DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

G. RANDALL GLADSTONE
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is November 30th, 2022. This interview with Randy Gladstone is being conducted for the Discovery 30th Anniversary Oral History Project. The interviewer is Sandra Johnson. Dr. Gladstone is at the Southwest Research Institute in San Antonio, Texas, and talking to me today over Microsoft Teams. I appreciate you taking time out of your busy day to talk to me for this project.

GLADSTONE: Thank you.

JOHNSON: If you could, share some information about your education and background, and when you first became interested in planetary science.

GLADSTONE: Okay. That's going back a ways. I was always sort of interested in science. First of all, I was born and raised near Vancouver, Canada, so I'm Canadian, and I'm still a Canadian citizen after many decades living in the States now. But I'm a permanent resident alien or something like that. But yes, I just remember always being interested in sciency things as a kid and watching the Moon landings on TV when I was little. That's how old I am, I guess. But I enjoyed watching those.

But then yes, I guess in high school I got interested. I was good at math, and when I went to university at the University of British Columbia, [Vancouver] UBC, I was always interested in astronomy, so I decided I'd major in astronomy. I decided I needed a backup plan, or somebody convinced me I needed a backup plan if it didn't work out, so close to that was geophysics. In the department at UBC they covered both those things, and you could do a major in both those fields at the same time. I decided that was a good idea, because as a backup plan I could work for an oil company if I didn't become an astronomer. That was a pretty good plan, and I did work for an oil company for a couple of summers out in Calgary, Alberta, where it's sort of the Houston [Texas] north place where they have a lot of oil companies gathered there. I worked for Sun Oil for a couple of summers towards the end of my undergrad years. But I still enjoyed astronomy more than geophysics, I think.

But after I finished my Bachelor of Science there I looked around and some friends of mine told me, "Oh, you probably want to go on to grad school," and I didn't even know what grad school was. But I asked my professors, "Where do you think I should go?" One of them said, "Well, you should go to the Jet Propulsion Lab [JPL]," and that meant to go to Caltech [California Institute of Technology, Pasadena].

I applied there and a couple other places but Caltech accepted me. To me it seemed like the best mixture of what I had learned as an undergrad would be planetary science, because it sort of combines geophysics and astronomy, so I applied to their planetary science program and got in and went to Pasadena after working another summer in Calgary. I flew down to Pasadena and started four years there and got a degree in planetary science under my adviser there, Professor Yuk [L.] Yung. I was his first grad student there. He was recently hired a couple years before I got there and he hadn't had any grad students go through with him yet. He's had dozens and dozens now. I'm lucky enough to be his number one son I guess as far as academic students go.

I learned a lot while I was at Caltech. It's a pretty good place to learn sciency stuff. That's basically where I got my education. Most of what I remember was taught to me by my adviser or the other people at Caltech. After that, as a postdoc I was required to leave the country again and spend a couple years away, so I went up to York University in Toronto and spent two years there as a postdoc working for Professor Jack [John] McConnell, a Canadian citizen, but originally from Ireland, and worked there for a couple years. Then went down to LASP in Colorado, the Laboratory for Atmospheric and Space Physics at CU [University of Colorado] Boulder.

This is going past education now. I don't know how much further you want me to go. But my whole history is basically pretty quick after that. I spent a couple a years there, and they were getting ready for the Galileo mission. They had instruments on the Galileo mission to Jupiter, and I was hired to work on that. Then it got delayed because of the Shuttle disaster, the *Challenger* [STS- 51L] disaster. After a couple years there I went out to University of California at Berkeley Space Science Lab up above the city. It was on top of a hill basically overlooking Berkeley, and it's a very nice view. The guy who ran it used to say, "That view is worth $10,000." Other people too told him, "Oh, well, we'll just keep the blinds closed if you give us $10,000 more." But that was a good place too. There I worked on Earth aeronomy, and that's sort of the study of the Earth's upper atmosphere. But also kept up on planetary and astrophysics.

They had a mission there that they were getting ready to launch called the Extreme Ultraviolet Explorer spacecraft, which was sort of an observatory, not as big as Hubble Space Telescope, but similar idea that they look at stars and galaxies at certain wavelengths. I was in charge of figuring out what they could do locally in the solar system, and even what noise and background they had to worry about in the Earth's atmosphere.

By the way I met my wife there too, so I had reason to stay around, and spent six years there, and then we moved, after my group that I was working with broke up there. I considered going to APL [Johns Hopkins Applied Physics Laboratory, Laurel, Maryland], but also had friends here at Southwest Research that were interested in hiring me. I looked at both places and we decided to come here, and I've been here ever since, it will be 30 years in a few months, so it's been a long time. It's a good place to work here. Lots of support and good people. I just never had a reason to leave.

JOHNSON: Nice part of Texas too.

GLADSTONE: Yes, it's very nice.

JOHNSON: I was reading some information and I came across the NASA Astrobiology Institute. Were you ever part of that?

GLADSTONE: No. The very first paper I worked on when I was an undergrad at Caltech was on the primitive atmosphere of the Earth, and it involved before there was life producing organic molecules with photochemistry, lightning, in the primitive atmosphere of the Earth. That paper still gets cited a lot among astrobiologists. But that was all I did. I guess I should say also as an undergrad the UBC program had us do sort of mini theses on topics, and the only other guy who was in the joint geophysics astronomy program and I did a joint thesis on the evolution of the

Earth's atmosphere, including the effects of life on it. I have often thought that that might have been one reason that I got accepted into Caltech, is because a couple of professors there were interested in the evolution of the Earth's atmosphere and thought that was a good match. I don't know if that's true or not. Anyway, that's all the astrobiology I've ever done.

JOHNSON: When you first started with the Southwest Research Institute were you working on any other NASA missions at that time?

GLADSTONE: Good question. I think I was not doing that at that point. That would have been in 1993 and we were interested in things and we would apply to NASA for grants in the IRAD Program [Internal Research and Development]. But there were no missions at all. One of the guys who hired me was Alan Stern who's pretty famous around NASA and he wasn't famous in those days, but he was trying to be, and he was working on a lot of different projects. Some of them were sounding rockets. We started doing sounding rockets not long after that. But I don't think I was actively involved in any NASA mission at that time.

JOHNSON: Looking at a timeline, it's hard to find information on when these ideas originated. But I know in 2004 President George Bush announced *The Vision for Space Exploration*. That kicked off a lot of projects, and going back to the Moon, because that was what he wanted. LRO [Lunar Reconnaissance Orbiter] became the first mission for that.

GLADSTONE: First and only, I think.

JOHNSON: Yes, between LRO and LCROSS [Lunar Crater Observation and Sensing Satellite] basically. Let's talk about that time period. This project will cover the New Frontiers Program too, and I read that you'd worked with these instruments or similar instruments on New Horizons and also on Juno. Was LRO the first one you worked on? Or was it the New Horizons?

GLADSTONE: It was neither of those actually. I was involved as a scientist in a mission from Southwest Research that was designed to image the Earth's magnetosphere called IMAGE [Imager for Magnetopause-to-Aurora Global Exploration, Medium Explorer mission][1]. That was launched [March] 2000. But I was just a minor player. I was interested in a lot of things on the Earth at that time, and I knew a lot about Earth aeronomy, so I was involved in that mission quite a bit around that time. After that I got asked by Alan Stern to be involved in New Horizons, again just as a scientist, not as an instrument provider. That proposal started around 2000 or 2001 for the Pluto mission. I helped a lot in the early days, I've been on it the whole time basically. The genesis of that Pluto mission was long before I showed up. It spent almost as much time getting into orbit as it did going to Pluto. It was started probably in the late '80s where Alan started working on that. But I got heavily involved around that mission proposal for New Horizons.

A couple years after that I got asked by Scott [J.] Bolton here to work on what became the next New Frontiers mission, Juno. New Horizons was the first New Frontiers mission. Scott Bolton and Dave [David J.] McComas worked together, and a lot of others, but Scott Bolton came from JPL to Southwest Research, but he proposed the Juno mission. That started off very

[1] NASA Medium Explorer mission that studied the global response of the Earth's magnetosphere to changes in the solar wind.

early, about 2003. The IMAGE mission and the two New Frontiers missions sort of predate my involvement on LRO.

But for the Juno mission Scott Bolton asked me to provide the instrument they wanted for that, and that was an ultraviolet [UV] spectrograph that I was well familiar with after New Horizons. We were also about that time thinking about the opportunity for the Moon with LRO. We hadn't written the proposal yet, but we were scratching our heads trying to think of how we could fit in with what they needed to do, which luckily involved observing volatiles in the polar regions of the Moon. We decided that we could really do that with our UV spectrograph instrument that could look at the reflectivity of the Moon and figure out if there's ice on the surface. It was a good way of doing that.

But also, I think a couple of us thought of this but I remember vividly reading a description of another instrument, a European instrument, on a mission to Mercury, where they said they were going to try and look at the reflected light in the permanently shadowed regions of Mercury and see if there was ice there using this skyglow Lyman-alpha. There's a glow of Lyman-alpha everywhere in the sky, so that if you could see Lyman-alpha it would never ever get darker than twilight. Even at midnight it would still be glowing everywhere. They had decided that they could see into these permanently shadowed craters at this wavelength of Lyman-alpha.

I read that and I thought wow, that's a great idea, we should try and do that for LRO. We tuned our proposal to go for that, to actually say, "We can see into these permanently shadowed regions," because it looked promising and it ended up being selected for that I think mainly. But it was also good at just even in sunlight looking for ice on the surface. But that I think was the thing that really got them, that we could see into those permanently shadowed regions. That's what we proposed and that was accepted with Alan Stern as the PI [principal investigator] of that instrument. Kurt [D.] Retherford was also working hard on that too. He had come to work for me as a postdoc a few years earlier here and he's still here. He's taking over for me on all these instruments.

JOHNSON: Let's just talk a little bit more about that process when they released the AO [announcement of opportunity] for the instruments. It was really a fast process for NASA. It wasn't an AO for LRO to begin with, but for the instruments that would fly with it. I think it was like around six months that they turned that around. Talk about the process and how an institute like yours works with NASA to get to the point of being selected.

GLADSTONE: I'm not sure I'm the best person for this kind of a question but I will try based on my experience. I was really new at it. I watched as other people like Alan Stern would plan out how to do the proposal. But he was always looking for opportunities to build hardware for NASA, and he saw this coming. He knew a lot about the Moon, and he had already written a huge review paper about the Moon's atmosphere that's still used a lot today. He was very familiar with the Moon and he's a big fan of all the Apollo missions too. He knew a lot about it and what they were looking for basically. He was already aware of all that stuff. He prepared us, Kurt Retherford and I, and we would have conversations where we would try to read tea leaves basically. What are they looking for? What do they need? Because we weren't a typical instrument to be on a mission. There'd be a straw man list of instruments suggested by NASA usually for these missions. Usually there was no ultraviolet spectrograph. We had to make a good case for it.

Alan had just come off another mission, a European mission called Rosetta[2], that went to a comet. They got selected for that, same thing. There was no UV instrument on the straw man mission and Alan wrote this proposal, he and Paul [D.] Feldman at Johns Hopkins University wrote a proposal and blew them away with the science that he could do with the UV instrument on there, and they got accepted. That was the first version of this instrument that we've built many of now that flew. He could sort of claim heritage for that. He said, "We're building one now for New Horizons, and we'll use the spare from that for LRO." That's what we did. But that made it very attractive to NASA because it's a lot cheaper to do it that way. It already has a high TRL, technical readiness level, that they really desire too. All the instruments, they want them to be ready to fly in space. It's a lot of work to get to that level. But we were practically at that level. We had to make some changes to the instrument for the Moon. The environment is always different wherever you go. You need to think about what environment you are going to be flying in. The Moon is hot and cold at the same time and things like that. You have to worry about a lot of different things depending on the situation. We considered all those things. I got to say yes, Alan was very good at that.

But we would have to go to people here at Southwest and convince them that this was a worthy proposal to send to NASA. So it was a lot of internal things that go on as well. I'm sure at other institutions it all happens the same way. You have to prove that this is a good project. But we did all that. We got accepted too. To me it was the first time I had seen it all. I wasn't so in awe of the process. I just thought oh, this is the way it always happens.

Right around that time we had a good string of luck where we had several instrument or mission proposals approved by NASA. The Juno one was accepted and the New Horizons one

[2] ESA's Rosetta mission was the first to rendezvous with a comet, the first to follow a comet on its orbit around the Sun, and the first to deploy a lander to a comet's surface.

had been accepted. I thought oh, this is nice, but I didn't realize how rare that kind of success was. It is fun to watch it in operation. How to do it successfully without having to do a bunch of failures first to learn the hard way. I sort of learned the easy way as far as getting into it. But it's the same process every time. You look at an opportunity and think can we even do this, can we get something that would contribute to improving our knowledge of the universe, basically in this area of the universe. Going to Jupiter for Juno, or going to Pluto, what can we bring to the table? If we decide that yes, we can really do something here, they don't know about this stuff but this is important, and we could learn about the atmosphere by watching the Sun go behind Pluto or something, we could determine what the atmosphere is made of and stuff that they really need to know.

Once we brainstorm a little and decide yes, the back-of-the-envelope calculations look good, we could do this if we just change this coating on the mirror or this coating on the detector or make the detector faster or change the bandpass a little bit of the wavelengths that we look at. All those things we think about, and we think about what other things could we do. We try to mine what we could get out of that instrument with as few changes as possible. But if we decide oh, if we just did this little tweak to the instrument, we could do all this extra stuff, so we sit around and think about that. Then once we have what we think is a good plan we have to, like I said, get the approval locally from the administration here at Southwest that yes, you're good to proceed on this, and our own vice president here at our own division has to approve it as well. But once we do that, then we write a proposal and we get a lot of support from various people on producing a good proposal. It's been well reviewed and people have critiqued the science and the engineering and all the other stuff and made it look good. Then we try it with NASA and see if we can get it approved.

JOHNSON: The instrument on New Horizons, that was Alice, is that correct?

GLADSTONE: Yes, that was called Alice. It was the same name as on the Rosetta one. There's a Rosetta Alice and a Pluto Alice. It's just a name, it's not an acronym. Confuses everybody.

JOHNSON: It's interesting that your group started with a basic idea and then you could adjust it depending on where the mission was going. But for the LAMP [Lyman-Alpha Mapping Project] instrument, once that started, what were your responsibilities as an investigator or scientist for LAMP that was going to be launched with LRO?

GLADSTONE: They started out pretty straightforward. I was going to be something like I am to this day on New Horizons where I was a scientist basically. I thought about the science that the instrument could do, how to get the best data back that we could with the instrument, and planned different campaigns for using that instrument at its target. But that changed when Alan Stern got called to NASA to be the head of science at NASA under Mike [Michael D.] Griffin. We were building the instrument. It was nearly built. But Alan had to go and he had to surrender a few things. He never gave up control on New Horizons. He retained PI-ship of that throughout. Because that was his biggest project to him, most important thing going.

But on LRO we had actually written in the proposal that we gave, the LAMP proposal, that if anything occurred to Alan, I would take over as PI, or as acting PI, I guess. We decided well, I guess I have to take over from you on LAMP. That's when I got formally introduced to being PI (or lead) on an instrument, where you have to answer for the quality of the hardware.

Things I know nothing about to this day basically. But I had to make sure it passed all its tests. It was at a crucial time because it was just going through what are called environmental tests before it's checked out for calibration and then sent off to NASA for integration on the spacecraft. So we were just finishing up those environmental tests. I remember almost immediately there was a problem with the detector. It suddenly went haywire and it was getting all kinds of background signals that were way way too high for the instrument to function with. There was a back-and-forth with the detector provider that we usually bought this same detector from, a company out in California that is still run by a guy I shared an office with when I was at the Space Science Lab at Berkeley, Ossy [Oswald] Siegmund, who builds a certain kind of detector that's really good at UV and X-ray wavelengths. That's what we've used in all the UV spectrographs we've built.

It was having problems, and it turned out to be a few little needlelike fibers that were on the detector, and they were acting like electron guns and firing off electrons into the detector. Ossy was able to take it apart and use tweezers and a microscope and pick up the little fibers and fixed it. He just knew exactly what the problem likely was and then went hunting for these little fibers and found them. Just a tiny little thing like that was enough to ruin our data quality if it hadn't been caught. Things like that I learned later and still learn today, they happen all the time. Something goes wrong that you weren't expecting and the engineers have to scratch their heads and figure out oh, well, it's probably this, and you go through a bunch of fault analyses and track it down and then figure out what to do about it.

There's always a bunch of these things. Usually it's all handled at a lower level. But the PI has to be a "the buck stops here" type of person- you actually have to make a decision occasionally where this is it, is it this way or this way. That's the only time it's really scary and thrilling (I guess) to be an executive decision maker, when you have to make one of those calls. This was sort of one of those times. It wasn't as bad or scary as other ones I've had. But it was big enough that we had to get the instrument put back together and shipped off in time that it wouldn't cause a problem for the rest of the schedule, and we did.

I saw the instrument LAMP through its final days here at Southwest. Then when it got sent to Goddard [Space Flight Center, Greenbelt, Maryland] to be integrated with the LRO I was involved in that process. Then after launch and the first year or two in orbit around the Moon I was involved heavily in the science that came back from it.

Then Alan came back from Headquarters and decided he wanted to reconnect with all his other stuff, so he became PI for a short time again on LAMP. Then he decided to hand it off to Kurt Retherford, who was well worthy of it at that point. Kurt has been running it ever since. But I think I ran it up until about 2013, so for I guess about six years or something like that I was the acting PI for LAMP on LRO. I got to do stuff like write the instrument paper for the *Space Science Reviews* issue or book and a couple of the flashiest papers on the first discoveries, watching LCROSS impact the Moon. We saw a plume and saw interesting stuff in that. Basically be there for the launch and everything too. It was a fun time to take over for a little while.

JOHNSON: Were there any other major problems or anything that you had to work through other than that one with the detector?

GLADSTONE: That's probably the most memorable one, but there were many others, I guess. There's always something where it's not quite what you wanted, or some other thing comes along. One of the other instruments has some issue that means that you're going to be affected by it. There were several. There was always something interesting going on on any mission I've been on. But LRO it was certainly true. We had one PI, I guess you're going to learn about that, that was put in prison for spying, or attempting to spy. Stewart [D.] Nozette on the Mini-RF. Things like that were pretty shattering to the team at the time. There were a lot of things that were interesting. But we had such great people on the LRO Project.

One of the things that's interesting, and I want to get to this now, because I'll probably forget it if we start talking about something else. But at some point, I was familiar, we were doing these different missions at the same time, and I became interested or noticed that there was a big difference in culture between the different NASA centers. It sort of amused me, but it was interesting the difference between Goddard and JPL in their approach to mission design I guess and operations. It seemed like the Goddard people, and this is Craig Tooley, the guy who ran the entire LRO mission, and Arlin Bartels, who was the payload manager, they were really good at the hardware, and they made sure that the LRO spacecraft was going to work. They tested it and tested it and were really good.

But we had a few days' transit time after launch to the Moon. I don't remember there being any big plan for what we were going to do once we got to the Moon. They had thought about it, but there was hardly any discussion of all the things we had to do at the Moon other than be there and take data. Whereas at JPL they probably didn't do as much on the spacecraft, and I'm thinking of Juno probably, there was a lot of work on it, but we weren't involved in it that much, but on the mission plan of what we were going to do once we were in space they did a ton of work, and we were always revising the mission plan. It was like the size of a phone book. I'm sure the LRO one was the size of a pamphlet. It was so much smaller. It's just a difference in culture in what they focus on. LRO, they focused on making sure the spacecraft was perfect, and perfectly capable of operating in a bunch of different situations.

They never had a problem with LRO as far as I know, or LCROSS. Whereas Juno, as soon as we flew by the Earth we went into safe mode because they had set it at the wrong values. Something like that would have never happened on LRO. On the other hand the LRO guys, when we were commissioning the spacecraft, I remember me and the guys who knew the instrument well went out to Goddard and it orbited the Moon (it still does!), about every 2 hours. It's always on taking data. But at the time we were trying to commission it and understand how to set the instrument parameters so that it would operate properly, there were a lot of things. You're flying into dark and then sunlight every hour basically. We had to be able to detect that transition and change the operation of the instrument appropriately. But I remember the ops people. We would have to write a form, and they were down the hall from us where we were at Goddard. We were basically alone in this giant room looking at this big board with all the parameters of the orbit and what was happening on the spacecraft, and we would have to write on a piece of paper what we wanted them to do to our instrument, the ops changes and stuff.

We had to take this down to the ops people who would command the spacecraft what to do and send up the command to the spacecraft. One time they said, "Oh, we can just get this command in if we send it in now." This is something we just wrote down like 10 minutes before. They said, "We can just get it in on this pass with White Sands [New Mexico] receiver and transmitter." We thought about that and then we went back. I compared that with how it would have gone at JPL. At JPL it would have had to have been approved of several times over a course of a month before that command ever went up to the spacecraft. It's very very different philosophies in how to operate and build spacecraft and run missions. It amused me at the time that we could just get this in. They said, "We won't be able to get the signal back saying they heard the command but we can do it."

I was like, "Wow, that doesn't sound like a very good way to do something." To send a command without knowing immediately whether it was heard or not. But anyway it all worked out and they knew what they were doing at the time. It was fun.

JOHNSON: That's really interesting. The difference in the cultures between all of the centers at NASA. It's just different. But that's a good description of that difference, I appreciate that. You mentioned that detector. You bought it from a company. For LAMP or for any of these instruments that you've worked on, how much do you purchase commercial off-the-shelf products?

GLADSTONE: It's a big variety. It depends what you can do. Our company does a lot of stuff. We could build just about anything right here if we wanted to, but we don't have the experts in certain areas. We don't have the expertise. If it's something like a power supply, a high-voltage or a low-voltage power supply, we can do that here. We could do it as well as anybody in the world. But occasionally we will subcontract low-voltage power supply from somewhere else. It depends on how busy people are around here. They're always busy is the answer. But sometimes you need to go to a vendor. A lot of things, I'm not the best person to know about this, but there's a lot of things called long lead parts, and detectors are one of them. We just knew we could never do it as well as these guys out in California could do it for us. Even though they charge a lot, it's worth it to us because we know it's going to work properly. We have a good relationship with them. There's other vendors that we have a similar relationship with.

Sometimes you make decisions based on political decisions. That's another thing about being a PI.

On Juno—I wouldn't have done this on LRO—but on Juno, I had hardware to build. One of the things that was different about Juno is it's a spinning spacecraft. In order to look at Jupiter when it wasn't spinning in the right direction, we had a mirror that would point off the spin plane and we were going to build that at Southwest Research. But a friend of mine called me from Belgium and said, "We'd like to be involved and contribute something to your instrument," and we decided that our scan mirror and motor would be the best thing for them to build for us, even though we were already set to go ahead and build it.

So I arranged to get a memorandum of understanding with NASA and ESA [European Space Agency] so that they could build this for us. There's a lot of stuff with ITAR [International Traffic in Arms Regulations] and things that you don't want to know about but were huge concerns at the time. Luckily, they're going away now. But at the time it was like if you don't do this right you're going to prison. Pretty sobering to hear that from people. It's things like you can't tell them that there's an element called tantalum. They're not allowed to know that. But, of course, they already do know that. They invented the periodic table over there. ITAR rules included lots of things that just didn't make any sense. You'd go, "This just makes my head hurt."

There were a lot of considerations. Even though they gave it to us, it ends up not being that much of a great deal. But it was still a good trade to make. Those are political trades that you could do and those involve vendors. Basically what I'm telling you is they gave us something that we could have built ourselves but it was a good trade scientifically and programmatically to do that.

Those are the kind of things you have to think about when you're deciding where to get parts or subsystems. Usually we have a list of who our best providers are, who we think of as the people we can go to for certain things if we need them. Like I said, sometimes you could do it just fine here in-house but those people are already busy on four other projects right now. They just can't get to it. You never know if you can do it for sure locally. Things like that happen all the time. Usually if it's a major subsystem then you should have thought of that ahead of time and it would be in the proposal that you've already made that partnership.

JOHNSON: You mentioned the team a few times. Let's talk about that team for LAMP and how the initial team was formed. About how many people are on the team for an instrument like LAMP and the kind of the mix for that team and their expertise.

GLADSTONE: Sure. We have this franchise on UV spectrographs that Alan Stern started back with the Rosetta instrument. There was a guy named Dave [David C.] Slater, who's dead now unfortunately, but he designed the instrument and built it. It basically has stayed the same design for a long time. We've modified it and changed the layout, but it has the same basic components. It even had the same grating for almost every single incarnation we've ever built. We're on the sixth or seventh now, I think. It's evolved a lot but we basically know what goes into it. It's the same recipe of things that go into it.

The same team has basically stayed with that instrument all the way along. There's people that come and go, but a lot of the faces and names are the same as they were on the very first one. I wasn't involved in the first one, Rosetta, at all. I don't know how it works on that one. But on the rest of them I've been involved. The guy who designs the flight software for the instrument, it's the same person. He's the same person that commands the instrument and knows how it works better, how to tell it what to do properly and to get the data back from Jupiter. Maarten Versteeg is his name. He's been on all these missions.

Then there's Dave Slater's successor, the guy who actually builds and tests the instrument in the lab, Mike [Michael] Davis has been on most of them now too. Other people are engineers that do the electronics, like Kristian Persson who's from Sweden. Maarten is from the Netherlands. There's a lot of us from other countries it turns out too. A lot of these people, their expertise is in one little area. Like we'll have a thermal engineer for understanding how the LAMP instrument will respond to the daytime and the nighttime on the Moon, and how big a radiator we might need to get rid of the excess heat so we don't use too much power. A lot of those people were the same all the way for the different missions. They would come into the next one and say, "Oh, looks like this is way different," and go away and think about it and come back and say, "I think we can just change this part."

Yes, it's hugely important to have that corporate knowledge in each project so that you don't make as many stupid mistakes (that you know you're going to make somewhere, but you want to keep them to a minimum). It helps a lot to have people who've done it before several times. I really, really benefited from that on LAMP and later missions as well, knowing who I could get to tell me what I needed to know about something, or to tell me if something was a stupid idea, or that might possibly work, or things like that, when we'd go to make tweaks to the instrument or try to operate the existing one in a different way. The people are hugely important. We have great people. Like I said, I've been here for 30 years now. Whereas before coming to Southwest I'd been at other places typically two or three years. That gives you an idea how much people tend to stay here for a long time. There's a lot of people who've been at Southwest for 40, 50, 60 years. It's mind-blowing to me but I can sort of understand because I'm becoming one of them. It's sort of like that for all our engineers. People come and go, but there's some people that don't go. They just are always here. That's what makes it a lot easier for us to succeed at this kind of stuff, is to have those people that know how to do it right.

JOHNSON: Before when you were talking about the different cultures, it made me think of that balance between engineering and science on these kinds of missions. You had engineers and you had scientists all on that LAMP team, and sometimes engineers think differently than scientists, and scientists think differently than engineers. Talk about that working relationship between scientists and engineers at your institute.

GLADSTONE: Sure. I would say that it's not just sometimes, it's all the time they think differently. There's a tension there. But we really work together all the time and we know each other pretty well after a single project. After multiple projects we know each other really really well. The nice thing about it here, and there's projects on New Horizons, it's done the same way there. But the projects that we competed with that we beat—and I think it's probably because of this close working relationship between the engineers and scientists here (and maybe at APL too), was different than would be done at JPL, where the scientists would tell the engineers what they wanted and they would go away and that would be the last time they interacted, and then the engineers would build what they thought they were told to build, and give it back to the scientists, and they'd say, "Well, this isn't exactly what we wanted" It would be a very marginal (if any) interaction between them. I don't think that's a good way to do it. I'm sure they don't do it that way all the time but I saw a couple of things like that where it looked like that's what they had done, and we don't do it that way.

We go to meetings every day. I will never know the engineering that well, but I do know that these people know about as much about science as I do about engineering, but we all know what we want to get out of it at the end. We work on that relationship a lot. It's fun; it's challenging sometimes. But the things that scare me to death don't bother them at all, and vice versa. It's amazing to watch these engineers.

The trickiest time, when we're all trying to work together and we're all crucial but doing different things, is during NASA (or ESA) reviews. When we are building the instrument, you first have requirements reviews, then a preliminary design review, and then the CDR [critical design review], the one when you're ready to build it, and during those reviews you're challenged by NASA and external experts on what you're doing. Watching the engineers in their specific field like the thermal engineer or the mechanical engineer get up and explain things, our guys were just brilliant at doing that. We would often have the panel come back and say, "That was the best presentation on fault control that I've ever seen," or something. Our guys would just shrug it off and say, "Yes, it's what we're supposed to do."

In those reviews you get tested on science too, so those were very stressful times, but they're also very inspiring times, because you got to see how other people responded under pressure, and it was usually a good thing (for us at least). But yes, that interacting with engineers is challenging for me, but we've all gotten really good and know each other pretty well and it has paid off to learn their side of things as well.

There's only one person I would say here that really did that flawlessly. There was a guy named Dave [David T.] Young who built mass spectrometers here and he was a first-rate scientist but he also knew exactly how to build instruments, like everything about it, not just at the "one-viewgraph-deep level" that I'm at, but at the deep level where he could actually go into the lab and build it himself if he needed to. Those people are rare. It's good to have those people around to inspire you.

JOHNSON: That made me think of a couple of things. One of them is, since we're talking about science versus engineering and how that melds, I spoke with Jim [James B.] Garvin at NASA and he described LRO as "rapidly conceptualized to become a Discovery-class orbiter at the Moon to do two things that were really hard to mesh, make measurements that science cares about and to produce engineering boundary conditions to design better, smarter, cheaper, and safer systems for people to go to the Moon." There was that science versus engineering with LRO to begin with. From reading about it, and LAMP as a result had to adjust, that first year, what was being captured was important, but things changed after that first year. So maybe talk about that a little bit. I want to talk about the results of course that you found early on and also observing LCROSS. But just that difference between the science versus engineering was even in the mission itself.

GLADSTONE: It definitely was on LRO in particular because it was not from the Science Mission Directorate [SMD] at NASA, it was from HEOMD [Human Exploration and Operations Mission Directorate] or whatever they called it back then, when they were thinking they were going to soon land people on the Moon and build bases. They needed—call it an engineering need for the maps of the Moon at 1-meter resolution of potential landing sites. That dominated LRO for a long time, and there were lots of people from that division that were much more focused on nonsciency things. They were interested in sciency things. One guy in particular I think about, he was just the greatest guy, is Mike [Michael J.] Wargo. You probably know his name by now on LRO. He was at NASA Headquarters, in charge of basically all that HEOMD stuff, and was our interface between the guys at Johnson [Space Center, Houston, Texas] that really thought they were going to land the people back on the Moon.

I don't think they really cared that much about the LRO mission but we were on their radar, so that he would be our interface with these guys who had bigger plans for the Moon. But yes, that was sort of a different flavor of the conflict between engineers and scientists where scientists think, "Well, we're the important people on this mission because we're going to do all this great stuff," and they're like, "No, no, the cameras are the important thing on this mission, and getting that map knowledge is all that we care about. Science doesn't mean anything." It wasn't said that way but you know what I mean.

That's what their focus was on, getting the engineering data for a future mission that is the important mission. That's what LRO is for. That was always there. Like we mentioned, I wasn't involved at the very beginning of LRO, other than as a scientist on a minor instrument. But later on when I had to go to all these meetings, and when we were integrating on the spacecraft and learning what it was going to be like and what things we had to figure out before we got there on how we were going to operate, a lot of that was conditioned by hearing what they expected us to be able to do, and that no, you can't do that thing, because we're not interested in that thing.

But it became a science mission eventually. It got transferred I think to SMD and so it changed a lot. But it was, yes, a different flavor of that engineers, scientists thing with the different manned and unmanned programs. It was fun to see all that stuff. Luckily Mike Wargo was hugely interested in the science as well. That made it nice. Jim Garvin and other people that were normally busy doing other things were very very interested in the science that LRO was going to get, so we got a lot of free rein to think about the science as well.

JOHNSON: You mentioned integration a couple of times. Maybe talk about that for a minute—because you went to Goddard for the integration—if any constraints were on the design for LAMP because of the way it had to fit with the other instruments, and how that integration went out at Goddard.

GLADSTONE: When we're building the instrument here, we have to make sure it works properly and then make sure it's going to work properly in the environment it's going to be in. You do all these environmental tests. Mainly you shake it on a vibrating table, and you test it for electromagnetic interference and things like that. Then you test it thermally. You heat it up and you cool it down in a vacuum chamber so that you understand if it's going to work or if it's going to break. Usually you find at least a few things that don't work right, like some electronics board fails at cold temperature because a tiny little weld snapped or something like that.

You fix all those things, and then you send it off to the spacecraft to be integrated. They do the same thing at the spacecraft level. They do all these environmental tests on the entire thing when it's all put together. First, they have to put it together. But then when they're done with that and everything seems to check out, they do all these large-scale versions of the environmental tests that we do here at the institute at the instrument level.

Those things are where you have to make sure that somebody's there for them to call on if something goes wrong with your instrument. They need somebody there to fix it right then because usually by that time the schedule is really really tight, because you have to meet a certain launch date. Not so true for the Moon thing I think, although I think they probably had a lot of constraints on that as well for LRO. But for planetary missions you usually need to go or else you're going to be a year or two waiting for the next opportunity.

The closer you get to the end of some schedule like that where there's a specific date that has to be met, the more things get hectic and crowded, and that's where a spacecraft integration is at. By the time you're doing that there's no time to be lost. You can't have a major screwup or else they'll just fly a mass dummy of your instrument on the spacecraft. So it's a good idea to help them out and be there and just give them the help they need to make sure that your instrument is working properly.

But we also want to test it and make sure it's operating properly. At some points in those tests or after them, mainly after the major tests on the spacecraft are done, we will go there to Goddard, or wherever it is, and test our instrument specifically to check out whether it's operating properly and whether it's still performing well. Sometimes instruments accidentally get covered with junk just by accident during integration. You don't want something to keep you from operating when you get to the Moon or somewhere.

So yes, by the time it's being integrated on the spacecraft it's more like we're support people for the spacecraft team. If they can do it, they want to do it themselves, but a lot of times we don't want to let them do it because we know how to do it right. So there's a tension there in the clean room. Usually it goes pretty well. We know what to expect when we go there. If we really want to do it ourselves, they'll let us. If we think they're good enough to do it on their own, we let them.

The things that go on last or get taken off at the last minute, there's always things called green tag or red tag items that either go on or come off, or there's multilayer insulation, MLI, that covers it that has to be—it's like a very complicated garment that's sewn up and then fitted over your instrument to make sure it is thermally stable. All those things have to be adjusted at the last minute by trained engineers and technicians. All that happens at the spacecraft level too. Most of that interaction goes fine, but there's always something that goes on where something's broken or something isn't mounted right, it's misaligned. Things like that happen all the time and you have to decide can we live with it, can we fix it, what are the options. I'd hate to be a manager of one of these missions because it must be that way every single day.

JOHNSON: A lot of decisions to be made in a short period of time.

GLADSTONE: Yes.

JOHNSON: Let's go back to the team aspect as far as working with the folks at NASA. Did you have a lot of interaction? You mentioned the LRO team itself. Did you have a lot of interaction with the separate teams? Was that on a regular basis, or did you meet often? I'm thinking about people also like the Project Scientist Richard [R.] Vondrak and Tony [Anthony] Colaprete with LCROSS. All these different teams. How did that work?

GLADSTONE: My recollection is that every so often, two or three times a year, there would be a team meeting where everybody would show up. We'd learn from the project people like Craig Tooley or Arlin Bartels or Rich Vondrak or John Keller, who took over from him eventually, and other people what the status of various things were. It would be a good chance for our team to get together with the other science teams, instrument teams, and learn what they were doing. That continued like that.

It's not unusual. It's that way on almost every mission I've been on where you get together every four to six months. Basically, the meetings serve just to renew acquaintances, but also get updated on what's happened recently that you need to know about and do stuff or make decisions for the upcoming crucial events. That occurred on the ground. Then that occurs once you're in orbit too. But it tends to go away from engineering concerns on the ground to being sciency concerns hopefully. If it's an engineering thing in space then it's a problem usually. But if it's normal ops there'll still be science team meetings after launch where you present new results and think about new opportunities.

That happened a lot with LRO because of the LCROSS especially, where we had an opportunity to watch LCROSS crash into the Moon. That took a lot of thought among all the teams. We had to think what instruments can observe it, and how would we observe it, and do we need to observe the actual event or can we look before and after, what do we want to look at. It was a brand-new thing. As far as I know, I think nobody had really done that with a spacecraft at another body looking at something else hit that body. It was new territory for me at least and maybe a lot of people on how do we do science on this thing. That worked out well.

Tony Colaprete would come and give us a talk about LCROSS. Even though it's a sister spacecraft, I didn't really know anything about it because I had other things to do. But he would come and tell us and we'd go, "Oh, wow, that's cool." Then we'd think, "Well, we could arrange it so that we're looking at you guys when you crash." We'd think, "Do we really want to look right when they crash, maybe we should start looking before they crash or after they crash, or do we want to fly through the debris that they toss up," that might kill the LRO mission right away. We had to think, if we want to look at it, then how do we look at it so we're safe and we can still see something interesting.

During several of these science team meetings we would discuss that particular event and how we were going to observe it and make plans. I enjoyed that the most because it was new stuff that nobody had thought of. How do you do this? You have one chance to do it and it has to work. You have to make sure that you get good data and learn something from it, not just what not to do next time, but actually get something useful out of it. So that was a lot of fun planning. Is this really the right way to do this? Maybe we should go over here and watch it from the side so we have a good way to look at it.

We eventually designed a pretty good experiment, and it worked sort of like we thought. We got very surprising results from it for our instrument anyway. I think a couple of the instruments did as well. Tony's mission went off like he expected too, so he got a lot of great data. But it was sort of a joint experiment that worked out well. Even though I think it was considered sort of a flop as far as the public goes because they didn't see a bright flash from the Moon. They were all told to look at the Moon and you'll see a big bright light go off. They were all expecting something a little more exciting than they got, I think. But for the science that we were able to do with LRO and LCROSS we learned a lot that we didn't know before. The scientists enjoyed it but maybe not the public.

JOHNSON: The science is what you were after, right?

GLADSTONE: Yes. But I mentioned that just because I think there were a couple of opportunities after that where folks on some spacecraft that had to be turned off and decommissioned realized hey, we could crash into the Moon too, we have enough fuel left to do that. We would try and do that, and Rich Vondrak was a big proponent of that because of what we'd learned from LCROSS. It got a lot of resistance at NASA. One time, I forget which spacecraft it was, might have been the one that mapped the cosmic background, WMAP[3] [Wilkinson Microwave Anisotropy Probe] or something like that, or another one. Anyway there was a European one once too that was possibly going to crash into the Moon. Rich and these guys were really interested in doing this and so was I, because our instrument had learned a lot from looking at the plume that LCROSS blew up. But Rich would say, "Well, we tried everything and they won't let us do it. They won't let us crash that into the Moon." It got worse because they got refunded some money from the launch of LRO around that time. Rich told them, "We could do it for free if you just let us do it," and they said, "No, you cannot do it!" For some reason crashing into the Moon had gotten a bad name higher up at NASA and they refused to let anyone do it even though it worked out really well scientifically. I think it was because they were embarrassed by the number of people that probably said, "Well, that was useless," or something like that. I really don't know the full story because I don't know anybody at that level that would have learned those things. But it seemed to me that they decided that we're never going to do that again after LCROSS. I hope not because it's still a great way to learn about the soil. If you want to see the soil and you can't go down and dig in it, you crash into it and throw some of it into the air and then you can learn a lot about it. It's not a bad thing at all.

[3] WMAP is a NASA Explorer mission that launched June 2001 to make fundamental measurements of cosmology—the study of the properties of our universe as a whole.

JOHNSON: Let's talk about the launch for LRO and at what point your instrument, you knew that it was working correctly, after LRO reached that point at the Moon where you could test all of that.

GLADSTONE: For scientists, the launch is just like being in a candy store. We don't have any of the responsibilities. We just have to show up and enjoy the event and maybe do a couple of talks to the public or something like that. But it's usually a huge amount of fun. That was no different for the LRO launch. I'd already been to the New Horizons launch, so I already knew ahead of time what to expect, but it was very similar. It was a great time for us to get our team together again first. Some parts of the team you don't see that often. We had people up in Boulder that did public outreach for us and so we got to see them and have a good time in Cocoa Beach [Florida] before going to the launch.

At the launch we got to sit together in a control room and pretend we were real NASA people, with headsets and computer screens to monitor. Then as soon as it was about to take off, we rushed outside and watched it go up into space. That was all fun to see from the inside how it works. But every launch I've ever been to has gone flawlessly. That's another thing that engineers and scientists have a difference of opinion on. When engineers say, "We have a launch window and target trajectory," the orbital engineers always hit the center of the target with no error at all basically. Even though they say the error is much bigger than that, as scientists we've learned to discredit the error budget of engineers, because we realize they make their errors way way bigger than they really are, because they're so much better than they think they are at doing the engineering that they always seem to nail the trajectory—orbital dynamics engineers do it perfectly. They never miss the target. That's even more true with the JPL folks, but certainly for LRO the launch and the few days it took to get there just went flawlessly as far as I remember. You might get another person telling you, "Oh, there were several problems." But I don't recall any.

Then when we had to check out our instrument, normally we would let it outgas, let all the gases that were trapped inside it escape into space for at least a month on our other missions before we would try to turn it on and start up these high voltages. Because if there's the wrong amount of gas in a high-voltage system it'll short itself out and fry the instrument. We never want anything like that, so we always give it plenty of time just to be safe before we try and turn it up.

But we couldn't do that on this mission because it was such a short transfer time. But it was enough time to get it going. I forget how long we waited. We probably were the last instrument to turn on on LRO. I can't remember. But any time you have high voltages you want to wait until everything is a better vacuum than you have on the ground, which doesn't take that long, but you always want to be cautious.

We were one of the last instruments to get commissioned. Usually that involves slowly turning on and trying different things like very simple commands at first like send me out what settings you currently have stored in your memory bank for this and that. We'll read those out and make sure that they agree with what we thought they would be. Then we try turning on low-voltage things like opening a door and closing a door on the instrument. Simple things like that. Then when we gradually work our way up to it, we open the door on the detector, which is a big thing, because it happens once and if it fails then our part of the mission is over. That kind of thing is mission-critical to us. Usually it goes pretty well, but it's a tense moment. Then turning on the high voltage is another tense moment and ramping it up so that it's at operational levels.

The way we detect photons in these detectors that we got from the guy in California, Ossy Siegmund, they require voltages of thousands of volts across a very short distance, so that's high voltage, and that's what we need to work perfectly.

The instruments I've been involved in have never experienced a high-voltage power failure, but they used to fail commonly in the early days of NASA. It seems like we always have to build redundant high-voltage systems, so we're always worried about it. But to me after a couple of decades now, I've never seen one fail, so I don't think of them as being a risky item anymore. We're starting to design versions of the UV instrument that only have a "single string" they're called. Now we can tolerate a single-point failure in the power supplies, because our engineers are so good at building them nowadays.

Once those things are up and running, then we're ready for first light, which is when you first take an image of the sky, where you can see a star or something like that. That's exciting too. That's another thing that usually goes pretty well, but it's a tense moment. Once you get the data down—it's also important to get the data down in real time so you can interact with the instrument in case there's something going on. Usually we'll send the command and then we have to see the result of that command. Did the instrument receive that command and execute it properly? There's a lot of real-time operations that go on in commissioning an instrument.

We would be at Goddard for that. That was during that time when we ran down the hall with this thing, can you send this up to the spacecraft, and they said, "Oh yes, we can just make it." That happened during commissioning. That was an intense time. But those are the kind of things we were doing then. Bringing up the instrument to its full capability and testing it at each stage to make sure it was ready to move on, and then getting the first real observations.

Each instrument would come at a different couple of days during the commissioning period. They would go through their thing. We didn't interact with the other instruments during that time. But we showed up when we were supposed to show up and did the same things that they had done with their instruments, bring them up to science readiness. That all went well. Basically for me, again as a scientist, I'm watching experts do this. If they have a problem, they sort it out, and I rarely have to involve myself in it. I'm usually writing a report or something that I have to do in the back of the room and listening to them, but usually it's going very well and we don't have to think about any emergency procedures.

JOHNSON: Let's talk about some of the observations and the results starting with LCROSS or the information or results that you were getting back before LCROSS. Or was LCROSS the first chance to get information back that you were going for to begin with on the mission?

GLADSTONE: I think LCROSS was just a month after we got there or something, so it was pretty soon. It was October [9, 2009]. We hadn't much in the way of regular ops. But we knew everything was working properly. We had this door that we would close every hour as we flew over the terminator where it changed from night to day on the Moon. Suddenly the signal was 1,000 times dimmer or 1,000 times brighter every hour. We had to open and shut a door as we flew over.

We had these little detectors that would figure out when the terminator is coming. "It's almost here, it's almost here. Oh, it's here, shut the door quick." That had to work properly. A lot of it was making sure that things like that were working as we expected or saying, "Oh, the setting is a little too low, we need to up the threshold for opening the door or closing the door."

It was mainly those kinds of things and noticing the science data that we got back and saying, "Ah, that wasn't quite what we were expecting. We thought it would be brighter than that." Deciding what that meant for operations.

But then LCROSS happened, and that was a big deal because this was like we mentioned earlier a one-time event, you had to get it right. We planned that observation to be off to one side. It had already impacted before we flew past it. The plume that it threw up had time to develop and wasn't just starting out. We wanted to see it in full plume shape once we went by. We also did another thing that was good in hindsight. I'm really glad we did it. We were thinking well, it's going to be a faint signal, so we want to make sure we see something. Let's do the same thing that we do when we fly by LCROSS. We'll also do it the orbit before and maybe a couple of orbits after. We'll perform exactly the same observation of the sky. We're looking at the horizon basically on the Moon with our slit aligned vertically so it's like a tall pole or something on the horizon. Just moving along the horizon. That's how we're going to try and look for the plume.

But we did that before LCROSS, I think for three orbits before and for seven orbits after or something like that, just to make sure we understood what was going on. It happened that we were in an orientation where it was almost identical stars going through the field of view on each of those orbits, which made the data analysis really a lot easier. We could just take the difference between what we saw when LCROSS happened and what we saw the orbit before where we know nothing's going on. We saw just clear as day the difference which was due to the plume of LCROSS. It showed up in different wavelengths in our bandpass.

One of them was very surprising. It was near 180 nanometers wavelength, and we didn't know of or expect any bright emissions at that wavelength. Then when we were looking at the data one of the scientists, a good friend of mine, Wayne [R.] Pryor, said, "I think there's a mercury line at that wavelength." We didn't think anything of it but we said, "Okay, we'll check it out." It turned out he was dead right. We couldn't find any other thing in that wavelength range. All the elements have different signatures spectrally of where they emit light and mercury emitted light exactly where we saw this emission in our data. It was brighter than anything else we saw. We just gave up and said, "It must be mercury."

Then another scientist on our team, Dana [M.] Hurley, she's at APL, she said, "I think I remember a paper by this guy that predicted there would be mercury in the polar regions." She dug up this paper by this guy named George [W.] Reed. He had been an Apollo era scientist and he was an African American space scientist and that's about as rare as you can get in those days. He had worked on Apollo soil samples where they screw a tube into the soil, the astronauts would, like a meter-long tube, and then they'd pull it out and it'd have this record like a core sample. A sample with depth of the soil of the Moon. He had analyzed that in Chicago at Argonne [National] Lab. He had measured the abundance of different elements as a function of depth, and he noticed that the amount of mercury in the soil fell off drastically as you got near the surface. At depth it would be a certain level, and then as you got closer to the surface of the sample, where the surface of the Moon would be in the sample, it decreased a lot. He wrote this little paper that predicted that all that mercury had come out of the soil and moved to the polar regions.

Mercury is so volatile. It's liquid. When I was a kid, you could roll around mercury in the palm of your hand and stuff you would never let a kid do nowadays. We used to play with it in science class. It's very volatile. Meaning it's easily turned into a liquid or vapor. He reasoned that it was so hot on the dayside of the Moon that the Sun would bake all that mercury out of the soil and it would hop around on the surface atom by atom until it reached a cold place. The cold places are at the poles. Once they got there that's where they stayed.

He predicted that it would be a lot of mercury in the polar regions, not just water ice would collect there, all this mercury. He wrote this little, short paper explaining this in 1999 and it was called "Don't Drink the Water." It was just such a great paper. Dana remembered this and she brought it up. It was like this is it. This is exactly what we're seeing. This guy predicted this would happen long ago and he got it exactly right. It just blew me away that somebody would make a prediction. Scientists do this all the time but they hardly ever get them right. But this guy nailed it perfectly. We were very pleased with that.

Actually one of the scientists I know contacted him and let him know about it. He was pretty old at that time. I'm not sure he ever appreciated it, but we really appreciated his work and had a great time with that. That was so unexpected to see that. Then in retrospect you think well, it makes sense that that would happen. But if you had to think of it ahead of time it would never have occurred to you. That was very fun.

We also saw other things that were interesting. This was a permanently shadowed region that the LCROSS guys crashed into in Cabeus crater. They were looking for volatiles. They were looking for water mainly because the bases on the Moon were going to use local water. That was the idea. They were looking for big tons of ice. But the thing that we saw was a lot of H2 emission, so molecular hydrogen. The thing was it came out so fast. We flew by, at least I think, 45 seconds after the impact, we were seeing the plume. It wasn't very long after it, and that wasn't enough time to convert water into hydrogen from the sunlight shining on the ice, which would normally produce that. There was no way that that could happen in that short a time because the Sun isn't that bright. It wouldn't have made as much hydrogen as we saw.

It meant that the hydrogen that we were seeing was probably hydrogen in the soil or H atoms sitting around waiting in this very cold place. It's 30 degrees above absolute zero. Not much chemistry goes on, so we were imagining well, maybe these hydrogen atoms were implanted by the solar wind and then when it warms up, when LCROSS crashes in and warms it up by a few hundred degrees, they just run around on the little grains of soil until they find another hydrogen atom and they combine with it and form the H2. But that would mean that it's not really water there, it's just hydrogen. That would affect the future plans for Moon bases.

I don't know if they ever appreciated that much. I mentioned that a few times at meetings, that we're not really sure it's water that you're looking at when there's other ways of detecting the hydrogen with neutron detectors. They've seen. There is one on LRO called LEND [Lunar Exploration Neutron Detector], and there's been other ones on earlier missions. They see a lot of hydrogen in the Moon's polar regions in the soil. They've always interpreted it as being water. They're usually careful to say, "We're not sure what form the hydrogen is in, it's probably water."

But since that experiment where we saw all this molecular hydrogen, I'm thinking I've always thought that maybe it's not water after all, it's just hydrogen. Which would be useful as rocket fuel too but it wouldn't be easy to drink. It would change the plans for their in-situ resource utilization, ISRU plans. Same with the mercury being there. It's not just water. If it is water it's going to be contaminated with other stuff that you don't want, so you're going to have to process it a lot before you use it for rocket fuel or drinking water or anything. But it turned out that also got people interested in our LCROSS results.

One guy named Warren Platts emailed me about mining on the Moon, and he said, "Well, all this stuff is already concentrated there. Let's just go to the polar regions." He wrote a paper that gold is not as volatile as mercury but it's pretty volatile too and over a few billion years he predicted that some gold atoms should have collected at the Moon's poles as well and maybe it'd be worth mining there. A lot of clever ideas come out after you do an experiment that's never been done before like that.

JOHNSON: Yes. Let's talk about the other results from LAMP. If you don't mind just describe to me how LAMP was going to achieve those goals and how LAMP was going to be able to look into the polar regions, into those cold dark areas, and how it would be able to detect things that you were looking for.

GLADSTONE: Sure. As I mentioned, there's this line called Lyman-alpha. It's really bright in the ultraviolet. We can't see it, but like I said, if you could see it, then at midnight it would still be pretty bright out because the whole sky is lit up with it. That was how we thought we could look into these permanently shadowed regions, is to use this UV light at Lyman-alpha that's always shining on it to look at it. That's what we did. Since we have a kind of large bandpass with our instrument too, we could look at reflected starlight from those permanently shadowed regions. So when we were on the nightside of the Moon we were able to open our door wide on the instrument and catch as many photons as we could from the source. It's still really really faint, so we have to add up not just one pass, but a month's worth of flying over the same territory to get enough of a signal to see the reflected starlight say at longer wavelengths and Lyman-alpha.

It's those longer wavelengths where the starlight reflects. There's enough stars that it's almost like the entire sky shining on the Moon faintly. If you've been out in a dark sky like at

[University of Texas] McDonald Observatory [Fort Davis, Texas] or something, it's very very dark. Trying to do anything by starlight, you know how faint it is. You're not going to read much by starlight if you had a book with you. But it's detectable and if you add enough of it up you can figure out what the properties of the thing that's reflecting it, namely the soil in these permanently shadowed regions, they reflect it differently than the soil outside the permanently shadowed regions. That means there's a different composition or different physical property of the soil in that region. It turns out both those things are true.

It turns out water has this ability in the FUV [far ultraviolet] to reflect very well longward of 165 nanometers or so. So at one point in our bandpass suddenly water would go from being very dark to being very bright if you looked at it. That meant LAMP was a very good detector of water, if you could see that signature, you would know if there was water in the soil or not. That's what we used. That edge, it's not near the Lyman-alpha line I was talking about. We had to use the starlight to do that. But if you added up enough of the starlight, we could see that the interiors of these permanently shadowed regions look different than the stuff that was sometimes seeing a lot more sunlight nearby just outside those craters. It was usually brighter, more reflective, and inside it was less reflective.

I've been away from LAMP for a while now, but I think this is one of the main results we got, was that the reason for the UV albedo or reflectivity of the soil being less in the permanently shadowed regions is because it was fluffier there. The soil is more fairy castle structure they talk about, more delicately structured. That allowed the light to get trapped in it easier and makes it darker, less reflective, than if it were packed down. That was one interesting thing we found out.

We also did see signs of the starlight edge, the reflection above 165 nanometers was higher in those regions as well. That indicated there was water ice on the surface at about a percent level or so, 1 or 2 percent, I think. Those were results that still sort of hold up that the LAMP instrument was able to measure roughly what we expected. We didn't expect the porosity thing, the fairy castle structure. That was a new result. But we expected to be able to see the water if it was there and it was there, we think. But it's a very tiny amount on the very surface of the soil.

I don't know if there's places where you could actually use it for a Moon base. We don't know that yet because we can't see below the very surface layer. They sort of do that with those neutron measurements. They can see a few meters into the soil and that's where they see all the hydrogen. There's still some indication it might be worthwhile amounts of volatiles there to use in a Moon base. But as far as the water goes, we're just seeing the very top layer. But we are seeing it at about the levels it's supposed to be at.

So it worked out. I think we did a lot of good science with LAMP. As I think I mentioned earlier, I sort of consider us to be one of the minor instruments on LRO compared to LOLA [Lunar Orbiter Laser Altimeter] and the gravity measurements that GRAIL[4] did later. Certainly LROC [Lunar Reconnaissance Orbiter Camera], the camera that measured everything down to a meter or less on the Moon with the NAC [narrow-angle camera] and WAC [wide-angle camera]. There were a lot of great instruments there. Diviner [Lunar Radiometer Experiment] that figured out the temperature of every place on the Moon at every time during the lunar day and night. I think of them as the main instruments on the Moon, but we were definitely a useful contributor to the science from LRO.

[4] NASA's GRAIL mission flew twin spacecraft—Ebb and Flow—in tandem around the Moon to map variations in the lunar gravitational field. The probes generated the highest resolution gravity map of any celestial body to date.

JOHNSON: Definitely. We have just a few more minutes in our time. But before we stop if you have a few more minutes, I just wanted to ask about the importance of outreach and education with these types of NASA missions for exploration. Being a part of the institute, were you involved in any of NASA's outreach and education efforts for LAMP?

GLADSTONE: I remember several when it was first at the Moon. There were several things we did. I think Dana Hurley and I both did interviews where we were asked on camera to explain about how our instrument worked and what we were going to see. I think those are still available on YouTube or something like that. Those kind of things were done by Nancy [N. Jones] at Goddard, and she was very very good at doing the outreach.

For the education part, all the teams as far as I know, and certainly LAMP, had their own outreach team. Our team, we have an office in Boulder that does a lot of planetary science too. Not in Boulder, but at the Denver Museum of Nature and Science we had a connection with a couple folks there. The main people were Gianna Sullivan and Polly Andrews. It's been so long since I thought about them. But they did all of our outreach, and we did museum exhibits of how our instrument worked at their nature museum up in Denver. Kurt Retherford or I would go up and give lectures to the public on the LRO and LCROSS missions and things like that. We did that on the instrument level like I just mentioned.

We also did it at spacecraft level with the people at Goddard. That did happen a fair bit. Education and public outreach was definitely a bigger part of NASA then than it is nowadays I'm afraid. They've tended to chop that down. Even though it's one of the things they do best. I don't understand it. It seems like something they should get back to because people really appreciate NASA. I think it's probably one of the shiny jewels of the government as far as an ambassador to the rest of the world or anything like that. The guy from NASA Watch always highlights it when some kid in a poor country is wearing a NASA T-shirt. It's like that's what you want to do. You want to be known as something good in the world. That's what NASA is.

JOHNSON: Yes, something for kids to aspire to.

GLADSTONE: Oh yes. I miss that. I used to do more of that too back in the early days of my career. We used to do those talks associated with the launch or interviews and things like that. It was fun when you got to talk to a school full of kids that were asking one question after another for hours and hours. That was the most useful thing, I think. But we did similar things on the other missions too where there would be instrument-related outreach and mission-related outreach and education. The education part was a lot of fun with talking to kids. But the outreach, just giving public talks, was also fun. We did a lot. I'm sure we did less than some of the other instruments did. I imagine the LROC guys, Mark [S.] Robinson and them, must have done a thousand of those talks by now. In keeping with our share of the science we did an appropriate amount, I think, but it was definitely a fun thing and I wish it was still done at the level it was done back then.

JOHNSON: I think we all do. I saw that Juno actually had citizen scientists processing some of the science data. I thought that was interesting.

GLADSTONE: Oh, and New Horizons has a Student Dust Counter as one of the instruments on board. The PI-led missions tend to be very innovative in terms of everything, but outreach is one of them where they think very carefully about what would be a good way to get the public involved in this mission. Sometimes it works out. For those two missions they really did a good job and led the way in doing that.

JOHNSON: Since we're at time I think it's probably a good place to stop. But I appreciate you spending some time with me today.

GLADSTONE: Sure. Fun. I hope I didn't misremember too much there. I'd always encourage you to go back and check wherever you can.

JOHNSON: It's a lot of good information.

GLADSTONE: Okay.

[End of interview]