics at UCLA and I've been around these folks, but I've never worked in their field."

And he said two things. First, "You are going to have to make some very tough decisions about downsizing the Earth Observing System. It will be easier for you if you don't have to do it for colleagues that you've known for thirty years." And the second thing he said was, "This office deals with some of the most controversial issues that NASA has." I think he mentioned global warming, ozone depletion, desertification. "And there is a whole group of people in the

Congress who believe that the scientists have cooked up these problems and are crying wolf and they're doing it to feather their own research nests." I'm paraphrasing a little bit.

And I said, "Well, why me?"

And he said, "Well, you are a first-class scientist with an impeccable pedigree." This I remember almost word for word, "I want the world to know that science is in charge of Mission to Planet Earth."

So with that kind of backing, and when the NASA Administrator says that to you, you say, "Yes, sir. I'll do it." [Laughs]

So then it got very sticky, and the reason is that actually my predecessor was very competent, Shelby Tilford, and a very good administrator, but Dan didn't trust him, and Shelby was resisting the downsizing of the EOS [Earth Observing System], which looked like it was budgetarily required. Dan and Shelby just did not get along. So Dan makes this announcement of me as Associate Administrator, and as it would happen, it came out on the same day that Shelby was being honored by his colleagues for his contributions to the Global Change Research Program, which were very great, actually, in sort of conceptualizing it.

So you can imagine what happened. The reaction was profound and very fast, and a group of scientists from the field called [Vice President Albert] Al Gore [Jr.] and said, "Look, this guy that Goldin wants is not from the field, and this is too important to be left to an amateur." So Gore then put a halt to the nomination, and I was in limbo for about a three-month period. I had just sort of burned some bridges at the university. The university would have taken me back and so forth. It was still not a good thing. So now the question was, what to do.

So the first thing to do was to, on my part, call up all the leaders of the field and ask them what they thought should be done. And as I called the scientific leaders of the field, there was, in fact, a fair degree of agreement about downsizing the multiple small spacecraft approach and so forth. There was a fair amount of agreement about that, not 100 percent, but there was a lot.

So then the other part of it was how could you assure the world that there was a competent scientist at the top of Mission to Planet Earth who knew the field. So I then suggested that we do something that the university would do, and that is put together a little search committee and search for the director of the Science Division, which was a job that was then open, and that we would do a national search, a national informal search, if you will. And then Goldin ran this through, I guess, OSTP [Office of Science and Technology Policy], and this was all agreed to. So then there were three people on the search committee: Dan Goldin; [Dr. D. James] Jim Baker, who was the NOAA [National Oceanic and Atmospheric Administration] Administrator; and then there was [Robert T.] Bob Watson, who was the OSTP Director for Environmental Science, and former NASA person.

So then we started considering—and I was working with them full time—we started considering the candidates, and it's amazing when the White House calls and says, "Would you be interested in accepting this job?" It was the White House doing the calling. It would be surprising the number of people that said they would be kind of interested.

And we ended up with two candidates. One was [Dr.] Mark [R.] Abbott, who's now dean up at Oregon State. And we actually offered the job to Mark, but his life position was such that he couldn't have spent full time at it, and it was going to be difficult. So it finally did not work out.

But then we offered the job to [Dr. Robert C.] Bob Harriss, who did become the Director of the Science Division. Bob is now at NCAR [National Center for Atmospheric Research] in Colorado, and Bob was the card-carrying first-class scientist, you know, Grade-A earth scientist.

So people then had the sense that it was okay. And by about November or December, this conversation having taken place in Dan's apartment in late August of [19]'93, it looked like it was going to go through. They talked to Gore and whoever else needed to be talked to, and then the whole thing was confirmed.

At that point, I started getting people from Hughes [Electronics Corporation] in Los Angeles calling me up. They wanted to talk to me right away, and I began to discover some of the difficulties with EOSDIS [Earth Observing System Data and Information System], which we never solved, not on my watch.

So then I turned up in Washington [D. C.] on January 6th of 1994. The other part of it was, before that, there was a big EOS review that I was invited to as the Associate Administrator designee, and this was probably in December of '93. While I was in Washington, Dan made sure that I did the right visits over at the White House. So first I talked to Watson, who had been part of this, and Bob Watson said, "Are you sure you want this job? You're going to have to make a lot of tough decisions."

And I said, "Well, what do you mean?"

He said, "Well, you have to cut the program's budget." You know, it was like $12 billion at that point. There's no way that they were going to pay for that.

So I said, "Well, okay."

And then I went on to [Lionel Skipwith] Skip Johns, who was another Associate Director for Science and Technology, the other part of OSTP, and Skip basically said the same message, "You're going to have a very hard time here, and politically it's going to be quite a difficult job."

Finally, I got to [Dr. John H.] Jack Gibbons, who was then the OSTP director, and Jack said something totally different. He said, "Is your wife coming with you?"

And I said, "Well, no, actually she's got a practice in Los Angeles. We're going to commute."

He said, "You know, we've got a lot of people like that in OSTP, and it works for about two years. It's kind of nice, you know, if your kids are grown and so forth. You can fly back and forth, romantic weekends, little vacations and so forth. You'll make it work. But after about two years, there will be a crisis. And then if you can work your way through that, then you're home free." Two years came and the crisis came and we worked our way through it and we're home free. So that was my introduction to NASA. I then took over at Mission to Planet Earth.

The other thing that Dan did, which I thought was wonderful, was he understood that I was a fine scientist. I was not of the field. I had been on all sorts of Academy panels and stuff, but I had never managed a $1.6 billion program. They engineered an IPA [Intergovernmental Personnel Act] agreement for me, by the way, and for reasons that I don't quite know how they did it, but I had all the executive authority to sign. So that even though I was an IPA, I was in charge of all the dollars in that division, and I don't know how they engineered that.

So there I was managing this thing with no experience at formal management. So what he did was he gave me two of the best damn deputies that anybody could want. One was [William F.] Bill Townsend, and Bill is now the deputy out at [NASA] Goddard [Space Flight Center], and Bill is a true blue NASA engineer. He went to Virginia Tech [Virginia Polytechnic Institute and State University], stayed for thirty years at Goddard and finally at [NASA] Headquarters, and was as honest as the day is long and would give you a completely objective engineering view, that you have this option, you have that option, this is how the teams will work together, here are the risks. And he would do all his homework, and he was the main interface for Goddard Space Flight Center.

The other one was [Michael B.] Mike Mann, and Mike was of a different type. He was a classic NASA management manager on the personnel side and on the institutional side.

So I had an engineering manager and I had an institutional manager, and basically the three of us made the decisions, not just me. The decisions that were ours to make we made together. If it hadn't been for this two-deputy system, I think I couldn't have managed. And it worked, I thought, quite well, and my job clearly was to interface with the science community as we worked through all the issues about EOS.

Of course, the first thing I had to do is learn the science, and there I was very lucky, because, you know, if you're sitting on top of the world's largest environmental science program in terms of dollars, you'd be surprised how many good scientists come and talk to you and how many of them have to disclose not only what they already know, but what they want to do, which is more important.

And lots of people gave me tutorials. Wherever I went, they'd sit me down and give me special sessions on the science, and probably after three or four months I had enough to go on. After all, there were all those Academy committees and there were all the NASA interior committees, all of which had scientists on them, and my job was to interface with them and gradually figure out how to get the science done.

The problem was a very big one. When the global warming emergency first appeared, in some sense the country overreacted, and the first thing they proposed was a series of six huge eighteen to twenty-four experiment spacecraft, each taking up the full weightlifting capacity of the Shuttle bay. And this was going to be the twelve to eighteen-billion-dollar program to the run-out. As it turned out, those eighteen to twenty-four experiments were proven to be very expensive to integrate. Each one of them was basically on the edge of the state of the art in terms of resolution, technology, a NASA special. But they couldn't manage the engineering. It was too complicated and the costs were getting out of hand. And, in addition, we had an Administration that was committed to balancing the budget, so they weren't going to look at that.

So we had to figure out how to downsize those experiments, and I had to figure out, as we downsize, how much of the true science—I've got to be careful—how much of the science we could retrieve and get done at a lower cost. And part of the strategy was to go to much smaller spacecraft. A Shuttle launch is half a billion dollars, and so if you can go to smaller spacecraft, you go to a smaller launch vehicle, and everything scales down. In many ways it's cheaper and more robust to have small launches than one giant one, more robust because if you lose one of the five, you've lost only 20 percent of your science, and cheaper because of the launch and other—the other aspect of it was that you could focus the missions' goals more clearly and the engineering became less complicated because they didn't have requirements conflict. That was a tremendous burden.

But you had to get through all of that while convincing the science community that the project wasn't going to hell in a handbasket, or the policy community, that we weren't abandoning our commitment to key issues of global warming.

The de-scoping took place in phases, and I can't remember them all at the present time, how they actually unfolded, but we made first cuts to what are now the TERRA and Aqua spacecraft, and those were still the last big spacecraft that Office of Earth Sciences will ever build. They are billion-dollar craft each. But they were to be launched on an Atlas [rocket], and they are. Then we decided that we'd go to Delta-scale launches and go to missions in the two- to four-hundred-million-dollar range, and more of them. So at one point what had been six missions became twenty-four, but what had been twelve to eighteen billion became six in the run-out.

During all of this de-scoping, which was kind of relentless, every few months there was another call to restructure the program, the question I had at all times was two things. The political question was, when would you arrive at a budget level that was sustainable? And the other question was, how do you know, in a given project, when you've cut enough? Because if you cut out too much, then you lose reliability, robustness, and all of that. As the Mars Program, the failures, subsequently indicated, that the faster, better, cheaper philosophy had gone too far. And I was very worried about that issue, even with Mission to Planet Earth. But I think at the end of the day, when I left the office, the budget for the program was stabilized and it's remained about the same. So I think I achieved my primary goal, which is to stabilize the budget and give the world a comprehensive Earth Observing System of some form. I think I achieved that. I think people will say that a good deal of the science was kept.

Some people will say, subsequent to that time, I left [Dr.] Ghassem [R.] Asrar, my successor, several serious problems. One of them was, I cut too much money out of—we cut too much out of the budget for EOSDIS, the data management system, and particularly the data acquisition system for the TERRA satellite. And Ghassem had to struggle with the fact that we had done it too much damage, and I'm not sure he's appreciated all that I did, because he had to clean up. But he's doing just fine now.

The other thing that he did that disappointed me but may have been the right thing to do was that we were originally going to plan three cycles of missions to go for a nominal fifteen years, and basically he stopped it after the first cycle. But, of course, just like Van Allen said, these missions will stay up for much more than five years, their nominal lifetime, and they'll continue to give some sort of data over a long period of time. They will gradually decline, and in the meantime, they'll all be up and running.

At the end of three years, my IPA was up. The university wanted me back, and I went back to UCLA. I didn't want to be an ordinary professor anymore; didn't seem like such a great job after I'd been dealing with generals and diplomats and the whole big wide world. And so I applied for and got the job as Executive Vice Chancellor at UCLA, which was the number two officer there. That's a big operation. They're about a three-billion-dollar-a-year university. It was a fine job for somebody, but I had fallen so much in love with the science that Mission to Planet Earth did and I felt so committed to working with issues like global warming, in particular, that when this job came up, this is a little jewel of an institution, but it is one of the world's leading scientific institutions in these fields, I just took it, and I've never regretted it, budget crisis or not.

I think I never would have done it without NASA. NASA changed everything. I was an ordinary professor, very self-centered, very focused on accomplishing—you know, "The goals of my field are my goals," and I got to NASA, and suddenly within a few months I had to explain to the world why it was important to do this, convince them, that I had to think about how science is connected to society and how science is connected to the development of environmental policy. I had to think about how science is financed. I had to think about the politics of it. And it was kind of amazing. Here I was in middle age, and then suddenly all of this information and this new way of looking at things, all of it came in in the space of about a year. It's just amazing and just a complete transformation.

A good deal of it was Dan Goldin. Goldin had his fans and his detractors within NASA. I was one of his fans. He was very rough on people, and he was very impatient with people who resisted the direction in which he was going, and he was very rough on people that he didn't think lived up to what he thought their potential was, and rough on institutions as well that weren't performing. But since he picked me and I picked him, we didn't have that problem.

My wife, a psychologist, told me one way to handle Dan Goldin is when—he was constantly pushing for change and constantly pushing for innovation and constantly pushing me to reduce the budget and get more science out of it, and he would have these ideas, and a lot of people would immediately react. They would say, "Boss, we can't do that." And Ellen told me, she said, "Nuh-uh. Tell him that you'll study it, you'll look at it, and that you will look at every faster, better, cheaper idea that he has."

And so that's what we did. I'd bring it to my loyal engineer and wonderful one, Bill Townsend. Townsend would call up Goddard Space Flight Center. Goddard reported to me. Townsend would call up Goddard Space Flight Center and say, "We've got another one." So then they would study these ideas, and the answer would come back often—not always—"We can move in this direction. We can go this far, but beyond this, the risk is unacceptable to us."

So then my job was to take Bill to Dan Goldin and sit there and mediate, and so long as Dan saw you moving in the direction that he was interested in seeing you go, then he would relent if you said. "We can't go any further." That's the way we dealt with it. Of course, that didn't mean that three months later he still wasn't on another idea about how to cut our budget. Many of these were very innovative and involved new technology and the assumption of risk, and I was always attracted to the creative part of this. So I was predisposed to hope that these ideas would work. This is my question about when would we cut enough. Whenever Goddard sucked it up and came back and said, "We can't do this," then I would back that and just say, "We can only go so far—this now."

So I think that for me, that was the key. Although I saw him do it to other people, he never once raised his voice with me. Never once. We always had a good—and we had long discussions. There were occasions in which we would get in the NASA plane and have to go someplace, and he'd want a four-hour discussion of where Mission to Planet Earth was going. So I'd get on the plane with him, fly with him once to New Mexico to give some report, fly with him back. We had eight hours. We'd discuss the whole future of the program, and always in a very philosophical, creative way.

So I was very pleased to work for that man. I saw him devastate other people, saw it happen, and that was very unfortunate. But it never happened with me. I realize, now that I'm part of the NASA Advisory Council, that Goldin actually controlled it and was effective with about half of NASA, and that was the science side. And his greatest desire, I think, was to be known as a great enabler of science, and, in fact, an innovator in science or an innovator for NASA science, I guess the best way to say it. He's not going to win the Nobel Prize, but he wanted everybody to know that his greatest thing was to make sure that good science happened at NASA. That's what he told me he wanted from Mission to Planet Earth, and I actually believed him, and I had lots and lots of flexibility to put in little dollars into good science projects. Never a question on that.

But he didn't tame the Human Space Flight side. And to be quite frank, with all your—I know you're from Texas, but Johnson [Space Center] beat him politically, and now the present Administrator is having to deal with that issue, and it's a tough one. Even tougher. And there is the politics, high NASA politics and high government politics. And we can talk about his problematical relationship with George [W. S.] Abbey. I don't want to say much about it, except that it was certainly there and obvious to all of us, and I didn't participate very much in that.

So I think Dan did a grand job. Office of Space Science did even better than Office of Earth Science under Goldin, added some very great programs. We did well. We stabilized our budget. We solved a major political problem in a very delicate area, and nobody questions that NASA should be doing work on climate any longer. And so we did our job, and he did a fine job with those two codes.

The other thing he did, I thought, that was truly exciting, at one point when I became Associate Administrator I had a good friend who's [at] the University of Maryland. His name is [Dr.] Roald Sagdeev, and Roald was the former Director of the Space Research Institute in Moscow, and a major figure in science and was an immigrant and had come to Maryland. When Roald heard I had this job, he came to me, he said, "You know how exciting it would be if the NASA Administrator were to hold science seminars at Headquarters? That would really tell something about what this agency is standing for, and you'd bring all sorts of people in from Washington to make a big difference."

So somewhere early in my job, Dan comes to me and France [A.] Cordova and Wes Huntress. France was Chief Scientist and Wes Huntress was A.A. [Associate Administrator] for [Office of] Space Science. And he says, "I've got to give a major talk before the American Geophysical Union, thousands of people, and I want to talk about an innovative program for NASA science. I want to find a theme that will knit together all of NASA science, the science that we do in Human Space Flight, earth science, astronomical science."

So we helped compose a speech for him, which he gave, a typical Goldin speech pushing the university community to do far more, to take far more chances with technology development and get far better data and not be so conservative, and he laid out a vision. But then he was still dissatisfied. He hadn't found his theme. So I can remember—and he talked to France Cordova that we should actually plan. He wanted a theme.

So France calls us in, Wes and myself and [Dr.] Harry [C.] Holloway, who was the A.A. for Code U [Office of Biological and Physical Research] then, and France calls us into her office about four o'clock in the afternoon and she says, "We're not leaving this office till we have a plan to get that theme." So about ten o'clock that evening, we had the following plan, that the Administrator would run a series of Administrator's Seminars at Headquarters, that we would invite world leading scientists in from all over the world, and we'd ask them to talk about their field and what they thought NASA ought to do in this field.

At the end of this time, maybe once every six weeks or so, a dozen of the seminars, at the end of this time we'd have a pretty good education and so would he. And he committed to going to every one. They were put on NASA TV. He actually moderated the discussions with the scientists on stage. These were Nobel laureates and other types of that rank, and Dan was sitting there asking them questions about their field and where it was going.

Well, it was pretty clear that about three or four seminars in, that he had already converged on what he thought the theme was. That was astrobiology. We know NASA has an astrobiology program, and it is now considered one of the more innovative things that any government administrator's ever done for science, which was to set up a new discipline. And he kind of just did it, and he used his power and the bully pulpit and the fact, actually, that France and Wes and I and Harry were all kind of behind it.

Dan's view was, you know, the search for life on other planets and the ability of life to live in space, the Human Space Flights, the preservation of life on Earth, which is Mission to Planet Earth, all of that tied everything together. And so it did, actually, and it was a very innovative program. This was one of the cases where his pushing everybody to be better and to be organizing to a different configuration actually worked. It put some money behind it, and it's now a going program. They got a Nobel laureate to head it up, and it's off and running.

The next thing that happened was that he sent us to Texas Medical Center [Houston, Texas]. We actually thought that such an institute [Human Space Flight] would work very well in Texas because of the culture of Texas and its commitment to Human Space Flight and all that. We wanted a standalone institute, but working closely with Johnson. That was the thought that we had. Johnson said, "No, no, no. It's going to be like the Lunar and Planetary Sciences [Institute] on us and we've got to control it." This was not what we had in mind, and so that idea died.

Then the next idea was—I had to recuse myself from this discussion. The next idea was to go to [NASA] Ames [Research Center], which had a first-class science group, and still runs the astrobiology program, that Ames should work with Stanford [University] and [University of California] Berkeley and build up a joint university-government center. This is an idea a few years ahead of its time. But the then-Director for Ames decided that he would kill this idea by overloading it and putting 500 NASA personnel on it, when probably the right number was about 60 from each, or 60 in total or 120, some much smaller number of just scientists in a standalone institute. And I was pretty sure—but I wasn't part of the negotiation, pretty sure that that was to head off the idea of a separately governed science institute on Ames' land.

Goldin finally achieved that goal for Ames when [Henry] Harry McDonald came in, because now they have Carnegie-Mellon [University] on the Ames campus and they have a cooperative program with UC [University of California] Santa Cruz, and astrobiology now is being run as a distributive science program, a dozen university participants with its headquarters at Ames. So the issues of control got solved, not through any effort of mine. But I thought that was one of the most interesting things that we tried to do. That was sort of a service that I gave as a scientist rather than as Mission to Planet Earth. It was just helping NASA out altogether.

I don't know, I've wandered. Is there a question you want me to come back—you've got that list.

JOHNSON: Yes, we can start going through some of these. With Mission to Planet Earth, if you don't mind, we can go back and just talk about the goals and the mission of the Mission to Planet Earth and some of the more global ramifications of the science, with the weather satellites and the agreements that you formed with other countries. Did you have a part in any of that?

KENNEL: Yes, that's very interesting. NASA's Earth-observing capacity from spacecraft, because the spacecraft observe the whole Earth at its surface, NASA is especially adapted to dealing with environmental issues of a global scale, of which the granddaddy of them all, of course, is global warming.

Much earlier than that, NASA had built, and still builds, all the satellites for the NOAA weather system, and much of the thinking for the Earth Observing System was to do for climate what they had done for the weather system, which was to build a regular series of spacecraft that come out and monitor the climate, monitor the weather.

But the Earth observing, for example, through Landsat got NASA into some sticky issues like the assessment from space of the depletion of the rainforest in the Amazon and in Indonesia and elsewhere. It was one thing to make the measurement; it was another thing to do the science and make the assessment; and it was a third thing to handle the political ramifications of something in as touchy an area where there were extreme statements going back and forth on either side of the issue, from the NGOs on one side and, shall we say, mostly the countries and the logging interests on the other, and how to steer a course. Headquarters' job and my job was to steer a course that said what science says about this, in full awareness of all the political and policy conflicts, but stick to the science. So in any difficult times, the mantra that I use [is that] the best politics is "good science."

So we [NASA] did all the remote-sensing observation. There was a very innovative satellite, TOPEX/Poseidon, which measures the surface of the ocean to one-centimeter accuracy from an altitude of 1,300 kilometers. Now, with that kind of accuracy, when the water warms up, it expands a little bit, and so warm water makes a little hump on the surface of the ocean about a centimeter or two high that you can measure. So when the El Niño develops in the western Pacific off the coast of Indonesia, the water is high there, and then as the El Niño warm water propagates across the Pacific, the satellite tracks it and predicts and also sees when it's going to hit North and South America, watches the warm water propagating. So there's very innovative work.

NASA did all the remote sensing for all the environmental problems that remote sensing can be applied to. I'm pretty sure there wasn't one that they missed that was technologically within reach. I mean, there are things that they couldn't do, but if they could do it, they were involved in it. So that gave us then a role to play in every single global environmental problem: ozone depletion, desertification, ocean circulation, global warming, atmospheric pollution. And so it was a very important job.

But it was also the case that remote sensing alone and the provision of all this data typically didn't solve scientific problems. It wasn't an automatic solution. So that all of this data that was being generated by the spacecraft needed to be converted into scientific knowledge. Some of that NASA paid for. Mission to Planet Earth had a 250-million-dollar-a-year science program, which is half of the NSF [National Science Foundation] entire geoscience program, and this $250 million was spent building an interdisciplinary network of research called earth systems science, which NASA basically, and Shelby Tilford did this—NASA actually invented this concept of earth systems science, along with a few other people. But because of the perspective of the spacecraft, we could see how the pieces of the Earth fit together, and we were interested in studying the contemporary era; that is to say, as it was fifty years ago and as it will be fifty years from now and how all the systems interact with one another—the Earth, the air, oceans, land, ice. And we had satellites for each one.

But we're [i.e., NASA] only a piece of the problem and only a piece of the solution. There are other parts of the problem. You had to deliver the data to people. So that meant that we needed a very advanced system to deliver all this imagery to the users, and that was called the EOSDIS. That program was an albatross from the beginning. It was too ambitious. The idea of an end-to-end system that one contractor could build was a mistake. But nonetheless, NASA had the responsibility, which it's gradually fulfilling, of delivering giant amounts of data into the hands of scientific and practical users. That was the second part of the story.

The third part of the story was that the remote-sensing data alone and by itself, without being supplemented with data from the ground, wasn't going to solve the scientific problem. It's pretty obvious, for example, in the ocean that a satellite can see the surface, but it can't see ten feet below, twenty feet below, and people who model the ocean need to know about the currents and other behavior, temperature and so on, at depth. So you needed other measurements. That was just to get a comprehensive picture of the science. And then finally, you needed all this data to be analyzed by multidisciplinary teams of scientists in general. That was an organizational problem. So that was the scientific goal.

Now, you asked me about the international work. Clearly, the Earth Observing System, as NASA had conceived of it, was a partnership in which about one-third of all the data would be collected by either the Europeans or the Japanese, and it would all be integrated together and ideally made available to the research community and that they would guarantee to do a piece of the job and so would the Japanese. And all of this was coordinated through something called the Committee on Earth Observing Satellites, which had been formed about ten years earlier and was a group of, I think there are now eighteen countries that do Earth observing from space, and they coordinate and they try to make sure that they don't both launch an identical satellite in the same time period. If you want to do it, fill in the data gap later. And so they originally had discussions like that, that were just the coordination of programs. Even that was hard.

So then when I came into Mission to Planet Earth, they had got to the stage where they actually could list every country's program and every instrument that would be flown, and the goals of every instrument and the specifications. So that a person wanted to know what would happen five or eight years from now in the field could look at this handbook that they developed and see that the French—"Oh, the French would be getting that data at that point," or EOS will get it. And, in fact, EOS was a good part, maybe half or two-thirds, of that overall effort, if you include the foreign partners that we had made specific agreements with.

I took a look at that, and I said to myself, you know, this is all very wonderful, but this is the space community talking to itself, and because I knew, and because we all knew from our EOS experience, that that data alone would not suffice to solve scientific problems. What was needed was an effort also to coordinate the collection of the needed supplementary data from ground-based or in-ocean instrumentation. And so the question was, how could you make this happen?

So what I noticed was that as part of this CEOS, Committee on Earth Observing Satellites, planning process, the Japanese had put into place some very ambitious plans, where on each of several major spacecraft they had lots of space still open, and they were saying international collaboration goes here. So they were advertising that they had a ride available for major instruments for foreign partners. And this was a challenge, you know, for coordination.

I used this challenge, and I went to Bob Watson, who was Director of the Environment at OSTP, and I said, "We've got to respond to this. The United States has to have a strategy for international collaboration that goes beyond EOS." Bob was Chair of the Committee on the Environment and Natural Resources of the NSTC, and they made me chair of an observations sub-panel with a large interagency group on it from NOAA. NOAA, NASA, DOE [Department of Energy], NSF, and Navy were the big players, but there was also EPA [Environmental Protection Agency] and typical NSTC kind of production. We just said, "There's going to be another CEOS meeting in six or eight month's time, and we've got to have a white paper."

So basically, Pierre Morel, who was then consulting with us at NASA, and I, with some help from Lisa Shaffer, actually wrote this white paper and, of course, had it reviewed through the interagency process, and I delivered it on Bob Watson's desk at OSTP, and about three or four days afterward, before I was to leave for this CEOS meeting, I got the approval.

This was a White House policy document, and it called for an integrated global observing strategy in which the satellite agencies would work with the other international groupings to try to tie together the fairly well coordinated set of space observations with a far less well coordinated set of ground-based observing capacity and data management. And that now goes by the name IGOS [Integrated Global Observing Strategy]. It exists. It still exists. The various groupings from the World Meteorological Organization, they're talking with the satellite people. It's a burdensome and slow discussion because of the size of the global effort, but at least there's a framework of discussion in which people from the space science, space part of the earth science world, talk to the people who are dealing at the international coordination level with the ground-based part.

Even if they fail to achieve tangible coordination, the existence of this organization is an assertion of the breadth of the types of data that you will need to make credible predictions about global change in the future, and it's put that broad understanding forth at the international level. So it's still out there. They still have IGOS meetings, and there are IGOS partnerships. In ocean science, I know we're part of one.

There's an experiment called the GODAE, the Global Ocean Data Assimilation Experiment, in which NASA and French satellite data on altimetry of the ocean will be combined by data that, as it turns out, Scripps participates in collecting from little automated floats and probes that probe the inner ocean, so we'll have it above and below the surface. We've combined the two together. And then there will be data management and modeling to tie them together. One day we all believe that the combination of satellites and floats will make an operational long-term system that will monitor the health and state of our oceans. And that's probably the first tangible project that the IGOS partnership develops.

It's still to be carried out, but NOAA continues to fund the floats part of this. NASA continued with the TOPEX/Poseidon follow-on, the Jason spacecraft, and NASA and NOAA are talking about making altimetry a long-term operational program for NOAA so that this kind of data will be assured, and NOAA, at the same time, is funding the floats and have built a big international partnership in a number of countries to deploy these floats.

So now we have an actual experiment that coordinates space and in situ data into the same data stream, tangible output. It all started with a little policy paper that Bob Watson signed off on. I mean, some of these ideas were there beforehand, but the actual political, big political, science political emphasis, I think, came because the White House pushed this idea.

JOHNSON: You touched on, a minute ago or earlier, about the issue of global warming and some of the hot buttons that were going on that you had to deal with. Did you have to specifically deal with groups that were saying what Dan Goldin told you, that scientists were just making this up to get more money, more funding, or did you have to specifically deal with any of those type of issues?

KENNEL: I think the more complex dealing, I had to deal with congressmen who listened to those groups, and they would also listen to me.

But in a slightly different area I can explain a little bit how the sensitivity works out. The Chairman of the House Science Committee in those days was a very powerful gentleman named Robert [Smith] Walker, who in retirement is still prominent in Republican politics. He's from Pennsylvania. One day NASA Goddard published a new result and there's a press release, and the result said that one of their satellites had just completely reverified our understanding of ozone depletion, it was definitely due to human activity and that you could observe the ozone circulating and explain it with the measurements from the Upper Atmosphere Research Satellite, UARS.

So Mr. Walker calls me in, and on the way down Dan says to me, or before I go he says, "Is that work peer-reviewed?"

And I said, "Sure. And we didn't do the press release until the actual paper came out in the AGU [American Geophysical Union] and the *Journal of Geophysical Research*."

He said, "Okay."

So I go to Mr. Walker, and he's very nice. But then finally he says, "Tell me about that paper." I tell him a little bit. And he wants to know, is it peer-reviewed.

And I said, "Yes."

He said, "Well, I'll tell you what my problem is. Every time you guys publish a paper on this topic and on global warming, about two weeks after you publish a paper I've got people in my office that want me to do things that I don't want to do, and they're telling me stuff and I don't know what to say to them."

So the warning was unmistakable: make sure that you don't go out on a limb on any of these issues that you talk about published data. And the other warning was: don't go out making a big deal about it; just publish it. That's the way I interpreted it. That didn't stop us from writing press releases. My belief was that if you understand something and the world needs to know it and it's, in your best judgment, correct, then it's important to get it out there, because all sorts of people are concerned and need to know. But it did make me be very careful about being scientifically as honest as I could be and then try to make us as honest as we could be. And I think NASA did a pretty job, on balance.

So in the global warming area, what we had was two kinds of problems. There was a growing consensus at that time, but not yet complete, that humans were, in fact, involved in the global warming that is preceding apace. At that time, I think the intergovernmental panel on climate change was saying that there is a discernable human influence on the climate. I think in their first report they said, you know, "Quite frankly, it's a very plausible hypothesis. Every scientist will tell you that if you're accumulating greenhouse gases, the Earth will warm up, on balance. But if you're looking for specific smoking-gun data, we don't see it." That was their first report.

Then the second report, which came out on my watch, during my watch at NASA—this is a U.N. [United Nations] panel and 500 scientists, they take two years to write these assessments—the panel came out and said there's a discernable human impact on climate. And this slight change from, you know, "Quite frankly we don't see the evidence, but it's highly plausible," to, "We're beginning to see some evidence" created a major change in the attitudes in the international community, less so in the United States.

Now, there were groups that were opposed to this view, and there were two kinds. One was something called, I believe, the Global Climate Coalition, which is the group that was funded by various industrial contributions, and there were about ten or so scientists who were a part of that group. Some of them were highly reputable. There was Fred Seitz, who's still with us. He's ninety-five years old, former President of the Academy. [Dr.] Ed David, a former Presidential Science Advisor. These people were skeptics. They didn't believe in the scientific consensus.

The Global Climate Coalition also had second-class scientists associated with it. Two of them stand out in my recollection: [Patrick J.] Pat Michaels and [Dr.] S. Fred Singer. These people were on point to counter with science, or have science arguments [against] the kinds of consensus statements that the U.N. panel was putting out. They got equal time in the media, and so the media, with their ethos of finding conflict, and illuminating the issue through conflict, had amplified the opinions of a small but vocal minority to the same level as a large but not very vocal majority. So there's profound conflict, and a lot of people thought that it was junk science, and you would read this all the time, that the kind of scientific work that's being done is junk.

Then the other kind of problem—so we had to cope with that by just saying the science is not junk. And you get all sorts of people. "Why are we spending $6 billion or all these hundreds of millions of dollars a year on the Earth Observing System?" My answer would always be, "So as to remove all doubts about the science."

So then the other type of problem we had was just as tough to deal with. There was a reputable and highly active scientist, [Dr.] Richard [S.] Lindzen from MIT [Massachusetts Institute of Technology], who in his own personal approach to science is a contrarian. If somebody well known says something, he's going to say the opposite and think that he's got a good chance of being right big because of that. So he was challenging absolutely everything that the scientific community—he was challenging every consensus statement that would come out. And Richard was as tough to deal with as anybody else, but I had to respect everything he said, so I listened to him and had to balance things out internally.

Then there was a third group of people that thought that NASA was spending far too much money on science trying to be objective about these political hot button questions, and they were perhaps overdoing it in that area and missing a whole lot of stuff that was much more innovative out there, because we were sitting there trying to be honest arbiters of this question: is there human imprint on the global climate? And they were saying, "We're missing a whole hell of a lot. Besides, what does it matter? The argument is what does it matter if human beings are creating climate change or it's just happening to ourselves? It's just happening naturally. We're living on a very crowded planet, and the climate change now matters a lot more to a lot more people than it did before, and it's our job to understand it, whatever the sources are. And there's all this other stuff out there that you're neglecting." So you had to balance all these points of view. And, of course, it's a shifting colloquy, you know, it goes back and forth.

But I think that in terms of the debate, the biggest problem I had was to work with congressmen who had read the newspaper and had seen article after article saying the science was junk. Clearly it was not, but clearly, if you made a lot of extreme statements or alarmist statements, you opened yourself up to going well beyond the evidence. So you had to be extremely careful in our public statements.

JOHNSON: You talked about launching on smaller and less expensive ways instead of putting the satellites and that sort of thing on the Shuttle. But STS-66 launched during the time that you were with Mission to Planet Earth, and that Mission was dedicated—

KENNEL: Yes. There was 59 and there was 47, and those missions were—the one that I think comes from me is SRTM [Shuttle Radar Topography Mission], which is the Shuttle mission that was launched well after the work that we did. But if I recall, I remember two missions that we did. One is with the Shuttle imaging radar, SIR-C [Spacebourne Imaging Radar], which I think was 47. And then there was 59 that we did, in which we tried out a light laser altimeter and we scattered off the atmosphere. Both of these used the capacity of the Shuttle either to lift the huge antenna into orbit, in the case of the radar, or to high power in the case of the laser. These were demonstration technology experiments, and they're expensive.

My objection was, the Shuttle only gives you seven days, in those days. So then later after SIR-C had flown, Charles Elachi came to us with a proposal for a reflight, another 250 million bucks to the United States Government, less to Mission to Planet Earth, to refly the same imaging radar with a few changes. And I said to him basically—I was nicer than this, but the basic message was, "Haven't reduced all the data you've got yet. There's only seven days' worth of data. It was a technology demonstration, and if you claim success, then you will have done your demonstration. If you don't claim success, then you've weakened your chance for reflight. So if there's going to be a reflight, you've got to do something new." So then I didn't quite know what to do with that, since I'd been so rough on him. But I wasn't going to forward a simple quarter-of-a-billion-dollar request for a reflight.

So I toss it in the hands of the National Academy of Sciences, and they, in their wisdom, appointed [Dr.] John [H.] McElroy, a former NOAA NESDIS [National Environmental Satellite, Data, and Information Service] Administrator and now head of the Space Studies Board. They appointed John to chair this committee, and we and they did one thing that was very smart, we put foreign collaborators on it, because I was hoping that we could get a foreign collaborator to help offset the cost of this thing.

So then JPL was put on notice and they got very creative, and they understood that there was a big truss that was down at Johnson that they could stick on the Shuttle, and then at the end of that truss they could put an Italian radar transmitter. Then we had the big one that was already on the Shuttle, and you could use this as a three-dimensional mapper and get resolved altimetry, you know, scatter from the Earth and get height resolved images as well as across the surface. You get a three-dimensional image because of the binocular vision. That was a good idea.

So then the question was how to pay for this, and it turns out that the military had been wanting to do one of these digital topography maps for a long time. There are obvious reasons for doing it. When we told them that we could go to plus or minus 55 degrees North and get most of the populated world with this, they joined the project. So then Bill Townsend loyally put together a deal with the military, with the Italians, and with JPL, and we flew that mission. But my objection was that the science per unit dollar on that Shuttle mission just as a reflight was not worth it. The demonstration that you could do it? Yes, because that then set the way for new science. But to just repeat it, no, sir. So we didn't do it, and I'm sort of proud of SRTM [Shuttle Radar Topography Mission].

In the turnout, the deal with the military proved to be problematical, because the scientists are objecting to their classification. I guess there have been problems with access to the data, but the data exists, and in seven days they got a map that they didn't think they were going to get for another ten years.

JOHNSON: I think the tape is about to run out, so let's stop for a minute.

KENNEL: Okay.

In all of these things, you know, you never know what your own role is. It's a huge cooperative endeavor, and especially a spacecraft is the work of thousands of people. And so when you're sitting there at the top the question is: what is your role? And part of it is certainly to express to the outside world the value of what you're doing and what you think you're doing, and if you've got problems, you've got to say that as well.

Part of it, I think, is just to make sure that at least in the people that you talk to, that there's been enough discussion, enough ideas assessed and evaluated that it crystallizes out and the decision finally becomes obvious; everybody kind of agrees that we should go in this direction. I used to wait a little bit until I got pushed to make the decision, and then if somebody started pushing me, "When are you going to make this decision?" that meant they were all ready to go and that they were all in agreement. So part of it was timing.

Then I think the final part of it was—and it has nothing to do with what you actually sometimes say, but that people perceive what you stand for and they will, because they're all interested, will propose to that. So actually what you stand for and what your values are ends up determining a huge amount, but it's all very indirect and it's not anything that you do; it's the way people perceive you. Obviously you have to say things all the time, but your values shine through, ultimately, and they do get expressed in the program, and my values were to get more science for the dollar and always, if I had a choice, to favor the science, and they knew that. They could have guessed it from the beginning. Right? Just from reading the CV [Curriculum Vitae].

Now, the interesting thing about technology was that Goldin's values were new technology for new science, and I think I told you how I dealt with that. I was delighted when new technology would produce a new capability for science. But when Goddard told me that they couldn't follow through or there was too much risk, I also went for getting the data.

I think I took a middle road on the technology development, and I certainly understood that if it were the case, that—and I can give you another example. Take your Landsat satellites. When I got to NASA in 1994, NASA was busy disentangling itself from a disaster of a cooperative agreement that they had made with the Air Force to build the next Landsat satellite. The Air Force wanted to put on an extremely high resolution imager and NASA would put on a lower resolution standard Landsat imagery on the same spacecraft, and the Air Force was going to pay for a good deal of this. And they just bailed out. So then the [later] resulting mission [cost], which was called Landsat 7, is now Landsat 7, I remember, Landsat 7 was $700 million. I don't know what it proved in the run-out, but that was the number I had in my mind. In those days, the Landsat program was outside the Earth Observing System. So suddenly NASA's going to be asked to eat several hundred million dollars more in cost, and yet the Landsat user community was demanding that we continue this data stream, which had gone on since the mid 1970s.

So Landsat 7 became a kind of albatross. It was very expensive. Landsat 5 had been run by NOAA, operated by NOAA, and Landsat 6, I believe, failed. So there we were with 7 and $700 million dollars, practical-user community, not a research-user community, by and large, and a great deal of expense. So the question was how to deal with that, and we decided to take it into the Earth Observing System and it would be part of the Earth Observing System. That was thing number one.

Thing number two, I discovered that the technology for the Landsat instrument had not evolved much, and so it was a big instrument. And worse that than, it was on a gimbal that rocked back and forth like this [indicates rocking motion with hand], so they could focus it and frame it, and this required a big structure. Then the big structure required a big launch vehicle, and soon you were at 700 million bucks. And so Goldin was livid, "I'm not going to fly that damn Landsat 7. It's too expensive."

And then he went to Congress and sort of talked to George [Edward] Brown [Jr.] and several other people in Congress, and he came back and said, "They won't let me not fly it. The data is too valuable." He was a very acute politician. He listened to every political voice and tried to balance it all out. So we had to do something, and I didn't know how to deal with it.

So he said, "Well, there are these people that have got very small short focal length—." This other thing was the camera with long focal length. "Short focal-length cameras that will do the same thing and they're electronically steered. So why don't you look at those." So we called in, I believe, the Mitre Corporation, who had been doing some of these instruments, I guess. They'd been working for the black community. So they came in and gave us a presentation on all the advantages, that we could go to a small spacecraft, etc., etc.

Then we went to the user community and we sort of started talking about small spacecraft, Landsat replacement observations, and the user community got extremely nervous, "It's unreliable. We don't know how this works. You can't prove that it's the same data, and my business depends on having a continuity of data." And it was quite clear that the user community wouldn't have enough faith in the system if NASA had just announced that Landsat 8 or whatever was going to be new technology. It wasn't going to work. They wouldn't buy it, and half the people there were buying their data. So the other half now wanted to use it for science.

So then there came along the New Millennium Program at JPL, and the New Millennium Program's purpose, it solved part of this problem of technology demonstrations. These are specifically for technology demonstrations, proof to the world it would work before you flew it on a more expensive spacecraft. So the New Millennium Program came along, and our first proposal to that was for this modern Landsat replacement instrument. Then the idea was that the Landsat would fly and then this New Millennium instrument would fly at approximately the same time, and the world would compare the images. And if the user community gained enough confidence from this inter-comparison, then the next NASA spacecraft would be new technology.

That's the path, I believe, that the system was on, that they have built the Earth observing, EO-1 that's up there, and I don't know how it's working out. But that's how, in that particular case, we handled the "faster, better, cheaper" requirement. And if it should work, then you are talking instead of $700 million you're talking $250 million to do the same job. So it's quite clear that new technology sometimes does bring your cost down, but you have to get through this barrier of risk, both real and perceived.

JOHNSON: One of the other programs, I think, under your tenure, you led the development of a coordinated educational program to increase students' understanding of Earth's environment. Can you share with us some of the details of that?

KENNEL: I don't know how successful it was. It was just sort of one of the things that we did somewhat towards the end of my time there. But, you know, no federal agency has enough budgetary clout, including the Department of Education, to affect K-12 education in the U.S. It's an immense enterprise run by the localities. So everybody, Department of Education, NSF, NASA, are all adding at the margin and enriching perhaps, but it's very difficult for them to change the fundamentals. So the question for every agency involved, especially just talking about science education, what can they do that adds or subtracts or will make a difference, given resources that are infinitesimal compared to what's required.

So when I got to NASA, I went out to a trip to Goddard Space Flight Center, and education's one of our missions. So they showed me around. And at that point, there was an elderly, about to retire, Goddard scientist, who had a favorite high school in Maryland, and he installed a satellite downlink station and the kids were analyzing the data. I thought, "This is great. This is very impressive. They're really learning a lot." Then I asked the following question, "How much did we spend on this download station, and what is the annual operating budget of the school?" And when I discovered that NASA was showering on this school about the same amount per student as the school district was for a limited project, I realized that this was not sustainable. This was a very concentrated way of spending our money, but it wasn't going to affect many people, and it's a great demonstration and made an old guy feel good. He had all the enjoyment of all these wonderful students, and that's all nice, but what was it accomplishing on a large scale?

So I challenged the Education Division to change and to look at this, and they went through a strategic plan. As I recall, it was led at that time by a fellow named Mark [A.] Pine, who was one of the wonderful NASA employees, he's just the truly NASA types. Mark came out to JPL, and he's now left NASA. But at that point, he was in this, and they went through and they finally decided that the best thing they could do, and this is, I think, almost a universal conclusion, the best thing they can do is to teach teachers, and enrich their intellectual grasp and give them curriculum materials and then they would teach. And I think the other part of the story was that we could teach the teachers that live near the NASA Centers, for obvious reasons, that that could bring them in, summer programs or whatever. So I think they started on the path of developing materials that would concentrate on teachers, and I thought that was about the right thing to do.

Now Mr. [Sean] O'Keefe wants to find a way to spread this to every school. "No child shall be left behind." And the question is, can NASA leave no child behind with its limited resources. And I don't know the answer to that, but that was how far we got then. As I recall, they were going to have teacher peer-review groups review what NASA did, which I thought was the right thing to do. I think that may have been an innovation, at least for us, in Mission to Planet Earth. I don't recall all of it, but I thought the things that came out of that plan were good and they had an appropriate sense of humility about the difficulty of the task. And, you know, we spent what, 20, 30 million dollars a year on it in those days. It's a lot of money. Small compared to a spacecraft, but still a lot of money, but still tiny compared to the challenge.

JOHNSON: Currently you serve as the chair of the NASA Advisory Council. Can you tell us a little bit about your role there and some of the issues that you're dealing with?

KENNEL: And how did that happen?

JOHNSON: How did that happen?

KENNEL: Yes, right. I came to Scripps. I'd been the Executive Vice Chancellor of UCLA from '96 to '98. I came on at Scripps in—well, my first full-time day on the job was April Fool's Day in 1998. So it had probably been here about a few months after that, and Dan calls me up and says, "Would you like to be on the NASA Advisory Council?"

And I said, "Yes, sir." The NASA Administrator calls, you do it. In fact, I had wanted to be on it. I thought I could play an interesting role.

So [Dr.] Brad Parkinson was chair then at that point, and so I sat, was a loyal member, and went to a number of the meetings, as many as I could go to. And in the fullness of time, Parkinson stepped down. His term was over. And Goldin had already said to me earlier, "Maybe I'll make you Chair of the NASA Advisory Council when Parkinson steps down." But that was not to be, because there was actually a change of Administration, which I think was somewhat unexpected.

So Parkinson has stepped down, and there they were with a new Administration coming in, and it was clear to everybody that Dan's days were numbered, but that he would have some still. They weren't going to get rid of a very competent NASA Administrator just like that. They were going to take their time about it, and they had other priorities and so forth. So there was this unspecified period of time in which he would be the Administrator still.

Now, just before all that happened, [Dr. Daniel R.] Dan Mulville called me up and said, "A lot of the people on the NASA Advisory Council terms are ending and so is yours. A lot of them are rotating off, but you've only served one term and you're a lot younger than they are. Do you want to serve another term?"

And so I said, "Well, yeah, but, you know, I believe that there's going to be this rotation of the NASA Administrator, and I believe every Administrator should have the ability to choose his or her own members of the Advisory Council." I said, "So please go back and relay this answer. I'll serve if asked, but don't you want to just wait and bank these positions until the new Administrator comes in?"

So about a month later, I got a phone call from Courtney Stadd, and Courtney said, "That was a very classy answer that you gave. I've checked around in the White House and so forth, and there'd be no objective to you being interim chair. Would you serve at least for a year till we figure out what to do with NASA?"

So I went to Courtney, and I went and had another interview with him and I said, "You know, there're two liabilities that I have for this Administration that are politically sensitive. One is the global warming issue. NASA is deeply involved in it, and I believe it has to continue to do that, and so is Scripps. And, secondly, Scripps is constructing your favorite spacecraft, Triana," which was very politically sensitive because it was traceable to an idea that Al Gore had had, and they had called it presidential pork.

So Courtney checked all this out and said, "Oh, okay. It's still okay."

And so I said, "Okay, I'll serve."

Then Dan Goldin came in and actually asked me, "Would you serve as interim chair?"

I said, "Sure." Especially since they all asked me now. The only thing I did different was I thought the people on the Advisory Council were so experienced, that we should have them talk more and have NASA talk less. So I didn't want death by viewgraph to happen to the Advisory Council, and so I made sure there was plenty of dialogue on the Council. As it turned out, as a result of that and also as a result of [A. Thomas] Tom Young and the work that he did with Sean O'Keefe when they were at OMB [Office of Management and Budget], we had a profound influence, for good or ill, on the course of the program and particularly on the present course that NASA's on with regard to completion of the [International Space] Station.

Tom Young's committee surfaced. Let me give you my interpretation of what happened. The Johnson Space Flight Center, under Goldin, had managed the Station to $2.1 billion budget. It's a budget cap. They could get some extra money if they needed it by eating the lunch of the science program, but it wasn't a very big lunch. So basically they had to manage the $2.1 billion program. If something went wrong, their contingency was "We'll just delay, you know, we'll do it when we can." They never, from their view, compromised on quality; they'd just do it when they can. So their contingency was to delay.

So somewhere towards the end of the [William Jefferson "Bill"] Clinton Administration, I gather that somebody was testifying before Congress, and they were asked, "How much money is there to completion?" This is something that I think the cost to complete had never really occurred as an instrumental management tool to Johnson. But that person got up and said, "We've got about four years to go, four times 2.1. It's $8.4 billion." So suddenly the number was out there, how much money there is to complete the Station.

So, several things happened. The new President finally comes into power. It takes a little while, as you know. And within a while, a number is floated by somebody—we all have our suspicion who—the number is floated that actually the cost to complete is like $13 billion. And within a week or so of that number being floated, George Abbey is fired as director of Johnson.

Two things happened. I think the present Administration treated that $8.4 billion estimate or any other estimate as an actual guarantee, a promise to the American people, whereas it wasn't nearly that well thought out. So they began to feel that they were double-crossed, that NASA was pulling a fast one on this Administration, especially when the number went to 13.1 billion.

So then I think at that point the NASA Advisory Council had this Station management team in place through Tom Young, who's one of the most competent and loyal people that I've run across, and Tom Young was looking at the financing of Station, the management and so on and so forth, and Tom reached the conclusion that Johnson didn't manage to cost. They didn't know what it cost. So OMB could go and the Congress could go and one day one person would give one number, the next day another person would give another number, both were making back-of-the-envelope guesses based on their experience, and there never had been a formal cost control and cost assessment of the cost to complete the Station.

In addition, there's a second problem. Everybody knew that if this were a normal project, after you'd built most of your hardware, the cost would go down. So how come the Station costs weren't going down? Why were we still at a $2.1 billion budget? Where is all that money going? Who's got it? The problem had been that it was not managed as a closed-end project. People were badged to a Station project. They work for Johnson Space Center and were taken in and off the project as needed, on an as-needed basis. And, of course, for many years the Center had operated according to this entitlement. They expected still to get it. And the new OMB said, "No, you shouldn't get it, because it's project tailing off. It's got a closed end, and you've got to tell us when it's going to end."

"End? The Space Station is going to end?"

So, "The project, I mean."

"And our phase in it will end? What do you mean?" I mean, this was the kind of response that we were first getting.

So I think that Tom and the Administration and Sean O'Keefe, when he was at OMB, worked out this core complete probation period concept, which we're now going through and it's had some success. But then when I got in, this is about the time I got into the leadership role, I also realized that there was another thing that Johnson had not thought about. It had not thought about how to do operations, that its next responsibility was not to building it, but to operating it. And this required a whole different cast of mind. When you're building something and you're an engineer, you ought to be in complete control. No mistakes. Zero tolerance. And they did very well by that. But now you're operating a hotel and you're bringing in scientists and you've got thousands more constituents than you ever had before, who want to use this thing, who don't know how to use it. And a completely different philosophy of management was required, openness and not the control, and they hadn't thought about the transition.

But one of the reasons they hadn't thought about the transition was that NASA had been creatively unclear about what the goals of the Space Station are or should be. Are they exploration? Advancement of the human spirit? Preparing for the Mars trip? Doing good science? Which are they? International collaboration? That's fine, when you advocate the problem in 1985, the reason we wanted to build a space station. But in 2002 Johnson should have had some sense we'll put so many dollars on international, so many dollars on the educational mission, this is how we're going to do it, and this is how we're going to operate. No clear idea. And a good deal of that comes because we, NASA, the science community, all the stakeholders, the system chose not to define clear initial goals for the Station. If it lasts for fifty years, as I expect it will, we'll accomplish all of them. But if it only lasts for the next five, what are the ones that you're going to put your money into first?

So the Young panel said science has got to be the primary goal. The science community is saying, "There's no way that we're going to get a $33 billion charge to our account. The science isn't good enough for that." That's probably true, but it is also true that given that the Station exists, science could be a very good first use of it. And so we've gone through this remap exercise, which tried to define scientific goals, tossed out a lot of bad ones and left in about half the original program goals as being reasonable for Station, made the extreme point that if you can't go beyond U.S. core complete, you might as well forget about the science.

I think that is a generally held view on the NASA Advisory Council. Certainly Tom Young believes that if we stick with core complete, the program goes into a death spiral, because there isn't enough capacity, astronaut capacity, to do the interesting things that would justify the continued expenditure on it. Even the 10 or 20 percent, if they're going to spend 500 million a year keeping it up there, not the 2.1 to build it but the 500 million, 500 million dollars is a lot of money, and you've got to find a reason. People have to understand the reasons why you're spending it. More than that, at some point it's going to be up there and people are going to say, "Why did we build it?"

There's a whole bunch of reasons why we built it, but there isn't one that's sticking in the public mind. So, beyond the definition of the science goals, which could well define a good initial phase, the really truly inspirational reasons why you should do Station are not clearly articulated in a simple way and they are not held in the public mind. You don't have a "faster, better, cheaper" mantra for the Station, something that simple that everybody says, "Oh, that's why we're doing it." You know, it could be it's for all the world. But they haven't thought yet at that level, and it's because we, they, haven't got a clear idea of how the goals all work together, what the priorities are, and when we'll accomplish them.

So that's where the NASA Advisory Council is right now, and the biggest threat of all, I believe, to NASA is the fact that we'll have this wonderful achievement up there that nobody will understand why we built it. That would be terrible. So, somebody's got to have a little courage. Somebody's got to start talking about things. One of the main reasons of having it up there is it's not entirely clear that astronauts can live a long time in space without running into serious problems. Should we actually be thinking of sending them out into space for five years at a time? Will they come back able to walk? Will they have any bones left? I mean, there are serious medical problems that emerge with long-term space flight that need to be looked at.

In my view, the NASA macho is so strong that they do not want to admit that there might be a problem with achieving these wonderful goals of going to Mars or whatever they have set out there. So there's a serious issue there. A research step, a biomedical step, a science step.

How long can human beings carry out complex activity in space for a long period of time and what do they need to do it? It's an issue.

OMB, under the Clinton Administration, said, "Never talk to us about Mars. It's too expensive. We're never going to go there in your or my lifetime, and we do not want to see it as a precursory goal. We don't want to see anything portrayed as leading to Mars exploration, because we are not committed to it." And I think that right now the present story is like that. But on the other hand, fifty or a hundred years from now, we may be talking about mining the asteroids. We may be talking about a lot of things for sustainability reasons as well as just human exploration reasons.

So I think it's important for our culture now to answer the question, can you live a long time in space and what does it take? So that's the main reason for doing it. Is it worth $33 billion when we're fighting a war on terrorism, and when it only costs $2 billion to do the [Human] Genome Project and, and, and. There's no answer to those questions. But if we could, over the next few years, clarify the goals of the Station, then it would help. It will require a major new attention by Johnson Space Center on philosophical things, its relationship with the broad global society on, you know, really understanding what it means to promote international collaboration beyond a group of a few thousand engineers, because soon that thing's going to belong to the world. And if the NASA Advisory Council can play any role in making this change of viewpoint, then we will have done our job, at least done the job that one little council of twenty people or thirty people can do, if we can just push it a little bit.

JOHNSON: Well, I think we've kept you past the time we said we would.

KENNEL: Well, is there anything more that you're dying to ask or are you just dying? [Laughter]

JOHNSON: No, not at all. I thought I'd ask Rebecca and Jennifer.

WRIGHT: I have one question, speaking of the Station. As a scientist, but now as Chair of the Advisory Council, how do you feel that the cancellation of the CRV [Crew Return Vehicle] to enable more residents on the Station will affect long-term science projects?

KENNEL: Obviously not well. You mean the CRV?

WRIGHT: The crew rescue vehicle.

KENNEL: Yes, the best solution, of course, would be to have a specific crew rescue vehicle. But this is another area where courage could be applied. I'd look at it this way. We don't talk about the risks of being launched on the Shuttle. We don't know what they are. We know that if you have a conventional launch vehicle, about one in fifty fails. And the Shuttle, being nonconventional, is much more reliable than that, and it could be one in a hundred, one in two hundred, one in three hundred per launch. Who knows? But it's definitely highly risky. So just getting up there, the astronaut accepts a certain amount of risk.

Now, if something goes wrong up there, the question is, are they willing—there are two questions. If they spend six months up there and you look at the failure rate and so forth of subsystems and whatever happens, how long does it take them in orbit to accumulate as much risk from the in-orbit occupation and de-orbit as they had on the launch? Okay? So that's some number. Secondly, they're all brave people. They're willing. They're going to do it. They're like the original explorers. And they're not like your average person on the street who's very risk-averse.

So the other thing is, are there other strategies? Can you have a safe haven? Should you perhaps keep a Shuttle ready to go to bring them down and accept the risk that there's a couple of days' delay while they're up there, and, you know, accept the risk that there won't be a completely immediate catastrophic failure?

So I think that there's a middle ground in there in which we look for solutions that don't require five or six billion dollars, but mitigate the current situation and ask how much risk do you accumulate with that mitigation strategy. Do the absolute best you can and then just ask them to accept a risk comparable to what they're already doing.

I don't know whether that all works out in an engineering sense, but I do think that the astronauts are very courageous people. I've never met one that isn't. They will accept the risk. I think that our sense of risk is very different now than it was prior to 9/11 [September 11, 2001]. We wanted to fight an entire war without a casualty in 1998. But now we've got 3,000 of them. So that people's acceptance of risk, perception of risk, may be different now.

Again, it gets back to the inspiration of the goals. I think if society were to see that this was for some really higher goal of society, then, of course, society would accept the risk that the astronauts are willing already to accept. So I think there's some room in there. Of course, technologically it would be better, much more secure, I think, to have your own vehicle. But I haven't seen the alternate strategies for how to mitigate in the absence of the CRV.

WRIGHT: Do you have anything, Jennifer?

ROSS-NAZZAL: I don't think so.

WRIGHT: Through your career, is there any one thing that stands out in your mind that you feel is your greatest accomplishment in dealing with NASA? Obviously your career with NASA isn't quite over yet, or your dealings with NASA.

KENNEL: Oh, I guess the thing that I point to is the first thing I got on, just the Science Advisory Group, the small role that as a young scientist I learned to play in the planning of the Voyager, that great experiment. That gave me the greatest pleasure as a scientist.

And then I think later on it was just the incredible personal growth that occurred in about a one-year time. I had to learn how to deal with Congress and the Defense Department and the White House and a big bureaucracy, and learned how to manage. What I used to say to people was that I got a master's in business administration from Dan Goldin and a master's in public policy from the U.S. Congress all in one year.

So it was the complete change in growth that I couldn't run this, couldn't take responsibility for a major scientific institution until I had done that, and I hadn't realized until after the fact how well I was supported, and they understood exactly. It's a good organization. They understood exactly, actually, what to do with me, and the two deputies were there for a very good reason, and it was just wonderful. Nobody else had two deputies. I was lucky. And that was because they understood what my shortcomings were, and they wanted to capture whatever scientific strengths I could bring.

JOHNSON: We thank you for talking with us today.

KENNEL: Thank you.

[End of interview]