NASA SCIENCE MISSION DIRECTORATE ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

DAVID MORRISON
INTERVIEWED BY SANDRA JOHNSON
MOFFETT FIELD, CALIFORNIA – MAY 9, 2017

JOHNSON: Today is May 9th, 2017. This interview with Dr. David Morrison is being conducted for the NASA Headquarters Science Mission Directorate Oral History Project at Ames Research Center in California. Interviewer is Sandra Johnson, assisted by Jennifer Ross-Nazzal.

Thank you for allowing us to come to your office and talk to you today. I want to start by talking about your background and how you first became interested in becoming an astronomer and what led you to your work with NASA.

MORRISON: I was interested in space and astronomy clear back into grade-school level, as I know a lot of young people are. I never outgrew that. So I ended up going to the University of Illinois [at Urbana–Champaign] and taking a physics degree, but with a real interest in astronomy and spending time at the observatory there.

I then went to Harvard [University, Cambridge, Massachusetts] for graduate school, in astronomy, and was fortunate to have Carl Sagan as my adviser. He was an inspiration in all sorts of things, including especially interest in planetary science.

When I went to Harvard I had no particular focus on planets. I assumed I would study galaxies or stars, but Carl Sagan really hooked me on planetary science. I particularly remember the title of the public lectures he gave, "Planets Are Places," which reflects the idea that he's written about many times, that we start out thinking of the planets just as points of light in the sky, while as exploration happens they become real places. We can ask ourselves what it would be like to stand on the surface of a planet.

That inspired me, and obviously that directed me toward NASA because NASA was in the process then of supporting ground-based telescopes as well as space missions. I remember watching from Harvard when the first Mariner [interplanetary probe] flights went to Venus and to Mars.

When I completed my degree at Harvard, I was hired by the University of Hawaii [at Mānoa] Institute for Astronomy, a place with strong NASA ties. At that point NASA was just completing the construction of an 88-inch telescope at Mauna Kea Observatory [Big Island of Hawaii]. And not too long after I arrived in Hawaii they began work on the IRTF, the NASA Infrared Telescope Facility, also on Mauna Kea. Ultimately I ended up being director there for four years.

I spent 17 years in Hawaii as a research astronomer with some teaching, but primarily a research appointment, and I focused on planets and infrared. One of the things that made Mauna Kea such a remarkable site for an observatory was its unique capability for infrared observations. The Earth's atmosphere, and in particular the water vapor in the Earth's atmosphere, is the greatest problem for infrared because it absorbs and emits. To get above a third of the atmosphere, which you are on the summit of Mauna Kea—where it's dry and cold air coming straight across the Pacific, not much water vapor in it—in effect opens up the infrared window.

We were pioneering in making infrared observations from the summit of Mauna Kea, and most of our work was directly relevant to NASA programs. We were almost entirely funded by NASA. Thus I had connections with NASA all the time I was at the University of Hawaii, even before I considered actually working for NASA.

JOHNSON: I thought it was interesting that you said when you got to Harvard and then Carl Sagan was your adviser. Did his reputation precede him at that point? What was that experience like, having him there as someone to inspire you?

MORRISON: Carl Sagan's reputation didn't really precede him for me. Remember, he was just a young guy then. He only had his PhD two years from the University of Chicago. He'd been out in California the past two years, beginning his integration of understanding life science—what we now call astrobiology—with astronomy. He arrived as an Assistant Professor at Harvard just after my first semester there.

Sagan introduced a graduate-level planets course. A few of us took it, and some of the graduate students scoffed at it or went and listened to a lecture or two and said, "No, this guy isn't serious." But I thought he was an inspiration, and so did a few other people, including Jim [James B.] Pollack and Jim [James L.] Regas and Joe [Joseph F.] Veverka, who all became Sagan's students as they progressed through their graduate career.

JOHNSON: You stayed with him even after he left for Cornell [University, Ithaca, New York].

MORRISON: I went with Carl for two years to Cornell University, the problem having been that Sagan was denied tenure at Harvard. Very common. The great majority of people who come in as assistant professors, at least in the sciences, don't stay. But it was traumatic for me, because I was in the middle of my thesis with him, and suddenly it was announced that he was leaving.

There was some effort among the Harvard astronomy faculty to get me to drop him as thesis adviser and change my topic, even though I was halfway through my thesis. I resisted that and Carl resisted it on my behalf. Ultimately it was agreed that even though he was leaving Harvard and going to become a faculty member at Cornell, I could continue my Harvard degree and finish with him, and that's exactly what I did.

JOHNSON: You mentioned that when you went to Hawaii the funding was coming from NASA. But you also at some point, I believe it was 1981, became an Acting Deputy Associate Administrator for Space Science on an IPA [Intergovernmental Personnel Act] appointment. Was that the first time you actually went to a NASA facility and worked? Or did you do something else before that?

MORRISON: I actually took two IPA appointments to [NASA] Headquarters [Washington, DC]. One was for two years in about 1975, within the Planetary Science Division. Not a senior position, primarily as a staff scientist, someone who'd interact with the regular NASA people. It was a lot of fun. It gave me a valuable perspective on what NASA was about and probably set me up for the later position.

I was a participant in the Voyager [robotic] missions in the late 1970s; I was in California at JPL [Jet Propulsion Laboratory, Pasadena, California] for the Jupiter encounter, and then for the Saturn encounter, which were just extraordinary experiences. We were getting the information live in real time from the spacecraft. At Jupiter a new image arrived every two minutes. Every one of those images showed things that nobody had seen before. You just couldn't tear yourself away from sitting in front of that screen and seeing new images come in one after another. I was having a great time, and the same thing was true with Saturn, although the images came in less frequently because of the larger distance.

Andy [Andrew J.] Stofan was the new Acting [Associate Administrator] for what's now the NASA Science [Mission] Directorate. Andy Stofan had been Deputy, and Tim [Thomas A.] Mutch had been the Associate Administrator of Science, using today's terminology. Tim Mutch was killed in a climbing accident in the Himalayas on vacation, and his body was not recovered. Under those circumstances, NASA wasn't permitted to advertise for a new Associate Administrator for a while. Andy Stofan, who was a wonderful guy, a longtime engineer operating with NASA going back to Glenn Research Center [Cleveland, Ohio] as well as Headquarters, was not a scientist. He wanted a scientist to come be his deputy temporarily until they could straighten out this whole thing.

Stofan invited me to come for less than a year to NASA Headquarters as the acting Deputy Associate Administrator, which was a high-level position, which I really didn't have much management background for, but I was an enthusiastic space scientist. That worked out just fine for me, and I think it worked out fine for everybody. At the end of that period I went back to the University of Hawaii and NASA appointed a new real Associate Administrator for the Science Directorate.

JOHNSON: Let's talk about Voyager for a little bit and any other involvement. I know you said that you got to be there at JPL when the pictures were coming in, but did you have any other involvement in the program itself as far as the work or anything that was being done with any of the research?

MORRISON: I didn't have any administrative responsibilities with respect to Voyager. I also was there for the Viking missions [to Mars] with even less official sanction. But as an active planetary scientist I was invited to come and spend some time at JPL. I will never forget the excitement of planning for that first landing on Mars in Viking.

The Mission Director, Jim [James S.] Martin, was responsible to safely land for the first time on the surface of Mars. He held meetings every day with the whole science team, as we were beginning to get the data as we were approaching Mars and then going into orbit. I sat in on them, although I was not quite a member of the science team. He appointed three wise planetary scientists to provide him advice on where to land: Carl Sagan and [Harold] Mazursky and Toby [Tobias C.] Owen. Every day they studied whatever data had come in, and there was a general meeting discussing what new information there was on finding a safe landing site.

The problem for Viking finding a safe landing spot was that we didn't have high-resolution surface images like we do today. An object a meter or two or three meters across, like this desk, could not be seen from orbit. Yet if the spacecraft landed on it or landed with one foot on it, it would be wrecked. The challenge was to infer what the smaller scale roughness and topography was from the images and other data from the spacecraft which did not have the resolution to see these details on the surface.

It was really a tricky question. The landing on Mars was delayed a couple of weeks while this process went on, and it was really exciting. Afterwards I got to know Jim Martin better and he told me the interesting thing—he said he had spent his last 15 years working on this project at Martin Marietta [Corporation]. Everything he had professionally was invested in this mission. He said, "If this landing was successful, I knew I would become a vice president of the company and make a lot of money. If it were unsuccessful I knew I would lose my job." That was just another perspective on how important it was to make this first landing on Mars a success.

In the case of Voyager (which was a flyby, not a landing mission), there wasn't any dramatic decision making of that sort, but there certainly were lots of interactions with the spacecraft to set up the observing sequences and look at the right things at the right time. It quickly became apparent that the moons of Jupiter and the moons of Saturn were equally if not more important than the planets themselves, with this tremendous variety of geology, for instance, on the four Galilean satellites of Jupiter.

The process was so interesting because it condensed the whole normal scientific activity into such a short time. Literally, we would get the images from one day, we would discuss them among ourselves on the imaging science team, and there would be a news conference the next morning at which some of the best images would be shown, and if possible some interpretive things would be said about them.

Just one example. Was Io [moon of Jupiter] going to be heavily cratered or not? Then that same day more higher resolution images would come in and might change the conclusion. The next day you would have another press conference and say, "Ah, we thought there were going to be craters, and now we see there aren't. Now we have a whole new idea of how the geology of Io is working."

This took place over a period of a week or so as the spacecraft approached Io. In ordinary science there might be a year interval between the updates, but with Voyager they were updating every day or two. It was really a fascinating experience, and it brought the scientific process alive.

JOHNSON: We can talk more about it later, because I know you've dealt a lot with educating the public about astronomy and astrobiology. But Voyager, you're saying those news conferences were happening so soon, and the pictures that the general public was seeing. Talk a minute about the reaction of the public and what you were seeing as compared to earlier in your work. Did it change as far as the interest and people wanting to see more, and being more excited about NASA at that point?

MORRISON: One of the great things about most NASA missions, and it certainly was true of Voyager, is that they took lots of pictures. The first Mariners didn't even carry cameras. That was because there were a couple of old-time astronomers who said, "You can't tell anything from a picture. We want quantitative information, we want to test a hypothesis. So we will suggest a hypothesis and design an instrument to make precisely that measurement."

Sometimes that worked and sometimes it didn't. In the case of the first Venus mission [Mariner 2] the primary question was whether the surface was hot, whether the radio emission that we were detecting was coming from a hot surface. So they designed a very simple experiment to look straight at Venus, look to the side, look back again. I don't mean look with a picture, but measuring the thermal emission, and that would tell them the answer to whether the radiation was coming from the surface or the atmosphere. It happened that the answer was found by astronomers on Earth while the spacecraft was en route to Venus. Without a camera, we didn't have much opportunity for serendipitous discovery.

Once our spacecraft started taking pictures and spectra with broadband instruments, there was this tremendous opportunity for discovering things that were never thought of. That was exciting for the public as well. It had been true in the Apollo missions, and now for the first time we were seeing pictures of Mars and of Jupiter and Saturn and their moons. It was an easy sell for the public. It made the solar system come alive.

It went back to Carl Sagan's "Planets Are Places." They were no longer just points of light. They were geological entities with all kinds of processes going on. Sagan was one of the best exponents of sharing these discoveries with the public. That was when he was going on Johnny Carson [*The Tonight Show*] with his examples. He would come in with the latest Voyager pictures, two or three of them, and show them to Johnny. Johnny would hold them up to the camera. That was a way of reaching a lot of the public.

The press conferences also were really dramatic sometimes and we had as many as 60 or 70 members of the press at JPL. It was a great big science party. Often the press focused on some individual scientists, not just Carl Sagan but others, and they interviewed them and they put them on TV. I think it was probably second only to Apollo in the kind of excitement generated in the public about NASA programs.

JOHNSON: It was interesting too, Carl Sagan being a scientist. Like you said, at that time he was appearing on Johnny Carson. He was on different shows, he was being interviewed. He was becoming popular, almost a rock star not to overuse that term. I don't think we had seen that in science before.

MORRISON: We certainly hadn't seen it in the planetary science arena. It was while the Voyager Saturn mission [flyby] was happening that Carl finished *Cosmos* [*A Personal Voyage*], his [1980] TV show that he'd been working on for more than a year. Suddenly that was added to the mix, because now *Cosmos* was shown weekly.

I went out with Carl to dinner near JPL shortly after the show started being shown. We were in a little quiet dark Thai restaurant in La Cañada [Flintridge], sitting in the back, talking to each other, and a guy came over from another table and said, "You're Carl Sagan, aren't you?" and how wonderful *Cosmos* was.

Carl turned to me after he left and said, "This is happening to me suddenly all the time." Once you have been in people's living room on their TV, they feel they know you. They can come up and talk to you. It was a mixed blessing. Sure enough, within a year or so Carl was traveling with a nanny for his children, with people to chauffeur him around, and he had received so many death threats at Cornell University that they really went overboard to protect him. His name was taken off all the university directories, was removed from the directory of his office building, it was removed from his door. The only way into his office was through his secretary's office. And Cornell posted a full-time policeman at his house in Ithaca day and night for about a year. So that is the downside of suddenly becoming famous.

JOHNSON: I guess what he was talking about, I assume people found it threatening.

MORRISON: Apparently. Or they're just kooks. If somebody becomes famous they attract this sort of thing. It changed Sagan's life very much, and for the better or the worse I don't know. But he wasn't nearly as accessible to the public, to students, and to friends after that, because of these safety concerns.

JOHNSON: You became the Chief of the Space Science Division at Ames in '88. What made you decide to leave the university and work for NASA full-time?

MORRISON: My career at the University of Hawaii had partly drifted into science management. I was the Director of the NASA infrared telescope, the IRTF, for four years. I spent two years as the Acting Vice Chancellor for Research at the University of Hawaii at Mānoa. This interested me. I was also constantly involved with NASA missions, NASA committees, flying back and forth. It's a long way to fly from Hawaii to Washington. It's pretty long flight to get to JPL, but to Washington it's no fun. You essentially have to spend one night on a plane to get there.

All these things joined in to make me feel that if I was going to spend all this time with NASA and NASA missions, maybe I should actually work for them. I knew Larry [Lawrence] Colin, who was the longtime head of the Space Science Division at Ames. I knew him, I liked him, I'd been to Ames a number of times. When I found out Larry was going to retire I decided I would apply. I got in touch with him, he introduced me to the leaders of NASA Ames, and after a couple of interviews I got the position with a minimum of paperwork.

JOHNSON: Let's talk about Galileo [Jupiter mission]. It launched [in 1989] right after you became the Chief of the Ames Space Science Division. Were you working early on in Galileo? You said you were on committees, you were doing things for NASA. Had you been working on that project also?

MORRISON: I had been involved in the planning of the Galileo project. Following on the success of Voyager at Jupiter, that was obviously the next thing to do. It was a terrific mission, but it had its problems. First of all there was a delay of several years because of a [1986 Space] Shuttle [*Challenger*, STS-51L] accident.

There were problems with the funding. We had gone through an issue with the [President Ronald W.] Reagan administration when they tried to cut the planetary program; at one point they had actually proposed turning off Voyager after the Saturn encounter and not even collecting data at Uranus and Neptune.

It was a very long struggle, but I did become involved as the Galileo Program Scientist at NASA Headquarters for a year or so at a very important time, because that was when we were trying to finalize the plan for the instrumentation. I was privileged because of my role at Headquarters to actually give the presentation to the senior management of what the proposed payload would be.

Selecting a payload for a planetary mission is a delicate balancing act because you have many more good ideas and good instruments proposed than you have either the money or the mass capability to carry. Trying to put together a balanced payload is not just picking the best instruments, but trying to make sure you have a balance of imagers and spectrometers and UV, ultraviolet and radio and all of this.

I was involved at that point in the original selections. Then afterwards back in Hawaii I was able to get a position as Galileo Interdisciplinary Scientist as sort of a reward for having done this work in setting up the mission itself.

JOHNSON: As you mentioned, there were a lot of delays. A lot of it was even political influence because of different constituents and congressmen wanting their areas to have those contracts, and of course the *Challenger* accident. But it finally did launch after you were at NASA full-time. Ames was responsible for the Jupiter atmosphere probe. If you would talk about what the expectations were as far as that probe mission, and then of course the Galileo mission itself and the timeline and what happened with it.

MORRISON: Galileo was designed to be a comprehensive years-long study of the Jupiter system. You had three groups of science instruments and approaches. One was the study of Jupiter itself. Another was a study of its magnetosphere and magnetic field, all of the issues of the space around Jupiter. The third was the satellites [moons].

I was primarily interested in the satellites. We really wanted to follow up on the discovery of erupting volcanoes on Io, which at that time was unique. We knew of no other place beyond Earth with active volcanoes. We also wanted to determine what the heck was going on with Europa, which was smooth, with no craters visible in the Voyager images.

Galileo was very ambitious. Part of the way you achieve that is you put on modern instruments which have high capability and high data rates. You put in a radio transmission system with high data rates so you can get back images every few seconds, or whatever the data may be that you're trying to collect.

Then when Galileo was launched and we discovered we couldn't open the high-gain antenna, most of that was lost. We had all the same instruments, all the ability to point, all the tour through the system, but we could transmit less than one percent of the data that were collected back on Earth. That was a terrible blow, just gutted the mission.

But yet we knew we still had these supercameras and superspectrometers. So we faced a very complex question of sequencing, of deciding exactly what was the most important data to collect and return to Earth. If you had a place like Europa and knew you could only get high resolution images of one or two percent of the surface, how are you going to decide which one or two percent to do? It was exciting but very difficult and tiring. You knew you had this one opportunity to use these terrific instruments but you were so limited in the data you could get back to Earth. .

The Galileo atmospheric probe, which I had not worked on, became interesting to me after I came to Ames, since it was designed at Ames. It was coasting on its own through this whole mission. It had been sitting on its own before and during the long delays before the mission was launched. No one knew if it could survive this long hibernation.

There was a big project here at Ames on the batteries, how to select long-lived batteries. There was a special room that was filled with batteries of various ages and they'd take them out and monitor how they were doing on the ground, because you couldn't monitor them on the probe.

Finally we reached Jupiter [December 1995]. It was an unforgettable day with everybody gathered at Ames for the event. I knew the people who'd been involved in building the probe. They put much of their career in this, they'd waited for many years.

The probe is a one-shot thing. It was designed to broadcast all of its information while it was descending through the atmosphere, but most of that data was collected on the orbiting spacecraft and sent back slowly. The initial direct-to-Earth transmission was just going to be a simple yes-no signal. Did it work? Was it in the Jupiter atmosphere? Was it taking data or not?

We all gathered in the Ames cafeteria, everybody standing around extremely nervous just waiting for that beep or no beep. I was a little shocked when the person who had been the project manager said, "I think there's a less than 50 percent chance it will work" after all the delays and all this inability to monitor how it was doing along the way.

Then that beep came back and boy, the crowd erupted in celebrations. The probe turned out to be extremely successful. The only problem with the probe—and it really isn't a problem, but it didn't comply with our expectations—was that it went into a very, very dry part of Jupiter's atmosphere, into a hole sort of, where there was very little water vapor, and it didn't encounter the water clouds that it should. But it went deep into the atmosphere and gave us a lot of information about atmospheric composition and cloud structure. It made us think, "Boy, we wish we'd had more than one probe so we could compare with other places."

JOHNSON: Was the information that was coming back from Galileo, because of the antenna problem, actually lost? Or was it just slower getting what you wanted?

MORRISON: The cameras for instance were capable of taking high data rates and recording them on a tape recorder. But there's no point in doing that, because you couldn't play it back. So yes, in principle Galileo could collect the data, but if you can't retrieve it on Earth it might as well not be there.

JOHNSON: Around that time, you were in charge of the Ames Space Science Division. That included astrophysics, planetary science, and exobiology. I want to talk a little bit about exobiology and NASA's role in that. You hear that term in the early '60s, then astrobiology became something in the early '90s. What were the differences between exobiology and astrobiology, and why was the term astrobiology substituted for exobiology?

MORRISON: Being the Chief of the Space Science Division at Ames was a really great position because we had many of the best scientists in NASA. Many of them were Carl Sagan's students. There seemed to be a link between Sagan and his students and they would come here for a postdoc [postdoctoral fellowship].

I got to know Hans [M.] Mark, who had been the Director of Ames earlier and by this time was in Washington, in the Department of Defense [Director of Defense Research and Engineering, and later Secretary of the Air Force]. He told me once, during a visit to Ames, "Dave, you have the best job in NASA for a scientist. There is no better place than the Space Science Division at Ames. Being the Chief of the Space Science Division at Ames is really the top."

Of course that made me very happy, and I told that story to my successors in that job, including the current Chief [Steve B. Howell] who just came on board a couple months ago. I went to him and I said, "Hans Mark said you have the best job in NASA for a scientist."

I was less familiar with the life science part. I had learned a little bit from Carl Sagan, but I had been primarily involved in ground-based telescopic observations, in spacecraft observations.

The issue of exobiology and later astrobiology is complex because you're asking questions about things that are not even known to exist. Ultimately the objective is to discover extraterrestrial life, but we have no evidence whatsoever even today of extraterrestrial life.

When exobiology was formed, the goal was to study the biology on objects beyond Earth, and in particular Mars. A lot of exobiologists were involved in the Viking landers on Mars. One of the most elaborate instruments in the life detection system was built here at NASA Ames.

But they didn't find life on Mars. We were actually on the surface with Viking so they could take some of the soil, collect it, put it into miniature wet labs basically, and do tests on it to see if there was any sign of metabolism. There wasn't.

It was not a for sure negative. We only looked at the two places on Mars where the Viking spacecraft had landed, and you could only take a scoop of soil within a meter or two of the spacecraft. You could only dig a few centimeters deep. You could hardly characterize the whole planet from that. But the conclusion that NASA came to, and the science community, was that Mars didn't have life on the surface. It didn't have the kind of life we hoped.

Carl Sagan was on the imaging team on Viking. He even went so far as to say, "Look, we must try to get any possible information." He said, "I want to look at every image taken of the surface from the lander spacecraft to see if something might walk past." He did this, and nothing walked past. In addition to searching for microbes, why not? But that's typical Carl Sagan.

Ames had been involved in a number of things in exobiology, going clear back to Apollo. Ames had been one of the places that got the early Apollo samples and analyzed them for evidence of organic compounds, searching for life or precursors of life or dead life or whatever it might be, fossil life. With the Mars missions coming up, Ames was naturally involved in Viking.

But there was this long pause of 20 years after Viking, before we went back to Mars. There really weren't any other obvious candidates for life until Galileo got data on Europa. Exobiology wasn't as vibrant a field as we had hoped it would be. There were people back before Viking who thought there really would be life on Mars, and we would have a chance to study it. It wasn't working out that way.

The perspective changed with the shift to astrobiology. First of all, we were no longer just looking for life beyond the Earth. We wanted to understand the history of life on Earth. We wanted to understand more about the origin of life so we could make inferences from Earth history as to what might have happened other places.

I was part of that discussion. We thought we had an opportunity, and we also wanted to reinvigorate the parts of NASA that could conceivably help us determine something about life and habitability outside the Earth. So we changed the name.

It would have been possible just to morph exobiology, but I think it's a truism in NASA, perhaps in the government in general, maybe in society in general—if you want to do something new, give it a new name, a new branding, and you get new money. If you just want to make an incremental change in an already existing program, you're unlikely to get any money. So that was part of the reason for going with astrobiology, but also we really wanted to give astrobiology a much broader footing in science.

The opportunity came because of the widely publicized discovery that there was some evidence for fossil organisms in a Mars meteorite [Allan Hills 84001], the famous Mars rock. It was a controversial finding from the beginning. But there was no question that the image showed things that looked like tiny microorganisms, like you would get from terrestrial microorganisms.

So what to do? NASA agonized for months over how to publicize this. The people at JSC [Johnson Space Center, Houston] who'd done the work were very anxious to release the information. NASA Headquarters and NASA Public Affairs were worried about what would happen and what kind of reaction there would be.

The JSC scientists did publish a paper in *Science* [academic journal] after going through the rigorous peer review one normally has. When NASA organized the press conference they insisted on having, in addition to the proponents who had done the work, an outsider scientist who was not part of it, who explicitly at the press conference criticized it as, “Look, we’re not sure. There are uncertainties here, here, here, and here.”

I thought it was a very responsible job that NASA did. It wasn’t a gung ho sort of publicity, but it still took off. The President [William J. “Bill” Clinton] had a discussion from the White House on how exciting this was. The Vice President [Albert A. “Al” Gore, Jr.] put together a committee of about 20 people that met for a day with him, including theologians, philosophers, as well as scientists, to try to explore the importance of this discovery.

That was part of what set us up for astrobiology, but there were other things. We for the first time had evidence that Europa had an ocean with perhaps more water in its ocean than we have in ours. There were thoughts about Titan and its possibility of supporting life on a satellite of Saturn. We were seeing the beginning of the genetics revolution and the ability to study life in much more detail at the genetic level. It just became a very exciting thing.

Dan [Daniel S.] Goldin was the head of NASA then, the NASA Administrator. He really got interested. For a while he was going around telling all the NASA managers they should go take a biology course or read a biology textbook. He was distributing biology college texts and saying, “We’ve got to all learn about biology.” I don’t know how many people actually did it, but that was the in thing at NASA for a little while. The White House had been very supportive. On that basis, and with Dan Goldin’s enthusiastic support, we decided to do introduce the new term, astrobiology.

Then the question of the content of this new discipline. I pulled together a fascinating workshop on the science of astrobiology with about 100 people here at Ames who met for two or three days. We managed to get five Nobel [Prize] laureates to come too—some were already at Stanford [University, California] and [University of California] Berkeley, so they didn't have to come very far, but some came a long way—to try to see how we could formulate this science of astrobiology and a program that was affordable that could get somewhere.

We ended up with a very broad definition. We said, "We're trying to do three things. We're trying to understand where does life come from, what's its distribution, and what's its future?" I remember taking that list to Dan Goldin when we were trying to get his approval, and I think we phrased it very simply. "Where did we come from, are we alone, and where are we going?" He said, "That's far too trivial-sounding. If this is a real science program you've got to phrase it differently."

So we went back and phrased it in scientific jargon, which I didn't think was nearly as good as "where did we come from, are we alone, and where are we going?"

JOHNSON: It's kind of funny coming from the man that did "faster, better, cheaper."

MORRISON: Yes, right. We officially adopted the terminology Golden wanted and unofficially we still used the simpler terminology. But the important idea was that astrobiology didn't deal with only extraterrestrial life. It could be involved with life on Earth. How did life on Earth start? That's the where did we come from, what are the general questions of habitability. Then we can apply that to Mars, to other places in the solar system, eventually even today we think of it in terms of exoplanets. So it is a very broadly based field.

A lot of the research that was supported was dealing with microorganisms on Earth. Some scientists went to the most difficult climates on Earth, the most Mars-like, like the Atacama Desert [South America], or the [McMurdo] Dry Valleys of Antarctica.

One of the most exciting discoveries was in the deep [Mponeng gold] mine in South Africa. You go very deep, it's very hot, you're kilometers below the surface, and some water leaks out. Like any mining people, they have to pump the water away. But they permitted scientists to go down two kilometers below the surface, collect that water, and they found microorganisms in it. Microbes in water that was very deep in the Earth, probably had no connection with the surface, couldn't be supported by photosynthesis.

The ultimate conclusion was that this deep ecosystem actually lived as a result of chemical changes caused by radioactivity in the rocks. If that's true, that gives us a whole different perspective on life. That says you can have life many miles below the surface of a planet. It didn't matter whether the surface was clement or the Sun shone, whether you had surface water, any of those things, it could be something deep down.

That doesn't tell us how it originated, and that's another question. Even if life could be supported deep below the surface, there's no saying that it could originate there. And it would be very hard to find on another planet, because we're not going to dig mile-deep mines on Mars in order to try to do that. But this discovery gave us a broader perspective that maybe astrobiology really mattered, and that we could think of life in ways that were very different from just the microorganisms on the Earth's surface.

The original roadmap I wrote for astrobiology also mentioned the societal parts. We said this is something that is very closely tied to issues of planetary protection, because if we find life elsewhere, we mustn't bring it back to Earth if it could possibly contaminate our planet. Also there's forward contamination. If you're going to Mars to search for life, the last thing you want is to find it, and find that the life you found was terrestrial microbes from a previous mission. There was a big debate about whether the missions that had already landed on Mars, including several Soviet missions that had crashed on Mars, might have contaminated the planet already. You have issues now of how you can transport organic material or life from one place to another.

We also acknowledged that this was a topic of potentially wide interest to theologians, to philosophers. What would be the implications if we found life elsewhere? I always thought that concern was interesting but perhaps overblown, because a century earlier it was generally thought that there was life on other planets. [Percival L.] Lowell, canals on Mars. It was just said, "Well sure, if there's a planet Earth, and then there's Mars and it has water, it's going to have life too."

That conclusion didn't destroy our civilization, that didn't mean an end of religion. It seemed to be taken for granted. But now after many many decades, nearly a century of saying, "No, there isn't life elsewhere or at least we don't know of any," it was thought that it could have a profound effect on society to discover life on another world.

I don't think that's an argument for or against doing it, but I'm just saying that when I wrote the first Astrobiology Roadmap we at least thought about some of these broader implications of astrobiology, and particularly of finding life elsewhere.

JOHNSON: Speaking of finding life elsewhere, the search for extraterrestrial intelligence, which NASA was giving funding to as early as 1971, and then there was a branch within NASA eventually in the late '80s. Talk about the SETI [Search for Extraterrestrial Intelligence] program, and the relationship with NASA and your relationship with that program. You mentioned earlier about changing names and maybe getting funding, and I know at some point they changed the name to High Resolution Microwave Survey, which didn't help save it evidently.

MORRISON: You're quite right; that new name doesn't just trip lightly off your tongue. I was fortunate because when I arrived at Ames Research Center and became the Chief of the Space Science Division, the Division included NASA's SETI program. SETI experts like Jill [C.] Tarter and Barney [Bernard M.] Oliver were in my Division.

It was amazing to have Barney Oliver there, because he had been vice president of Hewlett-Packard [Company] and was one of the most respected engineers and scientists anywhere around Silicon Valley [California]. Absolutely an intellectual giant, and here he was supposed to be reporting to me, young guy as head of the Division. I certainly didn't ask him to obey all the NASA rules and regulations, which he thought were ridiculous anyway.

But the point is we did have a SETI program of a dozen or so scientists and engineers, and we were developing the receiver systems, which are the critical part of it. You're searching for very faint radio signals, you don't know what they are like. You therefore need an extremely broadband receiver that's sensitive to all different frequencies and cadences that might be coming in. That's expensive and it really profited by being in Silicon Valley where the most advanced electronics of that sort were being designed.

We were a part, in fact the leading part, of the NASA SETI program, which was scheduled to begin on Columbus Day in 1992, the 500th anniversary of the [European] discovery of the New World. Two parts—observations being made at Arecibo [Observatory] in Puerto

Rico and at the Deep Space [Network] facility [Space Flight Operations Facility] near Pasadena [California].

There were two NASA SETI programs, and they were done largely independently. JPL operated the one that would ultimately be putting receivers on the Deep Space Net, and we here at Ames had the arrangement, to use part of the Arecibo time for our part of the survey.

It was cool. I was down at Arecibo on the day when we first turned on the receivers. I think it was extremely exciting because you realized that if you could find any evidence of intelligent life, you could leapfrog over all these other questions. You didn't need to know about the life chemistry, life origin, whether there was water on the planet. If they were out there and they were intelligent and they had enough technology to transmit a signal that we could receive, they're living in a whole different world.

I think you probably know that [science fiction writer] Arthur C. Clarke's third law was that any sufficiently advanced technology will be indistinguishable from magic. That's kind of what we thought. We are not necessarily going to interpret what comes in, and we may never be able to. Two-way communication will not be possible, but just finding clear evidence of an intelligent technological civilization elsewhere would be extremely dramatic, perhaps more dramatic than finding microbes on Mars.

SETI has a lot of public appeal, although in general I think the public doesn't understand that you won't have a two-way conversation. Almost certainly any signal you receive would be from hundreds of light years away, and so the delay time—even if you could transmit back and they could transmit to you—would be many centuries. But that doesn't mean you can't learn from it.

The example that's often given is we cannot have a two-way communication with the ancient Greeks, but we've learned a lot about them from reading the things they wrote. You at least might be able, if you could decipher it, to get a tremendous amount of information, even though you didn't have a two-way signal.

That depends of course on whether there's anything out there transmitting, and now you ask, "Well, why would they transmit? Would they just put out a beacon that would reach millions of stars in the hopes that maybe someone would answer?" Or would you have to overhear their own internal transmission?

The question then of course is, "Well, what about us?" We have been transmitting for 70 years at levels that might be detectable at a nearby star. Our FM [frequency modulation radio], television, and especially our radar signals, which are the most powerful signals Earth transmits.

SETI is a daring thing to do because you have absolutely no way of estimating the chances of success. But you do SETI because you think even if the chances are very low, it might be successful, and it could be so revolutionary that it would be silly not to do it.

Now you worry about analogies. Is that like going into Las Vegas's [Nevada] gambling area and putting money into something that has an extremely low chance of winning? Of course a lot of people do that. Anybody who buys a lottery ticket is taking that kind of chance because the chances of winning $1 million on a lottery are extraordinarily low, but they do it anyway. By analogy, surely we could put 0.1 percent of the NASA budget into a SETI program and that would be justifiable, and it could be revolutionary.

We started observations at Arecibo, but just a year-and-a-half after the program began, in 1993, the Congress terminated it. And didn't just terminate it by cutting the money. They forbade NASA to have anything to do with SETI, told NASA we could not even help private organizations interested in SETI. For years there were annual questions and sometimes visits from staffers from the Senate who wanted to make sure that we hadn't somehow surreptitiously helped the SETI Institute or some other organization with federal money.

We were able eventually to surplus some of the electronics and let the SETI Institute have it. But in general we were watched carefully to make sure we didn't do this horrible thing of spending a little bit of NASA money on SETI.

JOHNSON: When it was canceled, why do you think they wanted to make sure NASA never had anything to do with it? What do you think that reaction was caused from?

MORRISON: I don't know. At the time I thought about it. I actually don't remember the details. It was Senator [Richard H.] Bryan who proposed this cut on the Senate floor and made comments about little green men on Mars, and we've already sent a spacecraft to Mars and there aren't anybody there, and so this is a waste of money. He clearly had it out for the SETI program. One powerful person in the Congress can have this kind of influence, so he could send his staffers out every year to Ames to make sure we weren't doing it. Most of my work was not directly impacted, but there were some people at Ames who lost their jobs. The [non-governmental] SETI Institute set itself up, and of course has carried on. I later on spent four years with a half-time IPA appointment heading the Carl Sagan Center at the SETI Institute, so I like those people a lot.

But even at the SETI Institute very few of the people or funding goes to the SETI project. The SETI Institute is primarily an astrobiology organization, and more than 90 percent of their money and their staff are involved in doing all kinds of astrobiology research that has nothing to do with SETI. The SETI part just struggles, partly because there's no federal funding, and it has to be funded by private donations. You would think that in a time like this when there are so many extremely wealthy people, private funding would be available, but it's been a constant struggle to even keep a single SETI program on the air.

JOHNSON: Going back to communicating with the public—Carl Sagan, of course his [1985] book *Contact*, I think for a lot of people was their first exposure to SETI and to radio astronomers and that whole field of study.

MORRISON: And the book and film were both very impressive, and the fact that they could shoot it [1997 film] at the [Karl G. Jansky] Very Large Array [New Mexico], that is so photogenic, and the fact that they had a highly photogenic scientist doing it. *Contact* was a powerful book and a powerful movie.

JOHNSON: Yes, and I think that conflict in there was probably a reflection of what was really happening in the world, the conflict between faith and science.

MORRISON: And the fact that the first response of the federal government to this discovery is to send guys with guns to say, "Stop, what are you doing?" Partly that again is the misapprehension that we're transmitting. We're just listening. If a signal is there, we either detect it or we don't, but it's not as though we're advertising ourselves to the universe. Or putting it another way, we are advertising ourselves inadvertently through leakage of our radar for instance.

It's also true that the space involved in searching is so large and the distances are so great that with our present receiver capability we would not be able to detect the leakage from Earth even at the distance of the nearest star. For us to detect a signal, we have to assume that somebody's going to beam something, a beacon, a strong transmission. If we're just looking for an analogue of ourselves, we couldn't detect our twin at Alpha Centauri [closest star system]. But we could detect a beacon, if there were one.

The other thing that I think comes out of that is frequently stated but maybe not fully understood, that it is extremely unlikely that we could find any other civilization at a similar level of technology to our own. If we're going to receive something, it will be somebody far advanced, or very different at least.

That makes SETI exciting, but it also means that you're very unlikely to get a two-way conversation going. They are going to be indistinguishable from magic in Arthur C. Clarke's way of saying. Sufficiently advanced technology can't be told from magic.

JOHNSON: In '96 you became the Director of Space at Ames.

MORRISON: Director of Space was a wonderful title. I still have a few business cards with that on it. I joked that it was a terrible responsibility: if I didn't do my job every day the planets would fall out of their orbits, the galaxies would explode. But then NASA Headquarters did a reorganization and created a division called the universe, and so they had Director of the Universe at Headquarters.

JOHNSON: Something to aspire to, right? Let's talk about your role in the development of the NASA Astrobiology Institute, which was established in 1998 I believe.

MORRISON: When we developed the concept of astrobiology and wrote the [1997] Astrobiology Roadmap, we already had in mind to have an Astrobiology Institute. This is a different kind of institute from what people mostly had at that time, which we call bricks-and-mortar institutes.

The idea traditionally would be if NASA is going to take up a new exciting field like astrobiology, then they should build a building, a laboratory. They should hire people. Or maybe they should contract it to a university. It should have a single organization. Take the Lunar and Planetary [Institute] in Houston as an example.

We said, "No, we don't think that's necessary." First of all it takes a long time to build a building and hire people and create a new institution. But more to the point, if we're on the cutting edge of science, then we're going to presume that the experts are primarily in universities and they have graduate students and postdocs. So it's much more intelligent and cost-effective to learn how to utilize the talent in the universities than for NASA to set up its own organization.

Hence virtual institutes. The NASA Astrobiology Institute was the first one. Later we established the Lunar Science Institute (where I was the founding Director) and then the Exploration Science Institute.

I think NASA's virtual science institutes have been very successful, and I'm proud to have been part of the concept not only for astrobiology but as a way of doing business. It is obviously less expensive to accomplish things through existing organizations like universities, and if you want a university to do some serious work in astrobiology you probably don't have to pay for their faculty, just give them enough to have a lab, to bring in postdocs, teach students, something of that sort.

The idea was to solicit competitive proposals for five years for organizations to become part of the Institute, although they would remain in their own place. We asked for those organizations to be multidisciplinary and geographically diverse.

We wouldn't just take one university in one town and say, "They're going to do it." The goal was to increase our footprint so that NASA could have a much greater influence if it worked through the academic universities. We wanted to try to have a NASA astrobiology group in as many different universities as possible. I think it's been very successful. We developed the idea at Ames of a virtual institute, and we have been the people who have created virtual science institutes, and tried to work out a way for it to make sense.

You need to be competitive. You need to have a way of selecting the very best teams, through 50-page proposals that are peer-reviewed. But then you have the challenge that once you've done that, you would like these organizations to collaborate and work together. So they start competing against each other, and then ideally you create mechanisms for them to work together.

One example is to fund graduate students that go to more than one institution. That is, they may be a graduate student at the University of Washington but then they will spend a semester or a year at Berkeley, say. So that you will get connection between two different academic organizations.

The other problem that we've had to face is one I think that everybody has dealt with who's tried to do virtual organizations. How do you get people to actually communicate, and better yet collaborate, when they're in two different places thousands of miles apart? I think most people have concluded, as we have, that it is very difficult to do if you have too many people trying to interact simultaneously.

When we have meetings of the different teams of the Institute, we have a dozen or so people online at once, and that's about the upper limit with modern technology to get conversations going back and forth between people. In fact, it's actually easier if you don't have that many people.

The other thing I think everybody now realizes is it's much more successful if you've already had direct contact with people. You can collaborate using video conferencing, telephone, email, but it's so much easier if you know the person. So you want to have opportunities to physically bring everybody together once or twice a year. Then remote communications work much better. That's been the experience at the Astrobiology Institute and the later institutes.

We had a review after 10 years of the Astrobiology Institute by the National Academy [of Sciences] at NASA's request, and they gave us very high marks on productivity. Not only that we've published a lot of papers, but the large number of those papers that had authors from different institutions or different teams. So we think that was quite successful. The Institute funding comes in a block grant to fund a particular institute, and once the institute has been established the team doesn't have to send in proposals over and over.

We were really fortunate to be able to hire Baruch [S. "Barry"] Blumberg as the first Director of the Astrobiology Institute. We already had set up the Institute, and [G.] Scott Hubbard had been the Interim Director for a year, and we had gone through the process of selection of the teams. We were just ready to actually hit the road running. Barry Blumberg, who was spending time primarily at Stanford University, even though his appointment was on the east coast, was really interested. He's one of those Nobel laureates who came to the workshop that we had to develop the astrobiology roadmap. He thought astrobiology would be fun. He had a successful career. He was already 70 years old, he was world-famous. He had a Nobel Prize for the discovery of the hepatitis B virus and the development of the countermeasures, a vaccine in particular. He said that probably he was responsible for saving at least 10 million lives from hepatitis B. Every year he would be invited to China, and they would fete him and practically have parades because China had such a problem with hepatitis and he had made such a difference to the health there.

Barry Blumberg was a wonderful guy and we all loved him and we all really profited by his example. One of the things he did from the beginning, that I've always thought was very important for understanding the way the institutes work, is he'd tell all the PIs [principal investigators}, "Look, you've each written a detailed proposal explaining what you would like to do. If at the end of your five years you have accomplished all you said you would and nothing more, you will have been a failure. You are not expected to do everything you said you would do. You're senior scientists, we trust you. The whole discipline is moving forward. You need to develop new ideas, you need to work together. You'll be judged on the basis of your total output, not whether you did what you said you would do in the proposal."

Some of the PIs said they'd never heard that said before.

JOHNSON: That is an interesting way of looking at it. It's an interesting idea of the virtual institutes. I can see how it would be something that would translate well. It would be something that would work well within the system because of funding issues with NASA, bringing in all these different groups.

MORRISON: Yes. Of course you have to justify to NASA that the money is well spent and that it wouldn't be better spent responding to individual proposals. We think we can demonstrate that. But it's not the usual NASA way of doing business.

JOHNSON: The Lunar Science Institute was renamed the Solar System Exploration Research Virtual Institute, which is a real mouthful.

MORRISON: Hey, you said it! I just call it the Exploration Science Institute.

JOHNSON: It's simpler that way.

MORRISON: Yes. The Lunar Science Institute was a direct response to NASA's interest in returning to the Moon with humans, but even more the sudden explosion of scientific interest in the Moon. Lunar studies had largely lain dormant for 25 years since Apollo, although work continued on Apollo data, especially the samples.

Suddenly this was changing. The Japanese had a satellite in orbit around the Moon [Selenological and Engineering Explorer (SELENE)], the Indians were sending one [Chandrayaan-1], the Chinese were sending [Chang'e 1].

The U.S. had several missions. GRAIL [Gravity Recovery and Interior Laboratory] to look at the gravity fields, the Lunar [Reconnaissance] Orbiter, which has gathered more data from the Moon than all the other scientific missions to that point added up, including extremely high definition images, and such sensitivity that they can photograph the inside of permanently shadowed craters just from starlight on it.

The Russians were interested, although they didn't quite get their mission up. Here at Ames we had the mission [LCROSS] to crash a spacecraft into the Moon, into one of the permanently shadowed craters near the pole, to see if there was water ice, which was very important for the long term perspective of lunar resources.

All those things came together in a Lunar Science Institute which was doing brand-new things just the way the Astrobiology Institute had. Many scientists had almost forgotten the Moon and suddenly here it was and we especially were encouraged by the students who could vote with their feet. The number of graduate students who went into astrobiology or went into lunar science, the number of theses on astrobiology or lunar science. The fact that the students formed their own professional organizations—LunarGradCon for instance was a Lunar Graduate Student Conference held every year. They did some interesting things.

Because it's multidisciplinary, they were willing to step up to the fact that they needed to learn multiple disciplines, to actually take courses in different departments. In order to stimulate the conversation, because science is full of jargon, they did some innovative things. When they would have the student meetings, just students talking to students, everybody in the audience would be given a red card. If the speaker said something or used terminology they didn't understand, anybody who didn't understand would put up the card. If only two or three people did, the speaker ignored it. But if half the people put up the card, then the speaker knew that she should stop right then and define those terms. It really helped give them the idea of being able to talk to a broader audience. That surely would carry over also to public communication.

JOHNSON: Speaking of public communication, you were one of the first scientists to warn the public about asteroids or near-Earth objects and the possibility of them hitting the Earth and having obviously consequential effects. Let's talk about that, in 1989, you co-wrote *Cosmic Catastrophes* [with Clark R. Chapman].

MORRISON: Yes. This is something I have devoted a great deal of energy to in the last 20 years. I find it fascinating scientifically, but also in terms of understanding risk and how to communicate risk with the public. When Clark Chapman and I wrote the book *Cosmic Catastrophes* in about 1988, we devoted two or three chapters to what we thought was one of the more plausible catastrophes -- impact of an asteroid with the Earth.

That of course was something that had become popular because of the revolutionary discovery of the Alvarez team [Luis W. and Walter Alvarez] that it was an asteroid impact that had caused the extinction of the dinosaurs at the end of the Cretaceous Period, 65 million years ago—one of the largest mass extinctions in history.

A few years later, when one of the TV programs discussed 10 things that could kill the Earth, asteroid impacts was number one on their list, because it had actually happened, and we know that it could happen again. When they did a similar program five years later, asteroid impact was not number one, it was number two. Number one was global warming. The argument there was not that it had happened millions of years ago, but it was happening now.

This book by Chapman and Morrison, *Cosmic Catastrophes*, a simple little trade book, didn't sell terribly well, but it was okay. We were proud of it. I was going around giving some talks, and I was invited to give a talk in Washington at the Congress to the space group there, primarily of staffers, but a few members. I gave this talk about *Cosmic Catastrophes*, about the asteroid impact in particular.

Clark Chapman and I had carried out the first quantitative estimate of the risk. That is, what is the chance that it will say on your tombstone that you were killed by an asteroid? That meant looking at the whole range of asteroid sizes and what kind of damage they could do.

We recognized that a meteorite, a small chunk of rock falling from the sky, is not a big risk. Things have to hit with cosmic velocity in order to have enough energy to produce a huge explosion. If I were standing somewhere and 10 feet away a meteorite hit, the worst it would ever do is just splatter me with mud. But if something could come down with cosmic velocity, it would produce an explosion.

We had to decide the question of when an impact would not just be a local problem but global, as the end-Cretaceous extinction event had been 65 million years ago. Obviously to produce a mass extinction you have to affect the whole Earth.

We had researched that, and we for the first time had a quantitative estimate of the risk. For instance, one of the things that we said that was widely publicized was that your chances of dying from an asteroid are similar to your chances of dying from an airplane crash. Airplane crashes don't actually kill very many people. The risk is very, very low. But everybody who's been on an airplane bouncing along has had a white-knuckle experience or something to make them realize, at least think about, their mortality. We were saying, "Think about your mortality in terms of asteroids."

I gave this talk to the congressional people. They were interested, and they—also encouraged by the AIAA [American Institute of Aeronautics and Astronautics]—said, "We think this should be seriously studied." They wrote into the 1991 NASA Authorization [Act] that

NASA should carry out two studies of the hazard: the risk of asteroid impacts, and the technology that could be used to protect ourselves."

I was appointed to head the NASA study on the risk. I was given free rein to do what I wanted. So I brought in half a dozen people from other countries as well as U.S. experts. We spent about a year debating it and ended up writing what we called the *Spaceguard Report* [*The Spaceguard Survey: Report of the NASA International Near-Earth-Object Detection Workshop* (1992)].

We understood that the mitigation approach must include surveys, because we knew we had to find the objects. If you couldn't find the object, no other form of defense or mitigation made any sense. We wanted to define what that survey would be and what its requirements would be, what size asteroids should be found, how many of them.

We named it the Spaceguard Survey after Arthur C. Clarke, who in his [1973] novel *Rendezvous with Rama*, at the very beginning where he was describing such a system for finding incoming objects, talked about a fictional impact in northern Italy that had wiped out Florence and Venice and killed millions of people. It said the people of Earth after that developed the Spaceguard program to guarantee that they would never again be taken by surprise with an impact like this. We thought if Clarke could write that in *Rendezvous with Rama*, we would take it. We asked Arthur if that was okay, and of course he said yes.

The Spaceguard Survey was proposed to NASA, and we provided estimates of how much it would cost. The idea is you would have to build new telescopes and support them. Following up the first Spaceguard Survey panel that I chaired was another in 1995 [*Beginning the Spaceguard Survey*] that Gene [Eugene M.] Shoemaker chaired. I was on it, and that was specifically trying to make a better estimate of how large the telescopes would need to be, where they would need to be located, how much they would cost. They concluded that a survey could be done substantially more cheaply than we had thought in 1992. If you can imagine a program that gets cheaper with more study instead of more expensive.

NASA finally started the Spaceguard Survey program in 1998, but NASA took a different and interesting approach. I had always assumed that the best way to do the survey was a top-down organization. You design optimum telescopes, you put them in right locations, you spend the money to have people do the observations. You bring them all together to a central organization so that you can find and track the objects.

NASA, partly because they didn't have a lot of money for the survey, ended up organizing it differently. They said, "Let's find existing telescopes and give grants to those people to do it, and let them figure out how to do it. It doesn't matter if the telescopes are not identical or if they have different detectors. We'll actually make it a competition. We'll say the money you get depends on how many of these near-Earth asteroids you discover per year. That will be more effective than being organized top-down."

That approach turned out to be very successful because some of those teams are extremely innovative. The group for instance at [Steward Observatory] Catalina [Station] in Tucson [Arizona] has located several telescopes that weren't being used—old telescopes or at least old optics—and refurbished them, put them together for a relatively small amount of money, put a focal plane system on. They have been the most successful discoverer of near-Earth objects.

Our objective that we stated back in that first *Spaceguard Report* was to find 90 percent of the asteroids large enough to cause a global catastrophe if they hit. Not a mass extinction, but to put enough dust into the atmosphere to block sunlight and produce an agricultural collapse for a couple of years. We had estimated that at about one-kilometer size.

We said, "The goal is to find 90 percent of the asteroids one kilometer or larger in 10 years." Of course some people said, "Why 90 percent, why not 100 percent?" But you never get 100 percent of anything. How would you even know that there wasn't one or two left? In any such system, the rate of discovery would drop with time as you found more and more of them. You could in effect retire 90 percent of the risk if you found 90 percent of these objects that were capable of causing a global catastrophe. That's what's happened. The survey has now found more than 95 percent.

JOHNSON: One of the things I'm assuming you found through all of that was the [99942] Apophis [near-Earth asteroid], that was identified in 2004 with a possible close flyby relatively soon, and then a possible return seven years after that. Do you want to talk about that for a minute?

MORRISON: While the observing astronomers were out finding these objects, there was of course a very important role for the dynamicists who calculate orbits from observations over a fairly short window in time. Within a month there might be three or four positional observations. Can you calculate an orbit? You have to determine an accurate orbit if you're going to decide if that could ever hit or not. While the observers were chugging away reporting several discoveries a week, the dynamicists were trying to interpret the hazard.

One result was two superb orbit analysis programs, one at JPL and one at the University of Pisa [Italy], that could take these observations and determine a prediction as to whether the object might hit, and what the probabilities were, and which objects were important enough that you should make an effort to go back and observe them again. JPL and Pisa check each other, and they both do this every day. Every time a new observation comes in, they re-compute the orbit for that asteroid.

It's a huge thing really, and the results are all online. So people who worry about, "Oh, the government is not going to tell us if there's something coming" just haven't looked at the system. You can go online to JPL and get the latest calculations, orbits, predictions of close passages for every known asteroid updated daily, and you can do the same thing at the University of Pisa.

One of the interesting discoveries was Apophis, which is a pretty big asteroid, several kilometers across, that was going to come by the Earth quite close. It was clear fairly early that it wasn't going to hit, but it was going to come as close as our geosynchronous satellites, would be visible with binoculars, even to the naked eye as it went past.

Then the dynamicists realized that there were several interesting consequences. When an object comes very close to any planet, its orbit is changed by the gravity. If the Earth's gravity changes its orbit in just the right way, it would come back in six years or seven years and hit us. Not high probability, but you couldn't say once it went by, "Oh good, it'll never be back." It's possible it could go through what's called a keyhole in which the orbit would be changed just enough by the Earth's gravity so that it would come back.

That was a subject of considerable interest in general, particularly for Apophis. We knew it wasn't going to hit us the first time, but could it hit us the second time, seven years later? It's taken years of observation to have enough precision to show it's not going to hit us the second time either.

JOHNSON: Size-wise, would it have been a global catastrophe?

MORRISON: Apophis would have been a global catastrophe. It would have caused so much dust in the atmosphere that you could have a global failure of agriculture. Of course one of the questions we immediately had to ask, "What does that mean? How much food is in storage, could you live for a year on what's in your refrigerator or your freezer? Could the world?" The answer is no. The people that would be best off would be places like the U.S. and Canada, because we have a lot of food in storage and on the hoof. If we slaughtered all of our herds you could get meat that way. But places near the equator where they depend on two or three rice crops a year, they don't keep much stored at all. Even losing one of those crops or losing two of those crops in a row would have a devastating problem in terms of famine. Then you assume that the consequences of famine might be pandemics, might be mass migrations, could be wars. You could have a collapse of civilization.

Clark Chapman and I tended to call this size threshold of a kilometer or so for global consequences a civilization-threatening impact. The effect would be global. The impact may have been localized to someplace like the size of a state in the United States, but if the actual physical impact caused agricultural failures around the world, then everybody had to worry. You couldn't just talk about defense as, "Let's defend the U.S." You had to worry about defending the Earth.

JOHNSON: In one of the articles you sent me, in 2015 you were in Italy. They were doing a what-if scenario to run through what would happen. Do you want to talk about that and what they learned from that?

MORRISON: Yes. There has been an International [Academy of Astronautics] Planetary Defense Conference six times now. It's been going over for 15 years or more. I've been on the organizing committees of each of those. They've gotten bigger and bigger. It is an international issue, and now it's an issue that's important at the UN [United Nations] also.

One of the things we did, as an education for ourselves and also for the public, is to say, "Okay, we can talk about all this. We can talk about discovering an object and predicting where it's going to hit and understanding how we might protect ourselves, but what would be the actual timeline for that?"

We did an exercise in 2015—and we're doing one again this year, a week from now in Tokyo. The folks at JPL, who are so good at orbits, generated a dynamically realistic orbit for an imaginary object that would come very close or hit the Earth, and examined what the observing opportunities would be. How quickly would you be likely to get enough information to have a firm prediction that it would hit or where it would hit?

We condensed all that into a week. Every day they would give us an update, "Three years have passed since the last one." At first all you know is the asteroid has a 1 or 2 percent chance of hitting somewhere on the Earth. Then, as that probability either goes away or goes up—but of course for the exercise it went up, although in the real world most of the one percent objects are going to miss—then you would find out that there was an impact corridor.

It's a little esoteric, but the intersection of the orbit of the Earth and the orbit of the asteroid tells you where the two would intersect, a line around the Earth. It still wouldn't tell you where it would hit, but you could say it would hit on this path. In this case the path (impact corridor) started in Iran, and then looped around the Earth all the way out into the Pacific [Ocean] crossing a number of major cities, like Delhi [India] and Beijing [China] and Tehran [Iran].

Then we tried to guess, as the information about the asteroid's size and composition improved and the risk corridor shortened, what the response of humans might be. We were a bunch of scientists, for goodness's sake. What do we know about this? But it was an interesting exercise, because you actually sense, even though it was all condensed into a week, the progression of knowledge.

At one point for instance it looked like the asteroid would hit somewhere between the South China Sea and north India. Of course there's a huge difference in population between hitting in the sea and hitting in north India. Both India and China are countries with relatively mature space programs, and the question is, would either of them try to deflect it into the other country? China would much rather it hit in India than the South China Sea, and India vice-versa. It made for an interesting discussion.

But then of course we had to say over and over and over to the press, "This is purely hypothetical. There is no asteroid coming, there is nothing going to happen. We are not experts in what the response of individual countries would be."

JOHNSON: Like the radio broadcast in the '30s [Orson Welles' *The War of the Worlds* (1938)].

MORRISON: Yes. We always have that problem, that communication problem. You can go to the JPL website for instance that lists every known asteroid and see which ones are coming close. There is apparently a group of people who want to create almost continuous scare stories about an object going to hit. They publish in what I call "newspaper tabloids." They publish in several British tabloids, India produces some of these. And they generate "fake news" on the Internet.

With communications now, even when some obscure newspaper in India announces that there's going to be an impact and we should all be scared of the end of the Earth, it goes around on the Internet. Fortunately not so much of it in the U.S. NASA generally ignores it, properly, because such stories are all absurd, they're absolutely fiction. But they constantly keep in front of some element of the public that asteroid impacts are possible.

JOHNSON: I know part of what you've done and what Carl Sagan did—he was interested in being a "baloney detector" as I think you put it in a paper you were writing about him. He fought against what he called pseudoscience.

I know you have a program that you've worked on a lot, *Ask an Astrobiologist* [blog]. You've also had to debunk a lot of false scientific claims, about for instance the end of the world in 2012. How did you first become that person here at Ames to answer these questions?

MORRISON: I'm sure that Carl Sagan did influence me in feeling that it was a responsibility of scientists to communicate not only the good things we do but also to counteract the falsehoods. The one that dominated over all the others was the story that the world was going to end on December 21st of 2012, when we would be hit by a fictional planet Nibiru. It was tied to all sorts of utter nonsense about the Mayan [Mesoamerican civilization] calendar, and the things that were said about the Mayan calendar were almost all wrong. People were making a lot of money. They would take rich people down to the former Mayan areas in Central America and give them luxury tours and explain to them how the world was going to end.

There was a huge boost of people building air-raid shelters, fallout shelters, selling emergency—the whole idea of do-it-yourself, save yourself when the world ends, became very widely held. There were people who decided that they could publicize that a particular mountain range in Spain for instance would be the only area not destroyed when this planet Nibiru hit us. Of course they bought it up and sold the lots at extravagantly high rates. Several hundred books were published, and a big Hollywood movie called *2012*. A lot of people were trying to make money from this fear, and it really angered me. Especially that it was such blatant misrepresentation used to scare people, and most of all of scaring children.

I became exposed initially because of my blog: *Ask an Astrobiologist*. Suddenly, even though it wasn't astrobiology, people started sending me questions about the end of the world. They were very disturbing. Like a woman who wrote and said, "My only friend is my little dog, and I'd like to know when the world will end so I can put her to sleep so she won't suffer." There were kids who wrote that they couldn't sleep, they were not going to school, they were throwing up all the time over fear. There were a few that said they were going to commit suicide, and in the UK at least one girl, teenager—did commit suicide over it.

What do you do when you get an e-mail from somebody who says, "I'm Jimmy, I'm 11 years old, I understand the world is going to end. I don't want it to end, why should my life be cut off, what should I do?" It was stressful. For about three years I was answering questions first weekly, then daily, then as it got closer to [December 21st] I would get questions at the rate of 10 or 20 a day coming in.

I, in effect, became the most prominent spokesperson from the whole scientific community on trying to counter this. I got good support from NASA, from the public information offices at NASA Headquarters and here at Ames. No one said it was foolish of me to do this, and I was definitely on the point of the spear in terms of dealing with this.

JOHNSON: Do you feel like you were able to convince these people after talking to them?

MORRISON: Some of them certainly. Most of the time you don't end up with a two-way conversation, but sometimes you do. There were 13-year-old kids who wrote back and said, "Thank you. Thank you, I feel so much better now."

JOHNSON: The Kepler [space telescope (launched 2009)]—you were instrumental in helping to sell that idea so that the funding would be available. That seems to be something that has to be done with the NASA budget a lot, especially for missions. Do you want to talk about that experience?

MORRISON: Sure. Every mission has to be marketed. It has to go through a lot of study to refine your understanding of what you're going to do enough to get a funding line. NASA has basically two kinds of missions. There are the missions that are run by NASA, often by JPL or Goddard [Goddard Space Flight Center, Greenbelt MD]—Voyager and Galileo and the Hubble Space Telescope were examples of this—and now increasingly there are also missions that are proposed by individuals or individual centers which are smaller cost. They involve an actual formal proposal evaluation.

The big missions will do competitive selections for the instruments on the spacecraft, but not the spacecraft itself. But more and more we're doing these things like Discovery [Program] that is PI-led.

Here at Ames we have a wonderful PI, Bill [William J.] Borucki, who for 20 years had been trying to sell the idea that we could discover planets around other stars by measuring with high precision the brightness of the star. When a planet passed in front of the star, if the orbit were aligned properly the brightness would drop, but only a very small amount.

It turned out it was such a small amount that except for large planets and small stars it couldn't be done from the ground. The noise caused by the atmosphere made that kind of precision photometry impossible, so it needed to be done from space.

Bill had gradually developed the idea. He ended up doing five Discovery mission proposals to NASA Headquarters altogether before his mission was finally selected. Each time they would say, "Well, an interesting idea, but the technology isn't mature, or you need to go back and study this." He would just roll up his sleeves and start all over and do it again.

Borucki was in the Space Science Division when I was head of it, so I came to Ames at a time that he would have already gone through a couple of these proposals. I had heard him give talks to NASA panels advocating this. I thought it was a wonderful idea, but I was very skeptical of the practicality of it, because you would have to observe an awful lot of stars.

Only on average 1 star in 100 would have its planet orbits oriented properly. If you wanted to find 10 extrasolar planetary systems you would need to observe 1,000 stars, and 10 probably isn't enough because not every star will have planets. If only a few percent have planets, then you'd have to observe tens of thousands of stars, and that's very difficult. Simultaneously observe 100,000 stars with sufficient precision to detect a drop in light when a planet passed in front. These are planets you cannot possibly see. You could say you see their shadow or you see their effect on the light of the star.

As Bill Borucki developed these ideas, with some colleagues here, I said, "We really ought to see if this is practical. We really want to make this happen. It would be so exciting. There is no other program that NASA could carry out that would answer the question of whether there are planets around other stars."

I put together a panel chaired by Jill Tarter of the SETI Institute with some really, really good astronomy experts, photometry experts, to do a careful query of Borucki and look at his plans and see what they thought of it. They spent a couple days, and they were really good. They were asking all the hard questions. He was answering them.

They came out—this is a nonadvocate panel—saying, "We think it can really happen. We think it's good." That made me believe it more than I could just trusting my own judgment, and I made sure that report was circulated at NASA Headquarters and to the science community.

Then a year or so later, the thing that they still were uncertain of was the stability of detectors, because you're having to measure such small changes in brightness. So NASA Headquarters gave us $500,000 and Ames put in another $500,000 from the Director's discretionary funds to do a laboratory setup to mimic the photometry system and try to prove whether the detectors were stable enough.

At this point I started talking to people at Headquarters about it. I talked to people in the Planetary Science Division and the Astrophysics Division. Eventually, once I became Director of Space here, I had access sometimes to Dan Goldin, the NASA Administrator. It wasn't much, but he came to visit Ames every few months, and I made sure each time he was here at Ames I at least got to give him an elevator speech. Not a full-up presentation, but just say, "Hey, Dan this really is a great idea. Our testing is going well. I really think that this could be something outstanding for NASA's future."

I'm sure there were lots of other influences, but I felt as though if I could only say one thing to the NASA Administrator on these visits it would be to push Kepler. Kepler was ultimately selected and flew, and has been in many ways the most remarkable mission NASA has ever done.

It is hard to compare Kepler with a mission like Cassini to Saturn or New Horizons to Pluto, but the fact is that it's fundamentally changed our understanding of ourselves in the universe. When people talked about SETI or looking for life elsewhere on other planets it was frequently felt that the solar system was probably very unusual and maybe only 1 star in a hundred or 1 in a thousand would have a planet, something like that.

Kepler has shown that when you look at the night sky and look at the stars, almost every star you can see has at least one planet. If there are 100 billion stars in our galaxy, there are probably 200 billion planets. That is a revolutionary discovery. We still have to do a lot to understand them or to characterize them, but we now know that planets and planetary systems are the rule, not the exception.

JOHNSON: There's been a lot in the news about discoveries in the TRAPPIST-1 [dwarf star] system, then the latest announcement by NASA a few weeks ago about Saturn's moon Enceladus.

MORRISON: We sometimes call it Enchilada.

JOHNSON: Easier to say—about the possible hydrothermal activity on the subsurface. When you go back a few years, Voyager has gone so much farther and now is in interstellar space.

When all of these things were being developed, and all of these programs like Cassini and Kepler—everyone has hopes that things will work the way they're supposed to work, but has it changed your ideas about what's out there?

MORRISON: It's changed my ideas, but not in a linear progression. When I started forty years ago with Viking, two landers on Mars, two orbiters, were followed in very short order by two Voyager spacecraft, which went to Jupiter and Saturn and Uranus and Neptune. We were having these experiences of data just flowing in. I thought that's what planetary science was going to be like, I thought there'd be big missions every couple years.

Then it really all fell apart, partly because of the Shuttle and the Shuttle accidents, but we didn't go back to Mars for 20 years. We didn't have any follow-up to Voyager except Galileo. Then it was delayed, and then its antenna broke. We sent two missions to Mars and they both failed in the same year [Mars Climate Orbiter and Mars Polar Lander].

I went through a period of thinking that we're going to have telescopes, we're going to have things like the Hubble [Space] Telescope, and that's great, but the planetary exploration has really almost stopped. It's now taking us so long, 10 years at least, to develop a mission. If it's going to the outer solar system, it takes more years to get there.

Then, in the last few years, we are having a second golden era of planetary exploration. Five spacecraft to the Moon, for goodness's sake, each one of them far more capable than anything we had back in the Apollo era in terms of orbiters.

Landers—China landing on the surface of the Moon, going to bring back samples from the lunar far side. India has a very successful lunar mission, Japan a successful mission. Japan has also had flights to near-Earth asteroids that and landed on the surface and brought back samples [Hayabusa mission]. The European Rosetta comet mission was spectacular. Kepler of course is magnificent.

So suddenly we're in an era of really rapid discovery again, and I think that's wonderful. I've lived long enough to witness the first golden era, then 20 years when we were not accomplishing as much as we thought we should have, and now another golden era.

JOHNSON: Do you have any worries about the political feeling of the time and some of the skepticism about science that we're hearing more now? Do you have any worries that these programs are going to be cancelled? I know in the [president's] 2018 budget the landing on Europa looks like that's something that would be canceled. There's some different things that they're talking about canceling. So do you have any worries that we're going to go into another one of those periods?

MORRISON: I always worry, I always worry. Given the situation in the Congress it's hard to have much faith that they'll do the right thing, but often they end up doing the right thing. It's Congress that's pushed a Europa mission. Although they aren't funding directly the Europa lander, I think it's coming along.

There's tremendous interest in Saturn's moon Enceladus. The New Horizons at Pluto was a fabulous mission. Compared to other things, these are not expensive, they really aren't. They're way down relative to almost any other government program in terms of funding. I have no idea what's going to happen, but there is no reason that we should slow down. We have a vibrant space science community.

I think the real questions for NASA concern humans in space, and are we going to go that route again. A lot of people take it as a matter of faith, and I used to, and maybe I still do, that human exploration of the solar system, going to the Moon and then to Mars, will happen. But meanwhile the results we're getting are not from humans in space, but from our deep space probes and our robotic telescopes in space. They have been fabulously successful.

JOHNSON: Hopefully the publicity and the fact that people are aware more and more of what's happening, which you've been a big part of. Before we close, is there anything that you would consider your biggest challenge in your career, or anything you would do differently if you had a chance?

MORRISON: In terms of doing differently, I did do one thing a little bit strange. When I left the University of Hawaii and came to work for NASA it was when NASA was not launching missions, and that was a disappointment. I had hoped that I would have more mission involvement once I worked for NASA, but there just haven't been that many missions.

It's a very great difficulty for young scientists, because even when they are selected to be team members on a mission, they don't get the money they used to. You could be a postdoc, someone just with a PhD, get a position on a mission like New Frontiers, and not get enough money from NASA to pay your salary.

But I've done fine. I'm very pleased with it. I'm thankful that I've also been involved in the struggle against pseudoscience—and today that means primarily a struggle against climate deniers—and that I have been able to talk to so many people and write. I've written multiple textbooks that are in use in the colleges.

One thing right now that I'm very proud of is that the college astronomy text that has been out for 20 years by Andy [Andrew] Fraknoi and Sidney [C.] Wolff and me [*Astronomy*] has just been completely updated and put online for free. In an era when other college textbooks cost $150 each, now a university can adopt our book, the students can get it on their laptop free, or they can print a copy. That's something that'll last, and that will help the next generation of young people learn about astronomy and space exploration.

JOHNSON: I think that's a good place to stop. Thank you very much.

[End of interview]