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

ANTHONY COLAPRETE
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is June 23rd, 2022. This interview with Tony Colaprete is being conducted for the Discovery Thirtieth Anniversary Oral History Project. The interviewer is Sandra Johnson. Dr. Colaprete is at NASA Ames [Research Center, California] and talking to me today over Microsoft Teams. I appreciate you agreeing to talk to me for this project. I'd like to start today by asking you to briefly describe your education and your background and how you first came to work for NASA.

COLAPRETE: Sure. I went to school at University of Colorado in Boulder, Colorado, and I studied atmospheric science. I got my PhD in astrophysical, planetary, and atmospheric science. My dissertation was on clouds on Mars, so I studied the formation processes and the impacts of clouds on the martian climate. One of the aspects of the studies I was doing at the time was showing that the clouds on Mars had definite impact and were of real importance to understanding the climate on Mars.

At NASA Ames, there was a group who had a martian general circulation model, which modeled the atmosphere of Mars and the climate of Mars. My adviser at the time, [Owen] Brian Toon, was originally from NASA Ames, and connected me with the lead of that group, Bob [Robert M.] Haberle. When I graduated, talking with Bob Haberle at NASA Ames, he said if I was interested in coming as a postdoc to NASA Ames. I said, "Absolutely, it's always been a dream for me to work at NASA."

I came to NASA Ames in 2000 to work with Bob Haberle and the rest of his group on the Mars general circulation model. I did that for about two years as a postdoc, and then at about that time I was putting in proposals to get grant money and won my first grant and at that point transitioned to a soft money position, no longer a postdoc but a research associate with the SETI [Search for Extraterrestrial Intelligence] Institute. Still working at NASA Ames but contracting through the SETI Institute.

That lasted for about three months, and that's when I got offered a permanent civil servant position at NASA Ames, and so that was August of 2002 when I became a civil servant. I still worked on Mars cloud science and Mars climate science, but also part of my hiring on as a civil servant was to help build up instrumentation for NASA flight missions. I had done this back in college. I had worked with the Space Grant College and the Laboratory of Atmospheric and Space Physics at the University of Colorado, working on instrumentation that went on sounding rockets and small satellites. That was one of the reasons I was hired as a civil servant, was they saw that value that I could bring to NASA Ames and the Agency was not only am I a research scientist, but I'm also an instrument developer.

I split my time more or less between doing instrument development, working on proposals to build instruments, get instruments into mission proposals, split that with doing basic fundamental research on Mars climate science.

JOHNSON: How do you feel that those earlier positions with that instrument development that you were talking about and that research prepared you for the LCROSS [Lunar Crater Observation and Sensing Satellite] mission that you would eventually be involved with?

COLAPRETE: Yes. That's a great question. There was kind of a confluence of factors that came in just at the right time. One of the things I was doing with the Mars climate research was trying to understand how a large asteroid or comet impact would affect the martian climate. I built into these general circulation models, which were these very large high-fidelity physical models, I built into that the simulation of a large 8-kilometer asteroid hitting Mars.

To do that I had to educate myself on what would really happen with a large impact and looked at how an impact like that would heat the atmosphere, how much ejecta, debris it would throw out, how much water would it excavate from the subsurface of Mars and put in the atmosphere. I researched that, I understood that. That was actually one of the first grants I got, was to do this work. That educated me on exactly what would happen with an impact.

I had a general understanding of impacts and impact processes. Then this opportunity to propose what to do with the extra 1,000 kilograms that was going with the Lunar Reconnaissance Orbiter [LRO] mission came around, and the proposal was, "Hey, we had to go to a larger launch vehicle," LRO did, so they put out a call for ideas, proposals. What could we do with that extra 1,000 kilograms that is headed to the Moon?

I was talking with colleagues at Ames. One of the ideas was just crashing the spent upper stage of that rocket that was going to the Moon, that was carrying LRO to the Moon, into the Moon and looking at the debris. I said, "Hey, I can estimate based on what I've been doing for the Mars climate model research. I can estimate the amount of debris and we can see if that made any sense." A colleague of mine who had done lots of Mars research before actually—Geoff [Geoffrey] Briggs is his name, he's now long retired—really is the guy who approached me and said, "Do you think it would throw up enough material?"

I did the calculations and I showed it to him and I said, "Yes, this is how much I think it could." Originally, we were thinking what if we were just observing it from telescopes on Earth. This was tried once before and has an Ames connection actually. The Lunar Prospector mission was an Ames-led mission in the late 1990s that actually mapped the hydrogen content in the surface of the Moon, the top meter of the surface of the Moon, and showed there was this excess hydrogen at the poles. As Lunar Prospector mission was coming to the end, as all good things do, they crash, and it crashed into the Moon. But they targeted it into one of these permanently shadowed craters, Shoemaker, in an attempt to see if it could throw up material and they could observe it.

They observed it actually with the Hubble Space Telescope [HST]. They didn't see anything. We knew we couldn't just repeat that experiment. The Lunar Prospector mission was a very small spacecraft, about 150 kilograms or so in size, and it came in at a very low incident angle, it was grazing. We worked with some mission designers, Ken [F.] Galal was in particular one of the mission designers, and came up with a wonderful orbit solution where we'd be orbiting the Earth, not the Moon. Lunar Prospector was orbiting the Moon, and that's why it had a low impact angle. But we'd be orbiting the Earth out at a lunar distance. It just so happens the Moon actually gets in your way while you're orbiting the Earth, and so we didn't crash into the Moon, it just got in our way. We weren't targeting the Moon, it just got in our way. But what it means is we're able to hit very steeply the south pole of the Moon. That imparts more energy, you get a larger more vertical ejecta or debris column that comes up. Plus, the upper stage of the rocket, the Centaur stage that was used to push LRO to the Moon, was approximately 2,300 kilograms, so it was a lot more massive, factor of 20 or so, than Lunar Prospector spacecraft.

When we looked at all that and said, "Hey, that's big enough and it's hitting at an angle that's steep enough that we can get a much different result." We did all the math and convinced ourselves yes, you could probably see it from Earth, this is good enough where if you use ground-based telescopes or HST you can make the observations. We started putting that proposal together.

At the same time, just serendipitously, some folks from Northrop Grumman had approached Ames about the same call, and one of them, Luke [S.] Sollitt, who was a scientist at the time at Northrop Grumman, met with me and Geoff Briggs and a few other folks at Ames and said, "We were thinking about putting a small spacecraft on this and in an orbiter and doing observations of the exosphere." We told him about our impactor ideas, and Luke said, "Well, what if you actually had a spacecraft that went with it and observed the plume?" We were all like, "Yes. Why not?" It turns out Luke Sollitt just actually accepted a branch position at NASA Ames after being elsewhere at the National Academy, so things have all come full circle almost.

We looked at that, we said, "Okay, what would that look like?" Northrop Grumman worked with NASA Ames to come up with a concept for the spacecraft, and I worked with Northrop Grumman folks, Luke and others, and folks at Ames to come up with a payload, an instrument suite that might work.

We put in both concepts, one where it was just the rocket crashing and you observed it only from Earth or from LRO, and then the other where you observed it from Earth, LRO, and the spacecraft that you brought with it. It was the latter, the spacecraft that came with it, that was the one actually that Dan [Daniel R.] Andrews was actually helping out with as well. Long story short, that was selected to go to a downselect of four, so four oral presentations and then final selection. It was just kind of having the right concepts in mind, talking to the right people at the right time. I was thinking about impacts and debris clouds for Mars. I could apply that to the Moon.

Talking with people who were thinking about other ways to make observations from small platforms at Northrop Grumman, it just kind of came together at just the right moment into this really unique concept. There's been other impact missions. Not many, but like this. Certainly nothing to the Moon. That's how all that Mars research and then small instrumentation and payload research came together to be built into the LCROSS proposal.

JOHNSON: That's interesting. Were both concepts competed originally, the one with the shepherding spacecraft and the one without?

COLAPRETE: Yes. Those went into the same call. I believe if I recall correctly 19 proposals from various centers and JPL [Jet Propulsion Laboratory, Pasadena, California] were submitted. We submitted at Ames several. The two that I was most involved with were the one that was the impactor that was just observed from Earth, and then the impactor that included what would become the shepherding spacecraft is what we call it in LCROSS. We put in both because one was a very low-end cost mission. The one that was just an impactor was very low-cost. There was almost no modifications to the upper stage. All the cost went into designing the trajectory and preparing ground-based facilities for observations and things like that. It was very low-cost. The other that you built a spacecraft to go with it was at the high end of the cost spectrum. We put in both, not knowing what NASA Headquarters [Washington, DC] would like.

Ames put in other proposals too. One was an orbiter that was really highly ranked actually in the review process, but ultimately it did not touch the water, and that was what was key to the selecting officials ultimately, that LCROSS touched the water. Albeit violently, it touched the water, and that's what they were looking for. They didn't want just more remote observations of what this might be. They wanted the most definitive result they could get of what the hydrogen at the poles could be. They thought LCROSS was the way to go with that.

JOHNSON: Using that rocket, the Centaur, the stage, that was an interesting concept too because it had the weight you were talking about that the Prospector didn't have.

COLAPRETE: Right, absolutely. Actually, looking back on it, it's quite brilliant, in that they offered you 1,000 kilograms, and one of the competitive concepts which was also an impactor used that 1,000 kilograms, but only that 1,000 kilograms. They were going to have LRO. It was very similar to our idea of using the Centaur as an impactor but they were using that 1,000 kilograms as the impactor. In a way LCROSS played the system in that we used the 1,000 kilograms to build spacecraft and didn't use any of that for our impactor. We used the 2,300 kilograms of the Centaur as the impactor. We got 3,300 kilograms out of the deal to the Moon. That was really smart.

One of the fellows who was on our team on the proposal and critical through the whole LCROSS mission was Pete [Peter H.] Schultz who's at Brown University. He's an impact guru. He's done a ton of experiments at the Ames Vertical Gun which fires projectiles, and was key to understanding the LCROSS results, etc. He was actually also on that competitive team which was led out of JPL. This happens often on proposals within the planetary science community. We're a fairly small community in the big scheme of things. You often find yourself on multiple proposals that are all competing within the same announcement of opportunity.

Afterwards JPL was very upset that we won because they thought their concept, which was just the impactor not using the Centaur at all, was very good. It was very good, but it was not actually—I think the fact that we realized you could use the Centaur as the impactor and have the shepherding spacecraft was definitely the aspect of our proposal that put us over the top over the JPL proposal.

JOHNSON: That's interesting. It's kind of brilliant too.

COLAPRETE: Yes. Like I said, it was the stitchwork of various people's ideas. As I recall it wasn't one person going, "Aha. This is how we're going to do it." It was oh, we can do an impactor. Yes, and we can do a small spacecraft. Oh yes. Then all of us coming together and saying, "What if we put them all together?"

Retrospective, I don't remember an exact moment that someone said, "Aha." It just kind of organically grew out of all of our discussions.

JOHNSON: Let's talk about the competition itself, because it wasn't a normal Discovery type mission competition in that you found out that this opportunity was going to happen because they had this extra weight that they could spare for LRO. It was kind of a condensed version of a Discovery competition. Talk about that, hearing about the opportunity, the proposal itself. You said you were working on two different versions of it. Talk about what phases and the downselect, and then going through that downselect process.

COLAPRETE: I honestly like these kinds of opportunities and calls because they're quick. They don't languish. They get to the point. I like that about them. But it was short-fused. There was some rumblings, rumors that this call was going to come out, and so at Ames there was a team put together. I forget what it was called. The Blue team or something like that, that was going to look at concepts and ideas. I myself was part of that. Ken Galal, Dan Andrews, Geoff Briggs, a number of folks at Ames. Anyone who was interested of course could participate as well.

We got together and just started throwing ideas out there, concepts out there, sitting down in front of a whiteboard and jotting down ideas, and started fleshing out the ones we liked the most. Then once the announcement finally came out and we saw we had just a few months to get the idea together, we got into a scramble. We downselected to a handful that we wanted to really pursue.

Within Ames, however, anybody was allowed to put in a proposal. I think of the 19 Ames put in I want to say probably more than 7 proposals. This was at a time when Ames was proposing to lots of things and just trying to get some opportunities to do some really unique stuff. The proposals weren't long because it was short-fused. It was 50 pages or less than that, I think. That's always harder. When you have not a lot of pages to tell a story it gets really hard, and that was one of the more challenging things, was how do we get our concept across and make it clear enough and demonstrate we've thought about it enough in this few amount of pages.

I remember working very much to the very last minute on those very late hours with everybody polishing the proposals and getting them together, the ones I worked on. They were submitted after three months and then we waited to hear the results, and again rumors started circulating that one of Ames's is going to make it to the next round. I remember talking to at the time Bob Haberle who was still my boss in the Mars climate group about it. He was like, "Don't get your hopes up."

I remember that very distinctly because I really felt good about this one. Of course, not so much now but back then it was if you wanted to get any insight into what might be going on you went to NASA Watch [blog website edited by Keith Cowing]. I was on NASA Watch every once in a while, along with the others who wrote the proposals. He somehow got that certain institutions were being considered. It didn't say exactly what.

But then I got a phone call that said, "You need to put together a short CV [curriculum vitae] that includes this kind of information, Headquarters wants this." I'm like, "Okay, I've never had that before." That made me even more excited. Then we got the call that yes, you've gotten downselected, and you've got three months, or two months, I forget exactly, to prepare an oral presentation at Headquarters, being one of four. Actually, two were selected out of Ames-ish. One was a Berkeley/Ames. That was Lunar Explorer for Elements and Hazards (LEEAH) I believe it was called. It was a dust—it was an orbiter, a really good concept. One was out of JPL and the other was out of Goddard [Space Flight Center, Greenbelt, Maryland].

We went, "Yay, okay, now we're in the final four." We prepared for that and got that all together, and went to Headquarters, made the presentation, and I felt really good about it at the time, just because the Chief Scientist for Exploration at the time, Mike [Michael J.] Wargo—it was a small conference room, not very big—was sitting not too far from where I was standing and presenting, and I could tell there was an immediate interest and connection, and he's a nice guy. I'm sure everyone else felt his same enthusiasm, but I could tell he really liked the idea. There was a lot of discussion. I had to be taken down offstage, if you will, because of the conversation. It wasn't me going long in my presentation; it was just a really dynamic conversation with the review panel. It was all very positive.

That was a really good sign. I know afterwards we all huddled together and Dan and I were talking. He said, "Yes, that was golden, you could have thrown your slides away. Really it was that conversation you had that was sincere and heart-to-heart and just piqued excitement. That was it." I think at the time we knew our other proposals were going to be just as good and fantastic, so we still didn't know. But then I forget exactly, a few weeks later, something like that, they again called for more information, for me to prepare more personal CV information for them. I'm like, "Okay, this is good, this is good." Then we got the announcement that we were indeed selected. There was a lot of excitement. Bob Haberle congratulated me right away. He said, "Ah, you were right. You got it." It was good.

JOHNSON: Were they collecting the CV information for the announcement itself?

COLAPRETE: Yes. Exactly. That's what they were doing, gathering information on each of the key leads on the proposal for the announcement.

JOHNSON: You were the principal investigator, but you were also the payload manager.

COLAPRETE: Yes. That's where that instrument background came in. LCROSS was a really small budget, and to be able to get it to all fit, we had to do some very creative things. One of them was me being the principal investigator [PI] but also the payload manager. We tried to flatten the management structure as much as we could, as much as it made sense. We had a very small science team, about six, seven people total, which is very small. We took a very different approach on the instruments. In the proposal we had I think five instruments called out. A couple spectrometers and a couple cameras. After selection we had a science team meeting at Ames, and we stepped back and said, "All right. We were selected. Authority to proceed starts now. This is the payload we proposed. Are we sure this is the payload we want to fly?" That's a very dangerous question to put forward to any scientist because they're like, "Well, we could do this and this and this."

Indeed, we started talking about things we could have done differently. This is part of the proposal and the work process. As you work through the proposal process, as you work through the step two selection, you get smarter. You start thinking more. You've had more time to think on it. We did make some changes to the payload, and so actually what we had was, like I said, two spectrometers, and I think three cameras before that went into the proposal. We ended up with five cameras, three spectrometers, and a radiometer, so the payload exploded. As the PI I was like, "Yes, this is going to give us fantastic data, fantastic science." But then as the payload manager I said, "And I have to do this all within budget and schedule."

I remember getting a phone call after selection from somebody from a traditional NASA instrument house saying, "Congratulations. Who is providing your instruments? We'd really be happy to participate if we could."

We started talking and I said, "You know, my budget is $3.5 million and I have to deliver it in nine months."

He said, "Oh. Okay. Thanks. Bye." He literally could not. He's like, "Don't want any part of that."

We knew we couldn't go with traditional instrument providers for this too. Part of the proposal, we had already been talking to some folks who were nontraditional instrument providers. It wasn't just starting at this moment. But we started calling around and going to places like commercial instrument providers that provided instruments for pharmaceutical production monitoring or asbestos identification, and handheld instruments that they used for tearing down drywall or doing building demolition. There's a lot of these commercial companies who have small instrumentation. They more or less mass-produce it, and use it in commercial ways, but of course it's not flight-qualified. But we started talking to them and we said, "Hey, you interested?"

What's great is a lot of these companies were very very small. They were personal, and you created this personal relationship and they get excited and they're like, "Yes, we're in, this is crazy but this is fun. We want to do this." That was really important. The fact that they were commercial companies, I remember talking to one particular instrument provider. His name is Dave [David R.] Landis and I later did another instrument with him on the LADEE [Lunar Atmosphere and Dust Environment Explorer] mission. He was at Ocean Optics at the time. I remember talking to him, "We want a UV-visible spectrometer, these kinds of resolutions, and you guys have some products that are very similar to this, but we would have to build an engineering unit and get it through qualification testing and evaluate it for ruggedization. We need that engineering unit in six months." Thinking wow.

He said, "Why so long?"

I'm like, "What? You can do it faster than six months?"

He's like, "We have to turn our catalog over in terms of product every six months; we have to keep selling and improving the product, otherwise we go out of business. I'll get it to you in three months." Definitely each of the instrument providers got us engineering units within about three months and it's because they're at a totally different methodology and process for making changes and pushing it through.

Now one of the reasons I could get away with working with these people and working in a very agile nimble way with these people was the payload on LCROSS could be descoped at any minute. The minimum success requirements did not include the payload. That was something that Dan and I talked about a lot. I said, "Okay, there's a risk here in that yes, if something goes really wrong, they could leave it off, fly mass simulators, and we don't have it. But at the same time if we make the payload part of the Level I requirements the amount of mission assurance and quality assurance requirements that then would be levied on top of them because they're part of the minimum success would mean we couldn't do anything with our $3.5 million budget." It was a trade we made early on. I agreed to it, we all signed it with Headquarters.

That really came down to just having trust in project management, Dan Andrews, trust in our chief engineer that they weren't just going to try to solve a problem by throwing the payload off even if the risk there is we have a mass growth problem, we need to save some mass, throw the payload off rather than trying to look at the spacecraft and solve the problem there. I had lots of conversations with them constantly about how things were going and staying on top of it. That was a risky proposition we made, but it worked out fine in the end. We were able to deliver the instruments on time under budget.

I credit our ability to do that to the experience I had back at the University of Colorado working for the Space Grant College. I built a number of instruments for small spacecraft and sounding rockets, and our budgets were like $5,000, $10,000. We'd borrow, beg, scrape together stuff, and were able to do some really good science. I knew it was possible at least at that point, and I was young enough that I wasn't at all completely bought into the spaceflight culture of it has to be expensive and all this. I was like, "No. Let's just do it differently."

That all worked out really well. The cameras were commercial cameras. One was a ruggedized camera that's used in Indy [Formula 1] race cars [MIRICLE TB2-30, made by Thermoteknix Systems Ltd.]. That came from the UK [United Kingdom]. They were so thrilled and excited. He actually came out to NASA Ames for the impact, the president of that company. The other cameras came from Ecliptic Enterprises. RocketCam was their product at the time where they'd had cameras on rockets and they'd filmed the exterior of the rocket as it launched. They were thrilled. They were out of Pasadena. They also build a data handling unit that controls all the other payloads.

The near-infrared spectrometers were from a company that, like I said, built handheld instruments for detecting asbestos and also measuring alcohol content in beer, very important. Then the UV-visible spectrometer was from Ocean Optics, my friend Dave Landis, he's my longtime friend now since then. Then we built an in-house radiometer. That was the one instrument we actually built in-house.

But we put it all together at Ames, and what was really cool too about the whole spacecraft concept that Northrop Grumman put together was it was built around an ESPA [(Evolved Expendable Launch Vehicle) EELV Secondary Payload Adaptor] ring which was just this adapter ring that connected LRO to the rocket. It was there already and they actually turned that ring into a spacecraft by putting panels on it.

Now originally all the hardware for the spacecraft, the command system, the power system, and the payload were all internal to that pipe. It looks like a giant sewer pipe, and originally at proposal time they were all in the interior. At PDR [preliminary design review] one of their mechanical engineers just had an epiphany and said, "Well, why are we doing it that way? Why don't we just put these adapter rings, they have little ports on them, why don't we put panels on each of those ports on the exterior? It's like a flower. You can lower all these petals, work on them individually, and then bring them all together. Then the fuel tank goes right in the middle." That change was made at our preliminary design review.

It was kind of late in the flow that we made that change, but that was a brilliant idea too, because what that allowed us to do at NASA Ames is we delivered the payload, we were doing all the payload work. We had one of those panels at NASA Ames and we could build up the entire payload on there, do all the testing there. Once it's all done and we're totally happy with it we packaged it and we drove it down to Northrop Grumman in southern California, and they were able to just basically pick it up and put it on the spacecraft. It just made for a very modular easy way to assemble and put things together. That was another really enabling aspect of the design, because otherwise we would have had to deliver each individual instrument individually, integrate them individually in some cases to the spacecraft. It would have been a lot harder for the spacecraft integration team. That worked out really well too.

JOHNSON: The fact that you were able to go to the commercial world and find these things mainly because you had this time constraint, but was that unusual at the time? Was this really odd for NASA to do this for these types of exploratory missions?

COLAPRETE: Not entirely unique, but I think we brought it to the next level. We went to these smaller companies, so a lot of the times Lockheed Martin for example and Ball Aerospace for example, they are excellent instrument providers and spacecraft providers. They've done this. This is what they do, in part. They're commercial companies, so it's not like they're some other government entity or anything like that. They've got smaller groups within them and that sort of thing. You could go to these companies and they might have a product. But what they're doing is designing it from scratch with you. Or they might have a design from a previous mission that they can apply.

But it's all done at a scale that is in terms of total numbers and turnaround time orders of magnitude less units and orders of magnitude more time than these smaller companies whose just livelihood depends on quick turnaround. That was the difference, I think.

Ocean Optics is a fairly large company too. But Dave Landis, the person we met, ran a small part of it. It really came down to finding the right people within a small company who could fully commit to engaging with us and hearing us out. It was very bidirectional. It wasn't NASA throwing requirements over the fence and saying, "This is what we need, you provide it." It was much more of a well, we see you have something that's kind of like what we need. What would it take to change it? Have you ever done anything to ruggedize it? We'd have that conversation and then there would be a back-and-forth. That's what I think was really different.

Now NASA does this through Small Business Innovation Research grants, SBIRs, and so on and so forth. This wasn't that. But in hindsight it was that. It was really working with these small companies and helping them with their product. For example, the near-infrared spectrometers had a certain seal on a chip that was very important and we were finding that in thermal-vac that would leak after some time and make the instrument not work reliably. We worked with them, the NASA engineers here worked with them and their engineers, to create a better seal for that, and they took that and implemented it into their production line, so their product is actually more robust and holds seal better. There was a lot of that back-and-forth where we could take our engineering expertise and apply it to what their product was.

That created this immense amount of trust which was critically important on these small timescales. All the contracts I did with each of the instrument providers were firm fixed price too. None of them were cost-plus. I remember our Headquarters executive Jay [Jason E.] Jenkins during a review, he asked, "What kind of contracts do you have?"

I said, "They're all firm fixed price."

He said, "What? Really?"

I said, "Yes."

He said, "They agreed to that? They didn't balloon the price?"

I'm like, "No." Some of the contracts, it was a certain amount of money to get the instrument to an engineering unit, and once that engineering unit was qualified, an option to buy "n" after that with each unit costing $10,000. The contracts were structured in a much more commercial way. It was like I want to build a widget and then we'll order "n" of them once we're done and satisfied with it.

That was important. There's tons of little things like that that were really important to get the payload out the door. We had nine months from the time authority to proceed where we changed the payload from five to nine instruments, we had nine months to deliver to Northrop Grumman. We did on time, and about $3.4 million was our total budget, and we came in at about $3.2 million or so, so it was under budget too. It was all because these people were working on firm fixed price contracts—the near-infrared spectrometer contract where we flew two of them on the spacecraft, the original contract to develop the engineering unit was $125,000 I think, that's it, and then each unit to fly that flew were $25,000 on top. Just ridiculously inexpensive in a traditional NASA payload sense, and every instrument worked. They all worked.

In part it's because LCROSS was a short mission, it was about 110 days in space. It wasn't very long. But nonetheless they all did great and didn't have a single issue with them the entire time.

JOHNSON: I'm sure it opened up opportunities for those companies too.

COLAPRETE: Exactly, absolutely. I've carried that forward, that approach to doing business, and started a flight instrument group at Ames where we do basically the same thing. Two instruments on VIPER [Volatiles Investigating Polar Exploration Rover], the instrument that I lead is developed through an SBIR [Small Business Innovation Research] in part. Part of it is developed with an SBIR with a company on the east coast in a very similar way, very similar fashion. Then the mass spectrometer, which is delivered out of Kennedy Space Center [Florida], about five years or so ago I met with them and talked to them about how we did the instruments on LCROSS because they were trying to get a mass spectrometer ready for flight. I said, "Who builds these commercially?" They threw out some names. I said, "Try this approach." At the end of the day, they did, and they're working with a company in New Jersey, INFICON, who does commercial mass spectrometers, but they've applied the same practice.

Basically it's like an SBIR but you're just working very closely with a small company and modifying a commercial instrument to fly in space, rather than developing it from scratch and it being $20 million. They're building these things for just a couple million dollars. They're very very capable. You're capitalizing on all the investment the commercial companies put in.

It's definitely a two-way street, just like the whole SBIR program is meant to be. I'm totally a fan of it. Works great. I apply it anywhere I can.

JOHNSON: Northrop Grumman, the relationship that your team had with them, I read in Dan's book about how the team members were actually embedded with some of the Northrop Grumman folks. That relationship, did that help with that communication and with that working back and forth with them, having that kind of dynamic embedded with them?

COLAPRETE: Yes. Absolutely. For me it always came back to being open and straightforward. I remember working with one of their lead systems engineers with the payload, and he asked me a question about one of the instruments early on, and I was like, "Well, to be totally up-front, we don't know." I forget exactly the topic of the conversation. But he was taken aback by how open I was to reveal dirty laundry we might have. Because he wasn't used to that. He said, "Oh, okay. We're going to be totally open? Cool. I like that." That was very early on. But that's part of it.

Then like you read, we had people embedded there at Northrop Grumman but also very much involved in all of our discussions especially with the payload. As I mentioned too, the scientists who were part of the proposal were from Northrop Grumman. They were part of the science team. There's a definite cross-pollination between the two teams that just built trust ultimately. I think that was absolutely critical because we had to be so dynamic, so quick, so nimble that you had to rely on everybody to help solve problems. You checked your ego. You didn't care if you looked like you didn't know what you were doing. You were like, "I've got to get this solved. Can anyone here help me solve this?" People would say, "Yes, I can help you." Then it went both ways.

That was really important. To the manager at Northrop Grumman and to Dan's credit, they created an environment where that was doable. In some instances management will stifle that and stomp that down just because they again are afraid of unwanted information getting out. An example of that for me was we took one of these commercial spectrometers. One of the first things we did was test how easy is it to break it. Part of the ruggedization evaluation. We had one on a random vibe [vibration] table. We were going to do a random vibration shake test of it. There is a film crew out to film it, this is one of the first spectrometers, and they want to get film of it. It shook and it was shaking. We picked it up and the test engineer picked it up. He went *Shake. Shake*. He looked at me like there's something loose in there. I said, "This is exactly why we do these tests early. We don't design and design and design. We test. We can look at it now, see where it failed, and immediately fix it." That was all filmed, and it actually was put in an interview and televised.

I got a call from, again, our Headquarters executive going, "Oh my God, why was that? What are you doing?" He said he was watching it and he fell out of his chair.

I'm like, "What? This is what we're doing. Not ashamed of it. This is exactly the right approach."

But he said, "No. This is not how we're supposed—we're supposed to hide the things when we break them. Or at least not televise them," is the way he put it to me. Just don't televise it.

I'm like, "All right. But it's good. We retested it. It was a mirror and the adhesive was not applied as well as it should have. We fixed it, retested, it's good."

He said, "Okay, just don't do that to me anymore. Let me know at least."

That's what it was. It was just very much open dialogue across the teams, all teams. I'll say that extends to even one of the most fulfilling aspects of it was the open dialogue with the LRO Project. From the spacecraft to the payload to the science there was this cooperation between the two projects. That was really interesting in the sense that Goddard had put in a proposal also for this, and they weren't selected. But we were riding along with them, and so they were obviously keenly interested in what it is they were going to be sitting on top of during launch. I was from a science perspective critically reliant on those early LRO observations. We knew kind of where we wanted to impact but the maps of the Moon at the time were not known especially at the poles to maybe 3 kilometers. While we can say, "We want to target there," we actually didn't know where there was to within plus or minus 3 kilometers in inertial space. It was really up to the LRO instruments and scientists to actually provide us that data.

The instruments on LRO also measured surface temperature, the topography, all these things that were really critical to us selecting an impact site. Normally in a mission like LRO there's a commissioning period where the orbiter gets into orbit and the instruments then start to check themselves out one by one. They're not actually quote, unquote doing science for maybe one, two, even three months after. LRO was going to be in orbit at least one year, so there was time to have a nice cautious checkout. That's exactly not what they did. They immediately started taking data. All safe, it was all safe, but they immediately started taking data and sharing that data with our team.

The PIs of each of the instruments and the project scientists were immensely supportive, immensely. I would have conversations at 2:00 a.m. with the PI for example of the Diviner instrument which was Dave Paige about best places to impact. The hydrogen sensing instrument

LEND, that PI, we'd have conversations. They'd share the raw data and say, "No, data is not perfect yet but want to make sure this is what we're looking at." It was fantastic. It was absolutely critical to us finding the right place to impact and understanding the lighting geometry, the slopes, everything. We had a team meeting where we had the full LCROSS team and then all the LRO PIs and project scientists participating, and they all shared data.

We launched and we had 110 days to impact. We had to pick a place where we were going to impact prior to launch. But I made it very clear to the Project and Headquarters that that's probably going to change as LRO learns things. Over that 110 days we were constantly learning and getting smarter. We had a team meeting. We made a final selection with the team that we're going to impact a crater called Cabeus A. Cabeus A was a small crater outside the primary crater of Cabeus and might have some hydrogen, was showing signs of could be having hydrogen. This was about a month and a half outside of our impact date that we made that decision announcement. Something about it didn't sit still well with me. We all made a decision, but the proper crater Cabeus, the main crater Cabeus, still was showing the highest hydrogen content of all the craters in the area we could impact. The problem was that it was so deep that the ejecta would have to get really high up, three plus kilometers, to hit sunlight for us to observe. That's why we weren't selecting it, we were selecting a crater just outside of it, Cabeus A.

We kept getting data over the next couple weeks from LRO and they were refining their elevation models. I had a postdoc working on some of those elevation models and looking at various angles to the Sun for Cabeus. There was something just in the back of my mind that was haunting me that no, Cabeus is where the most water is, hydrogen at least at the time as we understood. This postdoc came to me, Jessica Barnes was her name, and she said, "There's this little saddle in the rim of Cabeus, a little dip. I looked at these dates which correspond to when we can impact. The Sun is moving almost like Stonehenge into that perfect position where it can shine through this little spot in the crater rim and illuminate just 800 meters above the crater floor." I'm like, "You're kidding me." We got the team together real quick, looked at this, that, and the other, confirmed it.

I remember then I'm like, "Okay, this is a big deal. We've already said we're going to Cabeus A, and we made a public announcement we're impacting here." I put on the table then to the team this new information, let's go for Cabeus. Some of the team members weren't happy. Cabeus A gave us a very low Sun angle, like just a couple hundred meters. It gave us the best opportunity to observe the ejecta cloud and all that. But I said to myself, "Well, it's not about observing the ejecta cloud. Our goals are to identify what that hydrogen is. If there's not sufficient hydrogen there, and there's a lot of uncertainty about that, then it doesn't matter if I've observed a brilliant, beautiful impact and it was in a dry area, we haven't met the spirit of our goals."

I was really troubled by this still because of a lot of different opinions. I remember having a conversation again with Dave Paige, the PI for the thermal instrument on LRO. He's like, "Tony, it's simple. Impact the coldest place." Cabeus. He's like, "Yes, Cabeus. Our temperature measurements say it's 40 kelvin where you're talking about impacting. That's the coldest place in the south pole." I'm like, "Okay, all right." I took that advice, put together a proposal, ran it by the Project. I worked with our mission planners too so the trajectories were all fine. Put it to the Project, and Dan was like, "Okay. If you think this is. Yes." Then put it past the Program Scientist, Ben Bussey was our Program Scientist at the time. He's like, "I love it, I love it. Put it towards the execs." We made the decision okay, we're changing our impact location. That was changed two weeks prior to impact. It was the right decision ultimately. It came down again to this cooperation with LRO. If we didn't have this kind of dialogue and this again openness willing to just speak frankly and freely, then it wouldn't have happened.

Another example of that cooperation was okay, we picked the spot, but how do we know we're targeting really where we want to target. LRO was building up this lunar terrain map with every orbit using the laser altimetry instrument LOLA. That was helping us. Those errors in our understanding of the locations of where things were on the Moon were collapsing quickly.

We came up with a process where three independent parties, we could look at a map and say, "We want to impact between these two points on the Moon." But exactly where those were in inertial space is what mattered because that's how we targeted the spacecraft. It didn't have any kind of visual targeting. It just flew in inertial space. We had the LOLA team on LRO. David Smith was the PI. We gave him a picture with an X on it. We want to target there. We took that. We gave it to our targeting science team. We want to target there. Then we gave one also to the flight dynamics group. Three independent people all came back with the coordinates based on their information as to where that was in inertial space. They all agreed enough, and that's how we were confident we could impact where we wanted to impact. It turns out we hit within 80 meters of where we were targeting. That worked out great. Again it was critical that the LOLA team who had the best information be a part of that, because otherwise we could have been off by hundreds of meters or a kilometer even.

JOHNSON: Have you experienced that kind of cooperation in other missions that you've worked on? Or was this something somewhat unique to the situation?

COLAPRETE: It was somewhat unique to the situation. I had when I was at University of Colorado as a student in the sense that you kind of had to find clever ways to just solve problems. You built this trust and this level of cooperation you don't have if you're coming at it in a very methodical way. The uniqueness of the mission, the timeline, all that I think played a big part in forming these relationships.

One more example of how it extended beyond just the science team and the LRO Project management was originally LRO had planned to be on the far side of the Moon during the impact for safety reasons. They didn't want debris hitting the spacecraft. Totally makes sense. But all the PIs on LRO wanted to observe the impact as best they could, and of course I wanted them to do that too. We worked a lot on debris analysis for what LRO might encounter. I was feeding that information and discussing it with the LRO lead systems engineer. Again very frank conversations. I don't think there's any risk to your spacecraft. Here's our calculations. He had people at Goddard look at them and they generally agreed.

Again this was just a week or so or two weeks before the impact. We changed the impact location; we're working with them. I remember I was in a dress rehearsal for operations and the systems engineer called me up and said, "Yes. We talked with everybody here and the PM [Project Manager]. We're going to change the orbit phase to line it up, so we'll be 90 seconds within view of the impact 90 seconds after it occurs."

I'm like, "Wow, okay." I said, "That's fantastic."

Then they let Headquarters know and they said, "Yes, we're changing the LRO orbit to do this."

Headquarters is like, "Okay, well, we'd like to have a discussion and look at it."

That's when the Project Manager for LRO said, "That's fine, we've already changed it, it's done." Again it was just a pure trust that allowed that I think to happen. They went ahead and they changed it.

Then Headquarters went, "Okay. If you're good with it and you're good with it, LCROSS is good with it, LRO is good with it, we're good with it." It worked out great.

Back to your question, it was the uniqueness of the mission that I think really just kicked it out of the traditional norms of traditional spaceflight missions that allowed these kind of unique relationships to happen.

JOHNSON: This was a piggyback mission. Do you think this mission in particular created more opportunities for further missions like that for NASA? I'm thinking of things like CubeSat or those types of things. Do you think those are directly related to the fact that LCROSS was so successful and those teams did work together so well?

COLAPRETE: Yes. It was a Class D mission, which was a more risk-accepting mission. It could accept more risk. I think that was important. That demonstrated that a significant planetary mission—it was under $80 million but still pretty significant—it definitely demonstrated how yes, a secondary mission with a higher risk tolerance is a totally acceptable tool to have in the NASA toolbox. In part I think it's led to ridealongs like the SIMPLEx [Small, Innovative Missions for Planetary Exploration] missions and Lunar Trailblazer is going to the Moon as a SIMPLEx mission. Luna H-Map [Lunar Polar Hydrogen Mapper] is on Artemis I, a CubeSat. It's the first SIMPLEx mission. So yes, I think it definitely led to more missions like this. It certainly led to the LADEE mission. At the time Alan Stern was the Associate Administrator for

SMD [Science Mission Directorate]. He came out to Ames to see LCROSS, and I brought him through the clean room and we were looking at the payload on the pallet. He was just totally impressed by how we put it together so quickly, the innovative approaches working with commercial companies both large ones like Northrop Grumman and small ones. He wanted to see that done again, and that really led to the direction of LADEE to NASA Ames, his impression of how missions like this, LCROSS, can be really successful and important.

Definitely, and I think it's even led in part to some aspects of the commercial lunar landing services program now, where there's payloads that are being put together for these missions that are following a lot of the same kind of steps we followed in terms of being nimble and quick. The proposal process for these payloads is very short, just like it was for LCROSS. The delivery schedule is very short. It mandates these same kind of steps. I do think LCROSS opened up the way for these kinds of missions.

The funny thing is Lunar Prospector was one of the first Discovery missions. Discovery was originally meant to be low-cost risk-tolerant missions. Lunar Prospector was very inexpensive. It didn't even have what you'd call a computer in it. It just had some logic boards. But it was very very successful. However, the Discovery Program became more and more risk-averse as time went on. Then there was a couple failures and that really pushed it into the Class B mission. It was no longer Class D. Mars Pathfinder, which was a Discovery, was Class D. Lunar Prospector. Class D existed before LCROSS but NASA had moved totally away with this class of mission, this size of mission, from it. LCROSS brought it back, I think. Especially with launch vehicles becoming less and less expensive now and rideshares more and more prevalent, it's maintained its value within the Agency, that is Class D missions. I think it's going to open up more and more. So yes, I think LCROSS brought it back.

Quite frankly, I think there were individuals within NASA who didn't like that it was successful. It was not in their particular—they didn't want to see NASA go back to any kind of risk-tolerant stance for any number of reasons. But luckily LCROSS was successful.

JOHNSON: Maybe for some of the same reasons they didn't want you televising those mistakes or those failures.

COLAPRETE: Exactly. Yes. Right. They didn't want any kind of show of failure. What we do is not without risk. It's hard stuff and it's okay if sometimes it fails. It's not like we're going out there. That's the other thing. One of the things LCROSS I think really did well is its risk management. That's one of the things I learned from it and I apply daily in all my projects, is saying, "I'm risk-tolerant," doesn't mean you're necessarily taking risks. It's actually harder than being not tolerant of risk, because you have to understand each risk more, you have to really appreciate the likelihoods and the impact, because you don't have resources to just hammer down the risk. LCROSS did that exceedingly well in my opinion. It's really a model I think other groups have borrowed and taken and applied towards their risk management process for these larger Class D missions. That was actually something I think LCROSS also brought to the Agency.

JOHNSON: There were other institutions that had some investment in LCROSS because they were going to be monitoring it, those ground-based places, the teams in those observatories, and then of course the ones that were monitoring from space. Talk about how they helped you and your team collect the data that you were looking for.

COLAPRETE: That was a really amazing part also of LCROSS. Jen [Jennifer L.] Heldmann was the lead for the observation campaign for LCROSS and that included ground-based telescopes on the west coast of the United States and South America and Hawaii and HST and a couple other Earth-observing satellites that could view the Moon.

It was a coordinated dance down to literally the second. I put in a proposal actually to get time on HST to make the observations. That's how we got that. It was selected, but we had to coordinate that observation just perfectly because HST has a hard time looking at the Moon because it's so close. It can only maintain a certain slew rate, pointing rate, and the Moon is moving by pretty quick because HST is in Earth orbit, and so we had to coordinate. We could pick the exact impact time by adjusting when we released the Centaur from the shepherding spacecraft. But there was an optimum release point so that we were at a certain distance at impact from the spacecraft observation point of view, LRO was coming round, and had to be in an optimal place for its viewing at the same time. Meanwhile HST was literally swinging around the Earth and catching it just at the right time. Then all these Earth observatories, as the Earth was rotating and the Moon was coming into view, were observing all at the same time. That was literally coordinated down to the 1 second, 1 second, they were all coordinated, they were all successful, they all had views at the right place at the right time.

That was phenomenal. We had workshops with all the astronomers talking about what to observe, what to look for. We had one of our team members, Diane [H.] Wooden from NASA Ames, go out to Hawaii and work on the IRTF telescope [NASA Infrared Telescope Facility] with Paul [G.] Lucey who's a lunar scientist to help develop visual ways of finding the crater. Astronomers typically hate the Moon. It's bright. It ruins their observations of the sky. They didn't know how to actually point their telescopes and find this one crater at the south pole of the Moon to point at. There had to be a lot of observational cues as to where they'd put their instrument slit and field of view and all that. Paul Lucey knew the Moon inside and out. He worked with Diane Wooden who was an astronomer and they together developed over a course of several months this methodology for all the ground-based observers to find the right crater. That worked out really well.

One of the hardest things though to do was to deal with one, expectations of what the impact cloud would look like, and two, deal with the fact that while these telescopes were very powerful, their fields of view still would have bright lunar terrain in it and they were trying to look into a dark place for the ejecta cloud.

We tried many many times saying, "Look, the blackness is going to go from black to a little less black. You're not going to see a big bright flash or any ejecta cloud like that. For the public, unfortunately, public affairs and Northrop Grumman had released some very beautiful dramatic pictures of this explosion and this, that, and the other. That's what people were expecting, and of course never happened. Immediately following the impact there was a lot of disappointment in the community, public, that well, I didn't see anything. Even the team teased me. They got an eye chart. You know those eye charts that you read and it says, "Can you see the plume now?" All the way down. I said, "Look, it was there. Just you wait. It's in their data."

That was one of the other things that I thought was pretty novel that we did, was astronomers tend to take data and sometimes sit on the data. If it's not super pressing, it can just get lost. They sit on it. It sits on an archive. We actually said, "There's funding from Headquarters to support these observatories to observe." But we made one of the criteria for observing, of winning the money to make the observation, as you needed to archive your data in the Planetary Data System so others could get at it.

That was important, in retrospect really important, because papers continue to get published. One just got published last year using that ground-based observations of the ejecta cloud. That was important. Those results actually turned out to be very important in terms of understanding the total extent and potential depth profile of ice in the impact area. While they were not very fabulous to look at from a publicity standpoint, from a scientific standpoint they turned out to be really important to make.

JOHNSON: How quickly after the impact did you start getting the data?

COLAPRETE: From the ground observatories?

JOHNSON: From the ground observatories or LRO or the places where you were getting it.

COLAPRETE: We got the data from our shepherding spacecraft in real time. We could see the spectra and the thermal camera. We saw the ejecta and all that in real time. That's a fun aspect of it. It was real-time commanded and controlled. The actual observing period for LCROSS from the shepherding spacecraft was about 4 minutes, and that was it. But in that 4 minutes' time in the Science Ops [operations] Center [SOC] it was Jen Heldmann who was on the phone with all the observatories. We had a grid lined up, because we still weren't 100 percent certain where it would impact due to uncertainties. There could be a tip-off error or anything like that. As soon as we saw the impact flash, with our instruments we could, we'd call out the grid like a bingo card, B-10. I called that out to Jen and so all the astronomers who had the same bingo, B-10, they knew exactly where to put their field of view, if there needed to be adjustment. Nothing was needed. We hit center, so we didn't need to do that. There was that real-time aspect that was really fun. Actually we're bringing that to the VIPER mission, this real-time decision making.

Another real-time decision was as we got closer, after about 2.5 minutes after the impact, we had a couple cameras that were in the near-infrared, and we had an idea that we might be able to see the bottom of the crater, even though it's in permanent shadow due to scattered light, in the near-infrared. Kim [Kimberly A.] Ennico Smith was the instrument operator scientist in the SOC, and she was talking to command in the ops center for it. She and I were sitting next to each other, and we were both talking. I'm like, "I think we should change the gain, the settings on these cameras."

She's like, "Yes, you want to do that?"

I'm like, "Yes, why not?"

She's, "Okay." She called in to command. The Moon is getting real—it was coming, coming in. She's calling in. She's like, "Command. Change near-IR [infrared] camera settings. OP one five."

Command comes back, "Copy. Changing. MIR." We had a mid-infrared and a near-infrared. He heard M.

She's like, "No. Near-infrared."

He's like, "Copy. MIR."

She's like, "No. November. November." She starts yelling, "November. November." Because the Moon is getting close.

He's like, "Copy copy November. Change it." We did that and it went saturated. Then all of a sudden, the saturation bled away and we could see the floor of the crater we just impacted into. We saw that in real time, and she and I were just like, "Uh." To this day those are the only images still of the bottom of that crater. They've imaged other PSRs [permanently shadowed regions] using similar techniques. But Cabeus is so dark that we were the only—only from our vantage point could that be done.

Then it impacts. We had a press conference one hour after impact at 4:00 a.m. in the morning to tell everybody what we saw. That's the bad thing about an impact mission. There's a sudden end and everyone wants to know what you saw. We televised all the camera data in real time out to the world. Again this was a very important—we did this during an early flyby of the Moon. This was very, again, important to me to be totally open. Headquarters was very squeamish about it. Management was squeamish about it. What if one camera fails? Then it fails. I'm going to tell them after the fact it failed too.

But that was cool. At the time it was the third most watched streamed event on the Internet of all time, like 5 million plus viewers or something like that. I forget exactly. In that press conference one hour later, we were scrambling to get our data together and get it summarized in some charts. Jen Heldmann was working with all the astronomy teams. They literally were giving her initial impressions and initial data in real time there too. Yes, we saw the flash. No, we didn't see the flash. Yes, we were on target. Just some simple stuff. Yes, we were on target and everything worked nominally. We were reporting out initial data one hour after the impact. Basically what we could say is yes, everything worked like it should. We impacted the Moon. We did see hot ejecta. We got our spectra.

I remember in that press conference one reporter in the way back said, "Well, you showed some spectra up there. You said to yourself, 'There's some wiggles in the spectra.'"

I said, "Yes."

She asked, "Do those wiggles mean you saw something?"

I'm like, "Something causes wiggles."

She's like, "Aha." Everyone is like, "Ooh." After that we all went to get some sleep. That was 4:00 a.m. to 5:00 a.m. We reconvened the entire science team and some of the ops team at 8:30 in the morning the next day so like 3 hours of sleep or something like that. We're still poring through the data. Immediately we started seeing things like emission lines from sodium. Emission lines from potassium. This looks like a hydroxyl emission. This is an absorption. We all kind of looked around and said, "Yes. We've got some really good data here. This is going to be fantastic."

The astronomy teams were coming in and saying, "Well, we're not seeing any obvious ejecta cloud," because the problem is the Moon. We're trying to do these long integration times to see really faint scatter from the dust cloud from the ejecta. But the rest of the Moon is so bright that a lot of times our dynamic range was insufficient to pick it up. That was kind of disappointing. But about three or four months later, Nancy Chanover, she's at University of New Mexico, had a grad student who was working on something totally unrelated, but what he was using was a principal component analysis that's able to pull out of images all the various components that go into making that image the image. He was doing it for some astronomy effort. He thought, "Ah, maybe I'll apply it to the LCROSS data that her team took." She took some data from their observatory.

Lo and behold, there was the ejecta cloud. Very clear. He was able to pull it out. Since then several papers have been written. They've seen that ejecta. Using that same technique they've got data from four or five different ground observatories. We got the data very quickly from all the observatories. HST also saw OH [Hydroxide] and iron signals and things like that, so we got that data too out and published within a year or so after the impact.

There was a lot of pressure on us as a team to make a public announcement about whether or not we discovered water, even congressional pressure being applied at the Administrator level as to what did LCROSS see, what didn't it see, was it a complete failure, we haven't heard anything. They told them, "The scientists have to do the science." But still there was a lot of pressure being put on the entire team.

We knew that we had to make some kind of a statement soon even if it was just that we're still working on it. About a month after impact I got the entire science team together and the LRO team. Oh. I should say that same morning that we got together after impact I did hear from one of the LRO PIs. He's like, "Oh my God. Yes, we saw the plume and we got spectra. We got good data." I heard from the thermal instrument PI. Dave Paige was like, "Oh, we see the hot ejecta for four orbits after the impact." We knew they got good data, we learned that the day after the impact, so really quickly. But then one month later we got the team together, and we'd been of course communicating and talking this entire time and doing the analysis. I established a rule with the team that we would not go public with any announcement prior to any published paper unless there were two independent methods of verifying that we saw water, because there could be water and we only have one method, we're going to wait for the peer-reviewed publication, I'm not going to take a chance and make a mistake.

Everyone agreed to that. In this meeting we were all presenting our summary of findings. All the LRO instrument PIs and then our instrument scientists and team members. It was all looking very positive. We did indeed have two independent indications of water. Sitting next to me in the meeting was our executive officer from Headquarters, one of our executive officers, and she was constantly looking at me like "Are you going to announce?" I said to the team, "All right, I want to take a vote here, are we confident enough to make an announcement and go public with this and really go against the tradition of waiting for peer-reviewed publication and then making an announcement?"

I asked for a vote and it was unanimous, they all said, "Yes, let's go public." All the LRO PIs did, all the entire team said, "Public. Public." Our Headquarters exec just got this big smile on her face and was thrilled. At that point though I actually called up the editor for the publication *Science*, and I said, "We've got a lot of pressure to go public with this. I talked to the entire team and they're satisfied just one month after impact looking at the data that we can go public and we can show definitive evidence for what our conclusions are with regards to water at least. But we plan on submitting papers to your journal."

She was totally understanding. She's like, "I want to know too, so fine. That's totally fine. This is not unprecedented. Totally fine. You can announce prior to submission even, prior to publication."

Like wow, okay, great! So then we put together the findings and did the press release and the press conference and said, "Yes, we indeed found water." Then the *Science* papers were published about one year later or nine months later. Then we wrote it. After that we discovered that we really saw all kinds of other things besides water.

JOHNSON: Because it was water on the Moon there was a lot of interest. Like you said, so many people were watching the impact. There's a lot of public interest that NASA doesn't always generate. It's not like it was back in the Apollo days. But talk about that. This mission had a little more public interest in it. Talk about having to deal with that press, and if you'd ever done that before. Or was this new to you?

COLAPRETE: I'd done a little bit of press work even in college when we would fly a student payload on the Space Shuttle that I was the PI for. But nothing like this. A number of us had media training, which was invaluable. That really helped definitely prepare a lot. There was a lot of work being done trying to manage expectations. That was hard. In the end we didn't do the best job of it. That's in retrospect a lesson learned that you got to get out ahead of that because if you don't it's almost impossible to pull back and correct expectations. That's in particular the size of the impact. You're going to look at the Moon and it's going to explode. I did get a lot of funny emails and I did save a lot of those emails from astrologers saying I was going to damage the whole zodiac. One woman said she was a druid, would visit me in my dreams as a panther. It was a big enough deal to get those kinds of interesting feedback.

The press release at 4:00 a.m., there was a lot, a lot of pressure. I remember a lot of media. First time I ever felt like there were paparazzi for us was at that 4:00 a.m. I had to get ushered out by one of our staff, because the media were chasing after us as we were leaving the conference room. It was pretty insane. It was. That's great. Not that I got the attention, but NASA got the attention and what we were doing got the attention. That was one of the reasons why I wanted the video to be put out there. I wanted people to experience it in the exact same way we were experiencing it. When I'm looking at those images that we're getting from our instruments, you are too. That's the engagement that I was seeking for sure, and it worked well, it really worked well.

After the fact, people were unhappy that they didn't see the plume, and I got some pretty negative comments back from some people. I remember shortly after the press release, I went to the yearly Lunar Exploration Analysis Working Group—this is one of the subject matter analysis groups that NASA sponsors, this one is for the Moon—and was presenting results, and there was questions and answers after that. One particular person, a professor in Florida, said, "Do you think you need to apologize for getting everybody whipped up in a flurry about watching the impact from Earth and then not seeing anything like that?" He was very upset about that. I explained to him that first and foremost it wasn't about creating a publicity event. It was about actually getting the science, doing the science. No, it didn't bother me if that didn't work out, because that was part of our learning. We went in not knowing exactly what we were going to see. It wasn't about just generating publicity.

Everyone appreciated that. But that was again a lesson learned for me, you have to be very careful in how you set expectations within the public but also within the professional community. I think from that point of view there was disappointment both public and within the professional groups.

I don't think that lasted long though. After the results came out and the papers were published, everyone really, I think, appreciated the significance of the finding, still appreciate the way it was portrayed and how we communicated it as openly as we could. Now 13 or so years later, I still get compliments from everybody, community and professionally, about how great that was. We're trying to infuse very similar approaches within VIPER so that we can again have people—it's also a real-time mission, just like LCROSS was. Four minutes and we're going in, so we can really pull people in and basically experience it the same way we're going to be experiencing it as best we can.

One of my favorite also fun things out of it was I got a lot of of course the UFO [unidentified flying object] conspiracists talking to me. There was one picture from the SOC of an OCOMM [Office of Communications] release. It's Kimberly Ennico Smith and me. She's pointing at the screen and I'm looking down. Under my hand is a picture of the crater with the bingo grid on it. It's a fuzzy picture and you can't really tell what it is. But the center grid kind of looks like a blocky building, and that immediately went conspiratorial that we were targeting an alien base on the Moon and that's what it was all about. At one point though it got bad enough that I got a call from Office of Communications, Ames, and Headquarters saying, "Yes. We've been monitoring this and it's starting to get way too hot, like 50,000 hits this article. Can you explain to us what this image is and what's going on?"

I actually had the same picture that was sitting on the desk on my computer still. "It's this. It's just a bad picture and they're looking at it at an angle." They're like, "Could you write something up on that?"

I'm like, "Sure." Then I did, and then I got an email from somebody who monitors these conspiracy pages.

He's like, "I monitor these groups. I'm not one of them. But I was wondering if you had an official statement that I could actually pass on to these groups to try to counter some of this."

I sent that to Headquarters. They said, "Oh yes, no, this guy is legit. Okay. Yes. Prepare something." I sent it to him, he posted it. It included a letter, my email response to him. I signed it, *Tony*. It all got posted on this page where some of this activity was. The guy running the page says, "Oh, sure, we're supposed to believe that some big-time scientist at NASA is just going to sign a letter, *Tony*."

Unfortunately though then my wife was monitoring it. She wrote on the chat, "Yes. He's a nice guy. He's not pretentious. Leave him alone."

I'm like, "No. Don't. Wave off. Don't get involved." But finally that died down. That was about probably six years ago. It was funny. It was interesting.

JOHNSON: You can't control what people are going to do, that's for sure. You can't usually tell them they're wrong.

COLAPRETE: No. I wanted to. But OCOMM said, "No. This is one where we should at least put something out there so we're not totally silent so it doesn't totally run away."

JOHNSON: Who was it that said, "I can give you the facts, I can explain it to you, but I can't make you understand it"?

COLAPRETE: Exactly. You can only go so far. It's like bringing a horse to water. You can bring him to the water but you can't make him drink.

JOHNSON: I read in an interview where you said, "I think we have as many questions as we started out with, which is always a good indication that you're doing the right science. More questions than answers." You made a statement a few minutes ago that by the time you wrote those papers you'd learned even more. Talk about that, how these kinds of missions not only answer the original questions, but they can create more questions, follow-alongs.

COLAPRETE: Yes, that's so true of science, the universe is as big, beautiful place. We're just scratching at what we can see and perceive in our limited capacity. Every time we do reveal something it often is unexpected. That's definitely the case with LCROSS. I like to say we went into LCROSS with our eyes wide open. The set of instruments we had covered a broad range of possibilities. There was some argument within the team to say, "No, we really just need to look in this one little part of the world here but do it really really well." In the end we decided no, we know we don't know a lot, and so let's go in with a much broader view. I think that was the right thing to do in that each one of those observations that are different enough from each other, one, they help reinforce each other, but also, they revealed things we had totally not expected. I think proof of that is there's still science papers being written about the LCROSS results. I'm working with a team now analyzing some of the data that hadn't been analyzed yet in a way that's revealing new things.

I mentioned the ground-based observations. They're still being analyzed and revealing new things. At the same time there's just recently two papers that came out using the LCROSS results that suggested two totally different origins for the water on the Moon. It's awesome, this is what science is. I looked at it from this perspective and I got this result, I looked at it from this perspective and got this result. Just shows we need to study things even more. We need to take the next steps. But at least now we know the kinds of questions we need to be asking, when before we didn't even know the kinds of questions.

It's always a good sign when there's more avenues to go down than there were when you initially started. LCROSS, it was what is that hydrogen? Now it's everything from what's the origin of the hydrogen, the organics, the sulfur compounds, what's the burial depth, what is this, that, the other, distributed evenly or not? Basically it branched out into a thousand questions. The LCROSS experiment, as we like to call it, contributed enough input to create a branch if you will in that question tree while also answering some of the fundamental questions down near the trunk, like what is the hydrogen. It's at least in part water. But it's also this, this, and this maybe. That's one of the branches that comes off of it. Yes. It's fun.

JOHNSON: As technology develops can that help, going back and looking at that data differently as the technology that we have develops over the years? Is that going to help find more information, do you think?

COLAPRETE: Yes. Absolutely. Like I mentioned this PCA [principal component analysis] of the ground-based observations helped. That was an application of a technique that no one had thought about doing. That's basically revealed things we didn't see before. Some of the more recent papers are applying new models of impact clouds and how ejecta clouds will evolve in space and using that then to interpret the data we had. It's one thing to say, "Oh, we saw hydroxyl." But it's another thing to have a model that explains why you saw what you saw. That's where we're at with a lot of this data. A lot of those models are state-of-the-art, require massive computational power. They're modeling hundreds of millions of particles, some of them. In some instances we're just looking at the data differently. Again we had nine instruments, so there's quite a few instruments on the shepherding spacecraft, and you can slice and dice that a number of different ways. Sometimes it's just taking a step back and looking at it from a slightly different perspective. I'm always impressed that 4 minutes of data continues 13 years later to be mined for results, and they're results that are being published. It's not just an insignificant little interesting tidbit. This is big enough to be written up in a paper peer-reviewed journal and published. As new techniques come along and new separate observations come along, raise questions that people are going back and looking at the LCROSS data.

For example there's a paper recently published in it was *Nature* I believe on the origin of the water on the Moon being cometary. They looked at LCROSS data in comparison to other isotopic data from the Moon and comets, so they were looking at Rosetta [European deep space probe] data, cometary data, and new lunar lab data in terms of isotope data from samples from Apollo, and then looking at LCROSS. It's not just even new technologies. It's actually new observations or new data simulation that makes you come back to the LCROSS data and ask a question differently, or it allows you to approach the data differently and make a different conclusion. That's definitely ongoing.

JOHNSON: Hopefully for many years. I appreciate you talking to me today.

COLAPRETE: My pleasure.

[End of interview]