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

MICHAEL H. NEW
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is February 23rd, 2022. This interview with Dr. Michael New is being conducted for the Discovery 30th Anniversary Oral History Project. The interviewer is Sandra Johnson, and I'm in Houston, Texas, and Dr. New is in Washington, DC, and talking to me over Microsoft Teams today. I appreciate you taking time out of your schedule to talk to me. This is going to be helpful, I know, for the project.

I want to start by asking you to briefly describe your background, your education, where your interest in exobiology came from, and then how you first came to NASA.

NEW: Sure, happy to. I started out life wanting to be a chemist, and I majored in chemistry in college. I then went to Columbia University [New York] and I got a PhD in chemical physics, which is sort of physical chemistry but with more math. When I was doing my PhD research, I was studying very very basic physical processes that drive some biology. In particular I was looking at the hydrophobic effect, which is why oil and water don't mix. I was doing that using computer modeling, computer simulation. I did a postdoc at the University of California [UC], Berkeley, where I was looking at biological electron transfers in photosynthesis. Then I started doing another postdoc with someone who I thought based on their address was at UC San Francisco. Turned out he was adjunct faculty at UC San Francisco but his offices were at NASA Ames Research Center. This was Andrew Pohorille, who's still there right now. I worked with Andrew I guess for like four and a half years studying a variety of things. But one of the things that Andrew studied was origin of life studies. He looked at the chemical and physical basis for how life could have gotten started on the Earth. That was my real introduction into origin of life studies and therefore exobiology.

Worked with him for four or so years, was hired as a civil servant at Ames and continued to work in association with Andrew. My research at the time was related again to just how did life start on the Earth. This is something that most people don't think NASA funds, but NASA is actually the major funder of it in the United States. Around 2001-ish a position opened up at NASA Headquarters [Washington, DC] to run the Exobiology Program, which I was funded in, and I thought it might be really interesting to try doing some programmatic management. I had done a little bit of people management, I was an acting deputy branch chief for about a year and a half, and I liked it. Which is a little sick, because it's really a very high responsibility low authority kind of job. But I really enjoyed it, and so I thought I'll try program management, so I applied for the position at Headquarters. Surprising to myself, I got it, and in 2002 I came to Headquarters to become the Astrobiology Discipline Scientist.

I ran the Exobiology Program from 2002 until 2017, and that's what brought me to Headquarters and the Planetary Science Division, because that's where exobiology is housed still to this day.

I've always been interested in a variety of things. I'm kind of an inch deep and a mile wide in a lot of ways. Running a program as broad as the Exobiology Program, which covers everything from the origin of habitable planets all the way up to the origin of multicellular life, was really appealing, and fit my interests and personality.

JOHNSON: When you were still at Ames, since the Discovery Program had already begun at that point, were you involved at all with the program at that point? Or was that later when you came to Headquarters?

NEW: No. I wasn't involved with Discovery at Ames at all. Peripherally since I was an acting deputy branch chief at the time, I was on the edge of some of the negotiations that went on regarding the Kepler [space telescope] mission. Kepler was an extrasolar planet detection mission. The principal investigator of that mission was Bill [William J.] Borucki from Ames. Around the time it was selected to happen, to go into development, Ed [Edward J.] Weiler, who was the Associate Administrator for Space Science at the time, decided that only two NASA organizations in his mind would be allowed to manage big missions like a $400 million mission like Kepler, and those were Goddard [Space Flight Center, Greenbelt, Maryland] and Jet Propulsion Lab [Pasadena, California].

When they selected Kepler, they gave Bill Borucki a month or two, and they told him to go off and choose between the two, because he had originally proposed that Ames should manage this mission. Being in branch management at the time, I was at some of the division and higher-level meetings about this choice between JPL and Goddard. That was the closest I came to the Discovery Program when I was at Ames. Ames has an old history of success in missions, but by the time I had gotten there in 1996, 97, a lot of that history was past. There were very few people left who had managed the development and operations of flight missions at the time.

JOHNSON: How much did you know about the Discovery Program when you decided to move to Headquarters and when you started working with it as far as the selection? What did you think about this way of choosing missions and to get more missions going?

NEW: The timing here is important, I think. I came to Headquarters in 2002. I didn't really start managing the Discovery Program till 2006. I came to Headquarters basically to run grants programs, and I started doing that, and looked around and realized that the really really interesting thing about working at NASA Headquarters was being involved in missions. I talked to my then division director, Colleen Hartman, about how I could get more involved in mission planning, mission development. There were a few possible false starts, and I knew about Discovery because—this is kind of strange, but—my cubicle was next to, shared a wall with, Susan Niebur, of blessed memory's, cubicle. We became friends, and she was running the Discovery Program at the time. That's how I learned about the Discovery Program and its originally novel approach to picking missions.

Around 2006, right before the 2006 AO [announcement of opportunity] came out, Susan Niebur resigned her position, and they put out an email saying, "Hey, is there someone who wants to step up into this role?" By that point I had worked on a few, I'd helped out with some mission stuff, and I knew the very very basics of how PI [principal investigator]-led missions were selected. I went to Andy [Andrew A.] Dantzler, unfortunately also of blessed memory, who was the division director at the time, and I said to him, "Look, Andy, I don't know a lot about this. But if nobody else is willing to do it, I'm willing to do it." That's sort of a refrain for a bit of my career actually.

A couple days later nobody had come forward. The AO was about to hit the street, and so he came to me and said, "Okay." He set it up with Paul Hertz, who at the time was the Chief Scientist. He had also run a couple of Discovery competitions in the past, and he'd gotten Paul to agree to mentor me in the role, and so that's how it all started. I didn't write the 2006 AO; I just ran the evaluation on it.

JOHNSON: Do you think that that background you had—you said you were working with grants programs—and that evaluation that you do with a grant program, helped with this position and the way Discovery was being run?

NEW: Oh, absolutely. There are a lot of differences and subtleties. There are subtle differences between how you evaluate grant proposals versus how you evaluate the science part of a mission proposal. But at a fundamental level it's all about the psychology and sociology of science evaluation. So, it was absolutely transferable. The part of mission evaluations that was really very novel compared to what I had been doing was the technical, management, and cost evaluation, which is the other part of mission evaluation, because when we evaluate missions for Discovery, or for any PI-led mission these days, there are actually two panels that meet. There's a science panel and then there's a technical, management, and cost panel. Fortunately for me and for everybody else who is in charge of these programs, the technical, management, and cost panels are managed by an organization now known as SOMA, the Science Office for Mission Assessments, which is a Headquarters function that has been delegated to Langley [Research Center, Hampton, Virginia]. They're professionals, and the people who work there, that's all they do is run these technical, management, and cost evaluation panels. They're lifesavers because they've done tens of these things over the course of their career. The people there, they know what they're doing. When you're a program scientist doing your first one, they're very very helpful to do it.

JOHNSON: Let's talk about that. As you mentioned, that 2006 AO had already been written. But it originally was released in 2004, and they didn't select anything at that point. Was it reworked for 2006?

NEW: Oh yes. The biggest issue in 2004 was that in 2004 AO they gave both a cost cap and a cost profile. The cap is the area under the profile curve over time, and it turns out that nowadays we'd say that giving both of those things as constraints overconstrained the problem. Because you might be able to find a mission that would fit in your cost box but it would have a different spending profile over time. You might be able to find a mission that had a similar spending profile in terms of 10 percent in the first year and 15 percent in the second year, whatever. But the total sum of that might be different. But constraining both together was just overconstraining. When they got to the end of the competition there were really nothing scientifically meritorious and scientifically appealing that fit in the box and under this profile.

One of the things they did when we went from 2004 to 2006 is we removed the constraint of a fixed profile. They said, "Here's a cost cap. The mission must cost less than this and must be developed in this many months. But we're not going to tell you how to divide that money up across those months. You get to tell us." Of course, not every spending pattern is accommodatable by our budgets, but we told the proposers, "Don't you worry about that, that's our worry. You propose the best mission you can propose given the total cost and the total time.

Then if we can't afford it, if we can't make it fit in our budget, well, we'll figure something out, and that may mean we won't pick you." But that was important to fix that, to relieve that stress. That was one of the biggest differences between 2004 and 2006.

JOHNSON: You ran that selection then, that selection process. Talk about that process of selection and how you went about running it in your position, what you did specifically.

NEW: Sure. Let me start with the big picture. We want to pick the most meritorious science investigation we can that advances NASA's scientific objectives while fitting into the resource constraints we have. To do that, when we evaluate mission proposals, we evaluate them on three different criteria, three different axes if you will. The first is scientific merit. That includes how important are the questions they want to answer, how well does this particular proposed mission fit into other things that we're planning on doing or our partners are planning on doing.

We also want to ask the question if this mission were to fly and were to get the data that they claim in the proposal to need, have they proposed a method for going from that perfect data to answering their key questions. That's scientific merit. That's evaluated by a panel of scientists. The Program Scientist, me, recruits that panel.

The next thing we look at is what we call science implementation, which is really asking the question—if you remember in the first thing I said, science merit, we talked about the perfect data. Now for science implementation, we're asking, "With the instruments you've picked, the mission architecture you're planning, all of that stuff, how likely is it that the data you're going to get from those real instruments is going to be close enough to this idealized data so that you'll be able to actually do whatever calculations you have to do to turn those data into the answers to your questions?" That's evaluated by the same science panel as the first science merit that I talked about. Again, that panel is put together by the Program Scientist, in this case me.

Then the third thing we look at is technical, management, and cost feasibility, where we now say, "Your question is meritorious. You've got a great way to go from perfect data to answer those questions. We've now looked at everything and the data you're going to get actually is pretty close to this perfect data. You're good that way. Now we're going to ask can you design, build, test, launch the mission to get those data." That involves looking at engineering issues. It involves looking at the management team, how experienced is it. It involves looking at the capabilities of the various multiple institutions involved in the proposal. It involves looking at whether your cost estimate is reasonable, and whether your schedule seems reasonable. That's done by this technical, management, and cost panel or TMC. It's a group of mostly engineers. They're recruited and that part of the process is led by an acquisition manager, who's somebody who works at the Science Office for Mission Assessments, SOMA, at Langley.

When we started getting the proposals in for the 2006 AO, in that AO, although Discovery was a Solar System Exploration Division program, it accepted proposals to identify and characterize extrasolar planets. That was Kepler for example. We got proposals in roughly three categories. We got proposals to study small bodies, so comets and asteroids. We got proposals to study planets, the Moon, Venus, and so on. We got a bunch of proposals to identify or characterize extrasolar planets.

Based on how I ran ROSES [Research Opportunities in Space and Earth Science] research grant proposal reviews, I took the proposals, I broke them up into these three bins, small bodies, planets, extrasolar planets. I went ahead and then worked with other program scientists both from my division and the Astrophysics Division to recruit scientists who would serve as science reviewers. I forget exactly how many, although I can dig out that information if you really want to know. But we had three panels. They met simultaneously in a hotel for a week. They went through all of the proposals. They'd all read them before they got there. They'd all written individual reviews, just like we do with grants. Then they discussed and voted and figured out what they thought was the best. Then from their scores and the scores of the technical, management, and cost panel, we came up with a selection recommendation.

That was my first exposure to mission science evaluations really. I did an extensive lessons learned at the end of it. One of the big, big differences between a—well, there's two big differences between a grant proposal evaluation and a mission science evaluation. The first is that most scientists have a natural understanding of what a good research proposal looks like, because they all have to write them and they all know what's gotten funded in the past and what hasn't. They kind of come mostly pretrained.

When it comes to mission science proposals though, very few principal investigators, very few researchers ever write or participate in the writing of one of these proposals, so they don't have a clear internal standard. That was one of the big differences.

The other big difference is that when we evaluate research proposals for grants, the criteria we're using are not especially structured. We'll say the scientific merit of the proposal, and we may say this could include this, this, this, this, this, and that. But that's about where we end it. For mission proposals it's much more structured in the sense that we say science merit of the proposed investigation, factor one; merit of the question, factor two; alignment with future plans, factor three; likelihood of success, factor four. One of the things I learned from doing that review was that when I would do another mission science review, I would have to have a process that was more structured than a grant review process, because fundamentally the kinds of questions we were asking the reviewers were just more structured, if that makes any sense.

That's how we did it. We looked at all the scores and so on. We found that at the time none of the extrasolar planet identification or characterization missions really fit in the cost box. The cost box I think was $425 million, maybe $475 million. When we did cost estimates, they were all in the 500-million-to-600-million-dollar range, which is retrospectively not that surprising, because I think the final cost of developing the Kepler mission was in that range. We didn't select any of the extrasolar planet missions to go on and do a funded concept study. We wound up selecting three missions. One was GRAIL [Gravity Recovery and Interior Laboratory], which was actually a pair of spacecraft that would go orbit the Moon for 90 days and generate very very high precision gravity maps of the Moon. Second one was Vesper, which was a mission to Venus that carried a really powerful spectrometer to interrogate the atmospheric chemistry of Venus, which we hadn't really done. The third was a mission called OSIRIS [Origins, Spectral Interpretation, Resource Identification, Security]. That was going to go to an asteroid and return a sample to the Earth.

We made those selections. I did the debriefing, meaning that I sit in a small room with mostly annoyed researchers who were not selected. I had to answer their questions about the review. In those days we would not hand over to the proposers a hard copy of the review forms. Instead, I had to sit there and they had to sit there and listen to me read very slowly everything on a form. This whole show came about because somebody seemed to think that this would reduce the risk of a bid protest. Maybe it did. We've only ever had, I think, one bid protest in any of our PI-led competitions, none in Discovery. But it was also remarkably tedious, and I always felt it was a little disrespectful to the proposers.

I did the debriefings. We then had a kickoff for the three teams that were going to go off and do concept studies. We gave them, I think it was six months. They went off and did their concept studies. Then they submit this concept study report, which is sort of a giant version of a proposal. In 2006 we weren't doing any of the mission proposal process electronically. So, for the first step I got a whole bunch of books basically delivered to my office. Then for step two I got giant dictionary-sized books dropped on my office desk.

We did an initial review meeting, and we sent off a bunch of questions. Then we did what's called a site visit, which is where the whole review team would show up at the facility of the proposer's choosing, and they would have seven or eight hours to tell us everything that we wanted to know about their mission. We did three site visits that year because we'd selected three Phase A concept studies. It was really interesting for a lot of reasons, one of which is that we don't do anything like a site visit for a grant review. It's really a unique feature of the process we use to select PI-led missions. Another interesting feature was that the kinds of information you'd get out of a site visit was way way more than just the technical information. Because you really got to see the proposal team working as a team or not working as a team. That was eye-opening.

I remember we went to the site visits and most of us had in the back of our minds a sort of rough ordering of the three concept studies, which we thought was more likely than the others to win. The site visits really scrambled that ranking because you could tell almost instantly which teams really had bonded and functioned as a team, which teams were way too dependent on the PI, or the PI was too dictatorial to give the team leeway. Really fascinating type of sociological information that you wouldn't get any other way.

We did site visits. We reassembled our review team to do one last review of everything. Then we wrapped everything up, put a bow around it. Came to Headquarters, sat down with my boss. We decided on what we would select based on the reviews. We selected GRAIL, which went on to fly to the Moon and produce exquisite maps of the local variations in gravity around the Moon, which led to all kinds of deeper understanding of the geological structure of the Moon. There are these things called mascons, mass concentrations, on the Moon, which were discovered in the run-up to Apollo. GRAIL was able to provide enough high-resolution data about them that they were able to figure out what they actually were. Intrusive dikes, lava that pushed its way into the surface and then solidified. But it's denser than surrounding material. That mission was led by Maria [T.] Zuber, who was one of the first female full mission PIs at NASA. Carle [M.] Pieters had been selected earlier as the PI of an instrument, something called M3 [Moon Mineralogy Mapper], that flew on the Indian [Space Research Organization] mission Chandrayaan-1 [lunar probe]. But the first full mission female PI was Maria, and not that this is connected, but she's gone on to be very very successful. I think she's the vice president [for research] of MIT [Massachusetts Institute of Technology, Cambridge Massachusetts] right now. Yes, that was the first Discovery competition I ran.

JOHNSON: You talked about the missions and their concept studies, but also three missions of opportunity were selected.

NEW: Oh yes.

JOHNSON: Talk about the difference between the two. Was that run similarly as far as getting them evaluated?

NEW: Yes, in those days we willy-nilly mixed the mission of opportunity proposals in with the full mission proposals for both sets of evaluations, the science and the TMC. It was pretty much exactly the same. I think we did some site visits as well for that too. I'd have to go check my notes. But it was pretty much the same except you don't have spacecraft to evaluate. The technical part of the evaluation is much simpler in the sense that you're *only* evaluating an instrument, not an instrument and all of the supporting machinery around it.

JOHNSON: You were talking about the teams. When they first decided to propose the principal investigator, the PI is the leader of that team, but they have to put together a team that will work with them. You need someone from outside looking in, you would need someone that can manage things or that can manage the side of the house that the PI wouldn't be managing, as far as watching funds and that sort of thing. Then you also need engineers that can actually build what you're wanting to do. Talk about those teams and the various roles in putting them together. You mentioned that when you went you could tell if those dynamics were either really good or really off. But discuss the dynamics a little bit and how they work in a proposal and what those roles should be.

NEW: The principal investigator is treated as the person responsible for everything by NASA for a mission. Principal investigator of a mission. Of course, that's not possible because you can't be both 100 percent chief scientist and 100 percent chief engineer at the same time. What happens is the principal investigator partners with someone who's a project manager. The project manager is an employee almost always of the institution that is going to manage the mission. That's rarely the institution that the principal investigator works for. Many of our principal investigators are university professors, and there are very few universities that are interested in or capable of managing a 400-million-to-500-million-dollar spaceflight project.

Very often university faculty will partner with NASA Centers or sometimes some other academic nonprofits like the Southwest Research Institute [San Antonio, Texas] or the Applied Physics Lab at Johns Hopkins [University, Baltimore, Maryland], and that institution will provide them with usually a small choice of project managers. On the day-to-day ins and outs management of a flight mission, the project manager is doing that. Discovery missions are big enough that the discipline of systems engineering becomes really important because missions are usually complex enough, especially nowadays, that you can't keep it all in your head. You need to bring some structure and rigor to how you engineer your system. The third key member of the leadership team is the project systems engineer. Then finally since often the principal investigator and the project manager are not at the same institution, very often there'll be a project scientist added to the team whose job it is to basically be the science interface with the project management part of the institutions, to be the liaison and the advocate for science.

Different missions have different unique histories about how these teams get put together. Sometimes a scientist will put together a small science team, and then go looking for mission management support. Sometimes a mission management center will have come up with an idea for a mission and go looking for a scientist to lead it. There are all kinds of possibilities in there.

The kinds of dynamics that you want to see is every member of the team, whether they're an engineer down in a subsystem or a scientist who's part of a team that's designing and testing an instrument, you want to see that they all are contributing, that they feel that they can contribute, to the mission. You want to see a principal investigator who's confident enough and trusts their team enough so that when you're at a site visit and somebody asks them a question, they don't feel obliged to answer it, they feel comfortable turning to someone and saying, "Well, Janet here is the engineer in charge of that. Janet, you answer the question."

That's the kind of interactions you want to see. You want to see them working as a team. You want to see them passing off answers one to the other to the other. You don't want to see the principal investigator answering every single question. You don't want to see a project manager who seems a little bit out of touch or overly formal. These are big complex missions, but they're often not big complex teams. The project manager of these teams really does have to be able to know this is the engineer who's doing this thing, and if I have a problem or a question, he or she is the person I'm going to go talk to. Not oh, well, I'll put a request in to the section head, who will put a request in to the group leader, who will put a—that's not the kind of organization you want to see in a Discovery mission.

They're not anywhere near as flat organizationally as they were at the start of the program. But you do still want a project manager who can walk around the shop floor, if you will, and know everybody, or most of everybody. All that comes out in just the way they answer questions, the way they look at each other, who sits where, which is kind of silly, but it's true. Yes.

The site visit is also often overlooked by proposal managers. These are the people that work for management organizations that guide the proposal through the various steps. Yes, a lot of this nontechnical stuff is often overlooked when they put together their site visit agendas and how they set the room up and all that kind of stuff. Yes. Site visits can be remarkably illuminating. Not about technical stuff, although technical stuff too. But just looking at how people interact, who's answering the questions.

I was at one site visit where the project manager answered almost no questions, but there was a junior systems engineer who answered all the questions. The review panel's unspoken advice later on was that the PI should ditch the project manager and make this systems engineer their project manager, because clearly, they were doing that job now. It's that kind of stuff you can't get from paper. Does that help?

JOHNSON: Yes, it does. I can see where that site visit would be very important, because for the future success of anything NASA does, you have to have a team that works. That makes a lot of sense. Once the mission is selected, how much interaction do they have with Headquarters while they're going through the process of trying to get that mission off the ground? What's the relationship between Headquarters and that group?

NEW: I need to ask you at what point in the life cycle. There's before they submit their proposal. There's after they've submitted their proposal and we've selected them to do a concept study. There's when they're doing their concept study. Then there's after we've downselected and picked a concept study to continue into development.

JOHNSON: Let's just talk it through. Talk about how Headquarters communicates and builds that support that these people need all the way through the process.

NEW: We actually don't build a lot of support early on. NASA doesn't help people write proposals per se. We do provide a lot of support materials in what's called the Program Library, which is now a website where proposers can go and find all kinds of things, all kinds of relevant documentation. NASA policies, costing tables for different services, things like that.

We do answer questions about the AO, about the announcement of opportunity. What does it mean? What do you mean in page 73 when you say there should be? Is that a requirement or not? That kind of question.

I should say the reason why we have very limited contact before we make a selection is because that first step is an honest-to-goodness procurement by the federal government, and there are tons of rules, regulations, and guidelines about how the government has to behave during a procurement. Mostly we have to be totally fair, totally transparent. Those requirements mean that we can't really be very supportive to proposers. We can be a little bit but we have to be equally supportive to all proposers.

Once they're downselected and they're now a concept study, that's when we first introduce them to the program office. Every one of our mission lines has a program office. In the case of Discovery, the current program office lives at Marshall Space Flight Center [Huntsville, Alabama]. The guys at Marshall are phenomenal. Originally most of them came out of rockets and human spaceflight, and they built a program office from the ground up for supporting PI-led missions. They've done a great job. They're totally focused on mission success, on science success. They're great.

After we select a couple concept studies, we have a kickoff we call it. We do a bunch of things at the kickoff meeting, one of which is they get face time one-on-one with folks from the program office. This is important for lots of reasons. Most immediately it's important because it's the program office that's going to issue the contracts to the concept study teams that fund their concept study development.

Along the way we have dedicated points of contact both before and after the initial selection to answer questions about NASA-provided services. We'll have somebody who can answer questions about launch systems, somebody else who can answer questions about using space on the outside of the International Space Station, things like that. We usually have somebody on call who can answer questions about the Deep Space Network, which is our radio communications network that's optimized for distances beyond the Moon.

They enter into the concept study phase, they get financial support, and they get a little more help, but not very much at all, from the program offices. At this point, it's no longer a procurement. We have a contract with each of the three concept study report teams, or five, however many it is that's been selected. Because they're on contract, they are NASA contractors. The next step when we downselect, we say we select something for further development into flight, that's not a procurement because we're not really doing a new contract. What it is is a process we use to decide whether or not to exercise options on the original contract. That gives us a little more latitude, which is why we can do site visits for example, why we can ask questions and have an interactive discussion with proposers, with concept study teams.

Then once they're downselected and we've decided yes, we're going to move this forward into flight development, then the focus of their interactions with NASA shifts. Day to day it's now with the program office, although every mission has at Headquarters a team of a program scientist, a program executive, and a program analyst who monitors the science, the engineering, and costing, accounting.

This Headquarters triumvirate works with the people at the program office, who work with the project people. That's where you start getting this much more elaborate interactions, much more support, because now they're official, they're officially a NASA mission. They're not a concept anymore, they're not a proposal anymore.

What we've done in Discovery is for the teams usually when we make the initial selection and we select three or five teams for concept study reports, we assign one program scientist to all of them and one program executive to all of them, to help them interact with the program office to answer questions, to get contracts squared away. Very often when we transition from the concept study phase to an actual next mission step, the program executive and the program scientist will carry over into that new phase.

Before we made the initial selection, the contact for the team, as I said, at NASA is necessarily kind of formal. That goes through the program scientist who's leading the competition.

JOHNSON: That's a good explanation. I appreciate you giving me that, because it makes sense that it would follow some of those early procurement type things that NASA has to do when they're buying something or letting a contract.

Let's talk about those announcements of opportunity and that process. Who's involved in coming up with the announcement of opportunity, and is there a team that writes it? Talk about that process a little bit if you don't mind.

NEW: Sure. Prior to 2010-ish, 2014-ish, somewhere in there, I'd have to check, every AO that was written, every announcement of opportunity that was written, was basically written from scratch. At some point in the mid-2000s when [S.] Alan Stern was the AA [Associate Administrator], the Chief Scientist at the time, Paul [L.] Hertz, was given the job to simplify AOs. He put together a team, I was part of that team, and we did a whole bunch of workshops with community members and decided that in addition to simplifying AOs we'd standardize them as well.

We created a standard AO template, and we've been using versions of that template since then. In the pre-standard-AO era, every program scientist working with an acquisitions manager at SOMA would draft the AO text, based usually on the last AO that went out. That process led to a certain amount of continuity too within AOs of a given program, because the 2006 Discovery AO was very similar to the 2004 Discovery AO, which was very similar to the 2002 AO. But it didn't lead to much continuity between AOs of different lines. The 2006 Explorers AO followed its own lineage back in time, and while there were commonalities with the 2006 Discovery AO, there were also pretty serious differences.

Now with the standard AO the program scientist is still the person who's leading the writing of the AO. The acquisition manager at SOMA is also the person charged with putting together the technical parts of the AO. But there's a lot less de novo writing to be done. The standard AO was written around a series of boilerplate text with options that individual AOs can flip or not flip, turn on or not turn on. Some of it is policy-based. For example, the Earth Science Division has a much more elaborated data management policy than the other divisions, so the AO has basically two sections describing data management requirements, one that's specific for Earth Science Division and one that's for everybody else.

Writing an AO is not quite only just filling in the blanks and flipping switches, but it does mean that a lot of the stuff you would have had to have written from the ground up you don't have to worry about. If you do need to add something new or novel or experiment or whatever, you can, because that's where you can focus your energy.

When I wrote the 2010 AO, this was before the standard AO, and so I gathered a team around me. I had a whole bunch of people from different parts of the Agency that were experts in something. When I needed somebody to help me craft the section on environmental compliance, I would go talk to somebody from the Environmental Management Division. They would help me. Then for the section on launch vehicles I'd sit down and talk to somebody from Launch Services Program, and so on.

We still do those kinds of consultations, but more often than not they're done at a higher level when you are making modifications to the standard AO. Unless you're trying to do something new and sporty in your AO, you don't necessarily have to gather a little kitchen cabinet around you. You write it. You probably iterate it a few times. Especially with your management, because very often your senior management will have things they want to do in the AO, new things, or they want to take lessons learned from the previous AO and not do things. Like the example we had about the 2006 AO where we decided not to enforce a profile.

Then when the AO's big policies are kind of settled, these are things like cost cap, duration of the Phase A study, amount of money we're going to invest in Phase As, concept studies, things like that, we have a SMaC meeting. SMaC is the Science Management Council. It's essentially all the division directors plus all the deputy associate administrators in SMD, in Science Mission Directorate, and a bunch of other people from other offices like procurement and legal.

The SMaC meets weekly, and at one of these SMaC meetings if you have an AO coming out, you'll give a short presentation about what the key policies are. Generally, you'll get some questions, but unless you're doing something really really new or really kind of sporty, you're not going to get a lot of pushback, if that makes any sense.

After you go through the SMaC, then you can put the AO into concurrence, meaning that there's a series of people who have to sign prove the release of the AO. Thomas [Zurbuchen], the Associate Administrator, is the last signatory. Usually, it's people in the division who have to sign off on it. I have to sign off on it. Sometimes the Deputy Associate Administrator for Programs has to sign off on it. Then Thomas gets it, the AA for SMD.

When he signs off on it there's a new thing going on now that we have to await permission from the ninth floor to release it, the ninth floor being where the [NASA] Administrator's office is. Then we put it out. We put it out on NSPIRES [NASA Solicitation and Proposal Integrated Review and Evaluation System], which is our big proposal submission and management system. We send out an email to 56,000 people I think is on the current e-mail list and tell them, "Hey, we've just posted an AO." Then they go off and look at it. Very often these days we will actually issue the AO twice, once in draft form, where we invite comments from the community on things that are unclear or things that are thought by members of the community to be unfair. They provide us their feedback. Then 60, 90 days later, sometimes more, we'll incorporate their comments and then issue the final AO. Then 90 days after that the proposals come in. Does that explain the process?

JOHNSON: It does. It seems like a very long process from the beginning to the end.

NEW: It's over two years.

JOHNSON: When you finish that one process from beginning the AO to making a selection, do you just start on the next one at that point?

NEW: It depends on the program. Discovery Program, before I started working it, it would issue an AO every two years. That became less and less sustainable for a bunch of reasons, one of which was the time of the program scientist. The other was that one of the metrics of success for the Discovery Program is given as the launch rate, not the rate at which AOs come out. So, it turned out that in terms of workload for everybody concerned, instead of issuing an AO every two years and making one selection, it's actually more efficient to issue an AO every four years and select two from that batch. When you do that, then you do get like a year, two years in between AOs where you don't have to do much as the program scientist.

Programs other than Discovery though have much quicker cadences. In the Explorers Program they alternate what they call Small Explorer, which has a low-cost cap, with a midsize Explorer, which has a higher cost cap. They do that alternation, I think it's two years or 18 months or something. Basically, you can actually wind up in a situation where you're finishing writing the next AO when you're finishing up evaluating the concept studies from the first AO, and that gets really draining. I'm glad that Discovery went to a four-year AO cadence instead of a two-year AO cadence, because it's just too much work.

JOHNSON: Yes. It sounds like it would be. That's why I was wondering if that's such a long process how you keep up, how would you keep up with what you need to do.

NEW: The last Discovery competition I ran, first I had an assistant to take some of the load off. I was relieved of every single other duty I had at the time. Basically, my duties were either loaned to other people or temporary people were brought in so that I could focus 100 percent of my energy on the AO, writing it, evaluating it, so on.

JOHNSON: That always helps.

NEW: Yes.

JOHNSON: Let's talk about the whole competition model of the Discovery Program as far as the scientific community. I know you weren't there at the beginning, but how did that change this idea of exploration as far as spaceflight and the opportunities? Including the availability of these other things that are tagged on. Like we talked about not just the missions themselves, but the missions of opportunity, and student researchers, and that sort of thing. Talk about this whole concept and the way this works and how it affects the science.

NEW: I know the history. I wasn't there for it, but I know the history of it. Prior to 1990-ish, the only planetary science missions that were launched were these giant massive Cassinis, Galileos, Voyagers. They took a long time to develop, they took a lot of time to get the money together to develop them. They were these giant Death Stars [moon-sized space station from the *Star Wars* movies] basically. They were huge. They had a lot of instruments, but they launched very rarely, negatively impacting the speed of small "d" discovery.

There were also a lot of budget issues too because at some point the Agency could not support these missions called flagships now, multiple flagship missions at once. Which stretched out the time between planetary missions even longer.

There's a number of good articles and books about the early history of the Discovery Program, but what happened essentially was the question came up. If we gave members of the scientific community the opportunity to propose missions, would they propose anything that was interesting or even vaguely feasible? They had a meeting in San Juan Capistrano [California], and it's the kind of meeting that you could never do today legally. Because basically people showed up with short proposals and they reviewed each other's stuff, and then we had a couple engineers who looked at some stuff and decided whether something was feasible, not feasible, maybe.

But in the end of the day what it proved was that there were ideas out there from the scientific community that were scientifically meritorious and feasible. That democratized missions in an amazing way, because now any scientist with the right soft skills essentially could nucleate a team around themselves, find a mission management center, get a commitment from a spacecraft builder like Orbital ATK, Northrop Grumman, and make a credible proposal to NASA. And if that proposal was deemed worthy in some sense, NASA would hand over huge sums of money relatively speaking for them to achieve their science.

Really it went from being a very top-down government-driven National Academies recommends we do a mission to do X and now we're going to go and Goddard is going to build spacecraft and all this stuff, to much more like hey, I'm over here at University of New Hampshire and I'm really interested in the aurora and here's a proposal to go study the aurora. Or I'm at Berkeley Space Sciences Lab and I want to send two CubeSats to Mars to look at the remaining magnetic field. Okay. If you can convince us that it's scientifically meritorious and technically feasible you got a shot at actually realizing that goal.

Essentially it was a big gamble on the part of NASA or on the part of the government because we didn't know that there would be lots and lots of really good ideas out there that were feasible. For all we knew at the time, there could have been just one or two. But every year Discovery gets 15 to 25 proposals. Of those lately a lot of them have been both scientifically meritorious and technically feasible. So, the last couple of AOs, the results have been an embarrassment of riches. It's not been a question like in 2004 of choosing between a couple of not very exciting missions that happened to fit the cost cap versus a bunch of missions that may be a little more exciting but definitely didn't fit in the cost cap.

Now it's oh, we've got a third of our proposals that are both scientifically very meritorious and very technically feasible. So now we give the selecting official, in this case the Associate Administrator, a real good problem to have. That's a testament to the qualities of the planetary science community. So yes, I think it made a huge change in the way that missions got viewed in planetary science.

The other thing I'll note is in my current position one of the things I do is I look at the demographics of proposing teams. Generally speaking, NASA's diversity in mission proposing teams is poor. The one slightly brighter spot is that the number of women who propose as PIs or are on science teams in the Planetary Science Division, so that's the Discovery and New Frontiers Programs basically, is much much higher than that of the other divisions.

I think part of the reason for that is that the Discovery Program made being on a mission or being involved in a mission in some way much more integral to your success as a planetary scientist than say the Explorers Program did for most areas of astronomy, astrophysics. You can be a perfectly well credentialed successful astronomer and really never touch anything that came from a mission. I don't think you can say the same thing about planetary science. Some of that is the nature of the science, but I think a lot of it is due to programs like Discovery, programs like New Frontiers, where all of a sudden you didn't have to be the named chair professor at Caltech [California Institute of Technology, Pasadena, California] to be heavily involved in a mission. All of a sudden, you had a good idea, you worked with people who were good, you could be on a mission. I think that's a really important legacy of the Discovery Program's founding.

JOHNSON: Yes. I agree. I think it would be. About on average say in the last 10 years, how many do you receive? How many proposals do you receive?

NEW: Between 15 and 25 more or less. It varies. I think the last year I did it we had 28. The AO before that we had 27. I think the last AO had fewer. I think maybe it only had 18. But it's still a healthy number of proposals. It's a lot of money invested by the way.

JOHNSON: Yes. I would think so. Just to do the proposals itself. To give you that book you talked about on your desk. Do a some of them use that proposal and submit again the next time an AO comes out? Unless the announcement is completely different.

NEW: In theory they could. It has happened. If you remember I said in 2006 one of the three mission concepts we chose was called OSIRIS and it was an asteroid sample return mission. We didn't pick it at the end of the day for further development because it really didn't quite fit in the cost box. But the New Frontiers call, which has a much much higher cost cap, among other things, came out eight months or a year after we finished the Discovery competition. Because it was only about a year in between, the PI was able to hold their team together for that year. They pretty much went from not winning a Discovery proposal to rewriting that proposal and turning it into a New Frontiers proposal, which was successful, and is the mission OSIRIS-Rex [Origins, Spectral Interpretation, Resource Identification, Security-Regolith Explorer], which now is on its way back with samples from a near-Earth asteroid named Bennu.

But because the time in between Discovery announcements is four years it's really hard to keep together a mission team for four years. You do see some proposals come back maybe once, maybe twice, but it's a long wait in between calls. By contrast, if you look at the Earth Venture Program in Earth Science, they put out an AO every 18 months. Those AOs, you do see the same proposals coming in. In the Explorers Program, because of their cadence, you also will see something come in one year as a SMEX, a Small Explorer, and then if they don't get selected, they'll come in two years later as a midsize Explorer, and then maybe if they don't get selected, they'll throw off some instruments and go back to being a Small Explorer. So, eighteen months to two years, you can probably keep your team together and continue pushing forward on your concept. But four years, it's hard, it's really hard. You don't get as much repeat business. Get some but not tons.

JOHNSON: Once things are selected, they do have a budget they have to live under, and that's the whole sense of Discovery; it's relatively low-cost exploration. But has there been instances where things haven't been able to stay under budget? How is that handled?

NEW: Oh yes. Kepler is a big example of that. I forget what the cost cap was for the AO that Kepler was selected under. I think it was under $400 million. Might have been $375 million or something like that. As time went on Kepler just got more and more expensive, and eventually I think its final cost was like 600 some odd million. In the past when a mission started looking like it was going to go over, we would hold a termination and continuation review where they'd basically have to convince us that it was worth continuing funding them.

That was before NASA adopted the 70 percent joint confidence level requirement. So, there's a requirement now that every mission at what's called Key Decision Point C, which is when we commit to a budget and we commit to a launch date, we have to set aside money for that project at the 70th percentile confidence that that's enough money to get them there.

Now most cost estimates when you do them just initially, you're somewhere in the 35 to 50 percentile range. Which means that you estimate it's going to cost $100,000 but there's a 50 percent chance it's going to be more than that. Seventieth percentile confidence means that you estimate it's going to be $100,000, you put aside $100,000, that's going to cover 70 percent of the time, it's going to cover the total cost.

When the Agency introduced that requirement, it raised this question. Well, we have this cost cap. The 70th percentile costing, budgeting, for a mission is going to have to be above the cost cap. Because cost caps are for the average cost of a mission, which is 50th percentile again. Where we've come down now is that the cost cap is rigidly enforced for your proposal. It is rigidly enforced for your concept study report. But when you get to Key Decision Point C, which is six months to a year after we've made the downselection, at that point whatever your 70th joint confidence level costs are, that's your new cost cap basically.

So you can grow a bit. But overall if you look at cost growth of most of our PI-led missions, not just Discovery but even Explorers and so on, the cost growth there has been fairly small, a few percent tops over years. They tend to be much better at cost containment on average than the bigger strategic or flagship class missions.

Partially it's because they're designed that way from the beginning. You say, "You've got to fit into this resource box," at the very beginning. So, your initial design iterations have to keep you in that box. By the time you get down to KDP-C, Key Decision Point C, hopefully you haven't baked in any major flaws in your system so that the cost is going to explode when you start trying to build it and test it. That's part of it.

Part of it is also because of this rigorous two-year-long evaluation process we go through, by the time we select a mission for flight, it's probably been more closely looked at than most strategic missions have been by skeptical eyes.

JOHNSON: And peers that will be able to find those things that might happen, right?

NEW: Correct. But I think there's a certain amount of mental space that's different between a standing review board and an evaluation panel. Standing review board is there in some sense to help the mission move forward. Evaluation panel is there to prevent bad missions from moving forward. There's I think enough of a difference in that mental approach that the evaluators, when they're looking at these concept study reports, they're digging in. They're looking at details really closely. Because they have a personal stake in the idea that they're not going to let NASA down, they're not going to let a mission go forward that will fail or will explode in cost or whatever. I think that's a very different mindset from being on a standing review board for a strategic mission, where you want the mission to succeed and your job is to be a critic, a nonadvocate, but still helping the mission to succeed. I think that that slight difference between helping the mission to succeed versus stopping a mission from failing is enough to alter the way these things are looked at. At least that's my perspective.

JOHNSON: We only have a few more minutes. But I was thinking about sometimes when those teams are formed. Do the teams always stay or have there been changes in those teams? Do PIs drop out or the managers drop out or get replaced? Because you had mentioned in that one that they needed to switch those roles. After it was selected does that ever happen?

NEW: Oh yes. Yes. The PI almost never changes between the proposal and the selected mission, except in the very tragic case where the PI becomes incapacitated, which has happened once that I'm aware of. But mission management centers will routinely change project managers or project systems engineers between the stages. Sometimes it's because the different people have different skill sets, and sometimes the project manager who's really good for building up a strong concept study is maybe not the project manager who's best at taking the project from concept study to the next stage. Sometimes it's because the project manager is perfectly fine for all stages but the institution has a higher priority mission that they want that person to serve on, and so they move them around that way. We've seen a lot of movement of project managers, and some movement of project systems engineers, for both those reasons. PIs do step down, usually after the mission has achieved its mission success, its prime mission. We are trying to encourage PIs when they finish their prime mission and they move on to their first extended mission proposal that at that point they bring in some earlier career people into higher management positions as a way to start training the next generation, because we are concerned that we don't have a solid pipeline of earlier career people who in 10 years' time will be able to step up and PI a mission with a lot of experience. They may be able to PI a mission because of their native talent or whatever, but to have had a bunch of experiences that lead to that is becoming I think harder to find.

JOHNSON: I think that's a good place to stop for today. I'm going to go ahead and stop the recording.

[End of interview]