Margaret “Peg” Frerking

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Erik M. Conway,
Interviewer

Q: This is Erik Conway. I’m interviewing Peg Frerking at JPL about the Kepler mission. I don’t even know what day this is. Is it the 24th of October?

Frerking: Yes, something like that.

Q: Okay. The 24th of October, we’ve decided. And we’re in a little conference room at JPL, Building 111.

So, Peg, please tell me a bit about yourself. Where were you born? How were you educated?

Frerking: I was born in New York City, though I did not live there for very long, moved around quite a bit. I went to MIT both as an undergraduate and a graduate student, majoring in physics, and then after that, I was a postdoc at Crawford Hill in Bell Laboratories, which I did with Bob Wilson and Arno Penzias, the discoverers of the three-degree black-body radiation using a horn antenna, *now* old antenna there. I understand they’re preserving the antenna, which I think is wonderful.

I then came to JPL in 1980 and was at JPL until 19—I mean 2022. [laughs] So I was at JPL for forty-two years. At JPL, the first half of my career I conducted technology research. I developed submillimeter wave instrumentation. Then the second half, I moved into project management. The first project I managed was the MIRO instrument on the European Space Agency Rosetta mission that rendezvoused with and orbited a comet. MIRO used the technology that I'd been developing as a technologist. It was very satisfying to see the receivers that I'd worked on for close to twenty years actually now to go into space.

After the MIRO instrument, I worked on a number of other instruments. I had just finished working as project manager for the U.S. contributions to European Space Agency Herschel and Planck mission, when I was approached by Leslie Livesay to work on the Kepler mission. This was around 2006. I said, "Sure. Why not?" I joined her team as instrument manager. At that point in time, the project management for Kepler had been shifted from Ames to JPL. Then once she left Kepler and Jim Fanson, who had been her deputy, became the project manager, I became his deputy project manager and held that position through launch and commissioning, Phase D. For operations, Phase E, the Kepler project management transferred back to Ames, where it originated and its PI worked.

After that, I moved into the Office of the Chief Engineer at JPL, motivated in large part by some of the challenges associated with Kepler, which turned out to be quite an interesting and complex instrument. I did such things as tried to understand complexity in JPL projects by studying problem failure reports and engineering change requests. I looked at a whole bunch of data associated with JPL project development.

Then after doing that for a number of years, I decided I ought to put that information to use on another complex project, and so became the project manager for the coronagraph instrument (CGI) on what is now the Roman telescope, taking that through PDR.

Along came the pandemic, and I moved into the Astrophysics Division, 7X, as the 7X technologist for a number of years, and then finally retired. So that's my history.

Q: So tell me about your receivers. I guess it was submillimeter wave technology that you were working on that went on Rosetta.

Frerking: Rosetta had 567 gigahertz radiometers, which is officially submillimeter, because the transition from millimeter wave to submillimeter wave occurs at 300 gigahertz. For my PhD thesis at MIT, I had developed an infrared, a 9.6-micron heterodyne radiometer, which I used to look at ozone. Heterodyne radiometers had very high spectral resolution, so I was able to resolve ozone lines in the Earth's atmosphere that hadn't been previously observed. Due to the very high resolution, I was able to very accurately identify the characteristics of those ozone lines.

When I went to Bell Laboratories as a postdoc, my research moved from the infrared to the millimeter wave regime. The team there were developing 115 gigahertz high resolution heterodyne radiometers to look at carbon monoxide in the interstellar medium, using the seven-meter antenna, which was on Crawford Hill near the Bell Labs Holmdel facility, but no longer exists.

Q: Which is Holmdel, right? Holmdel, New Jersey, yeah.

Frerking: Holmdel. Right. So, we started working on developing superconducting detectors for the millimeter wave regime. These are very sensitive detectors and could be quantum noise limited; that is, they could detect a single photon.

So when I came to JPL, my first job was working on something called the Microwave Limb Sounder, an earth-remote sensing instrument, on the Upper Atmosphere Research Satellite. This was the first Microwave Limb Sounder, and it had several millimeter wave heterodyne radiometers - at 63 GHz, 186 gigahertz, and 205 GHz - measuring ozone, water, and carbon monoxide. I was developing new technology, but I was also the cognizant engineer for these millimeter wave radiometers on this instrument. This combination of both technology development and flight instrument development gave me a good understanding of how to transfer new technology into operational flight instrumentation.

Simultaneously, I was working on instruments that flew in the Kuiper Airborne Observatory out of Ames, to look at water in the interstellar medium, and so we targeted the 500 gigahertz regime in the submillimeter wavelength region where there are transitions of various isotopes of water. These radiometers had as their mixers the very sensitive superconducting detectors originally developed at Bell Laboratories. Heterodyne radiometers also required what are called local oscillators, which meant you had to have a laboratory source near that frequency, so those were challenging as well. We developed multiplier sources for those. Now, I mean, I could see in the future that there would be something like lasers, and there was something discovered about that time, quantum well oscillators, so I started looking at quantum well oscillators, but I was doing those at 10 gigahertz, really low frequencies. They're now making quantum well based oscillators in the terahertz regime —this was way beyond what I ever did—, which I think is fantastic. They should enable these heterodyne radiometers throughout not only the millimeter and submillimeter regime, but the far IR, and provide *very* high spectral resolution, which is cool.

Q: Really cool.

Frerking: It is cool.

Q: And eventually will be useful, eventually. So, okay, we're supposed to talk about Kepler. So you come on to the Kepler project in around about, you think, 2006.

Frerking: Correct.

Q: And you're instrument manager.

Frerking: Manager.

Q: Not the cognizant manager.

Frerking: It was a complex organizational structure as well as a complex instrument. Both the spacecraft and the photometer – consisting of the telescope and the focal plane - were all being built at Ball Aerospace. When JPL was brought on as the overall project manager, Ames retained the contractual role for the photometer, whereas JPL was the contractual manager for the spacecraft. So there was some interesting dynamics associated with that, and at one point it was decided that it would be important to have one reporting path for the project, that JPL also have oversight of the photometer, but not contractual management. So that's the role that I had. It was called instrument manager for the photometer. I worked very closely with the people both at Ames and at Ball to develop the photometer, which was fascinating.

All the instruments I've worked on have been both interesting and challenging and have fond places in my memory. But the Kepler photometer was unique because, in principle it used mature technology. Its focal plane used CCD detectors. NASA had been flying CCDs in space since the 1990s. However, what was *very* different from previous focal planes was having 96 megapixels, an order of magnitude more pixels than previous focal planes . That meant that there were 42 CCDs arranged in the focal plane the size of a very large pizza. What *hadn't* been done previously was the scaling up from 1 or 2 CCDs to 42. In addition, the fine guidance sensors were also in the focal plane, and so there were additional CCDs for that, which were operating in a different mode. There were interactions among the various CCDs and there were interactions between the fine guidance sensor and the detectors used for signal detection, which was an interesting complication.

Further, you had to fit these in a very small volume, for the time, and so there was something like 20,000 discrete electronic parts - resistors, capacitors, transistors - associated with the readout of these 42 CCDs into 84 channels. Well, anyway, lots of challenges. And they had to fit in the shadow of the secondary mirror of the telescope in order not to take away space for the collecting area of the telescope, so they were constrained into this small volume—it was about a cubic foot. So we had these 42 CCDs which were arrayed in about a foot and a half by a foot and a half, and underneath those, hidden so they wouldn't obstruct the view, were these 20,000 discrete resistors and capacitors and conductors and transistors mounted on 10 circuit boards each of which was *crammed* full. That led to a number of challenges in addition to the packaging in a limited volume.

Q: I would think there’d be a noise challenge.

Frerking: Well, I’ll get to that. In order to get low noise, the CCDs were cooled to about minus-95 degrees C. I may not be remembering the correct temperature. I could look it up. But cold. They weren’t really cryogenic, so they could be cooled passively with heat pipe-fed radiators. The thermal design was critical to get the CCDs cold enough in order to have low noise. Every mw of power dissipation mattered. So, the designers decided that one of the readout amplifiers that followed the CCDs would be turned on only when needed. Therefore, it would require less power. *But* there’s a downside to turning things on and off, because they never achieved thermal equilibrium. So these amplifiers were unstable, thermally unstable, and that led to, in some of the cases, low-level oscillations. Now, these low-level oscillations were quite low. However, we had sensitive detectors, and so the detectors were very good at detecting these low-level oscillations. So in retrospect, we lost the advantage of going so cold with the CCDs by adding noise in the readout electronics due to this turning on and off of the amplifiers leading to these low-level oscillations.

One thing that was cute, I should say—I don’t know if it’s cute, but interesting about CCDs is, of course, they’re very good at detecting oscillations since their pixels are read out sequentially. As these amplifiers heated up, these oscillations would change in frequency - they’d chirp - and you could actually see the oscillation move from one pixel on the CCD to another, to another, to another, resulting in low-level Lissajous patterns in the images. There were also a number of these type of artifacts that were a result of the constraints that arose from scaling up from a small CCD to one that could detect exoplanets around more than over 100,000 stars simultaneously. Identifying and understanding these artifacts took a while to figure out. It was a puzzle, you know. It was a fascinating puzzle to try to figure out what in the world was going on. Unfortunately, by the time we figured it out, it was really too late to fix anything. This often happens in spacecraft that have to launch within a constrained time and budget. So, we had to decide how to deal with it.

It turned out that only about a third of the amplifiers oscillated. Why not all of them? This had to do with there are slight variations in parts and that the design of this system was close enough to oscillation so that the variation in parts actually could lead to some oscillating and some not. But also, it was very temperature dependent, so as amplifiers were heating up, the oscillations changed in frequency. That was the chirping. But there were also regimes where there were fewer of these going into oscillation than other ones, so we ended up actually heating up the detectors.  Detectors should get better as they get colder. What we were seeing was the detectors were getting better when we heated them - telltale signs with detectors, a red flag that something not quite right was going on. What it also did was give us a tool to help us reduce that noise. So, anyway, one of the things we did was actually heat up the focal plane a bit, in order to get out of the region where there are more oscillations than others, and by doing that, we reduced the oscillations down to a smaller number, affecting a smaller number of CCDs. This, of course, increased the intrinsic noise of the CCDs. The remaining artifacts were somewhat repeatable, so could be calibrated out in software, which was the other approach.

I should say also Kepler was a fantastically successful mission, as you know, and the objective, the requirement was to be able to measure Earth-size planets around Sun-like star, and that was what set the requirement on the detectors. Kepler has detected many of those. So, it’s not as though the decision to “use as is” significantly hurt the mission. It did have an impact on the amount of time it took to see the smaller planets, but it didn't prevent it from doing the mission. You have to have the understanding of the instrument and the understanding of your science objectives in order to make those types of "use-as-is" decisions in a rational manner.

But, anyway, it was just sort of fascinating to see how the whole thing played together. You wanted to have the noise as low as possible. Ideally, you want the noise dominated by your detectors and not the readout electronics that follows the detectors, but in this case, because of these oscillations in some amplifiers, it was dominated by the readout electronics rather than the CCDs. So that actually allowed you to heat the CCDs up a little bit because you weren't getting the advantage of the colder CCDs anyway.

Q: So did you find that during the test program on the ground or is that all after launch?

Frerking: We found most of this during the test program on the ground. The test program on the ground went from individual CCDs with an individual readout circuit to two CCDs with one fully populated readout board. There were ten of these readout boards, but given time constraints, we tested two CCDs plus one board. That was done fairly early on. We unfortunately did not  see the oscillation at that level, because maybe we were testing one of the boards that didn't oscillate.

Next we integrated and tested the DAA, the Detector Array Assembly, which had the all the CCDs and all the readout boards, and *that's* when we saw the various artifacts and began to understand the temperature dependence of the oscillations.

Finally, the whole photometer was integrated and tested in a very large thermal vacuum chamber. The artifacts were quite obvious there as well. So, it was a progression of tests, but by the time we really understood what was going on, the photometer was fully integrated and aligned. To correct the artifacts would have required taking things apart and subsequently retesting in thermal vacuum, which would have delayed launch by at least a year. We were constrained by temperature control, by volume, but you're also constrained by schedule and budget, so that wouldn't allow going back and fixing the readout electronics.

Q: You'd know what to do.

Frerking: Well, one would never, ever do it again, because we would not be using those discrete parts on ten circuit boards. We'd now be using ASIC or specially-designed FPGAs.

Q: We have far different electronics than even—what was this, only twelve or fifteen years ago?

Frerking: This was 2006. So, the design was actually done before we got them, so it's now close to twenty years ago.

Q: A lot has changed since then. So, let's see. Jim told me about some other troubles, about getting the composite structures made and mirror coating done—

Frerking: Yes.

Q: —an overall problem, and then about the verification and validation campaign. So, what else? You've told me about the electronic noise problems.

Frerking: In addition to the noise problem, we had our share of other challenges as well. We couldn't demonstrate the end-to-end test of the photometer with the spacecraft within our cost constraints. It was originally planned to conduct an end-to-end test to verify that we could actually measure eight parts in a million – the signal size of the Earth in front of a Sun. So, in order to do the equivalent of an end-to-end verification, we had to do it piecewise. One of the critical aspects of that was if you're going to do something piecewise, you have to have a good model of what you're expecting to have, and this is something Riley Duren was very instrumental in, so you should talk to Riley about this. I assume you will.

Q: Yes.

Frerking: Riley put together this model that could be used as a verification at individual steps along the way to show that it would achieve the required performance. There also was a test bed that could generate very small Earth-planet sized signals basically by running a small current through a resistor in a pseudo sky image. The signal was detected with early-on prototypes of the CCD detectors. That meant that the flight detector specific artifacts weren't present. Not only did the system require the capability to detect very low signal, it also had to be very stable over a long period of time. The way Kepler observes exo-planets is to measure changes in the brightness of a star when a planet passes in front of it during its orbit around the star. and the planet is in front of the star once every orbit. The requirement is to detect Earth-like systems three times. Therefore, the stability requirement is for three years. This turns out to be a real driving requirement - to be able to get the stability where you could see a few parts in a million change in Sun-like star intensity over three years.

How in the world could you verify that? We used models to try and understand what drove the stability of the instrument as well, and those models led to a certain thermal stability. The sensitivity was driven by the absolute temperature, but the ability to be able to do repeat measurements of a transit of a planet required that it be stable over a very long time. That required not so much that you had an absolute temperature, but a very constant temperature. Another associated requirement was that the telescope pointing was also very stable and repeatable.

The whole mission concept was designed to provide the required stability. An Earth-trailing orbit about the sun, rather than a more traditional Earth orbit, was used so that the effects of either the Earth or the Moon was removed. If you're in a low Earth orbit for instance, the Moon would shadow the Sun, changing the thermal power on the photometer. And the spacecraft was designed so that one side always faced the Sun—it had solar arrays wrapped around the barrel of the solar shield. This permitted the telescope to avoid pointing at either the Sun or the Earth-Moon system in order to get the stability required.

So, it was a *very* clever design. Further, the telescope rotated with the sky, so that each star in the field-of-view was always on the same pixel. The pointing accuracy was stable enough so that a star would stay on that pixel. It was a challenge to come up with a way of verifying both the thermal and the pointing stability requirements. The verification was largely tied together by models put together by the system engineering team. First the prototype system verified sensitivity to detect Earth-sized planets orbiting about a Sun-like star. Then you measured characteristics at various stages of integration, and they had to meet the level of integration requirements. That was part of the validation, in that those level of integration requirements were derived from the end-to-end model.

And it all worked. Over a thousand planets were detected in the first four months of science operations. Now, these early-detected planets are fast orbiters, so you didn't have this *long-term* stability requirement. It worked well, that modeling. Subsequently, in excess of 4000 exo-planets have been observed including the Earth-like planets orbiting about Sun-like stars with yearlong orbital periods.

Q: The fast orbiters are kind of crazy too. You mentioned having to work with both Ames and Ball, so tell me about them separately. What was it like working with the folks at Ames, and maybe talk about the difference between the way Ames was doing things and JPL would have.

Frerking: So, when we started, it was a shock, you know. This was Ames' baby, a well-deserved baby. The PI was at Ames and he'd been working on this for fifteen years. Ames proposed the Kepler mission with Ball and were selected with Ball. "Why did JPL have to come in, the big elephant, or whatever"? So, there was an initial uneasiness as we got to know each other. Leslie Livesay, the JPL project manager, organized a workshop where everyone got together.

At this point, I was only supposed to keep track of progress, but all of a sudden, Leslie says, "Well, Peg is going to be the instrument manager."

And I said—I didn't say anything. I said to myself, "*Really?*" I mean, how is this going to work? So, I figured, "Well, all right, I'll just get in there and try and understand things the best I can and get to know the people."

I've got to admit, it was one of the best teams I've ever worked on, after we got through that initial shockwave. So, I started trying to really understand. And you can see it's an interesting instrument to understand. It's basically a puzzle. Anyway, I worked with Ames to understand their concept of what it was. There were a number of people that I talked with just to get enough background. And the science team was fantastic.

At Ball, the technical people were fantastic also, a group of people I really enjoyed working with. So, after we got to know each other, it became what I would call a seamless team, as seamless as anything I've seen at JPL. I spent about half-time at Ball, so it required a lot of being there, but it was *really* good people to work with.

Ball has some tools that JPL doesn't have. Ames certainly had the expertise in understanding the science and photometers, so it actually ended up working out fine. Everyone was really interested and excited about the mission. It was ground-breaking. It was an *exciting,* not only mission, but spacecraft and telescope to work on.

One of the issues was that it had so many pieces, keeping track of it was hard. One of the actions I did was to understand how it was to be put together, integrated, and captured in the schedule. The schedule, developed by Ball, had over 100,000 lines, so figuring that out took a while. It was very good schedule and I liked it because it identified lots of detail, much more detail than most JPL schedules do. So I could follow what was going on or what in principle had to go on, on almost a day-to-today basis. It's just that it was so big that figuring out how to manage it was a challenge. Part of the issue also was that initially it had some funny connections, certain things were tied, linked, that didn't need to be linked. But once that all got fixed up, which it did, then we came up with various tools to try and be able to trend progress based on a smaller number of critical steps that we could track a little more easily. We developed tools to monitor the slack in the funded schedule reserve and use that as a basis of how well we were progressing with regard to our budget as well. Ball had some really good schedulers, people who worked the schedules. So, we could get data from the schedule weekly, as opposed to what we do at JPL, which is monthly, and that monthly information goes into EVM [Earned Value Management], but the latency of a month sometimes has a big impact. Getting the weekly schedules in association with the tools we developed to analyze those schedules made it so we could be on top of problems as they were happening. We could see the problems coming up, as opposed to in the rearview mirror, and that was sort of fun too. It was another piece of the puzzle.

Q: So, you were under earned-value management.

Frerking: Yes.

Q: Because I think that was introduced by Discovery around 2001 or something.

Frerking: Right.

Q: I keep forgetting to ask people about it. [laughter] So you found it useful, though.

Frerking: The earned-value measurement, it was very useful from the point of view of presenting where we were to other people. From the point of view of being able to see forward potential problems, it was less useful. What was very useful was the schedule with that level of detail, because you could, after all the linkages got worked out, you could really see what was going to drive something in the future, and when you could see that, then you could put a focus on it, and in many cases fix it. So, I think having that detailed schedule with the shorter cadence and the tools that allowed you to get your arms around it actually allowed us to fix a lot of issues before they happened, basically by refocusing attention to the things that were going to drive activities coming up. Does that make sense?

Q: Yes.

Frerking: And that was actually fun too. It was part of the puzzle. Coming up with things like that was useful.

Q: You said working with Ames and Ball were great, but you didn't name any names, so tell me who in those teams were people I should talk to.

Frerking: Eric Bachtell at Ball. Doug Caldwell at Ames.

Q: I know the name from some other context. Ames isn't that big, as you know.

Frerking: He was great. Jon Jenkins, Charles Sobeck, those are sort of the people at Ames. Scott Tennant. Scott Tennant, the Ball photometer manager was *very* helpful to work with. The thermal guy, maybe Charlie Toich [phonetic]. And Eric Bachtell was the Ball lead system engineer. John Troeltzsch actually became the Ball project manager, but he started out as the spacecraft manager. Jeremy Stober. I don’t know if that helps.

Q: No, that’s good. I’ll ask if I can get anyone at Ball to talk to.

Frerking: Oh, and there was a guy, Vic Agrabright, he was the one who did the electronics and the CCDs, and he was brilliant, Vic Agrabright.

Q: Okay. If you can look them up, that would be great. So let’s see. What’s your favorite memory from the Kepler project?

Frerking: There’s one other topic to talk about before that. I don’t know if Jim mentioned this or not. It’s sort of a technical thing. At launch, Jim, Leslie, all the high-level folk, went off to the launch site, and I got to stay at Ball to “catch the spacecraft”, we called it. The spacecraft came off at a slightly different angle than expected, so it started spinning faster than anticipated, and it was spinning at a rate out of range of the design. During launch, almost everything’s off, so the power draw from the solar panels was low. The  solar panels were mounted all around the surface of the spacecraft so the power generation was going up and down, up and down, up and down, and because the power draw was so small, the power eventually went above the overvoltage limits. This triggered a fault mode that stopped charging the batteries, basically opened the circuit between the solar arrays and the batteries, which is what you want to do in an overvoltage situation. The rotation slowed down but before the initial overvoltage fault cleared, a second level of fault protection was triggered. The spacecraft has a main computer and two subsystem computers that were redundant. The second fault triggered a switch between those two subsystem computers, which then made the spacecraft go into safe mode.

Q: It should have gone into safe mode!

Frerking: Exactly.

Q: After all that, it should.

Frerking: The spacecraft was designed, post launch, to detumble – stop any rotation of the spacecraft, then point the solar arrays at the sun, the communication antennas at the Earth and transition the computers from launch to safe mode. All of this worked, but it was in safe mode with a switched subsystem computer, so that was an anomaly.

But before launch, there had been a review, as there are many reviews prior to launch, where a review board had gone through and looked at the readiness for launch. This was a phase review, so it was a Kepler level review. It was found that the procedures to respond to various fault conditions weren’t in place. An action from the review was to ensure all the procedures were in place. The spacecraft procedures were written, but what wasn’t written or addressed was how to act, what process to follow when you got into not only a safe mode, but different types of fault conditions. So, we went through, roughly the six months prior to launch, putting together these procedures.  This was a good thing we did, because then when the launch anomaly happened, we had a procedure for it and we followed it and it worked.

Anyway, so now you wanted me to talk about what was my favorite memory.

Q: Yeah.

Frerking: I think working with the team. I *really* liked working with Leslie and Jim and Riley, and I thought I haven't mentioned them Tracy Drain and Louise Hamlin.

Q: I know Louise.

Frerking: She was at Ball. She was the JPL representative at Ball.

Q: Oh, okay. I did not know that.

Frerking: She wasn't a Ball person. She was a JPL person, but she was living basically in Boulder. Anyway, they were just a great group of people to work with and led to us being able to work with Ball and Ames as well as we ended up working with Ball and Ames. I hope Ball and Ames think we were good to work with. [laughs]

Q: I'll try to talk to them. We'll see what they ultimately think.

Frerking: But the other thing was, it was *never* boring. It was a big puzzle to try and understand this—I won't say complex, but complicated spacecraft, and getting it all to play together. It was one of the highlights of my career at JPL. And then, of course, it had such fantastic results.

Q: Yes. People will forget that it was also nearly canceled for cost overruns.

Frerking: Yes. Well, by the time it got to JPL, I think there had been two potential cancellations. While we were there, there were two more. We were really directed to stay within budget and on schedule and come up with a strategy to do that and be able to demonstrate we had a strategy to do that.

Q: So did those cost impacts affect what decisions you had to make?

Frerking: Oh, for sure. If we hadn't had that cost impact, we would have fixed the amplifier oscillations. We did fix some amplifiers that had radiation sensitivity, but they were caught a lot earlier, but we could have fixed the oscillating ones as well. We could have done that end-to-end test. There was a whole descope list, and almost everything on that descope list was executed. If it had been JWST, all those things would have been addressed. So, we went with a lot of "use as is" and a lot of secondary approaches to verification and validation.

Q: We know how well it worked out, so—

Frerking: "Use as is" worked.

Q: Yeah, "use as is" worked, once you characterized it enough so you could understand the problems.

Frerking: Yeah.

Q: But there's a cost to that as well, of course.

Frerking: Well, there's definitely a cost in software, but that was done post launch.

Q: A different pile of money.

Frerking: Different pile of money.

Q: So, let's see. What did you learn from Kepler that you took forward into your future work? You did a number of things.

Frerking: Oh, well, you know, the schedule stuff I took into future work. I choose to become the Coronagraph Instrument project manager in order to apply the lessons learned from Kepler for managing a complex and complicated project. For instance, the coronagraph instrument uses deformable mirrors to correct the observed wavefront error in order to be able to image a planet. I used the same thought process I used on Kepler to try and deeply understand how conronagraphy works so the project implementation could be set up for success under its various constraints. This included a focus on instrument modeling. Further, I tried to ensure adequate time for testing the instrument at each level of integration in part because of the step-by-step characterization that was done on Kepler.

Also, one of the things I tried to maintain in that schedule was significant thermal vacuum time. Unfortunately, that was shortened somewhat. Anyway, I think that being able to have as much time looking at the instrument to understand the interactions, the crosstalk, the idiosyncrasies, if you will, is important for future operations.

And so, the coronagraph instrument has got a similar issue. I hope they have enough time to really characterize it. They're just now putting the whole thing together. But what the coronagraph instrument had was a test bed, as did Kepler have a test bed, and that test bed I think is *very* important and will be very important for these complex instruments to understand operations, and especially for an instrument like the coronagraph, where there's a lot of interplay with a computer system to do the optimization of the optical train. Having a testbed in place that allows parallel operation during flight instrument integration and once launched, on the ground, is especially important for complex instruments.

Q: Not just run time, but time in which you're doing some sort of characterization testing.

Frerking: Right. Yeah, not just making sure that the computer doesn't run off its counter. [laughs]

Q: Right. Not just to meet a set of hours that are specified by the design principles.

Frerking: Another Kepler lessons learned, as you pointed out, is that things don't have to be perfect. You can use "as is" and get very good results.

Q: That's a good lesson. Anything else before we end? Because we're out of time.

Frerking: Oh, okay.

Q: So, my famous last question is, what didn't we talk about that we should have?

Frerking: Hmm. There are reaction wheels issues. That would have been—you should talk to someone such as Riley about reaction wheels.

Q: Yeah, not just Kepler, a whole bunch of JPL missions got harmed by reaction wheels.

Frerking: Right. Talk to Riley about reaction wheels.

Q: Okay.

Frerking: I guess this was one of the first missions that relied on Ka-band, so the traveling wave tubes, the 32 gigahertz frequency ones—although traveling wave tubes have been around—

Q: A long time.

Frerking: —a long time, getting one that—well, actually they built them at this frequency forty years ago, but it was a forgotten thing. There were the usual problems with radiation. You always have to worry about radiation on parts. But I'd say reaction wheels.

Q: Okay. Well, thank you.

Frerking: All right.

[End of interview]