NASA DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

PAUL L. HERTZ
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is March 1st, 2022. This interview with Dr. Paul Hertz is being conducted for the Discovery 30th Anniversary Oral History Project. The interviewer is Sandra Johnson and I'm in Houston, Texas. Dr. Hertz is joining me from Burke, Virginia, over Microsoft Teams. I appreciate you talking to me today for this project.

HERTZ: Thank you.

JOHNSON: First of all, I'd like to talk about your background. If you could briefly describe your education and how you first became interested in astrophysics and that first work that you started doing for the [US] Naval Research Laboratory [NRL, Washington, D.C.].

HERTZ: I grew up during the Apollo era, and I was hugely interested in human spaceflight. I remember John [H.] Glenn's flight[1]. My father was very interested in human spaceflight. I grew up in Atlanta, Georgia, and we drove down to Florida and saw three of the Apollo launches just watching from the beach, as members of the public. As a kid I was very interested in astronomy. I had pictures of the planets that I drew all over my walls, and I clipped out of the newspaper everything about astronomy or satellites or NASA. Like everybody else who works at NASA, I was pretty good at math and science and I wanted to be a scientist.

[1] Mercury-Atlas 6 (MA-6), Friendship 7, first US crewed orbital spaceflight

When I went off to college, I majored in physics and I did my senior project in astrophysics, and then I moved on to a PhD in astrophysics and became a professional astrophysicist. I did my undergraduate degree at MIT [Massachusetts Institute of Technology, Cambridge], majoring in physics with a second major in math, and did my graduate work at Harvard [University, Cambridge, Massachusetts], a PhD in astronomy. I did my thesis analyzing data from a NASA satellite, the second High Energy Astronomical Observatory [HEAO-2], also known as the Einstein Observatory, and it was one of the earliest PhD theses at Harvard that was based entirely on satellite data instead of data from ground-based telescopes. I was an early adopter of NASA science, an early user of NASA astrophysics satellites.

I did a postdoctoral fellowship at the Naval Research Lab [in Washington, DC], and after that was hired by the Naval Research Lab as a staff astrophysicist. Then in 1997 I was invited to come to NASA Headquarters on a two-year detail to help manage the Astrophysics Program. I really enjoyed that detail very much because, at that time, I had been doing research for 20 years, and this felt like a really exciting opportunity to do a slight career change. I really enjoyed it. When my detail was over, I went back to NRL, but in 2000 I applied for a job at NASA and was hired back to NASA, became a NASA employee.

Around 2000 is when I first became involved in the Discovery Program.

JOHNSON: Let's talk about when you first started there, and you first became involved in Discovery. Talk about what your position was and what it involved and then the Discovery involvement too.

HERTZ: When I was on detail at NASA one of the things that I did was manage some of our announcements of opportunity [AOs]. In NASA science we have a class of missions called principal investigator or PI-led missions. We put out announcements of opportunity, which are calls for proposals from teams led by a principal investigator that proposes a science investigation including the entire mission it takes to realize that science investigation. It's one of our highest levels of calls for proposals, because you're not just proposing a research project, you're proposing science and the entire mission to do that science.

I had managed some of those calls during my detail. When I came back to NASA and they hired me, one of the things they hired me to do was to manage these announcements of opportunity, the solicitation, review, and selection process. At the time there was a Discovery AO that had just been released, and I came in and became the Discovery Program Scientist, amongst other duties, and that meant I was responsible for managing the handling of proposals. That includes the evaluation of proposals, then the selection process, and finally the implementation of those missions.

JOHNSON: Which AO was that?

HERTZ: It was the 2000 AO, and I don't remember its number[2]. I do know which missions we selected.

JOHNSON: Let's talk about that. Which ones were selected?

[2] AO 00-OSS-02, Discovery Program 2000, released May 19, 2000, to select the 9th and 10th Discovery missions

HERTZ: This was the AO that led to the Kepler [Space Telescope] and the Dawn [space probe] missions.

JOHNSON: You said you were doing that in ’97 when you first came in, you were managing that.

HERTZ: Yes, but not for the Discovery Program. I was managing AOs for the Explorers Program which does missions in heliophysics and astrophysics. Discovery was my first planetary science AO.

JOHNSON: When you came back and took that role were you just doing specifically Discovery only? Or were you still working with the Explorers Program in 2000?

HERTZ: I was doing both. I was a specialist in AOs, amongst my other duties.

JOHNSON: Talk about that time, because I know later you were on the AO simplification and standardization effort, but since you were doing both, were those AOs for both programs similar? It was a different type of exploration science. But beyond that, was there a lot of difference between those two?

HERTZ: The process is very similar. Back then we ran most of our AOs with similar processes. In fact, it’s not a surprise because Discovery was the first program that solicited PI-led complete missions, and as we added additional programs into NASA science to solicit complete PI-led missions, we modeled the processes in those programs after the way Discovery was doing it.

As Explorers moved to PI-led missions and then we added additional programs like New Frontiers or Earth Venture, they were all modeled on the processes of Discovery. So yes, it's very similar running the Explorers AO process or the Discovery AO process.

JOHNSON: When did Explorers start with the PI-led model, and going through the same process?

HERTZ: It was in the mid '90s[3]. Discovery started in the early '90s.

JOHNSON: As far as the AO, were you involved in also writing them? Or running the program?

HERTZ: I was involved in writing. Not that Discovery AO. By the time they hired me in to take it over it had already been written. I wrote quite a few Explorer AOs in my day. Then I became the Chief Scientist in the Science Mission Directorate [in 2004]. I had responsibility over all of our AO processes, and I standardized the process.

JOHNSON: Let's talk about that first one you said with Kepler and then Dawn, and you were the Program Scientist. Let's talk about that and maybe some of the details of that process. Walk us through the selection and then how that was set up in those early days.

HERTZ: Generally, the process is we issue an AO. We announce far in advance that we're going to issue an AO, so that the community knows to assemble their teams and to put together their concepts. We issue a draft AO so they can see what's going to be in it. Then we issue the final

[3] The first Discovery AO was in 1994

AO, which is a formal government call for proposals. We give them 90 days to respond, and we receive from a dozen to a couple of dozen full mission proposals.

We then assemble two parallel sets of peer reviews, a science peer review and a feasibility peer review, what we call the technical, management, and cost review or the TMC review. The science peer review is made up of scientists from the community who are not involved with any of the proposals, and can serve as unbiased, unconflicted reviewers. They review all the proposals in great detail. We usually spend about a week, and we used to spend a week all together at a hotel doing these peer reviews. Now we do peer reviews virtually since the pandemic started. The science peer review panel reviews every proposal for its science merit and also for the feasibility of the science plan. Science merit is, assuming this thing works, is the science really worth doing, how important is it. Science feasibility is, assuming you build it exactly as proposed, is it going to deliver the science, will it give you the data you need to answer the questions you ask, are you in the right orbit, are you visiting the right planet, are your instruments detecting the right wavelengths. All those questions about, assuming it works perfectly and you collect the data you propose, will you be able to answer those science questions. That's the science peer review.

As I mentioned the reviewers are scientists. Many of them are instrumentalists, and some of them are people who analyze the data from missions, because we want people with expertise on all parts of the life cycle of a science mission.

The second peer review, the technical, management, and cost feasibility review, is made up of technical experts who have expertise in the building of spacecraft systems and instrument systems, also people with expertise in project management, system engineering, mission assurance, communications, all the different things that it takes for a space mission to work.

They review the same proposal independently. These two teams don't talk to each other. The question they're answering is, given the plan that the team has proposed and given the cost that they proposed and the schedule they proposed, are they going to be successful, or is there something they've forgotten about, have they proposed something that takes longer or costs more than they think it will. Whereas the first set of grades are on an excellent, very good, good kind of scale, the second set of grades is on a high risk, medium risk, low risk scale. It's really about assessing the chances that they can deliver this mission as proposed within the cost and schedule box that they proposed. The TMS review panel also takes about a week.

When I started doing this, I did not have a lot of experience with building missions. But I learned a lot from these reviews, because here were some of the world's experts talking about what's feasible, what's infeasible, what's hard, what's easy. Sometimes they would find a problem proposal. Then they'd go, "Yes, but that's easy to fix. It won't cost them very much. They'll have to change their plan, but they'll change their plan and they'll still be able to do it." Or they'll say, "Wow, they're going to have to change their plan. When they change their plan, the cost is going to double, so they're not going to make it within their cost box. Because that thing they proposed, we don't think it's going to work." Very very interesting reviews.

At the end of the two sets of reviews, you have two sets of grades, one on science merit and feasibility, one on mission feasibility. Those then come to NASA Headquarters, where a committee is assembled called the categorization committee. The categorization committee takes the grades and the reviews and assigns every proposal into one of four categories. A Category 1 proposal, it's a proposal that's highly meritorious, highly feasible, and recommended. A Category 2 is feasible and meritorious and recommended but at a lower priority. A Category 3 is a proposal where the science is meritorious but the proposal needs more work before it can be considered feasible. A Category 4 is a proposal that's not recommended for whatever the reason. This committee meets for a couple of days going over the reviews of every proposal in great detail to assign the categories.

Then a second committee is convened at Headquarters, called the steering committee, made up of different people. They review the process, and make sure that the process was a high-quality process, that no disqualifying mistakes were made, all the reviewers were unbiased and unconflicted, that all the paperwork is in sufficient detail to justify a decision to spend hundreds of millions of dollars building this mission, and that the categorization results are well justified, and that it's ready to go to the selection official.

Anywhere in the government when a decision has to be made, there's one person whose job it is to make a decision, committees don't make decisions. In NASA science the decision authority is the Associate Administrator for Science. But he always puts together a selection board to debate the choices and to give him advice on what to do.

The next and final step is that selection board meeting where the Category 1 proposals are presented. If the selection official requests, the Category 2 proposals can be presented as well. But only those, because those are the only ones that are recommended for selection by the categorization committee. Then a debate is held; every proposal has strengths and weaknesses, every proposal has risks associated with it. In the Science Mission Directorate, science is the most important consideration, but so is delivering within the cost, because these are cost-capped missions.

After that debate the selection official makes a decision.

JOHNSON: That's the decision for that first level, right? When they start working.

HERTZ: In the Discovery Program all the competitions are two-step, and so that selection is to select two, three, or four missions who will all conduct a funded mission concept study, which takes up to a year. At the end of that year, they will submit their concept study report, and that concept study report will be subjected to review. This review consists of not only the reviews of an integrated science and feasibility panel, but it will also involve a site visit to the mission team to make sure that we fully understand what they're proposing, how they're proposing to do it, we can ask them about the things we think they might be challenged by and see if they have solutions in mind for those problems. Then the review committee writes up a report, they provide the same two sets of grades. Science feasibility and mission feasibility. It comes back to the selection board for a downselection decision, a decision for which of those missions proceed into a flight build.

JOHNSON: Did you go on any of those site visits in your position?

HERTZ: Absolutely. I went on 39 site visits I think over my career. It was great. It's fascinating. It's just so interesting. First of all, you're hearing the advocates talk about a project that they're advocating for, proposing for, that they've invested a goodly fraction of their career into. First developing the concept, then doing the science. Oftentimes they had to develop new technology, new detectors, new ways of doing business. Maybe a new sampler or something like that. Then putting the whole thing together into a proposal and bringing in the spacecraft people and the mission operations people. The instrument people, the science people. Getting them all to figure out how is it going to work, how are we going to build it, can we estimate the cost, can we make a case that we can do it for the budget that we've been allocated.

Listening to those people explain it, so fascinating, you learn so much. I was a total fan of the site visits.

JOHNSON: When you're visiting those sites, do you get some insight into how those teams work together and how effective they might be if they were selected?

HERTZ: Absolutely. One of the interesting things you can observe is when the review committee challenges the principal investigator or the project manager and says, "I think you have a problem here, how are you going to address it?" How do they engage their team? What kind of leader are they in real time? Are they the kind of person who thinks they have to know all the answers because they're in charge? Are they the kind of person who defers to their subject matter expert to answer that question for you, and steps back and trusts the subject matter expert? Are they the kind of person who has a confab with the subject matter expert to make sure they're okay with what they want to say before they say it? You do observe the leadership style of that project's leaders during something as stressful as a site visit. It's incredibly stressful for them because usually we have twice as many missions in the competitive mission concept phase as we can afford to take into a flight build, and so they have only a fifty-fifty chance of going forward to flight.

They've invested lots of time, lots of effort. But they've also invested a huge amount of their priority, what's important to them. This is what they want to be. They want to be the PI of a NASA mission. The stakes are very high for those individuals. It's very stressful. To see how they perform as a leader under those stressful circumstances is informative.

JOHNSON: Let's talk about the AO when you were working on that effort to simplify and standardize that process. Why was that necessary at that point? Is it something that had been talked about before? I know Alan Stern, was it around 2007, made the decision, 2006, 2007, and initiated that effort. Is that something that was pretty obvious that it needed to be done?

HERTZ: This is as you mentioned around 2007 when I was the Chief Scientist of the Science Mission Directorate and had responsibility over all the AO processes. I was no longer a program scientist anymore, but I did work closely with all the program scientists who were running AOs.

It wasn't obvious to me that the AO needed simplifying. I had been part of a team that wrote a proposal to an AO when I was back at the Naval Research Lab. That was quite a few years ago by that time. I wasn't the principal investigator of that NRL proposal.

Alan Stern had been a principal investigator. He was the new Associate Administrator for Science at NASA. He had proposed several missions, and his mission, New Horizons, had been selected and it had been built and launched, and was on its way to Pluto at the time he was the Associate Administrator. New Horizons took nine years to go to Pluto. He had been the PI building it, and he was going to be the PI when it did science, but he had some spare time in between, so he became the head of science at NASA.

Alan, who had lots of experience proposing, comes into my office. He said, "Paul, we have to simplify the AOs. There's over 1,000 requirements in every AO."

I said, "Alan, that's crazy. There's like 50 requirements in the AO."

He said, "No, you're wrong. Do your homework."

I called up places that frequently proposed to AOs, like Goddard Space Flight Center [Greenbelt, Maryland] or Jet Propulsion Lab [Pasadena, California]. I said, "I know that for these last few AOs, every time you have an AO you guys dissect the AO and make a list of all the requirements so that you can make sure that your proposal responds to all the requirements of the AO. Would you please send me your lists from some of our recent AOs?"

They go, "Sure." They send me their lists. Holy mackerel, they had found 1,000 requirements. Because we were sloppy in our language, in our writing. We were going, "Well, you should do this, and you should talk about that, and don't forget that you're going to have to do such and such later on." All these things got converted into requirements. Everybody spent a lot of time making sure they followed them.

Now many of those things they called requirements weren't important for the purpose of selecting a mission. They might be important five years from now when you're building the mission. But we didn't consider that those were requirements for the proposal purposes.

We decided, or Alan told me, to simplify the AO. We used the word simplify at that time. I ran a national process where I reached out multiple times to the proposing community to talk about what should be the requirements in the AO. We had an internal team that went through several AOs, and they weren't standardized at that time. We boiled it down into what we thought was the important part.

Every time we thought we had a requirement we numbered it. We said, "Okay, if it's not numbered it's not a requirement." We were very careful about verbs. We put all the shalls into the requirements. We tried to make everything outside requirements to be descriptive language and not look like a requirement.

We ended up with around 100 requirements. Then we had several workshops. Proposers came in and they told us what we missed, they told us what shouldn't be a requirement. They did a lot of work. They told us, "We don't understand the way you wrote that." We went back to fix it and make it less ambiguous.

At the end of about nine months, we had a simplified AO which was now standard. Ever since then we, me and my successors in the job, have maintained that simplified AO. We call it the standard PI-led AO template. It's posted on the web for anybody who wants to see it. It has what we call knobs in it. For instance, it has a place where you fill in the blank for what your cost cap is. It has choices of words: Are you going to allow nuclear power sources or not allow nuclear power sources? Is this a one-step AO or a two-step AO? What are the limits on contributions from foreign partners? We call those knobs, and we built in the knobs to the standard PI-led AO template.

Future program scientists when they write an AO, they're supposed to only turn the knobs and not write new language. Before that time, every program scientist would tweak the AO and improve it. That meant that every AO was different. The organizations that were proposing would have to read it and figure out what was different.

We believed that by simplifying it not only could we reduce the amount of work writing a proposal, but by standardizing it we could also reduce work by proposers not having to figure out what's different about this AO from former AOs. I think we were successful in both cases, and we've maintained that process of simplifying and standardizing ever since.

JOHNSON: Has it changed since that time or has it been updated or is still pretty much the same?

HERTZ: It gets updated regularly. New requirements come along. For instance, there's new requirements about commanding security. You have to have encrypted uplinks and things like that. The way we cost things at NASA has changed. What facilities we have for communications have changed, the launch vehicles have changed. Most of the substantial changes have been either because NASA has changed the way it's doing something, or NASA has added new requirements that have to be flowed down to the proposers. Sometimes it's because somebody has done something on a mission, we go, "Oh yes, that was a really bad idea. We need to forbid that in the future." There are a few of those in there too.

JOHNSON: That's interesting. When you were leading this effort, who else was on that team and how were they chosen? Did you get to choose them or were they assigned?

HERTZ: The simplification and standardization effort? Let's see. I'll go back in history a little bit. When the Discovery Program was first founded as the first PI-led mission, it was run out of Headquarters, but we leaned on the Goddard Space Flight Center for some technical support in evaluation. But the Goddard Space Flight Center also proposes to AOs. For the second round we established an office at the Langley Research Center in Hampton, Virginia, I don't remember what it was called back then. Its current name is the Science Office for Mission Assessments, SOMA. It had a different name back then [Science Support Office].

We put it at Langley because Langley didn't submit proposals to our AOs, so we could draw on the technical staff at Langley to help us evaluate AOs without creating a conflict of interest for those people who might be reviewing proposals from their own Center. By the time I was Chief Scientist, the office at Langley, I'll call it SOMA because I can't remember its previous name, the SOMA office was established, and so the Director of the SOMA office, Brad Perry, was my cochair in this simplification process. I drew on a few of his staff members to help me with this process.

The people at Headquarters, the program scientists who ran AOs, were subject matter experts who had a lot of input into this process.

JOHNSON: Let's talk about that. Once a proposal is accepted and they've gone through the concept studies and then their mission is selected, how does NASA ensure that those teams that are chosen are going to work well and successfully as it moves forward? How does Headquarters support those missions at that point?

HERTZ: We can't ensure that they're going to work well together and be successful. But that's one of the things that we evaluate—and I want to be clear that a best practice for an AO is that you're very crisp about your evaluation criteria. When we were simplifying, we made sure that there was a link between every requirement and an evaluation criterion, so that we didn't ask for anything we didn't need, and we told the proposers in advance exactly what the standards were that we were going to evaluate them against.

One of the categories of evaluation is management. We do ask for each of the members of the team, each of the organizations, as well as the individuals who are in leadership, to tell us as part of the proposal why they are qualified to do the work this proposal assigns to them. If there's an organization that's building the spacecraft, an organization that's integrating the instruments, an organization that is developing the widgets, and it's important enough to call out widgets in the proposal, then they tell us why that company is good. That's one of the evaluations. One of the things we evaluate is whether we think that that organization has the capabilities to be successful at the work they're proposed to do.

We also ask them to tell us what their organizational structure is, how they will manage the flow of work direction down and then the flow of problems back up to be solved, who's in charge, how would they solve problems and hand out reserves. All those standard project management activities we ask them to tell us how they'll do that, and we evaluate that. One of the things we look at is whether we think this team understands the work they have to do, whether they have the capabilities to do the work they have to do, and whether they have organized themselves in a way that we think they'll be successful at doing that work. We try not to pick anybody who doesn't get a checkmark in each of those areas. That's the due diligence we do in advance.

JOHNSON: Were there any standardized roles for principal investigator versus a project manager, and those roles within those groups? Was anything standardized, or do they figure that out for themselves?

HERTZ: We don't tell them what those roles are. We want teams to self-organize in a way that they can be successful. However, there are common and almost I guess traditional divisions of labor between the principal investigator and the project manager. First of all, the principal investigator is in charge. These are principal investigator led. Secondly, the PI, unless they're also a project manager, should be delegating all the day-to-day work to the project manager to manage all the systems as they get developed and integrated and to be balancing out the schedule and the budget. The PI needs to retain final authority on making trades when things don't go right, are we spending more money on this or changing our plans here or there. The principal investigator retains the ownership of the science requirements, and the project manager should be coming to the principal investigator for permission if they have to make a decision which impinges on the mission's ability to deliver science.

Generally, the PI manages the science team. The project scientist works for the PI, who will be managing developing the mission operations, the science operations plan.

JOHNSON: Those PI-led missions, are those missions and also the missions of opportunity, are they run the same way?

HERTZ: Generally, yes. The mission of opportunity is just a smaller class of PI-led missions.

JOHNSON: Let's talk about that PI-led model. There are some unique challenges I would assume for these PI-led missions. Let's talk about the competition model and using those PI-led missions for Discovery. Then, like you said, it's been adopted for these other programs now. What is the benefit of running a program like that?

HERTZ: In a traditional procurement, the government writes out the requirements and says, "I need this widget or this service, and here are the requirements you have to meet if you're going to deliver this widget or this service." We put out a request for proposal and companies will bid to do that job for us, to do the job that we specify. But we know exactly what widget or what service we need when we put out that call. That's a traditional procurement with a request for proposals.

An announcement of opportunity is where we ask the proposers to write their own statement of work. We say, "We want to do a kick-ass planetary science investigation. We don't know what planet we're going to go to. We don't know what kind of spacecraft. We don't know if it's a flyby, a lander, a sampler. We don't know any of that. We just want it to be kick-ass science. Would you please write a statement of work for a kick-ass science investigation, and then simultaneously propose the mission to do that science investigation?"

An AO doesn't solicit a widget, doesn't solicit a mission or a spacecraft. It actually solicits a science investigation. We ask the proposer to tell us that science investigation just like any research proposal. Then we ask them to propose to build the mission that is required to do that science investigation.

I'm the Director of Astrophysics right now. We do PI-led missions in astrophysics. I don't believe that I know what the best PI-led mission we should do next is; I don't know if it should be a mission to study dark energy, a mission to study exoplanets, a mission to study the evolution of galaxies, a mission to study weird stars or black holes. I don't know. I think there's a lot of smart people out there in the community who do know.

We put out an announcement of opportunity and we say, "Please propose the absolute best astrophysics investigation you can think of that can be done for under $300 million. Then propose to build the mission for under $300 million that can do that investigation." Instead of a few of us at NASA trying to figure out what's the best thing to do, we engage the entire national [science] community in thinking up the most innovative, the most exciting, the most compelling science missions, and then the people who are the innovators and the advocates, we ask them to build it, operate it, and deliver the science and the data for the benefit of the world.

JOHNSON: When you were at the NRL and you were researching and then this came about like you said in the early '90s with Discovery, and this whole concept of doing these PI-led missions, how did the general science community react to that? Was it something they'd been asking for?

HERTZ: I think I was too young to be able to answer that question, and not connected as well back in the early '90s. I will say a couple things. First of all, before NASA switched to this PI-led mode of soliciting entire missions, NASA would specify a mission and the science it was going to do, and then NASA would solicit for instruments. Instrument teams would propose to build the instruments on it. We still do that for our biggest missions. For instance, the Hubble Space Telescope. NASA decided it was going to build a large space telescope that worked in the optical, ultraviolet, and near-infrared. Then it put out a call for instruments that would take advantage of it. It said, "We think it'll work great if it has at least one imager, at least one spectrometer, at least one photometer." They got a bunch of proposals and picked the best to make together the instrument complement. They were also doing that for the small and medium size missions also.

I don't know what drove the innovation to solicit for an entire mission and to compete entire mission ideas against each other. But the Discovery Program was the first one to do that. Before they put out their first AO, they had a workshop where people could come and talk about their ideas. Then they funded some teams to mature some of their ideas so that that first AO had a bunch of great proposals come into it. That allowed them to get going really well in the Discovery Program.

The Explorers Program adopted that for astrophysics. When I was at NRL I was part of a proposal in 1995 to an astrophysics AO for a medium-class Explorer mission. We were not selected. The mission that was selected was way better than ours in my opinion in hindsight. We had a great idea. But somebody else just had a better idea. And it was in a completely different area of science [than the area that we proposed].

By the end of the ’90s the astrophysics decadal survey prioritized continued use of PI class missions through the Explorers Program as a way of doing the small class missions as one of the highest priorities in astrophysics. I am not sure how it started, but I know that those of us who were writing proposals were excited about the opportunity. I can’t answer your question whether the community as a whole like it.

But by the time we got to the end of the ’90s, the community as a whole not only liked it, they thought that one of the most important things to do was to continue what was still a bit of an experiment at the time. Every decadal survey in every area of NASA science since then has endorsed the PI-led process for identifying the small missions. Instead of some other process to identify top-down what a small mission should be, every decadal survey has recommended PI-led AOs as a high priority.

We know that not only does the community like it, the community thinks this is one of the most important ways of getting the best science out of the NASA dollar.

JOHNSON: Those teams that work together for these projects and missions, we talked about the PI and the project manager. But they also involve engineers to build what they want to do with the science. Those roles are somewhat diverse. Looking at it as an outsider, it’s interesting to me to see how those roles relate to each other, and I would think there would have to be a lot of cross-learning as far as the scientists to the engineers. Can you talk about that for just a moment? Or any experience you have or any instances of seeing that work?

HERTZ: The PI is responsible for putting together an entire team that has all the skill sets necessary to be successful in this mission, and that means if he doesn't work at an organization that has the capability of managing a space project, then he needs to team with such an organization. A lot of Discovery missions have university professors as PIs, and there's very few universities that are capable of managing a Discovery class space project. They often will partner with an organization that does have that capability such as the Jet Propulsion Laboratory, the [Johns Hopkins University] Applied Physics Lab [Maryland], the Goddard Space Flight Center, other organizations. Those aren't the only ones.

Then they also have to partner with instrument providers, and then they also need to partner with somebody who can build a spacecraft if the NASA Center is not going to build the spacecraft, and usually the NASA Center doesn't. There are companies out there who build spacecraft for a living, usually less expensively than NASA does. Because you're fitting in a cost box, you're looking for cost value in all your partners.

It's the PI's job to put this team together, and they have to do this very early because this is the team that has to mature the concept to the point where you can describe it and convince somebody it can be built. That's more than the back of an envelope. Although we only give them about 40 pages to do this – the proposal's not a novel.

Many of these PIs have a great idea, and instead of putting the team together themselves they'll come to a management organization and bring their idea with them and have the management organization put the team together for them. Because places like NASA Centers and other organizations that are really good at project management, they have relationships with all the skill sets that are needed to deliver a mission.

JOHNSON: You mentioned when you first came that you inherited or became the program scientist for the Kepler. Talk about that and the role of a program scientist as opposed to what you did after that.

HERTZ: First of all, I am an astrophysicist, I'm not a planetary scientist. When I inherited that AO, I can't remember, I think I got it a few weeks before proposals came in. Certainly, I had no role in writing the AO. But proposals were coming in and I had to put together the peer review. I'm not a planetary scientist, so I did have to find planetary scientists at Headquarters to help take responsibility for the different panels, and I would work with them to identify the right subject matter experts to be peer reviewers.

I inherited an acquisition manager at the SOMA office at Langley who was putting together the technical, management, and cost review. I worked with him, though I am blanking on his name [R. Wayne Richie.]

But it was my job to see this process through successfully to a selection. That meant getting the right people on the reviews, and then when we had the review, giving instructions, managing it, making sure that the outputs of all these reviews was fair, was credible, and was clearly written. Any of us who have run peer reviews with scientists know that with scientists, it's not their favorite thing to write lots and lots of words. They like to be as terse as possible. They'll say, "Okay, this is excellent." Then they'll only write five sentences. You need to say, "Tell me why it's excellent. Tell me all the good things about it."

Making sure that the written reports actually reflected the discussions that took place, where the strengths and weaknesses were floated and the team congealed around the important strengths and important weaknesses, making sure those got written down, was one of the most important jobs of the people running the peer review.

Then the job is to coordinate that process of categorization, steering, and then selection. That's bureaucracy. Getting people together and putting that material in front of them and ensuring that they have the right kind of discussions. The selection board meetings as the Program Scientist are fun because that's an example of managing up. I say fun as a 20-year bureaucrat. I've got a black belt in bureaucracy now. If I didn't think it was fun to manage up in a bureaucracy, I probably should have had a different job long ago.

But the selection meeting is organizing a review for a roomful of people who outrank you and who have the authority to make the decision. I didn't have the authority to make the decision. I didn't even make recommendations. I just put in front of the selection board the findings on the top proposals.

Then they would create the pros and cons and talk about the trades and what's best for the program and things like that. Then the Associate Administrator would make the decision. But figuring out the right way to put all that stuff in front of them, that's about communications. You want to communicate clearly. Going into that meeting I was the expert on those proposals. I needed to transfer my knowledge to the people who had the responsibility to make the decision. The ability to communicate was a challenge. It was fun. That's the job of the Program Scientist. As I once described it, you start off with all these proposals. You've got thousands of pages of stuff. Then the peer reviews come and they boil down those thousands of pages into the most important strengths and weaknesses and write their reports on the proposals.

Then you have the categorization committee and they pick out the strengths and weaknesses that allow you to discriminate between the proposals and use those to identify the

Category 1 proposals. Then you bring forward to the selection board just those discriminating features of the best proposals.

It's really my job to create this pyramid where the most important information ends up at the top, and all the stuff that won't matter for picking the mission—it's going to be really important for building the mission but not important for picking the mission—to make sure that we don't get distracted by all that stuff.

That's the job that anybody who's working to gather information and then boil it down and present it to somebody in a manner that's actionable, that's what you have to do. But it's about mission and space science. It's definitely creative.

JOHNSON: One of the appeals of Discovery early on was that it was something where the cost is capped for the concept studies and then for the final mission. But as with anything else, sometimes it may not work out the way people think it's going to work out. How often has that actually happened and how is it handled if anything overruns or needs more money to continue developing something, and so much money has already been invested, and what do you do at that point?

HERTZ: That is one of the hardest problems we have to deal with, because every time we build one of these missions it's something we've never done before. It's the first of its kind. There's no algorithm for predicting the cost. It's a bit of an art. We have a lot of tools for trying to estimate the cost dollar but they're all estimates. We don't really know how much something costs until after we build it and we add up how much we spent on it.

If you've ever redone your kitchen, the guy that did your kitchen probably has done 100 kitchens, and they probably gave you an estimate, and how often do they overrun their estimate? They've done it 100 times before. Every time we build a bridge we overrun, it seems, and we've done that 100 times before. We've never done these missions before, and so estimating the cost in advance is not an exact science.

We try to make sure the missions have reserves and headroom between their estimate and their cost cap. People who come in without that headroom are at high cost risk and we take that into consideration. We try to pick the ones that we think can be done within the cost cap. Nevertheless, some do overrun. Then you have a hard problem.

We've never canceled a Discovery mission because of a cost overrun. We have canceled missions in other PI-led AO-initiated programs because of cost overrun. But I'll tell you an anecdote about Dawn and its cost overrun. Dawn was overrunning during its build phase after it was confirmed. Dawn was proposed to visit two asteroids. There were a bunch of asteroid proposals that competed with it. Dawn was the best of the bunch because not only was it an excellent asteroid proposal, but it had proposed a way to visit two asteroids within the cost cap instead of just one, and to orbit them, not just fly by them, spend significant time and really do a fabulous investigation of both Vesta and Ceres. But it did overrun. One of the proposed descopes was to descope it to a one-asteroid mission.

As the Program Scientist it was my responsibility to say that I thought that was an inappropriate descope because it wasn't the best one-asteroid mission that got proposed, and if we turned Dawn into a one-asteroid mission then we would in some sense violate the selection that we had made where Dawn was selected because it was a great two-asteroid mission, not because it was the best one-asteroid mission.

As Program Scientist my recommendation was either terminate it or give it the money it needs to visit two asteroids, but I thought it would be unfair to the competitive process to do it the other way (descope it to one asteroid). But because it needed more money, that was money that wouldn't be available for the next mission, it was going to delay the next AO in order to use mission money which we would have used to start the next mission, but now we'll have to use it to finish Dawn.

We actually took the choice to one of our advisory committees, and it's made up of members of the community, and said, "What do you as the community think would be more important? To hold Dawn to their cost cap and cancel them or to give them the money they need to be successful and delay the next missions because we're going to delay the next AO to free up the money in the budget?" They recommended that we give them the money and finish it, which we did. Dawn in fact was a fabulously successful, really truly outstanding Discovery mission.

JOHNSON: Over the last 30 years technology has changed. As you mentioned, the missions that would actually use some type of nuclear power, or the propulsion would be different.

HERTZ: Off the top of my head, I don't think we've ever done a Discovery mission where we provided radioisotope thermal sources. Those have been used in the Mars Program. They've also been used in the New Frontiers Program, which is a program of PI-led AO-initiated missions. Both New Horizons and Dragonfly (from the New Frontiers Program) are nuclear-powered.

JOHNSON: Okay, and there are other technology changes. But how has that affected Discovery or even these other types of programs as far as the cost? As technology advances does it help with the cost or does it make things more involved going forward? For example, the Shuttle flew with 1980s era computers with some upgrades all the way through, and some of these Discovery missions are such long duration, the technology will change while it's gone. But talk about the technology changes and how that's affected the program.

HERTZ: Certainly, technology advances, and certainly costs come down for heritage technology. The great thing about the AO and the PI-led mission process is the authority we give to the PI, the responsibility we give to the PI, is to make the trades within the cost cap to get the most science and the best science. In this case, the case that you laid out, you can either have more instruments where the technology is old enough that the costs have come down, or you can have the brightest shiniest newest technology where the cost hasn't come down but you can do something that nobody's ever done before.

Depending on what your science is, one choice or the other choice might be the right answer. For instance, on Dawn, which we've been talking about, the new technology on Dawn was the solar electric propulsion system that it had, which is what allowed it to go into orbit and leave orbit and go into orbit again, because it wasn't using chemical propulsion, it was using the solar electric propulsion system. The Sun is its power source and so it doesn't run out of fuel nearly as quickly. Its payload, as I recall, and if somebody tells me I'm wrong I'll accept that, was heritage instruments that had not been used close-up on asteroids before. By using the new technology in the propulsion system, they could go someplace that had never been gone before, use instruments which weren't new technology but were the right instruments for a new place to study these asteroids. That's a choice you can make.

Or you can choose to fly brand-new technology never been used before. InSight, the Mars lander, has done that with its seismometer. That was brand-new, never flown technology.

JOHNSON: Outreach to the public and outreach to educational facilities, schools, universities, is important to NASA, to have that support and also the support of the Administration and Congress. Some of the missions of opportunity have been student-involved operations. If you don't mind talking for a minute about how important that outreach is to Discovery and mission success.

HERTZ: I'm going to correct some terminology.

JOHNSON: Please do.

HERTZ: I know you won't mind. The student-built instruments are what we call student collaborations. They are an opportunity to fly something that is done by students as a ride-along with one of our Discovery missions. Missions of opportunity are a class of PI-led missions and they're not done by students. Mission of opportunity is a completely different conversation which is mostly not about Discovery.

I think that Alan Stern flew the first student collaboration on his New Frontiers mission. He had a dust counter instrument that was built by students and it was attached to the spacecraft, so it measured how many dust particles hit the instrument as the satellite flew from Earth all the way out past Pluto, so it measured how much dust there was in the interplanetary space of our solar system. You could see that there's more dust closer in, and a lot of dust around the asteroid belt, and then less dust further out. That was really good science that was done, and it was done by students.

One of the rules of student collaborations is that it's optional science. If it doesn't work then the mission still achieves full science success. Because students aren't professionals, there's a higher risk their instrument won't work. For all Discovery missions we give the PI the opportunity to propose a student collaboration. We incentivize it by telling them that the first so much of the cost we will pay for above the cost cap, and then if it costs more than that, they'll have to pay for the balance within their cost cap. But we incentivize it up to something like one percent of the cost cap, which is several million dollars of incentivization.

The purpose of student collaborations is to create an opportunity for students to get hands-on experience with spaceflight hardware and incidentally to do some really good science. It's a great idea, but not every mission has a student collaboration. Not every PI proposes one and not every PI proposes one that we think is worthy of selection. I don't have a list in my head of all the student collaborations we've done, but I do remember the one on New Horizons.

You also mentioned outreach. GRAIL [Gravity Recovery and Interior Laboratory] was a Discovery mission, it was a pair of satellites that orbited the Moon, had laser ranging between them, and as the pair orbited the Moon, the Moon's gravity would make them accelerate and decelerate, change distance between them. We're measuring the distance with the lasers. We mapped the gravity field of the Moon with so much precision, we could determine what the interior structure of the Moon was, as well as where there were mass concentrations close to the surface.

But in order to do that science you didn't need a camera. The Sally Ride Science Center proposed adding a small lightweight camera to GRAIL that would be controlled by students. Students would choose where on the Moon to point the camera and take a picture as GRAIL orbited and did its non-imaging science. That was a great success. That was outreach, because students didn't build the camera, it was an off-the-shelf camera, but students operated it. Students had to do the research to choose what to point it at, and then look at the data, look at the images, and draw some scientific conclusions. They had to be looking for something and then they had to analyze it to see if they saw what they expected. This was all mentored by the professionals at the Sally Ride Science Center. It was very very successful. That was outreach, which hopefully inspired some students to go into STEM [Science, Technology, Engineering, and Math], maybe even go into space science and build missions for NASA. But that's another example of the way we can leverage these science missions to advance other objectives which are important to the nation, like training our next generation of students, inspiring our next generation of students.

JOHNSON: Do you ever take part in that type of outreach with students and younger people?

HERTZ: I do give talks to students frequently, public talks. I'm not a hardware guy, so I've never been involved with building anything with students.

JOHNSON: No. But there are other ways to reach students and make them understand that astrophysicists have a place at NASA.

HERTZ: Oh, sure. Of course, most of the astrophysicists in the country are university faculty members and directly involved with training the next generation of scientists. You asked me to talk about astrophysics (which is not the science of the Discovery Program). One of the great benefits of astrophysics and astronomy is that it's very popular. Nonscientists who have to take a science class in college will often take an astronomy class. The astronomy class is sometimes one of the biggest classes at universities, the highest enrollment, because so many nonscience majors are taking astronomy as their one required science class. Which means that the professors of those classes have a really great opportunity to teach people who aren't going to be scientists what science is about, to teach them why scientists believe the things that we believe in science. It's not because we read it in a book. It's because we did experiments and observed that this is the way the universe really works. It's driven by data and experiment and hypothesis testing and all those sorts of things.

Many astronomy professors know that their responsibility is to help these people who are going to grow up to be businesspeople and voters and parents, what the role of science is in our society.

JOHNSON: Let's talk about some of the decisions. Have there have been any Agency decisions that you know of that have impacted the Discovery Program, or any of the other programs? You mentioned that the other programs started adopting this PI-led way of doing missions. But are there any decisions the Agency made? Or even congressional support or anything like that that you can think of that could have affected or did affect Discovery?

HERTZ: I'll mention a few. Congress is very supportive of NASA in general, bipartisan support, NASA is not a partisan issue. Many members of Congress are supportive of space science, supportive of planetary science. They're supportive of the PI-led missions. Part of their support is motivated by the decadal surveys which prioritize the PI-led missions. Most of the members of Congress who are interested in NASA science pay attention to the decadal surveys. They trust the National Academy [of Sciences] decadal survey process to make really good recommendations as to what the priorities should be in NASA science.

That is one area where we get support for Discovery, because everybody agrees that it generates great science in a cost-effective way and it incentivizes innovation, it incentivizes getting the science for the lowest cost that science can be gotten for. All those things are attractive to our supporters in Congress.

One of the decisions which affects all of our PI-led missions is the availability of the appropriate launch vehicles and what they cost. Over the last 30 years, which launch vehicles this nation builds and NASA can buy to launch its missions has changed a lot. Thirty years ago, the Delta II was the standard and every Discovery mission launched on the Delta II. The Delta II is no longer manufactured. Nowadays there are a range of launch vehicles that are available. There are new companies that are building launch vehicles like SpaceX building the Falcon 9 and the Falcon Heavy. But there's also the traditional companies like one that builds the Atlas V (the United Launch Alliance).

Which launch vehicles are available kind of quantizes how heavy you can build something to go to a particular orbit. We make that information available to the proposers so they can design missions that can actually be launched for the amount of money that we are setting aside to launch their mission. That changes over the years. We don't drive the launch market. We launch a couple of Discovery missions every 10 years. We launch a handful of those other smaller missions every few years. That doesn't drive the launch vehicle market. The launch vehicle market is driven by the commercial market.

But we leverage off that market. As it changes, we have to change. Somebody could look and see if average Discovery missions are heavier or lighter nowadays. I don't know enough about launch vehicles to know. I think they're heavier because I think the Falcon 9 is more powerful than the Delta II, so we can afford to launch heavier Discovery missions, which means you can cram more science into the mission that you launch.

Then the third thing I was going to say. You started with Agency decisions. Some of the most important players in the proposal process are our NASA Centers. Many PIs partner with a NASA Center. Most Discovery proposals come from a NASA Center.

Our NASA Centers have to plan for the future without knowing what mission is coming along. You mentioned earlier on in this interview the competition model. NASA has evolved. For instance, 30 years ago we were not a full-cost agency. All our civil servants' salaries were budgeted separately from whatever projects they were working on. Their labor costs weren't charged against the projects. They could work on all their proposals. If that proposal didn't win, they could move and work on another proposal.

But nowadays we're a full-cost Agency, so the project you work on pays your salary. Whether a center wins a project or not affects how they're going to use their workforce in the future and what projects people will be working on. At NASA Headquarters, we simultaneously manage a full and open competition where no preference is given to NASA Centers, where it's all about the merit of the proposal, and we know that as an outcome of the selections we make we're going to have to make sure that there's projects at all the Centers for the people to work on. That has complicated workforce planning at NASA. But we accept the complication and we have not backed down on our commitment to the competition model as a way of picking these small missions, because as I told you earlier, we get the best science, the most innovative proposals, for the most effective costs by running AOs to pick our missions.

JOHNSON: In that same line, let's talk about lessons learned from Discovery. As you said, we talked about the competition model. But are there any lessons learned from the program itself that you can think of that you would like to talk about?

HERTZ: As we said early on, Discovery is the model for the AO-initiated PI-led missions that are the majority of missions NASA scientists do nowadays. Everybody else has been following Discovery. Discovery pioneered the process. Discovery pioneered the idea of a site visit, for instance. Discovery pioneered the idea of a two-step selection. Discovery pioneered selecting two at a time, which is a great cost saver for everybody, proposers and NASA. From a process point of view, we're all just tweaking and improving and modernizing and simplifying the Discovery process. I can't think of any other generic lessons learned, nothing comes to mind.

JOHNSON: As an astrophysicist and not a planetary guy, are there any missions that you would have liked to have seen chosen that maybe didn't go in any of these programs?

HERTZ: Let me mention the one that did go, which is Kepler. For a period of time, the planetary program included planets around other stars, and so proposals to study planets around other stars were in play for Discovery. That's what the Kepler mission is. The Kepler mission was proposed and selected through Discovery. It changed everything about our understanding of planets other than the ones in our solar system. Bill [William J.] Borucki proposed Kepler [to the Discovery Program], and he proposed it multiple times [when it wasn't selected]. He kept getting "This is a great idea but we don't think you can build it, go back and do more work." When he proposed it we had discovered only a couple, a handful of planets around other stars. We didn't know if planets around other stars were common or rare, because the only planets we knew about were planets bigger than Jupiter, closer to the star than Mercury. The so-called hot Jupiters, because they're the absolute easiest to find, so of course we found them first.

We didn't know if Earth-like planets were common. We didn't know if solar systems like ours were common, because the ones we were finding sure didn't look like ours. We didn't know anything. Kepler, by finding over 4,000 planets around other stars, including families of 5, 6, 7 planets around the same star, Kepler told us that planets are common around other stars, rocky planets in the habitable zone [like Earth] are common around Sun-like stars, the conditions for life are out there, are all over the place, and that it was worth it to go and start looking for those kinds of planets and studying them. Kepler changed everything about how we think about planets around other stars, it was tremendously fabulously successful.

The Kepler mission was enabled by building a photometer, a camera, that could measure the changes in the brightness of a star to 1 part in 1 million, because that's how small the change in the brightness of a star [like the Sun] is if a small planet [like the Earth] passes in front of it. The first few times Bill Borucki proposed it everybody goes, "You can't build something that's good to 1 part in 1 million." So, he built one in the lab and demonstrated it [under stable lab conditions]. Then Borucki's team came up with an innovative orbit where Kepler didn't orbit the Earth. Kepler was pushed away from the Earth, it was orbiting the Sun, drifting around the Sun behind the Earth, in a place where it was absolutely thermally stable. If you go around the Earth you get warm, cold, warm, cold, warm, cold, and your camera won't stay stable. Kepler floated out in interplanetary space where the sunlight was always constant, the temperature never changed. It was very innovative, it was great, it made the Kepler mission possible.

Were there any others that didn't get selected that I wish had? When I was the Discovery Program Scientist, I had already done a couple of Explorer AOs. You do an astrophysics Explorer AO, and you're going to put a telescope in orbit around the Earth, you need to propose what kind of telescope, what wavelengths, what instruments, what science. Fabulous science but pretty straightforward missions. I got over to Discovery and people are proposing stuff to go all over the solar system. People proposed things to crash into comets. People proposed things that are going to zip from one asteroid to another. Going to dip down into the atmosphere of Mars, going to sample the atmosphere of Venus. It was just like science fiction. As an astronomer, I just was totally jazzed by all of the Discovery mission concepts.

The ones I thought were cool but never got selected were the ones that were going to bring back samples. There was a while where people were proposing missions that would fly out to Mars, would fly one time around Mars dipping down low enough to fly through the atmosphere. They would collect some of the atmosphere and some of the dust that's in the atmosphere and then come back to the Earth and bring back a sample from Mars. The cheap way, without ever having to land on the surface and blast off again. No, it's not a rock that might have fossils in it, but it is stuff from Mars. I thought those were pretty cool, and I'm sorry that none of those got selected just from a coolness factor.

JOHNSON: We talked about what it means to inspire kids to come work at NASA and having that involvement with students, but what do you think Discovery Program's impact on society in general would be as far as what NASA has been doing?

HERTZ: I think the science is the impact. Discovery missions like the Mars Pathfinder with the Sojourner rover, which was our first rover on the surface of Mars. I remember Sojourner, even though the rover didn't go very far, it was there and it took pictures. That was amazing. Discovery missions like the NEAR [Near Earth Asteroid Rendezvous] mission, which was our first mission to orbit an asteroid, and then it landed on an asteroid. The landing wasn't part of the original plan, but we ended the mission by doing that. That attracted all sorts of interest in asteroids and in what NASA could do.

So many of our Discovery missions, they've done things that nobody's done before. They do it in a modest size package. The accomplishments, and then the science, is what the public notices and associates with NASA. I don't think—you can tell me I'm wrong—I don't think the public notices whether there's a principal investigator or not. But they do notice when you land on an asteroid for the first time, you put a rover on Mars for the first time. All those things, they do notice that. I think that's great.

But there's a second impact. I do think that because we're incentivizing innovation, the technologies that get developed and demonstrated in the Discovery Program then go on to be the basis for the technologies we use in our planetary flagship missions. Missions like the Perseverance rover or the Europa Clipper are using technologies which have been demonstrated in the Discovery Program. This feed forward of innovation in smaller packages where you're taking a little bit more risk so that now you can put it into your flagship missions where you really don't want to take risks, that's the right way to do technology development. Discovery is a part of that.

JOHNSON: Let's talk about your position now as Director. Are you involved with Discovery in the position you've held for the last 10 years?

HERTZ: Absolutely. As a division director I sit on the selection board for our AO selections. Every Discovery selection, every Discovery downselection that comes up, I sit on the board. I get briefed by the Program Scientist of the strengths and weaknesses of these missions. Then I engage in the discussion with my fellow division directors, other members of the board about the pros and cons and which way we should go forward.

I remain incredibly interested in Discovery missions. They're still cool. They still have that wow factor every time. I participate in full in these discussions and help the team do the trades between the pros and the cons, the various science areas, and help figure out the right priorities for the next mission based on the full and open competition model that gets us there. We don't have preconceived priorities going in. We let the proposals drive those priorities.

I can truthfully say that I have helped move the decision in directions that it might not have gone without my participation in the discussions. I think it's one of the most sobering responsibilities that we have as senior NASA managers, making decisions about our next missions.

JOHNSON: I would think it would be very sobering. That's a lot of money you're handing over.

HERTZ: That's correct, it's a lot of money, and it's taxpayer's money. We're all committed to making sure that it is well spent. It's appropriated to us by Congress for the purpose of doing science, in this case the purpose of doing planetary science. We all take very seriously our responsibility of making sure it is well spent, and we do the most compelling planetary science we can do with that funding.

JOHNSON: I read that you were planning to step down as Director.

HERTZ: I have announced my plans to step down after more than 10 years as Astrophysics Director. In fact, the advertisement for my job to hire my successor has been open since December and will close later this month. When my successor is chosen and arrives at NASA, then I will step down and hand over the Astrophysics Program to my successor.

JOHNSON: Are you planning to retire at that point or are you going to stay on?

HERTZ: Not yet. I'm going to move to the front office, be a senior adviser to the Associate Administrator, and continue kibitzing on the Discovery Program.

JOHNSON: That's good that your voice will still be heard. As far as your career, and your career with Discovery, because you've gone from those early days and walking in after an AO had already been released, and becoming the Program Scientist, all the way through helping to pick these missions, is there anything that you're most proud of during those 22 years?

HERTZ: Out of the the things we've talked about I think I'm most proud of the AO simplification process and creating the standard AO in the first place. During that period, I led the effort to really standardize best practices across the entire Science Mission Directorate for our AOs. Not only the standard AO but standardizing what are the rules for the peer reviews, establishing conflict of interest rules, formalizing the rules for categorization and steering committees. Then actually even helped write the charter for the selection board, we created the selection board in the first place.

When I was Chief Scientist, my job was to make sure we had quality processes, and so I'm very proud of some of the processes I put in place that have made the Discovery selection process as good as it is.

JOHNSON: Conversely, is there anything that was most difficult for you?

HERTZ: I think a lot of decisions when missions got in trouble. The Dawn example that I spoke of, that was really really hard. Because I didn't think the middle road was appropriate. I thought we had to either put up the money or do without the mission. The Kepler mission overran significantly. It turned out to be harder than it should have been. That was a difficult discussion between Discovery and astrophysics, because the Kepler mission was very important to astrophysics. Astrophysics ended up paying for a good bit of the overrun because it was too big of a burden on Discovery. Soon after that they decided that the study of planets around other stars should be in astrophysics and not Discovery. So now our newest exoplanet mission, TESS [Transiting Exoplanet Survey Satellite], is an Explorer mission, not a Discovery mission.

JOHNSON: That's interesting. As someone that's not involved directly with the science of NASA, other than talking to people like you, it gets a little confusing on what program the missions are in, because I read about them like everybody else does, and they sound really cool.

HERTZ: Yes, and I groove on that kind of nerdiness. That's one of the things I like about working at NASA Headquarters, is knowing and paying attention to those kind of details. Those details matter because the money has to come out of some budget, and somebody has to be the manager, and direction flows up and down. For everybody else out there who's paying attention to the science, I don't think they need to care about which program each mission came out of. The science is awesome across all of it, whether it's Earth science, whether it's heliophysics, astrophysics, planetary science. It's all awesome. We're doing PI-led missions in all of those disciplines.

JOHNSON: There are so many now compared to before Discovery. In the '80s there were one or two of these kind of missions. Then after Discovery started it just exploded.

HERTZ: Right. In fact, this is a lesson learned from Discovery. I'm going to put it in context. One of the motivators for Wes [Wesley T.] Huntress creating the Discovery Program back in the early '90s was that planetary science was having these flagship missions like Galileo, like Cassini, like the Mars Orbiter, they were coming once every 10 years. You're betting your whole program on them. Then when Mars Orbiter failed there was nothing for another 10 years. Wes Huntress said, "We need smaller missions that launch more frequently." That's where the Discovery missions came from. They did the same thing over in astrophysics and heliophysics when they created the Explorers Program, or they changed the Explorers Program to be smaller missions instead of bigger missions. The goal was to do smaller missions more frequently. That is something that Discovery gave to us.

Nowadays Discovery missions are kind of medium size. We've learned that we can do missions even smaller than Discovery. The planetary program has a program called SIMPLEx [Small, Innovative Missions for Planetary Exploration], where they're doing missions which are one-fifth the cost of a Discovery mission, and they'll be doing good planetary science. There's only a few of them. None of them have launched yet, but that's what we're looking at now.

Over in astrophysics we're doing things even smaller than Small Explorers called Pioneers, which are SmallSats, that'll do good astrophysics in a small package. We're all flying CubeSats nowadays. We have a lot more missions now because we've learned how to do really good science in the small packages. Whereas when Discovery first started, all we were doing was big missions.

JOHNSON: What do you see as the future of Discovery?

HERTZ: I don't see it changing at the core from what it is now, which is modest-size competed PI-led planetary missions. I see us innovating so that we can go more places with Discovery in the solar system than we've gone before because we figured out how to do things a little bit cheaper. We make our instruments lighter so we can launch them further and faster into the solar system. We improve our communications so we can get more data back. We're able to build onboard laboratories like we put on the Mars rovers so that maybe we can do some in situ measurements at different places in the solar system without having to bring samples back to Earth. I see the science expanding tremendously because we'll continue to innovate.

Launch costs will come down and that'll allow us to do more science per dollar. All of these will contribute to Discovery evolving. But at its core I think it'll stay the same program it is because I think at its core it's so successful. Everybody is imitating it.

JOHNSON: Before we close, is there anything that you can think of that you'd like to talk about or mention, or anybody that you'd like to mention?

HERTZ: I didn't know if you had Dave [David B.] Jarrett on your list of people to interview. Dave Jarrett was I think the first Discovery Program Manager out at JPL [Jet Propulsion Laboratory, Pasadena, California] when the program was at JPL. He's retired, but I have his contact info and I'll send it to you.

JOHNSON: Okay, that'd be great.

HERTZ: Another one I'll send you is Steve Brody, who is also retired. He was the Program Executive for Discovery when I was the Program Scientist.

JOHNSON: Okay, yes, that would be great.

HERTZ: I hope you talk to Wes Huntress.

JOHNSON: We've talked to him in the past and I know that Susan Niebur had interviewed him. But that's been 10 years ago or more, probably around 2009. It might be worth talking to him again. But we've interviewed him for another project in the past too, but it's been a long time.

I appreciate you talking to me today and hopefully we didn't take too much of your time, and hopefully you enjoyed it as much as I did.

HERTZ: I did enjoy it. Thanks for the opportunity.

[End of interview]