NASA Johnson Space Center Oral History Project
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Conrad Wells
Interviewed by Jennifer Ross-Nazzal
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Ross-Nazzal: Today is September 19, 2018. This interview with Conrad Wells is being conducted in Houston, Texas, for the JSC Oral History Project. The interviewer is Jennifer Ross-Nazzal. Thanks again, for coming by today. Last time you were here, we were in the middle of talking about the test, and you needed some time to think about how you wanted to present those details.

Wells: Our job was to test the primary mirror and make sure that the primary mirror was working right, and that it was properly aligned in the system.

There was another team of people: [J.] Scott Knight, Ball Aerospace [& Technologies], a lot of the people working on the science instrument teams from [NASA] Goddard Space Flight Center [Greenbelt, Maryland], and the Goddard image scientists, and Space Telescope Science Institute [Johns Hopkins University, Baltimore, Maryland], they were, at the same time, doing testing through the entire telescope. They were shining simulated starlight through the telescope and imaging it, kind of separately from us. We were in our own little world, and they were on the table behind us. But, both of us very engulfed in our work. Often when we were doing work, they would be off, and we would switch off, because if we were changing the mirror, that wasn't so good for them. They wanted a stable mirror to do testing for a day or two, so we would get a day or two off, which was very welcome.

Also, Harvey arrived, I think right before we were phasing the mirror. Things were working pretty well. Our software that we worked on for the previous decade but really the last four years that we were here, intensely in the last two years, very intensely, appeared to be working pretty well. Looking at the data, I thought I saw some peculiarities. Working with Scott Knight from Ball Aerospace, looking at the data, it appeared to me that the software was telling us to decenter the mirror. We wanted to put the mirror in one place, and as we were, the commands that we would calculate should be telling us to stay about the same centroid, about the same center of the telescope. Looking at the data, it was decentering the mirror. It was moving from where it was supposed to be.

I was talking with Scott Knight about that on that week, and the reason I know the days of the week is that Harvey arrived on Friday, and my kids were off of school on Friday. It was probably Tuesday, Wednesday, Thursday. I was talking, and this would be August. Harvey came on the 24th, I believe, August 24th, Friday the 24th, or the 25th, probably the 25th, because that's the day I wasn't there. So I was discussing the results with Scott Knight and looking at the data. It did look like it was going to decenter the mirror, and we were trying to figure out what to do, but still still moving mirrors and making measurements.

It'd take hours. It would take maybe 10 minutes to make a measurement, and another 15 minutes to transfer it. Within another hour after that, you'd really understand what you wanted to do. You'd give the commands to the mirror team, and they would take another 15 minutes to get all their commands done. They would issue the mirror commands, and it would go through the flight software. Some of those moves took four hours to make, so it was a pretty long cycle time, really. Some of them were an hour. Some of them were maybe 15 minutes at the shortest duration, but the bigger moves were four hours.

If there was a mirror-move error, which would happen at least every couple days, they'd have to go back to the telemetry and figure out what was wrong, to find exactly what happened. They have to figure out the root cause before we could move forward, and that could take sometimes, in the early ones, like eight hours, an entire shift, for them to figure out what really went wrong, to get the software settings and the databases corrected so that they could make another move again. They don't want to lose the actuator positions; that's very important information that they keep correct. So, we would be chugging along and all of a sudden, we'd have a 4-hour mirror move with an 8-hour delay in it. Next thing you know, you go home and come back the next day.

Scott and I were looking at the data, and we thought there was something peculiar. Then we had another engineer, actually the architect of the software, named Gene Olczak. He left, I think, just in time. I bet he left on Thursday, because it was before Harvey. He got out and drove up to Dallas to get out of here, because flights were getting hard to get. He had some ideas. He had been trying to figure out if the moves were right or wrong, and he called me on the way to the airport on—that's right—he called me on the way to the airport on Thursday night.

So I called James [B.] Hadaway, who is one of our senior [advisors]. James and I were kind of teammates. He's a professor at the University of Alabama, Huntsville. If you wanted to extend interviews, that could be another person you could interview if you wanted. I called him, and James is a very intense [person]. When he's working, he's working and he doesn't want to be disturbed. He's a brilliant man. I was trying to explain to him what Gene was trying to tell me. Gene had just left working with James, and he was on the way to the airport but he reached me. I think he changed his mind on some of the conclusions he'd made with James.

And so, James was confident; through Gene's work that day and the day before or whatnot, James was confident in the way the settings were being done. Things were good. I was still questioning it.

Friday, my wife was out of town, and Friday, my kids were off of school because Harvey was arriving. So I couldn't go in, and as it turned out, that was the last day that we made mirror moves. The final alignment of the telescope was done on first and second shift and probably through third shift that Friday. Those moves were very long moves, because we started decentering the mirrors for the first time. We were just tilting them. If you think about two mirrors next to each other, and you're phasing them, you've got a piston term to bring them flat with each other, and then you've got a tilt term to tilt them with each other, and then you can also decenter them. We needed to decenter them by millimeter-like numbers. When we started moving the mirrors in millimeter-like lengths, the move durations went to 4 hours, 6 hours.

So we got in one or two moves a shift. In fact, I think they did two moves and decenters all day, three optimized it. The process, as we defined it, you would go through the process of aligning the mirror. In theory, we were then going to go back and redo it. In theory, we were going to do that, but then Harvey arrived. We had aligned the mirror, and we did it well enough.

One of the engineers, Ben Gallagher, a mechanical engineer from Ball Aerospace, was looking at the interferogram which is how we measure. We use laser light to bounce off of the mirror, and we get a topographic map of the mirrors. He looked at this one mirror, B3, and said, "By golly, that doesn't look right to me." We probably had, on the Harris Team, on the alignment team—I'd probably noticed that but I hadn't really thought about it much. Ben looked at it that night and said, "Something's wrong here." Lo and behold, when they went and looked at the mirror, that mirror, it looked bent. It wasn't right. It wasn't correct. So that was one thing going on.

They had also made these moves that, it will turn out, had decentered the mirror. The software settings—we knew by the end of the following week or the middle to end of the following week, that there were some settings that we had set wrong. We had still aligned the mirror—the mirror was still phased properly—but it was decentered from where it was supposed to be by about half a millimeter. That's your pencil lead, your half-a-millimeter pencil lead. We were supposed to do it to a tenth of a millimeter, so we were five times off of our target.

They noticed that this mirror was bent, so they wondered what it was. We also had targets on the sides of the mirrors to align, to use something called photogrammetry, where, I think I mentioned, we are able to measure the positions of all the outer mirrors using these targets that are attached to the mirrors. In order to do that, that little target sticks out of some insulation, basically a sheet of Mylar on the side of the mirror. We now know that what had happened is the target was hung up on the insulation. So as we were moving the mirror, as the telescope cooled, that insulation hung up on the mirror and was distorting the mirror.

The next day, on Saturday, just as Harvey was hitting, Joe [Joseph] Cosentino from Harris [Corporation], and Scott Knight from Ball, and Ben, and a number of people, they'd moved the mirror down a millimeter and saw the figure get better. They moved it down two millimeters more, and it pretty much got better. It went back to the shape it was supposed to go to. At that point, they had basically discovered that there was contact between this insulation and the mirrors, which there really was not supposed to be. In this case, it was fine. They moved it back, and everything was fine.

Poor Joe, I think he must have worked on third shift that day, or something, and then, they were sleeping in the conference rooms on air mattresses. Jim [James] Tersigni, who I think you interviewed, was shuttling people back and forth from the hotels. They came in to Joe's conference room and said, "Okay, we got to get you back to your hotel." He was flustered and went. He'd left his computer at Johnson, and he didn't get it back for five days. He was stuck in a hotel room. So Harvey arrived on Friday the 24th or 25th, and we didn't go back to full staffing until the 31st.

There were four, five, six days of skeleton staffing where they had two 12-hour shifts a day, and they didn't have any of the alignment team there. We said, "Okay, we're done aligning the mirror. Let's do the tests that we had planned for putting light all the way through the telescope." Those teams went to work, and they discovered that, as they did it—basically, they're putting light through the telescope and imaging it down until they have these star images, simulated star images. The data was showing them that these images were moving around, and they weren't supposed to be moving around. They were supposed to be very stable. So they discovered this, and it was a big concern. What was going on? It appeared that it was happening in about a 40-minute period. It was kind of sinusoidal; it would go up and down and up and down and these mirrors were moving—certain mirrors, not the entire telescope but certain mirrors—the spots from certain mirrors were moving, on a regular 40-minute period.

When we got back on the 31st, there were two concerns. Number one was, after we finished aligning the mirror on that Friday, they would do a photogrammetry run. So you'd measure. We would phase the mirrors. We'd measure with photogrammetry. We would fix it. We would use the photogrammetry to optimize the alignment, then we'd re-phase it, and then you'd do photogrammetry again. That was an iterative process that we had been doing for the previous two weeks. After that final alignment, they went and measured it by photogrammetry, and lo and behold, we were 451 microns misaligned. It's exactly what Scott Knight and I had been talking about earlier in the week. I was afraid it was going to happen, and it ended up happening.

Not the end of the world. The team looked at the data. Joe Cosentino and Gene Olczak, the software engineer and then the software architect, looked at it and figured out the one setting that we didn't have done correctly. We reanalyzed the data, and we showed exactly why we got to where we were. When you have anomalies during the test, you call it a problem report.

We filed that we had problem report, and we presented to the test review board and said, "Look, we did misalign it by half a millimeter. We understand why it was misaligned, and we can correct it. We can correct it. The mirror is properly aligned to itself, so it's properly phased. It's nicely phased; it's just misaligned in global position. We know why it's misaligned, and we could fix it, but it would take us a minimum," I said, "of 16 hours," but in reality, it would have taken us probably 24 to 36 hours, so a day to a day and a half, and if we had a couple of move errors, it could have been two days.

The team, we really wanted, James and I and Joe, and Gene, we really wanted—and Scott—we all really wanted, from an emotional perspective, to realign those mirrors and get it done right. We'd been doing this for 15 years, and we wanted that mirror to be aligned correctly. From a test and bigger picture perspective, we had done what we needed to do. The mirror was aligned. It was aligned correctly. It was aligned to itself correctly, and we could verify the actuator range. Our number one goal was to make sure that when we launched the telescope that there's enough actuator range on orbit to put all the mirrors together, and we proved that. So we had done that.

We didn't have to perfectly put it back this half millimeter, with all the schedule risks that might have happened. Lee [D.] Feinberg and the team at Goddard, and the team as a whole, said, "No, we're good. It's going to be fine. We have bigger things to look at."

Simultaneously, there was a team looking—I don't even think I was quite aware of the second effect yet, maybe I'd heard rumors of it. There was some sort of instability in the mirror, and an instability in a telescope is the last thing you need. I think I'd mentioned that as we were cooling the telescope down, we would measure the primary mirror and see how it changed as a function of temperature. You would correlate that to what the models say it would do. If those two things agree to a fair degree, to some model uncertainty factor, which we were carrying a factor of 1.6, so a 60 percent model uncertainty—it's pretty hard to make these models for exactly how the structure is going to behave from 52 Kelvin to 49 Kelvin, over 3 degrees.

It meets the requirements that we need, but what we uncovered during that testing—we did that test twice. The first time we did it, it was right on. It was perfect. Model and prediction were two straight lines with a slightly different slope and well within the model uncertainty factor. The second time we did it, the measurements were all over the board. Offline we were busy then re-phasing the mirror and doing stuff, but other people were trying to understand why that was. What had happened during those periods of instability? It turns out they had turned instruments on and off. They had turned electronics on and off in the telescope and that, the action of doing that, changed the primary mirror drastically, an unacceptable amount.

That information, combined with this oscillatory behavior that they were seeing in the imagery—there were teams of people, instrument teams. The thermal teams, they were thinking this was a thermal effect. What was happening thermally when they would turn the instruments on and off? What was happening thermally in this 40-minute cycle? What is changing in a 40-

minute period on this telescope to cause these oscillations? I think because of the first anomaly of the instruments, people were looking at that. There's the telescope and the instruments, and there's an instrument electronics package. As they started wondering, that instrument electronics package, the temperature of that was actually changing with a 40-minute period. That started to make some sense, or it made sense to this 40-minute number.

We came back on the 31st. There were two things pointing to these electronics: turning the instruments on or off changed the temperature of these electronics, and switching from the A side to the B side—everything has redundancy on the telescope, so there's two sets of electronics for every instrument. When they would switch from the A side to the B side, the mirror figure would change. If they turned them on or off, the mirror figure would change, as well as the temperature in the electronics. With this 40-minute period, the temperature of the electronics was changing. So we came back and on the 31st, right after Harvey and deep into, "Why did the mirror get misaligned? Okay, we solved that problem; we're okay, and now, why is there this instability?"

And so, we started taking measurements, much more frequently than we had, than we'd ever, ever had. I think we started by taking a measurement every 5 minutes for an hour or two, and then we did it every 1 minute. We did it as fast as we could, which is about a minute. We did that for an hour or something. Lo and behold, we could look at the mirror and we could see something called astigmatism in the mirror, which is like a potato chip. It's a change in curvature of one axis versus the other. If you think about a potato chip, it's bending differently in those two axes. We would see that behavior change. We were able to totally correlate the periodicity of the two. The peak of the temperature and the astigmatism were changing right in line with each other. It became apparent that the electronics were designed with a 1-degree temperature band, which was pretty incredible, a very large number for a telescope that's supposed to be stable to a tenth of a degree or sub-tenth of a degree, and they have a piece of electronics that's varying by that much.

We tightened up the temperature on that. We tightened up the range on that from 1 degree to a quarter of a degree, and then to an eighth of a degree, and we saw this effect diminish greatly. We knew that it was correlated with that instrument, with those temperatures, and eventually we got to the point where we were able to take data something like every 30 seconds, so we were able to really get a lot of data, and really show this. There were teams working around the clock trying to figure out how that temperature way far away—that's a meter and a half away; it's 6 feet away from the surface of the mirror. Why is that changing the surface of the mirror? That story goes forward till the very end of the test. We didn't get to solve that until the very end of the test.

Another instability appeared. The chamber is 20 degrees Kelvin, but our interferometer, the equipment that we have in the top of the chamber to measure the mirror, is in this warm box up in the top of the chamber. That has a little shutter that opens up to measure the mirror, and when that shutter would open up, it would actually heat up. It would heat the telescope ever so slightly. It was not supposed to have any effect on anything, but it was having a huge effect on the mirror figure. Basically, the mirror was potato-chipping; the two wings were potato-chipping up, and nobody really understood why that was happening. And here we are; it's one year [later]. That was August 2017, and now here we are in August of—or September of 2018 and there's still a Failure Review Board going on for that, for that effect.

When they recently announced that it was launching two years later and they were another roughly billion dollars over budget, there were a few sentences in there that hinted at what was going on. There was a comment in there, "some mirror instabilities were uncovered during the testing," that was the limit of what was said in that article. We're still analyzing that right now. I was analyzing data this morning about it.

The heat, as it turns out, the heat from this hole, this 8-inch hole in the top of the chamber, was heating up the insulation on the outside of the telescope. That insulation is very thin, so it's easy for the temperature of that to increase.

I think one of the amazing things that definitely is part of the story is the vastness of this team and the excellence of this team; everybody had their role. We were there to align and measure the mirror, but yet in the background there were people analyzing this thermal data. Even though we'd measured this instability during the thermal distortion test, probably not quite a month earlier but three weeks earlier or two weeks earlier, people had been looking. I had written it off already, but there were people still looking at that data and trying to understand that data. It was that perseverance for excellence and of understanding that really made the test a success and the characterization of this telescope a success, the continued work that was going on three shifts a day among telescope scientists, among optical engineers, among mechanical engineers, thermal engineers, and analysts.

The ability to measure temperature—they realized that when we opened the shutter, the temperature of this stray light baffle on the outside of the primary mirror was going up. I believe the number's something like 8 degrees, a very big number, because it's so thin. It's just a ten thousandths, fifteen thousandths sheet of black Kapton. It would go up in temperature, and as that was happening, the mirrors were tilted, so what caused that, we really didn't know. Our job was to try, at that point, to just measure it. We would close the shutter, let everything settle for a day, and then we would open the shutter and basically measure for something like 24 hours straight. We would open it up, measure. We figured out ways of measuring basically once a minute for an hour, or twice a minute for an hour, and then we figured out, well, we can just do that. We're able to write the data directly to a disk, and we could take as much data as you wanted. I think we measured for two or three hours of data, and it took eight hours to analyze one hour of data, so we had three different computers trying to analyze that.

The teams, there was a balance. There's a delicate balance between the engineers who want to find everything out, and the management who wants to bring the test to a conclusion. I think there's the balance of the dollars and cents, but there's also the fact that every day that that telescope is at 20 degrees Kelvin is a risk. We had the hurricane. During the hurricane, at one point, I think some electronics got wet and they lost telemetry to one portion of the chamber electronics, and the system almost shut down. It didn't. There were people there. It's a risk. During one of the tests, we had a water main break, and that shut down the refrigeration system. If something had happened like that during this test, it would be a very bad situation. You have to understand that it's a risk. You want to find out everything you could find out, but it's also a risk every day you're at cold temperatures. There was a delicate balance there.

We took this data, and we could show that the figure of the mirror changed during that period. We did that test twice, actually. We had the same effect each time. At that point, we pretty much had measured and done everything that we thought we could do. What else could we do? Well, we could do it a third time, but we would get the same answer. We had tried everything. We had taken as much data as we could and gathered the data. After the test, we can try and analyze it. We had two forms of instability. We had this oscillatory behavior that we were able to diminish, and then the teams were, "Okay, well in flight we could tweak the electronics to even get better control" so the effect didn't seem that bad. There's [this] bigger effect, that the temperature change on orbit was 16 times less—it only goes by half a degree on orbit, versus this 8 degree, so it's a factor of 16. You eventually get to the point where the managers say, "Well, you guys have done everything you can do. We're going to stop the test now." We had to present that we had good data.

One of the things you do at the end of a test before you have a consent to warm up—I've been dwelling a little bit in the last half hour about all the instabilities and the problems, but we successfully aligned the primary mirror. Every test that we were supposed to run—I think there were something like 32 tests—we successfully ran them all, and minus these instabilities all of those tests passed. We were supposed to check that the science instruments were properly aligned to the telescope, and they were. We were supposed to properly phase the primary mirror, and it was properly phased. It was misaligned, but as I discussed, we could back that effect out. The instruments were all working properly. It was really a highly successful test, and we had acquired all the data that we thought that we could do to characterize these two effects. So, we had a consent to warm up, and everybody agreed to go to warm up. We started warm up.

This photogrammetry system, essentially [there] are four cameras inside the chamber, and there were other teams thinking about how to use that data. It's probably 300 people working on this test, so another group of people said, "Okay, let's take pictures." There were people wondering, because of the temperature change on this stray light baffle called the frill—that was a smoking gun, certainly. That temperature was changing. It was the only temperature changing when we opened the shutter, so that was a smoking gun. So they took pictures of it at cryo [cryogenic] and then as we warmed up, they took pictures of it. What they uncovered is that at cryogenic temperatures, you could see that this material was under strain. You could see like lines in a dress. You could see it was being pulled, there were stress lines in this material, and at 100 degrees Kelvin, you could see that those stress lines were reduced.

There was a theory at that point that the stray light baffle was, at cryo, was under strain. As it warmed up, it was releasing that strain. That baffle is mounted to the same structure that the mirrors were mounted to, and therefore it was able to tilt the mirrors. Now, we should put it into perspective: these mirrors are tilting by a unit. They're tilting by maybe 100 nano-radians. It's incredibly small. The change in height of one side of the mirror to the other is I'm going to take a guess of five ten-thousandths of a human hair or something, very small numbers, but it would affect science, the ability to acquire science data. We took pictures of it, as we went cold, and we tried to take some data optically as we were warming up. I don't think any of that did much, but we did. For the mirror measurement team, my team—we had to work a lot.

The test was supposed to be over a week after we did our final alignment, and we'd be done. Then it turned out, warm up is a month. At the end of warm up, when we got to ambient temperature, but still at vacuum, we repeated this stability test of the 40-minute stability and the effect was still there. So now it wasn't effective cryogenic temperatures; it was still there at ambient temperatures. That was in probably early October, or middle of October, and then they repressurized the chamber and started going in and doing inspections. They were looking at doing inspections on the whole telescope to see if there had been any hardware interferences where things might have bumped into each other, what did the insulation look like, was there any peeling back, was there any, what was going on.

Probably for two weeks or so, these inspections went on and uncovered that, in reality, there was no hardware interference. That was one of the dangers of this test, bringing this whole thing cold. Nothing bumped into anything else, so that was a really good thing. They did see how these photogrammetry targets did interfere with the insulation on the sides, and the insulation on the sides appeared to not have as much compliance. It wasn't as loose as it was really designed to be. It should have been by design.

They tried to understand what was going on with this electronics compartment, and we tested for another, seems like another week or two, and they tried a couple of things. We always saw the effect. This electronics compartment was being [supported] by ground support equipment. In flight, it [would be] deployed and held very loosely from the telescope. [During the test, on the other hand], we were holding it very rigidly. It turns out that this 40-minute oscillation, once we held this electronics compartment loosely, it didn't affect the wave front really at all. That pretty much closed [the issue of the electronics compartment heating]. By Halloween last year, we knew that that effect wasn't [going to be] a problem on orbit.

This primary mirror instability of the heat, the changes in temperature affecting the edge of the mirror, affecting the mirror tilts, is still under active investigation. We see that they've opened up a lot of the holes, and they've created more compliance in that structure and removed some tape. We can't retest the telescope. We can't bring the telescope back here to Johnson and do another 100-day test. They don't want to drastically change the design, because then you really would have to retest it.

There are some analysts from the Smithsonian SAO, at Harvard-Smithsonian [Astrophysical] Observatory: Lester Cohen and Mike [Michael J.] Eisenhower. They are fantastic mechanical analysts, just the best you can find. They've analyzed it, and we found ways to correct, by analysis, the design that we believe will basically ameliorate what we saw in tests. The fact that we're a year later and we're still looking at this. A month after the test, it seemed like, "Okay, we're ready. They've done a few things and we're ready." I thought maybe, "Okay, they're ready to end this investigation," but you know what? We didn't end the investigation because—maybe it would be okay, [but we all wanted to know the root cause and how to fix it].

[We all] kept looking and kept working, and because of that perseverance, [the team] kept uncovering different effects, "Oh wow, this tape that they put across this seam, is actually causing the forces." Even though we created this compliance, that tape is actually [the issue]. Mike realized that that tape that he sees in this picture—Chris [Christopher A.] Gunn, the photographer at Goddard, took thousands and thousands of pictures of the telescope. They're able to look through these pictures and say, "Okay hey, there's a piece of tape there [that does not belong there]." Mike would then analyze it and realize that that piece of tape is causing strain with temperature.

It's a testament to the perseverance of the team and the dedication of the management to allow the team to continue to analyze these effects, and it's not cheap. A lot of the team is still going. We have two or three meetings a week on these effects that pull a lot of people in, and they're dedicated to getting the answer right. The team at Goddard, Lee Feinberg and the whole telescope team, and Bill [William R.] Ochs from the management level, they're dedicated to getting this right. I think that's the real story that comes out of this, and I think maybe that will. It hasn't really been presented out in the public yet. I think after it launches and is working well, I think that's a story that can really be told, that they spent an extra year analyzing these effects and doing inspections and making minor physical improvements.

We're still considering additional improvements to this stray light baffle to prevent these effects, to ensure that we get good science. What it affects the most is something called coronagraphy, which is when you look at a star, and you want to see a planet in that star. That's very challenging. The star is as big as this table, and the planet is the size of a dime or a quarter. What they're able to do is block the light of the star and see the planet. In order to do that, the telescope has to be incredibly stable, and that's the type of science that would be affected by this. In particular, what they hope to do with this telescope is to block the light of the star, look at the planet, and do spectroscopy on that planet, to see if there's water, or if there's hydrogen, or [helium]. That is the incredible science of the telescope, [what] it's going to be able to do. That's why we need to get the stability, why we're still doing this one year later.

The test has been over for a year. The telescope was probably supposed to be on its way to the launchpad by now. I like the ending of that story, the ending of that. I think it was a lesson to me that I thought, "Why are we still investigating this?" But, you have to have the patience. You have to listen to all the people and what all the people are saying. Listen to them and let them have the time to do what they need to do, and if you do that, the type of scientists that we have working on these programs are going to get it right. It comes down to the people and letting the engineers do what they need to do, notwithstanding the cost and bearing the cost of the program. They haven't cut the corners. They haven't cut a short in that work, and that's a testament to the type of work that needs to be done to make these missions a success.

That story will be told, I'm sure, when it's a good story in the end and we have a telescope that's working, and it's doing this great science. If it gets out there and all the deployments work, or [it's] as stable as it's supposed to be—or maybe that'll get lost, who knows? Once you're getting good science, that gets lost. It'll be someone who writes the book someday, for the people who want to know the inside story, and that's really, I think maybe it's the opposite of what happened with the Hubble Space Telescope.

ROSS-NAZZAL: That's what I've been thinking about this whole time that you've been talking about this.

WELLS: Right. On the Hubble Space Telescope, they had two pieces of data. These were interferometers, so it's the type of work that I do. They measured the primary mirror. They had one piece of data that said the primary mirror was good, and one piece of data that said the primary mirror was very flawed. They buried the data that said it was bad and took the data that said it was good and didn't have a test program that would have found it at the telescope level. They didn't really do a system-level test like we did here. The Perkin-Elmer Team that won the Hubble Space Telescope did not have a system-level test. The Kodak Team that bid the project had bid a telescope-level test that made their—who knows why it was really chosen. The rumors I've heard is the cost of that extra test is why is was thrown out, and why they chose the other team. In the end, the Kodak Mirror is at the Smithsonian, and the Hubble Mirror is up there.

Each telescope has its advantages. The Hubble was serviceable, and they were able to fix it. The James Webb Space Telescope, the primary mirror is deformable. If we had an error just like the Hubble, a static problem like that, this mirror is very forgiving, the James Webb Space Telescope, because you could tilt the mirrors to compensate for that. It wouldn't be perfect, but as long as it was stable, it would be fine.

There are other [telescopes in the works]. We're working on the next space telescope, the WFIRST Telescope, the Wide Field Infrared Survey Telescope, that we're working on now. It's an infrared telescope, but it has a much more sophisticated coronagraph in it. It has stability requirements that are orders of magnitude tighter than what we need on the James Webb Space Telescope.

The type of lessons that were learned in this test—you need to worry about the temperature of insulation and the strain just from a piece of insulation and the strain of wires. It turned out the 40-minute oscillation, the physical mechanism that was causing the distortion was being pushed through the wire bundle, that the wires could actually—the wires, not structure—forces in the cables, can distort the telescope. So I think the lessons that were learned are going to pretty readily be passed on to the next missions, the next mission, I would say, at least, because it's the same team. It's the same Goddard team, and it's the same team from Harris, from former Kodak working on it, as well as a team from Ball Aerospace working on it, so, those people will be quite familiar with it and will look out for it. As you go out, the decadal survey in 2020, they're going to come out with what's the next generation of space telescopes; that maybe starts in 2030 or 2035. Hopefully, these lessons will be understood by those people as well, which, frankly, the lesson from the Hubble is probably why management was given the freedom to really investigate this and giving the engineers the freedom to investigate these effects to the end.

It's funny. It's a good story. If all this analysis is right, it's a really good testament to the thoroughness of NASA in this project, the contractor teams and NASA on this project.

ROSS-NAZZAL: Absolutely, yes, I think that's great.

WELLS: Another year later, yes, we were just in the meeting this last week. It was actually the one-year anniversary of this particular Failure Review Board actually being formed, because that makes sense. It's September, so that makes sense. We probably discovered these instabilities in August. You start in a test with a problem report that can then turn into a PFR, a Preliminary

Failure Review, and then it becomes an FRB, a failure. Right now it's a full-on Failure Review Board, with a full committee. I'm an adjunct member. Some people aren't there. I'm a voting member of the board. So it's been a year, and it's still going.

ROSS-NAZZAL: Well you want to get it right.

WELLS: You do want to get it right.

ROSS-NAZZAL: You're not going to be able to service it like Hubble; it's going so far out.

WELLS: Yes, the other lesson, and that is where the failure was here: is that the smallest things actually matter. Unfortunately, the thermal management system, the TMS, it turns out, was one of the last things done, the thermal baffling. Not a big deal. Well, turns out it was. So, you never know what it's going to be.

ROSS-NAZZAL: Yes. Well, it's a good story, and I appreciate you coming by and sharing it with us.

WELLS: Yes, I think it probably could be told by someone in a few years, when it's time to tell, but telling it now I think is a good thing, and while it's fresh in my mind.

ROSS-NAZZAL: Oh, absolutely, the memory fades after awhile.

WELLS: Yes, it was amazing. You're on your own little world. I was just worrying about phasing the mirror and trying to spend a little time with my family and get some sleep. There were all these people working in the background that I didn't even know about. Come back from Harvey, and there were people had all these theories already. People had been working the whole time through Harvey, it turned out, and somehow managed to get food, and water, and shelter, as Jim Tersigni has told you. Yes, in the end, we got pretty much all the data we could have gotten. If we had tested for another month, we wouldn't know any more than we know, I think, right now, so, it was a very successful test.

ROSS-NAZZAL: Well, I look forward to seeing what happens next.

WELLS: Yes, we hope.

ROSS-NAZZAL: I'll be watching it a little more closely now.

ROSS-NAZZAL: Is it 2023? I'm trying to remember. The administrator came out [and announced the date].

WELLS: Right, it's two more years from now. I think it's 2021.

ROSS-NAZZAL: Is it? Okay.

WELLS: Spring of 2021, because it was going to be 2018. It was going to be this fall they were going to launch, and then they announced to 2019, pretty quickly, and then they did the two-year delay.

ROSS-NAZZAL: Oh that's right; it was only two years. I don't know why I was thinking that.

Well: That delay, once it got to Northrop to do the final stuff, the final observatory integrations and things, the fact that they were behind schedule and had challenges really came forward. This was in the report as well, "They had an anomaly in the vibration of the spacecraft." The spacecraft is the spacecraft electronics and the solar panels and the communications electronics, as well as the sunshield, and when they put that through an acoustic test to simulate the noise of launch, they found nuts, bolts, and washers on the floor. That's why you do the tests. You do the tests, and that's something that's readily fixable they found. The design was correct; some of the drawings were wrong. That's a readily fixable thing, and they also cleaned the propellant lines with the wrong type of solvent, so they have to replace those. Again, it's a fixable thing; they found the problem. It's delays, and the marching army costs.

ROSS-NAZZAL: Yes, nobody works for free, that's for sure.

WELLS: Nobody works for free, and there are teams of people who have been training to launch this telescope and commission it. In the fall of 2018 I was going to maybe get on that team. I'm not on it, but now those people, they're still doing exercises. They have to stay fresh. It's the same team that ran the test here, a lot of the same people. They have to stay fresh with the software systems, and the flight software and things, so they're still doing—I don't know if they do it quarterly, or—simulations of commissioning the telescope a couple times a year, so that when they're ready in two years, they're still going to be mentally sharp to know what's going on. I imagine, if you're on that team, that's a challenge. Now you've got a two-year wait to have to do your job.

ROSS-NAZZAL: It's interesting. I spoke with Begoña Vila, and she mentioned she was going out there because she's in charge of the science instruments, so still working that aspect.

WELLS: Yes, I think they still have some challenges, I'm sure. Hopefully they can get the spacecraft right, because I think the telescope is going to be okay.

ROSS-NAZZAL: I hope so, thanks to your work.

WELLS: All right.

ROSS-NAZZAL: Well thank you.

WELLS: Good.

[End of interview]