NASA Discovery and New Frontiers Oral History Project
Edited Oral History Transcript

David J. Lawrence
Interviewed by Sandra Johnson
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Johnson: Today is March 22nd, 2024. This interview with David Lawrence is being conducted for the Discovery and New Frontiers Oral History Project. The interviewer is Sandra Johnson. Dr. Lawrence is in Maryland and talking to me today over Microsoft Teams. Thanks again for joining me today to do another interview. When we were talking last time, we talked about a couple of missions. We discussed MESSENGER [Mercury Surface, Space Environment, Geochemistry, and Ranging] and Psyche. But during that interview you mentioned that your instrument team proposed to be a part of LRO [Lunar Reconnaissance Orbiter] but you weren't selected and that there were some interesting things going on that you might want to talk about it later. I thought we'd start today and maybe talk about that LRO proposal.

Lawrence: Okay, yes. That was during my time at Los Alamos [National Laboratory, New Mexico], and we were coming off of the Lunar Prospector mission where we had made the measurement of enhanced hydrogen abundance at the poles of the Moon, and there was a big push on the Lunar Reconnaissance Orbiter mission to really expand on those measurements.

One of the big goals was how do you gain better spatial resolution of the hydrogen at the poles of the Moon, because from the Lunar Prospector data, the spatial resolution was sufficiently broad that you couldn't isolate the hydrogen to any particularly permanently shaded crater. If you want to move forward both scientifically and exploration-wise, you need to understand if there is water at the poles of the Moon? How much? Where is it? This was a key measurement to get better spatial resolution.

At the time, we had the idea to try to improve your spatial resolution by putting a collimator on the neutron detector. The basic idea behind that is instead of having your neutron detector look at neutrons from all angles coming up, you use the idea of putting a soda straw on your detector so that you can just see in a very narrow angle where the neutrons come from. Then, in principle, this should give you information about the spatial location of where the neutrons come from, and you can narrow it down sufficiently so you have better spatial resolution.

We worked on that problem for quite a while. We had some ideas, and the Lunar Reconnaissance Orbiter, or LRO, mission was coming up and NASA was going to put out an announcement of opportunity to get instruments. We did a lot of work at Los Alamos on how would you make a collimated instrument. One of the big challenges it turns out is when you restrict the angle of neutrons coming in, you restrict the number of neutrons you get and your statistics go down. You might have good count rate when you have a wide-angle detector, but when you restrict it your count rate goes down a whole lot, and you might not have enough statistics to do your measurement.

Another problem is whatever collimator you build is never going to be perfect. Ideally you want the collimator to block the neutrons from outside that angle you're looking at. But neutrons, they're kind of squirrely creatures, they can get through anything, and so you can never build a perfect collimator. We were trading off that idea. You also need a much bigger detector because your count rate is smaller, so you need more counts. Your collimator is a lot of mass. Lots of mass and lots of area are not necessarily compatible with spaceflight instruments.

We worked on a proposal for some time, and we had what we thought was going to be a reasonable instrument. We put a proposal in for the LRO instrument competition, and we included a neutron detector that was a collimated instrument, and also included a gamma-ray detector from APL [Johns Hopkins University Applied Physics Laboratory]. We had worked on MESSENGER as I mentioned before, and there was a MESSENGER flight spare, and so we included that. That would be an additional measurement that maybe can get good information about the poles.

We put in our proposal. I was principal investigator for the proposal. That was the first time I was PI for an instrument proposal. When you do these things, you don't know what your competitors are. But other organizations of course can put in proposals as well.

When the announcement came out for the selected instruments, and I think it was sometime in the timeframe of [December] 2004, it turned out we were not selected. Part of the game when you do NASA instrument work is you write a lot of losing proposals, and that's just the name of the game. If you can get a small fraction of what you propose, you're doing really well.

On one hand it's not unexpected. On the other hand, it stings when you've put a lot of effort into it. What happened was it turned out they did select a collimated neutron detector, but it was a neutron detector that was provided by the Russian Space Agency [The Lunar Exploration Neutron Detector, LEND]. We were familiar with this group because they also had a neutron detector on Mars Odyssey. The one on Mars Odyssey was very similar to the neutron detector that was flown by Los Alamos, and it was always an odd thing about why there would be two neutron detectors doing pretty much the same thing on Mars Odyssey. My understanding—and I don't want to get too much into this here, because it's more secondhand information, but that it was not a solely technical reason for choosing the second neutron detector on Mars Odyssey. But there it was. It was part of it.

We also had history with a possible neutron detector on the Mars Science Lab. That's the Mars rover. We had instrument development funding from NASA to do a small neutron detector on a possible Mars rover, which is funding to develop the idea, and so I had such a grant. One of my colleagues, Rick [Richard C.] Elphic at Los Alamos, had another instrument development grant for a higher technology level neutron detector, in preparation for possible announcement of opportunity for instruments.

As that one got going, we were told by people at NASA Headquarters that if we wanted to propose a neutron detector for the Mars rover don't bother, because they are taking a neutron detector that will be provided by the Russians—the same team that was selected for LRO, and the same team that had the neutron detector on Mars Odyssey. That was a bit disconcerting given that they invested in us and then they came back and said, "But don't bother, we've already preselected." We proposed for Mars instrument anyways knowing we weren't going to get selected. We weren't selected. The Russian instrument did fly, that's the DAN [Dynamic Albedo of Neutrons] instrument that's on MSL [Mars Science Laboratory].

Back to LRO. When we heard of the selections, again we were disappointed, and then you start to be curious. Okay, what was it? This must be a really good instrument. What's it all about? Soon afterwards, there was the 2005 Lunar and Planetary Science Conference, they have two-page abstracts that come out, and so an abstract came out that described the Russian instrument. When I read the two-page abstract there was enough detail in there that I could pretty much figure out what this instrument was about, because I had studied these types of techniques for a long time.

It became immediately clear to me that what they proposed was not going to work. They were promising—I'm just going to throw numbers out there—well, let me put the numbers that we promised. We promised that we were going to be able to measure 1 weight-percent water equivalent hydrogen for a spatial resolution of 10 kilometers. We had a detector that was about 900 square centimeters to accomplish that measurement. Big detector.

The Russian instrument was promising spatial resolution of 5 kilometers, a factor of 2 better, and a hydrogen sensitivity of 100 parts per million hydrogen, which is at least a factor of 10 better sensitivity, and their detector area was 70 square centimeters, more than an order of magnitude smaller. The physics doesn't work.

I immediately got concerned. How did this happen? Turned out when I was at Los Alamos, we had some of our program management there that were supporting NASA programs, and they were concerned about this. The Mars instrument was preselected; here we are having another program where a contributed instrument is selected. What's going on?

Typically you can't go and protest NASA decisions. That's not the way it works. But we were trying to figure out how can we at least question it and see what happened. As we huddled together with our management, one thing worth questioning is what was wrong with our calculations that made us think we need such a big detector to get poorer performance?

The boss of my boss knew one of the high-level associate administrators at NASA at the time, and so we arranged to send a note to that person at NASA to say, "We understand it was selected, we can't argue with that, but we would like to understand how the selection was done, because this will question our own way of determining how these instruments work. If we have big problems in how our calculations work and how our sensitivity calculations are figured out, we need to understand this not only for our NASA work but for national security work." Because at Los Alamos the work that we did there applied not only to basic science but national security problems.

We got a little bit of traction and there were some conversations back and forth. There was some willingness from NASA to at least have a discussion. Then at the time I think the NASA Administrator changed. This was when Mike [Michael D.] Griffin came on board. One of the managers at Los Alamos actually knew Mike Griffin personally and so was able to continue the conversation in a little more detailed way.

Over the next couple months, and I don't know how this really happened, but NASA ended up convening a review board to investigate this. The review board had two members. One was Glenn [F.] Knoll. Glenn Knoll is a world's expert in nuclear spectroscopy detectors. Then the other person was a professor at MIT [Massachusetts Institute of Technology, Cambridge] named Dick [Richard C.] Lanza. Basically, people at NASA appointed them to review what's going on. When Los Alamos says this instrument is not going to work, are they correct? Or are they wrong? Because if it's not going to work as planned, then that's something NASA probably should know.

In the course of this investigation, Glenn Knoll and Dick Lanza traveled out to Los Alamos and talked to us and had a big, long interview. Glenn is great. He has a very famous textbook for this. He's a physicist's physicist. He knows how to ask all the probing questions and find all the problems. We had worked up our whole analyses about why the Russian detector wasn't going to work and had a lot of questions, found some problems there. We fixed them. This happened over the course of I think summer of 2005. I am sure he went and interviewed the Russians and other people from the U.S. that they were collaborating with. During the course of our last meeting with him he mentioned: "I don't see any major problems in your analysis."

Okay. Let it lie for a while. Then we go to the fall of 2005 and I had another colleague in NASA community. Right now I'll just say the name, Paul [D.] Spudis. He died a few years ago, so no one can come back and give him a hard time. He let me know. He was involved in LRO and he let me know that there was a memorandum of understanding that was signed between NASA and the Russians in Moscow sometime in the fall of 2005 that basically stated, "Everything is fine with the instrument. It's going to go forward."

When he heard that, and I can't remember all the detailed version of events, he went back to the organization that was overseeing the review and said, "What's up? Is there any report?" I think it took a little bit of effort but finally we got a copy of a probably 10-, 15-page report. It was a NASA person that wrote it. This was at the time an organization out of NASA Langley [Research Center, Hampton, Virginia] who pulled together the report using Glenn Knoll and Dick Lanza as subject matter experts. This other NASA employee wrote the report. We finally got a copy. We read it and studied it and found its conclusion was the Russian instrument is going to work but when you read the report it was riddled with errors.

We went through and did a detailed study of that report. One of my managers at Los Alamos, Herb [Herbert O.] Funsten at the time—he's still there—was able to arrange a meeting with the head of the NASA review organization. We were actually able to arrange a meeting out at NASA Langley with the head of that review organization. Glenn Knoll attended the review as well as the person that wrote the report, just to go over the report.

We went into this meeting and it ended up being a 3-hour meeting. Long meeting. The gentleman, the head of this review organization, he started the whole thing out with a caveat that said, "We're having this meeting as a courtesy to you but just so you know, this is not normal, and nothing will change in our decision regardless of what we talk about."

I had put together with my colleagues at Los Alamos a long package of information about the report, about our study, and we went through it page by page with the author of the report, with Glenn Knoll, and as we continued through that discussion, we came to the conclusion that the report technically was not correct. The big saving grace for them was the requirements that were originally written for the mission were unclear, so there really weren't strong requirements to be measured against. So when you say it doesn't work, by what standard? There wasn't a standard.

Later on I went and talked to the author of the report. He was a very kind person and I said, "You have problems here." I never saw if that was ever corrected or not, but that's where it lay. Very discouraging. But this is the way the world works sometimes unfortunately. That's chapter one.

JOHNSON: Did it work? What happened once LRO flew?

LAWRENCE: That's chapter two. In the meantime, I think that was all of 2005. We had that meeting with the review folks somewhere probably early 2006. In the meantime, I ended up leaving Los Alamos and came to APL. I think LRO was launched in 2009. That was while I was at APL.

One interesting thing—this was before LRO actually launched—APL had, and they still have it, an interesting internal funding program called a Janney Fellowship, where if you have done a lot of work for something but you don't have funding to actually write a paper, they will provide a little funding to finish up the paper. I had done all this work on this collimated neutron instrument, and it was more than just that the Russian detector won't work, it's a lot of the theory behind this idea.

I wrote an application to this Janney Fellowship to turn all that work into a paper. I got funding to do that. Part of the reason for doing this was I had been asked to be a reviewer for a journal called *Astrobiology*. For some reason *Astrobiology* was doing a special issue on instrument papers. Don't know why. But one of the papers I got asked to review was a paper about the Russian LRO neutron instrument.

I know this gets a little tricky by being named as a reviewer. At the time, I did not give my name out. Sometimes when you review a paper you put your name out. Sometimes not. This was many years ago. But I'm saying that right now.

I reviewed the paper for *Astrobiology* and found a lot of problems with it because I'm pretty well-versed on it. The way I gave my review is I didn't immediately reject it but said, "Here are problems. You need to fix these problems before the paper can be published." I think the review went a couple rounds and, in the end, it was published over my objections in this journal. I objected to the editor—it was a guest editor at the time—and said, "I have a problem with this."

The editor, she came back and said, "Well, if you want to publish a rebuttal paper in the same journal, feel free to do so." I had the funding to do that, I ended up writing a paper about how it in my estimation didn't work. It was basically the entire set of the body of work that we presented to NASA a couple years prior.

After LRO was launched and they ended up putting their initial data out, I saw some of the results, was able to look at the data, and immediately realized it's not working the way they planned. The maps of the poles were mostly noise. We were able to get some data and I was able to analyze it. There were new things about the instrument that I hadn't anticipated. But when you get the data, you can see oh, it's operating this way, it was actually measuring fast neutrons more than anyone had anticipated. You see evidence of that by what the maps look like at the nonpolar portions of the Moon.

I had the data, and it turned out there was a lunar meeting coming up. I wrote an abstract and said, "Here's my estimation, what I can tell from the data, and it's doing exactly what we thought it was going to do as far as collimating neutrons."

When I put the abstract in, we ended up getting contacted by the LRO project scientist. Not very happy with us, and came and talked to us here at APL and said, "Why did you publish that without talking to the authors, the people first?"

My response is it's public data, I don't need to, there's no requirement that you have to do that. It became a public thing. But one interesting output from all of this was the LRO project scientist wanted to try to broker some sort of negotiation or peace. Mike [Michael] Wargo became involved. He's no longer with us. But he was trying to broker some negotiation.

A lot of that ended up in a very fascinating meeting at NASA Ames [Research Center, Moffett Field, California] at one of the Lunar Science Forums. This was now the summer of 2010, and by this time I wasn't the only one involved. I had a number of my colleagues who knew what the situation was and were helping out to do analysis, and we had about five or six of us in a room with the PI of the Russian instrument, Igor Mitrofanov, and a couple of others from his team, to try to talk about what was going on with the instrument. That was one of the more bizarre meetings I've had in my career, where we were really trying to take the tack, honestly, of "This is our analysis of the data, we may not have it all right, if there's things we got wrong, please let us know, and let the data tell us where things are."

As part of the meeting, the Russian PI lectured us to say how dare we go public with this information. The proper appropriate scientific way, he said, is to talk to people privately. Because if we make it public it will be apparent that this technique doesn't work and it hurts everyone. So be quiet and don't talk publicly about this. Yes. Not a whole lot came out of that. I learned a little more information about how the instrument worked and it had bigger problems than we realized. Even in terms of the electronics where the neutron peaks showed up where you didn't see it and you should have seen it, just technical details that were highly problematic.

That meeting, it didn't really broker any peace as some of the folks were hoping. Fast-forward to a paper, and this is another thing where it gets tricky. I was asked to review the first paper that was being put out by the instrument in the journal *Science*. It's a high-profile paper. I already had experience reviewing LEND papers before. I had another colleague that had reviewed one of their instrument papers, again pointing out problems. The problems weren't addressed, and the paper was published.

For this review—and this is the first time I've gone public that I'm the reviewer, some people know—it went two rounds for my involvement. As I was doing before, I pointed out there are problems. I did not immediately reject it, but these are errors that need to be corrected and additional information that needed to be provided.

The response came back. There were minor corrections but not dealing with the main issues. I sent back another comment saying, "You need to make these corrections and provide relevant information before the paper could be published." I never heard back. But what then happened was a few months later I saw that the paper was published under a different title. They published over the reviewer's objections.

I complained to the editor. The editor did allow us to publish a rebuttal in *Science*. So, there's a paper that I and some colleagues published in *Science* that's a rebuttal. Then of course when that happens, *Science* allows the original people to have their own response to the rebuttal, so that happened.

One of the things that's in their published *Science* paper, the final version that I did not have an opportunity to review, is they have a problematic figure. They have supplementary material which is not the main body of the paper and where they put additional details in. They put a plot in that supplementary section where they show what the response of the detector is as a function of angle going into the detector. One of the figures that I had in my *Astrobiology* paper is when you have wide-angle neutrons coming in, the collimator is not working. You have a lot of response. The collimator is kind of semitransparent; a lot of response at wide angles. That's the main reason why the detector doesn't work; because you have so much background coming in from the outside that the little bit of neutrons you get looking through the soda straw is just not enough to overcome the background.

That was one of the main figures in my *Astrobiology* paper saying why it didn't work. They had a figure in the supplementary material where they show the response of the detector as a function of angle, but they only show the very narrow angles as you're looking through the soda straw. They don't show anything at the wide-angle responses. It turns out I had also received copies of information from my colleague Paul Spudis that showed similar figures that were presented at LRO team meetings that showed the response at wide angles coming into the detector.

It turns out that one of the figures that I'd seen that was presented in this gray literature from the team meeting was nearly identical to the one that was in *Science* magazine. They were showing in their original figure that they agreed with our results that the response at the wide angles was indeed semitransparent. But for the figure that appeared in *Science* magazine, the wide angles were whited out. You can look at it and it's the identical figure as presented in the team meeting but with the wide-angle portion whited out.

JOHNSON: It's amazing.

LAWRENCE: That figure appeared in the paper after I had reviewed the paper, so I had never had an opportunity to see that version of the figure. That identical figure was also published in *Space Science Reviews*. In a similarly whited out fashion. That in itself tells you, tells me, problems.

JOHNSON: It makes you wonder what the reason behind it all, what those reasons are that they had to choose that team.

LAWRENCE: Yes. One can speculate. I'm not going to do this here. That's more of a conversation over a beer. There's more chapters to this by the way.

JOHNSON: Go ahead.

LAWRENCE: Actually in hindsight—at the time it's very upsetting when you're younger and more idealistic to see this stuff happening. It is. It's very upsetting that this occurs. But I recognize all human beings are flawed. All of us. I'll just say that I am sure I did not act perfectly during all of this. Probably if I look back, if I had a tape recording of things I said during some of those times I would cringe. That's who we are as humans. Not claiming any wonderful things. It's just sometimes things are presented in your lap and you have to take the step forward. Again that reality about the doctored figure; I haven't gone public with that, although enough people know that it's out there. It still bugs me. I really don't know fully what to do about it other than the situation will play out as it plays out.

Over time I continued to be asked to review papers. I had other colleagues that knew of this and started publishing papers about the LEND data as well. There's now a whole literature. There's anywhere from five to six to seven papers that discuss the LEND data that all describe it from different aspects about why it doesn't really do what the LEND team claimed it did.

I think one of the unfortunate results for the scientific community is there's a lot of nuances in this, and they don't know the details. They just look at it like it's a food fight going on. Clearly this technique probably doesn't work, and there's an appetite to want to ignore this issue. Nothing I can do about that except just try to do the right thing. Again there's a lot of papers out there that dispute it. As I mentioned, I was asked to review a lot of papers. The pattern continued to be the same. One is asked to review a paper, you make comments, you offer suggestions. For the most part they're ignored and the paper gets published.

There's one exception and this is another part of the story. It's kind of an adventure. As the LRO mission was moving forward, don't know how it happened, but there's a gentleman named Roald [Z.] Sagdeev. He is an older Russian scientist who is a nuclear scientist, very well known, rose very high in the Russian science community. He was involved in a lot of the nuclear treaty discussions during the [President Ronald] Reagan era and met with high-level national leaders during that time. It ended up after some of those years when he was working with [Mikhail] Gorbachev and Reagan, he moved to the United States. He's now a professor emeritus at University of Maryland. I think that might be one reason he was asked to join the LEND team, because he was in the U.S., very well-respected scientist, and if he could be on the team and actually show that things work for the instrument, that provides a lot of credence.

Turns out I heard that he was giving a talk about the LEND instrument. His talk was going through and showing how it worked. I looked at that, had my thoughts and comments, but you'd be very careful about going up against someone that's such a good physicist. I am not a good physicist. He is.

Interestingly he ended up writing a paper that was submitted and I got asked to review it. For this one, I reviewed it and I put my name to it. As part of my review—and I found problems with it—but I said as part of my review I would be more than willing to sit down and talk so that it's just not a back-and-forth on paper but let's actually have a dialogue and get to the bottom of it. Because if he was saying it worked, that really does make me question what I was doing.

One thing led to another and it turned out he had a second house out in Santa Monica, California. One of the reasons he was out there was he was undergoing some cancer treatments at UCLA [University of California, Los Angeles]. I happened to be out in California at JPL [NASA Jet Propulsion Laboratory, Pasadena] for a meeting for another project I was on, for the Dawn Project. It's all Discovery Program stuff. We're all linked to Discovery. This was when I was in the middle of this review, sometime in 2011. I was in email contact with him and he was willing to have discussions. He says, "Why don't you just come over and we'll spend an afternoon and talk about it at my house?"

I took an extra day and I drove down to Santa Monica from Pasadena and met him at his house. It's in a very posh neighborhood of Santa Monica. I met Sagdeev. This is a big deal. Sagdeev is a great gentleman. Very kind. So much appreciated him. It turns out he had another Russian scientist there that happened to live in San Jose who was working in Silicon Valley but originally Russian. This other scientist was helping him out on some calculations and I think doing a lot of the computer work. But Sagdeev is certainly the brains behind a lot of this.

Anyways, we sat and chatted for a number of hours, the three of us, in his kitchen in a rental house that he was at. We went through all this, and as we hashed out all this discussion—it was a great physics discussion—came to the conclusion that our original Los Alamos analysis was correct. There were still some things to discuss. Still some things to work out. We finished that meeting with action items on both sides that we would get back to each other on. I'll get to that in a bit. But there's an interesting interlude.

We were finishing up the meeting and Sagdeev said, "Do you want to stay for dinner? I'm going to walk over to my doctor's house who lives about a block away. I have my own rice pilaf that I made and I'm going to bring that for dinner." The other gentleman was going to go too. Sagdeev apparently, his ethnic origin is Tartar. I don't know all about that but he was saying, "The rice pilaf, that's one of our ethnic dishes and I'm bringing it over." It turns out his doctor is also Russian and this is a whole Russian community that was in Santa Monica.

JOHNSON: Interesting.

LAWRENCE: I'm thinking okay, that's fine, but I'll be honest, there's a small amount of trepidation of hearing stories about things that happen when you get too involved in some of the Russian communities. Putin was in power at the time. Just let it be there.

But everyone was very kind. We walked over and got to the doctor's house and there were already a number of people there. The doctor in particular wasn't there yet, but they were all Russians, I was the only American. The other scientist that I interacted with, he took me aside when we got in and he said, "Dave, feel free to enjoy yourself but for your protection, don't drink a lot of vodka."

Anyways, at one point the doctor finally arrived. He's this gregarious person and this crowd was fascinating because they started telling stories. I don't know if they had vodka yet but

I followed that advice very carefully. The doctor was talking about how he was on the phone a few days earlier talking to the doctors of [Dmitry] Medvedev's wife. At the time Medvedev was president of Russia. The doctor, whose house we were at, was a consultant to the wife of Medvedev. This is the crew I was hanging out with.

Then we went in and had dinner. The dinner was Sagdeev's rice pilaf, and then they ordered P.F. Chang's. A whole lot of P.F. Chang's. There was about 10 or 12 of us sitting around the table. There's a couple more people coming in and they had their dogs. It was a great community. All talking stories. As the evening wore on, Sagdeev and some of the others were telling their stories about when they were hanging out with Gorbachev and Reagan. Here I am thinking oh my goodness, what have I gotten myself into? The reason I was here was saying that the Russians were incorrect!

In the end we finished the evening. I walked back, got in my car, went to the hotel, was just reflecting. This was just an amazing thing. I've never seen these people again. Very thankful at some level for that, and again they were very kind. Nothing against them. But the interesting thing was as far as Sagdeev's paper goes, I heard later on that the paper was withdrawn. We did our action items. I sent the information. But quietly the paper was never published. This is the only time that that has happened in my reviews of LEND papers.

I have such a ton of respect for Sagdeev. He has lived through many many difficult things. Apparently, they had some symposium a couple years ago at University of Maryland where he did an interview with a newsperson, and you can see it online, and he tells some of the stories that he went through during his time in Russia and it's harrowing. I so honor and respect him. That's just a little bit of an adventure going through all this, having my interactions with him.

JOHNSON: Quite memorable I would imagine.

LAWRENCE: Yes. That paper was withdrawn. To this day there's been no official description of how things really work. I understand there is at least one other paper from some U.S. investigators on that instrument that were basically acknowledging it really didn't work as well as it should have. I don't know if that's been published. There's still people in the community that use those data. I've had many people now come up to me and say, "Yes, Dave, we think you all were right. We don't want to say it too loudly." That's kind of where it lies.

An unfortunate aspect to all of this is it has slowed down our ability to get at the answer of what's really going on at the poles.

JOHNSON: That's what I was going to ask you. If the data is not what it should be and people are still using it, the whole point of the instrument, and it's still there, LRO is still going.

LAWRENCE: Yes. One of the other things I realized after doing all this analysis that I found really interesting was the instrument that we proposed would not have worked either. The reason is there's still too many neutrons coming in from outside the collimator. But the background coming in from the outside is also itself sensitive to hydrogen variations in the same way as the collimated neutrons are. I'm convinced that would have made even our instrument have poor performance. But I hope we would have been more honest about it.

But that's hard. When you're in a position and you put so much effort into these instruments, to come and find out it didn't do what you thought it should have done. This is tough stuff. I understand it. Anyways, there's still efforts to make this measurement. How can we make measurements better? I'm convinced now that the only way to make this measurement is you take your normal neutron detector but instead of collimating it you just need to go very low altitude. You just need to get really close.

There's been moves afoot over the years to try to do something like that. NASA did a CubeSat mission that was going to possibly do a down payment on that. It didn't work for various propulsion reasons. But yes, I look back at it and I really don't regret any of this. I've learned a ton about human nature, about scientific integrity, about how you respond under pressure. I'm actually quite thankful for the experience. It's an adventure. Never could have planned it.

JOHNSON: Yes. All of our experiences we learn from, that's for sure. It was quite an experience.

LAWRENCE: Again this is something that people that know me, they know the story. But it's nothing I've really written down because it is sensitive. But now I think it can be important to tell it.

JOHNSON: Yes. It seems like it would be important to have it out there.

LAWRENCE: I have no desire personally to be in the forefront of that. I'm very thankful that there's a number of colleagues who have come alongside, have been able to see what's going on, and helped out. For me it's really about what's the truth, how do we do science well and with excellence. When we make mistakes, we own them.

JOHNSON: The world we live in as opposed to the world those Russian scientists live in, it's a little different.

LAWRENCE: It's hard. I don't want to minimize any of that. My father actually, he is a physicist, he's retired now, worked at Sandia [National Laboratories]. He ended up working on a program that was through [U.S.] Department of Energy funding Russian scientists after the fall of the Soviet Union so that they could continue good science work. He had some just wonderful experiences. But it is a different world. They have wholly different pressures on them that I can't even imagine. I don't want to stand in judgment of that.

One of the neat things about science is when it is done well it doesn't matter what our feelings are. It's outside of us and it's objective. We don't have to look at ourselves or look at each other. We can all look out at something else and see what's the answer, what's it telling us, and that's exciting.

JOHNSON: Yes. It's definitely exciting to get the results, especially when they're good results.

LAWRENCE: Yes. That's the story.

JOHNSON: You mentioned Dawn in there and that you were working toward that. If you want to go ahead and talk about that mission. That I think had a lot of ups and downs too, from the downselect in 2001 until it finally launched. But talk about that process and what your work was on that.

LAWRENCE: I was more of a side player on that but I did get involved. But aware of what was happening. The original instrument lead was my mentor back at Los Alamos, Bill [William C.] Feldman. What they were doing was trying to design an instrument that would be the next step from Lunar Prospector. But there was reasons you needed to do it in smaller resources, not as big mass.

The instrument was designed to be more compact. One of the key people that ended up being the instrument lead, Tom [Thomas H.] Prettyman, he worked with us also on Lunar Prospector. An extremely good planetary nuclear scientist. I've known him for many years. He had some new types of detectors they were trying to figure out for technology demonstration, cad[mium]-zinc-telluride detectors that were part of the Dawn instrument. As that instrument design and build moved forward, the instrument leadership got moved from Bill Feldman, who was late in his career, to Tom. It was more the two of them that had to live through all those ups and downs, cancellations, coming back, all of that kind of stuff.

In fact one of the things, I remember the stories—I wasn't a part of it, but I don't know if it was both Bill and Tom, it was at least Bill—they were out in Virginia for some key meeting or review on September 11th, 2001. We all know what happened then. They ended up driving home back to New Mexico because you couldn't get any flights. Of course that's very memorable. It was around that timeframe.

I was not deeply involved in the design of the Dawn instrument. But by the time I got to APL, they had a participating scientist call from NASA where you can write a proposal and become a formal science team member of the mission. I wrote a proposal for the Dawn at Vesta and I was selected, and so was very grateful for that and able to be a formal part of the team.

That was a very exciting time. Tom Prettyman at the time was playing the lead role as he should and came out with some really exciting results. I was able to help out on some of the analyses and also go to various meetings, so that's why I was out there at JPL for a Dawn meeting when I met with Sagdeev.

Then also there was a separate proposal call for the Dawn at Ceres that I also was able to be a part of. That was a good experience to be part of that. Again not as deeply involved. I wasn't really involved in the operations in the same way that I was for MESSENGER [Mercury Surface, Space Environment, Geochemistry, and Ranging]. That's the way these missions happen. You play different roles for different missions at different times.

JOHNSON: I know that after 9/11 I read that it delayed some of the work. There were a lot of starts and stops on that mission trying to get to launch.

LAWRENCE: It was a huge success and I've now learned every mission has these things. Saw it on Psyche. We're seeing it on MMX [Martian Moons eXploration]. We're part of the MEGANE [Mars-moon Exploration with GAmma rays and Neutrons] instrument. I don't remember how much I talked about that before.

JOHNSON: Go ahead if you want to talk about the MMX and MEGANE. That was a little different because it was a mission of opportunity. Maybe just talk about that and when you got involved in it and the background and the proposal.

LAWRENCE: Right. This is a mission by the Japanese, it's called Martian Moons eXploration or MMX. Their goal is to bring a sample back from Mars's moon Phobos. That's the larger of the moons. Phobos and Deimos are the two moons. As part of that mission in addition to the sample return, they want to do comprehensive remote sensing of Phobos because there's a lot of information that can be gained not only from getting the sample but from orbital reconnaissance. The main science goal of that mission is how did Phobos and Deimos form. Did they form as a giant impact, some big body hitting Mars? Or did they form as maybe a captured asteroid? That's the main goal.

It turns out one of the ways that you can help constrain that is measuring the composition. For instance if you had a big giant impact, you would think that the material would have gotten really hot and you would cook off a lot of the volatile elements like hydrogen or chlorine or potassium. If it was a captured asteroid then you might have higher abundance of volatiles. It's those kinds of things.

As part of that, the Japanese wanted to fly a gamma-ray/neutron spectrometer. This was a little interesting because Japan has their own group of people that have done gamma-ray spectrometers, even high purity germanium. But for whatever reason they latched on to the idea of the MESSENGER instrument. They saw what the MESSENGER instrument was. They saw its success. Apparently, they went to NASA and said, "Japan can't pay for all their instruments. Would NASA be willing to fund the development of a gamma-ray spectrometer?"

Apparently, NASA said, "Sure." I think part of it is when you work together on these missions NASA then would say, "Can we be in on a part of the sample?" When you have these international collaborations, things go both ways. For whatever reason when NASA came and decided to put out their announcement of opportunity somehow working with the Japanese they said, "Okay. A type of instrument would have these types of mass, power, performance constraints." It looked very similar to the MESSENGER instrument.

The way these competitions work is NASA then put out announcement of opportunity for anyone to propose an instrument. Because we did MESSENGER, and we had Psyche under our belt as well, we thought we'd be in a good position to propose, and we did. We proposed an instrument that was very similar to what we were planning for Psyche, with a lot of MESSENGER experience. My understanding is there were a number of other proposals submitted. Don't know the details. But however it worked out, we were selected—this was in 2017—to do a high purity germanium detector and a neutron spectrometer, I would say 70 percent similar to what we're doing on Psyche. There are some key differences that interestingly make all the difference in the world when you're doing an instrument build.

Just to say build-to-print are not easy words, because when anyone comes up to you and says, "Oh, we're just going to build-to-print what we did before," don't believe them. When you're doing one-off custom builds for planetary space missions, everything is a new build. Yes, you have similarities. That really did help us on MEGANE because a lot of the gamma-ray instrument, the germanium detector, the anticoincidence shield, the phototube, were pretty much identical. That gave us a big leg up.

The big difference is the cryocooler. We had to have a different cryocooler than we did for Psyche. Psyche we were able to have the mass and power, and money is important, to do a pulse tube cryocooler. This is one that's pretty much infinite life or should be very long life. For the Japanese spacecraft they just didn't have the resources for that, so we had to come back and use the version that we used for MESSENGER, which is a Ricor cryocooler. It has a more limited life. But also on the spacecraft we couldn't be on a boom. There is, depending on how you count it, anywhere from 10 to 14 instruments on the spacecraft. The Japanese now are working with the Europeans and they have a small rover on it. There's just a lot of components on the spacecraft. It's the resource constraints that we have in a way that we didn't have for Psyche that really have caused a lot of the challenges for the build for MEGANE.

However it worked out, we actually now have a delivered instrument in Japan.

JOHNSON: I saw it announced just recently, right? Just a few days ago.

LAWRENCE: Yes. There was a nice release that came out. One of the challenges for MEGANE, as we did the build, we had more than our share of anomalies. I think some number of them are just due to different things we've had to do compared to what we did for Psyche. Even simple things. For Psyche, we had all the gamma-ray spectrometer on a single mounting plate. But for mass reasons we couldn't do that for the Japanese mission. We had a number of boxes on their own, and that has implications in terms of grounding and things like that.

We have a delivered instrument, and it works. It's not as perfect as we would like. This is just a watch item as we move forward. But also the launch of the mission was supposed to be 2024 and the Japanese announced in December that it was delayed by two years to 2026. The main reason was the Japanese H3 rocket, which the mission was going to go on, it wasn't ready. They had an initial launch back in 2023 that failed. But now a few months ago they had a successful launch of the H3, so that's on track. I think there is some appreciation to have a little extra time for the rest of the mission to get everything going. That's a live thing right now as we're talking.

That whole experience has been an interesting adventure in its own right. Working with an international mission is an interesting experience. We definitely have cultural differences between Japan and the United States and we're working through that. Everyone's very kind, we very much appreciate it, but it can take some amount of time to work through communications and how we work through chain of hierarchy and things like that. And we have a very good working relationship with our NASA colleagues because this is a fully sponsored NASA instrument. It's in the Discovery line, so it's considered a Discovery mission of opportunity. We work closely with our NASA colleagues as a team and they help in the interactions with our Japanese colleagues. We all want to see a successful mission and we're getting there. But we will see in a couple years how it all plays out.

JOHNSON: Did you go when it was delivered?

LAWRENCE: I did not go for the delivery but I have been out there many times. Some of it is just a cost issue. When we delivered, we sent a lot of our engineers out, and in fact we're on track to be sending some out in a few weeks to install the instrument on the spacecraft. I've weighed whether it would be wise for me to go out or not. The old proverb don't let the PI touch the hardware. I'm not sure how useful I would be in a practical sense. You just have to make those calls as it comes to you. But there'll be still many trips out there to Japan.

JOHNSON: It's interesting that you said that about don't let the PI touch the hardware, because that's one of the things I usually ask people. There are different ways of communicating. Of course you have international communication but you also have that communication between the scientists and engineers. Sometimes some of the people I've talked to on different missions, they've had somebody that felt that was one of the things that was their job, was being the go-between and making sure that everybody's on the same page because of that. Maybe talk about that for a minute. Some of that communication, you on the science side and communicating with the engineers that are actually building these things for you.

LAWRENCE: Right. By training I am an experimental scientist. I did the high-altitude balloon flight. Put all the electronics together. It was great fun. As you learn the longer you go in your career, the harder it is to stay in the lab. One of the things I so admire about my mentor Bill Feldman was he continued to be able to stay in the lab, playing with the oscilloscope, while he was doing his other work. I have not been able to do that as well as I would like. But having said that, so far, we've been able to maintain a team here at APL where a number of the scientists and engineers are still doing that. I do like to get in the lab even so. When we do campaigns like thermal vacuum or calibration, I really try hard to make sure I'm a part of that so I can still have my hands if not on the hardware—I'm not turning the bolts—but at least at the computer doing the analysis, running some of the experiments, or helping out with some of the experiments.

I think that's really key, trying to be involved as you can, because the further disconnected you get, the harder it is to understand what are all the small little problems that everyone's dealing with. Doing a lot of that engineering work is tough. It's really hard work. We do have extremely good engineers.

We're also working on how do you train up the next generation. Bringing some of the younger people on and mentoring them well. But the communication, a lot of it is—I'm even finding in the last number of weeks—you bump into people in the hallways. You have the discussions that are the unplanned ones where things come up that lead to another thing, that leads to another, and you make some big breakthroughs.

I think a lot of that communication, there's the formal stuff, but it's as much as coming into the lab, hanging out where you can, and trying hard not to get disconnected. But the pressures to pull you to be disconnected keep coming. Email is a complete tyranny, trying to keep up with that. Meetings are their own tyranny where there's no end of meetings that you feel you need to attend. Some of the meetings to attend are really the ones where the engineers are talking, and you just come in and you are the fly on the wall, so you can get a sense of okay, what is it that's going on, what's the thing under people's skin that needs to be resolved. Sometimes you can't do anything about it. Other times you might be able to. Then just being interested in a lot of the seemingly small details. That's in the end important.

JOHNSON: A lot of what you mentioned, meeting people in the hallways and attending meetings, a lot of that changed when you were working on MEGANE because of COVID. Talk about the impact of that on trying to keep working and building an instrument and getting it delivered on time during a worldwide emergency.

LAWRENCE: No fun.

JOHNSON: Two words, right? No fun.

LAWRENCE: Yes. It was hard. For MEGANE it was interesting. We had our preliminary design review for MEGANE right after COVID started to hit. We conducted, if not the first, one of the first remote reviews that happened at NASA. We're a small project, not a big deal. But we had to pivot pretty quickly to go from a planned on-site meeting to one of these key program reviews online and try to figure out how do you do this.

I think it worked okay as far as it goes. One of the things we did, and I don't know if people have kept it up, was we basically said—for these preliminary design reviews they tend to be two-day, three-day affairs where you have many different talks, where you go through all the designs. One of the things we decided early on, was for those presentations that were pretty straightforward and there weren't going to be a ton of questions, we just made them hand-in. Then we allowed extra time for the presentations that we thought were going to be more meaty and need more attention to detail and provide people additional time to get up and move around. You didn't have a 9-hour day. It was a 6-hour day or something like that.

In the end I think it worked okay. But long-term it's not good when you're just you and your screen. I remember for MEGANE when we had our confirmation review. So confirmation review occurs after PDR [preliminary design review] where NASA commits to doing the mission. My understanding, normally the way it works, is the team goes down to NASA Headquarters, meets with all the NASA officials, and if it goes well, which most of the time it does, it's big and celebratory, it's very exciting. We did our confirmation review online. That was fine, we were excited, it worked well. But then you're done, you close your screen, and you're just looking out your window. Kind of anticlimactic. I'm not saying anything that everyone else doesn't know. We all lived through this.

But you have to search out and make opportunities to have those hallway conversations. For APL, we were very fortunate. We ended up coming back into the lab more quickly than many organizations. For us, one of the main things was the final build for the Psyche instrument. We sometimes needed special permission to get in, but our team was there doing our work. That was very encouraging to have everyone there and hashing things out and working it. Really you cannot replace those conversations online. It doesn't happen.

JOHNSON: Yes, it's a different world definitely.

LAWRENCE: Yes, they need to be embodied. That's a bigger thing than just our space science. But as a human being I think it's very important that human interactions occur with full human beings.

JOHNSON: That's true. In the time we have left maybe we can talk about Dragonfly. You're involved in this. Of course this is New Frontiers, which is a different amount of money as well as a different way of selecting. Talk about that, how it got selected, and what your role is.

LAWRENCE: Fascinating mission. This was the one that I think a number of us were shocked that it got selected, because it's such a crazy exciting mission.

JOHNSON: Very different, yes.

LAWRENCE: Yes. Our involvement for that—well, my particular involvement was through Ralph [D.] Lorenz. He was at the time the project scientist; I think he's project architect right now. He works here at APL. This was a hallway conversation. I've known Ralph for a long time. As a scientist he—just say he does everything. He's written a ton of books. He can talk on any scientific topic with great intelligence. When we both have time and we see each other in the hallway we just chitter-chatter about any sort of thing.

For APL, originally we had a proposal to do a mission called TiME [Titan Mare Explorer]. It was going to be a boat that was going to go to Titan and land on some of Titan's lakes. It made downselect but was not selected. A lot of the folks that were working that proposal were trying to figure out okay, let's regroup, how can we do something on Titan. Because a lot of those folks, they've spent their careers thinking about Titan. It's just an incredibly fascinating place. It is the only moon in the solar system that has an atmosphere and liquid on the surface and dunes and yet they're just totally different. The bedrock is water ice. I explained that to my kids. Imagine that, go outside and the rock is water ice. They can't wrap their brain around that.

I guess they were coming up with this idea of okay, well, one of the best places to fly a helicopter in the solar system is Titan, because it's very thick atmosphere, and it's lower gravity. They latched on to this idea of flying a helicopter. Then Ralph came by and said, "Could you use any of your gamma-ray techniques for measuring elemental composition?"

I said, "Sure, but you can't use cosmic rays because cosmic rays can't get through the atmosphere to initiate your gamma rays and neutrons. You got to bring your source with you."

At the time we were also working on a New Frontiers proposal called VICI [Venus In situ Composition Investigations]. This was a proposal that would have gone to Venus. We had worked with colleagues at NASA Goddard Space Flight Center [Greenbelt, Maryland] and they were going to fly a neutron generator with a gamma-ray detector. They were going to oversee the neutron generator, we were going to bring a gamma-ray detector, not germanium. You can't do a cryocooled detector on the surface of Venus. It's just a category mistake. It's way too hot. But we had already done some of the legwork for doing a neutron generator so we were already working with the Goddard team. When Ralph just threw out the idea, I immediately said, "Hmm, I think we have a possibility here," contacted the Goddard folks, and pretty quickly we had an instrument idea that could go on Dragonfly.

The other interesting thing of this that as a physicist you like elegant ideas, so our germanium detector needs to be cooled to cryogenic temperatures to operate. For Psyche we have this fancy cryocooler. MEGANE a limited life cryocooler. You need to get to 90 kelvin-ish, but you can go all the way up to 100 kelvin. It turns out the surface of Titan is a constant 94 kelvin.

We had the idea well, you can just put your gamma-ray detector on the outside of your rotorcraft, and it's just going to be passively cooled by Titan's atmosphere. That's what we did. Before long they were moving forward with the proposal and we were fully part of that proposal team. It really was the result of a hallway conversation between me and Ralph Lorenz.

JOHNSON: Was the proposal, working toward that and toward selection, since you've had experience with doing that for Discovery, was it different for New Frontiers? Or was it pretty much the same kind of process?

LAWRENCE: A lot of similarities. The experience that you have does help. You can move through things maybe a little more quickly. But every proposal is its own wild ride. Dragonfly was no exception. I don't think some of our gamma ray/neutron team were quite as involved in the proposal process as we were on Psyche for the simple reason that we just had a lot on our plate. We were very busy. We did what we could. I think everyone was very shocked and surprised that it made downselect. Very exciting. There having the experience from Psyche was very useful because when you do a downselect, you have a yearlong concept study report where you spend a year fleshing out all your views. Then you end up having a site visit, and it's this one-day-long extravaganza where you put forward—the proposal review panel has your Step 2 proposal. Then they come and ask you questions and you put your best foot forward. Having had that experience on Psyche, having just done a site visit for Psyche that was successful, it was kind of interesting to be able to bring some of those ideas to come help for Dragonfly.

For me personally one of the interesting things is that for Psyche they actually contracted with a speaker coach to help all the people because you have different team members that will come and give presentations. They contracted with a speaker coach just to help you speak better when you're giving a high stakes presentation.

I brought that idea up to the people here at APL and they said, "Yes, that sounds like a good idea." It turns out—funny how this happens—we have a family friend, and she does a lot of different things, but one of her jobs is an actor and actress coach. We had them over for dinner at one point and I said, "We're working on this potential site visit and we did speaker coaches for Psyche. What do you think? Is that something you might be interested to do for Dragonfly?" She jumped at the chance. APL actually hired her and we had a few days of training with her about how do you become a good confident speaker when you're in these high-pressure situations. We went through the site visit. We were competing against the Comet Surface Sample Return. Lo and behold, we were selected. The rest is history.

It's been its own wild ride. Some of the challenges recently for Dragonfly have just been NASA budgets and trying to fund things at the level that you need to be able to move forward. Of course when you're doing mission instrument builds, when you're in the Phase B portion, that's always the time where it's wild and rough. Some things you thought were going to work aren't going to work as well. You need to figure all that out. We're in the middle of that right now for

Dragonfly. But we're still alive. Things are still moving forward. We're confident that things will work.

JOHNSON: It's interesting, the speaker part and bringing someone in for that. Because I know when I talked to some of the folks at Headquarters, the people that were involved in the selection process for some of the missions, they were talking about those site visits and how important they were. Sometimes on paper things look a certain way, but then when you go and look at the team and talk to the team and see how the team interacts with each other and that communication that they have will be a selling point for that. Being able to present yourself and you have teams, people that you've worked with over and over and over again on these instruments. I imagine that helps a lot as far as when you're going through that.

LAWRENCE: It does. But it was interesting. For Dragonfly we have our small group of people doing gamma ray/neutron but the broader team is totally different than Psyche. They all develop their own flavor and aroma. But I think one of the things about Dragonfly is it's such an exciting different mission that it's very easy to build up the enthusiasm about it. Which is fun. It is exciting. But it is very challenging, and that's I think one of the things, and I've seen it with the team now as we're really getting into the guts of the mission, is you cannot lose sight of all the fine details, because when you do spaceflight, the fine details will get you if you don't pay attention to them. As we're moving forward, I am seeing that people are working very hard. When you find things that you thought were going to work might not work, owning up to them early, trying not to be upset about it, but saying, "All right. So how do we make it work, and do it in a way that is encouraging in a positive manner?"

JOHNSON: As an instrument lead or PI for instruments that are flying on NASA missions, how important do you think public outreach—like you said, this one is exciting. It's different. I think people will get behind it. But once it gets further along and it gets closer to being launched, that excitement will probably build just because it is so different. But NASA tries to do public outreach and education. Is that part of what you do also? Your institute? Or is that anything you have to be responsible for?

LAWRENCE: For outreach. JPL of course, they're right next to Hollywood. I don't mean this in a disparaging way. But they have access to a lot of very high-quality production type of things. With the rovers and all the things they do, they're very good at it.

Historically APL, that's not been as forefront in our minds. I think some of it is we're not only a NASA center. We do other work, national security work. Over the years APL has recognized the value of public outreach and has done it pretty well. One of the big successes was the Pluto New Horizons mission. The outreach that was done for that just hit very well. I was not involved in that mission. I was just a spectator. But one of the most amazing fascinating missions to get pictures of Pluto, a planet that we'd never seen before. For me that's some of the most iconic pictures, is seeing the landscape of Pluto backlit by the Sun as the spacecraft was going away. Just astoundingly amazing.

Outreach. People are always thinking of it. Yes, it's implicitly in your job description. But different people go about it different ways depending on their personality. Some leaders are big into social media, Twitter, all that kind of stuff. That's not me. If nothing else, I have no bandwidth to deal with that. I would screech to a grinding halt if I had to keep up with that.

But as there's opportunity, I do like to give talks in various organizations and places, give talks to undergraduate students at smaller colleges. Colleges that my kids attend. Grove City College, I've had two kids go there, and I've given talks up there. Sometimes I think it's the smaller venues that have more bang for their buck because you can look people in the eye and talk to them and have actual conversations about things. You can see their eyes light up and they get excited. Many different ways to do outreach. Yes, it's part of our job. There are small budget items for doing that. There's more formal things, set up websites, do news releases and all of that. There's a tension between overhyping things, I don't think we want to do that. I'm uncomfortable with that, but you don't want to say nothing.

You mentioned there was just a story out about delivery of the MEGANE instruments, I thought that hit it kind of nice. Straight down the middle. Not overhype. But this was a big deal, delivering an instrument to Japan.

JOHNSON: It was. It's an interesting mission too.

LAWRENCE: Yes.

JOHNSON: From your perspective and working on the science with these missions, what do you think the impact of the Discovery Program and New Frontiers also, of these missions, the impact of this model of competition? Discovery, more so than New Frontiers, has been described to me by other people that it's like the Wild West. You can basically propose anything in Discovery. But what do you think that impact has been for the science community to be able to get the science out there more often than it was before these programs were available?

LAWRENCE: That's a great question. To some level it's hard for me to really address what was before because as I've said before, most of my career has been doing Discovery work. Put that there for the moment. But I would say it's been a huge benefit. Hearing some of the stories before my time when you had a lot of big missions from NASA that were very centrally planned, stakes were very high. They were few and far between. Seemed there was not huge opportunities to be creative and entrepreneurial.

When you mentioned Wild West, there's good things and not so good things about that. Some of the good things is you're able to give opportunity to scientists to be creative, to come up with ideas that people might not have figured out. I would suggest it gives opportunities to a wider swath of people than might normally have been done. I think you can actually see that in all the selections of the Discovery missions that have been done, and I would suggest even New Frontiers. I'm not sure a centrally planned mission would have come up with Dragonfly. That took people saying, "Oh my goodness, I wonder if we could do this." Then having the possibility of going forward with it and maybe even succeeding. I think that's a big deal.

A challenge of course is over the years Discovery has gotten bigger and bigger. That tends to set things in stone and can tend to stifle some of the creativity. But you're not going to get away from that. You will always need to revisit. I know there's been some attempts in NASA to come up with a what I'll call Discovery light. Is there a place where you can do missions at a lower cost point that can still have the creativity but allow more risk?

That was one of the aspects with Discovery and New Frontiers. There's some willingness to take more risk than otherwise you might do. You have to accept the fact that you're going to have failures. Discovery has had some. That's the tension we always live with in doing this business. But yes, I think it's been a huge benefit on the whole for doing this Discovery model.

One challenge, and I don't know if I've talked about this before, is the PI model in some ways is good because it sets the authority in a particular place, not in some bureaucracy, and there are some good things about that, because it can help at its best streamline decisions and enable decision making to take place. A challenge is the pressure on the PI is huge. I'm only an instrument PI and it can be a lot of weight, but the pressure on mission PIs is really big. If you're doing that model sometimes there's not a way around it. The really good PIs, they know how to distribute the authority and the pressure and the weight and really be successful. But I think that's tough.

JOHNSON: What do you think it takes to be a successful PI? Not just for the instrument but for a mission too. What are the qualities of those PIs?

LAWRENCE: A little bit crazy. You have to be willing to give up a lot. Goodness, that's a great question. I don't know that there's a uniformly perfect answer because I've seen all sorts of different personalities. It's good to be willing to be involved in the details. I don't know. I don't have a great answer other than you know it when you see it. It's a tough job. I respect the mission PIs. It's tough.

Let me just say one funny little story about Sean [C.] Solomon, MESSENGER PI. He did a very good job. But sometimes with some leaders you don't know what's behind what all they're having to deal with. You can tell that they have a lot of stress, a lot of weight on their shoulders.

But it was maybe two years ago when I happened to bump into him at Lunar and Planetary Science Conference. MESSENGER was done. He was done. I had never seen him so bouncy and happy.

JOHNSON: Different person than you worked with, right?

LAWRENCE: Yes. Not taking away how he was tough, and he had to be. I appreciated him. But he had a lightness to him that I hadn't seen before. MESSENGER was fully successful, and it had its own harrowing experiences all throughout. That was a great mission. What an amazing thing to do that, go through all that suffering as it were, and come out the other side and still be strong and be glad about it. I thought that was just cool.

JOHNSON: All that weight lifted off his shoulders.

LAWRENCE: Yes.

JOHNSON: Right before we close, one of the questions I like to end with is thinking about your role with Discovery and the different missions and the instruments and now New Frontiers, but at this point, if you had to point to one thing what would you say you were most proud of?

LAWRENCE: Lunar Prospector. To be able to be a young scientist, work with Bill Feldman, be on a really small team, and see this map of thorium that no one in all creation had ever seen before. This is something on the Moon that people have looked up for all of human history and seen. We got to see something and understand hydrogen at the poles of the Moon, all of that. I guess that was one of those things. I remember telling myself and maybe a few others, "If nothing ever else happens, this is enough." It was not only the maps of the Moon. It was the people that we were a part of, and it was just fantastic people that were really tight, really enjoyed what we did, and it was a unique experience. Knowing you're never going to repeat that but being thankful to be a part of it. Everything else has been gravy.

JOHNSON: That's good to know.

LAWRENCE: Yes.

JOHNSON: Is there anything we haven't talked about that you wanted to mention or anything else?

LAWRENCE: I don't think so. I just feel very grateful to be able to do what I do. Never would have guessed it. I wanted to be an astronaut. Now I'm doing this. You never know when it's going to end. Be content with that. Yes, very grateful. I feel like it's all the people around that you have the pleasure to work with that have been the real power, the real excitement behind it.

JOHNSON: I think that's a good place to stop. But I appreciate you talking to me.

[End of interview]