DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

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INTERVIEWED BY ERIK M. CONWAY
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CONWAY: Okay, my name is Erik Conway, I am talking to Jim Fanson. I think you're the project manager for SPHEREx [Spectro-Photometer for the History of the Universe, Epoch of Reionization and Ices Explorer] now, and before that you were the Kepler project manager, is that right? How did you join the Kepler mission?

FANSON: I joined the Kepler mission as a result of a problem that the project was having coming out of a major budget rescission I think in 2005. They had like a 50 percent budget rescission by NASA, so they let go of a lot of the team at the contractor Ball Aerospace and at JPL [NASA's Jet Propulsion Laboratory, Pasadena, California], and they wound up spending money, but not getting a lot of work done. I think it wasn't a terribly well-organized situation after that impact to the project. It became clear that they were not going to have enough money to finish the project, so JPL put together a team that was cochaired by Leslie Livesay and Pete [Peter C.] Theisinger to review the situation for the project.

They began to assemble a new management team, and I was brought in by Leslie as part of that assessment team, and then I got a position as the deputy project manager to Leslie as we worked through a replan and tried to get the project on a stable path forward and get NASA to agree to support the plan going forward. I came on board, I think shortly after Leslie became involved in that assessment process. Leslie, ultimately, became the project manager for a while, and then I became the project manager after we had delivered the photometer part of the flight system. It was a period of trying to respond to implementation difficulties, spending resources faster than was sustainable, and the need for a different approach to the project to get it back into a controlled, stable forward plan.

CONWAY: What had been the prior project setup that was causing it to not perform within budget, granted that you already mentioned there was a rescission too?

FANSON: The Kepler was an interesting arrangement. It had been proposed by Bill [William J.] Borucki who is a scientist at NASA Ames [Research Center, Moffett Field, California]. He had been at NASA Ames since the Apollo Program. He *is* an interesting individual. There was a struggle for a long time to figure out how best to detect extrasolar planets or planets orbiting stars other than our Sun, and there are various approaches to that.

What NASA did was to organize a workshop or various teams to study various options, and Bill Borucki had read a paper some time ago, I think, written by a physician of all things, who had this concept for discovering planets around other stars by looking at a large number of stars. Just due to the random statistics of how planetary systems are organized, some of those stars would have the plane of the planet orbits lined up with our line of sight from the Earth, such that planets, when they orbited the star, would pass in front of the star for a period of time as seen from the Earth. It would block some of the light from the star causing the brightness of the star to dim very slightly for a period of time and then it would return. Every time the planet would orbit the Sun and pass across the disc of the Sun, you'd see this repeated diminution of the star's light. From that, you could make some assessment of what the size of the planet was, what the orbital period was, and how far away the planet was from the star, all kinds of interesting things.

He became an advocate for this approach in the NASA formulation era for how to detect exosolar planets, and he came up with a mission concept, which he proposed to NASA. The difficulty, in part, was that the NASA review panels had a hard time believing that a measurement as sensitive as what you would need could be made. We're talking about changes in the brightness of a star at the part-per-million level, and stars themselves fluctuate at that level just naturally. NASA had a lot of skepticism that such a measurement could be made. So they would not select the mission, but they would give Bill some study money, and they would say, "Go off in the laboratory and prove that you can do this," or "Take this to a telescope and prove that you can do that." I think he proposed the mission five or six times before it was finally selected by NASA.

The home institution, if you will, of the Kepler Mission was always the NASA Ames Research Center where Bill Borucki was located. But by that time the mission was selected, NASA had essentially decided that robotic mission developments would be led by either the Goddard Space Flight Center [Greenbelt, Maryland] or the Jet Propulsion Laboratory. While other centers had done missions in the past, Ames had managed the Pioneer missions and Langley [Research Center, Hampton, Virginia], the Viking mission. At this time frame, Ames was told, "Yes, we'll select this mission, but you have to work with either Goddard or JPL as the lead center." I think Bill Borucki wanted to team with the Goddard Space Flight Center, the Ames Center Director wanted to team with JPL. In the end, JPL became the mission implementation center.

But interestingly, NASA decided, in the spirit of that PI [principal investigator] institutions often have the role of delivering scientific instruments. They said, "JPL will be the lead for the mission implementation." (And JPL said for them to do that, they have to have the contract for the spacecraft, which was procured from Ball Aerospace) But they told Ames, "You can be still in charge of the contract for the science instrument," which we called the photometer on Kepler.

It was basically a wide-field telescope, it had a Schmidt corrector plate, basically a Schmidt camera. It was a sophisticated telescope with a very large focal plane camera for that time. It had to make these very difficult measurements, these part-per-million photometric measurements.

It was really, the hard part of the flight system. They said, "Ames, you can still be in charge of that," and they had selected Ball to supply that. So we had this really strange arrangement where there were two NASA Centers involved—JPL, which held the contractual tether for the spacecraft and the observatory integration and test, if you will, and Ames that held the contractual tether for the photometer. Both of those contracts coming into the same company—it was not the best organizational structure for clarity of decision making and who was in charge of what, coming into the same company. Whenever we had a change in something that affected the interface, for example, between the spacecraft and the photometer, it involved often a change in two contracts with two different contracting entities at two different NASA centers, and it was cumbersome.

Part of the issue was that there was this complex organizational management structure, if you will, for the mission. In my experience, NASA often has difficulty in the robotic mission field when it splits responsibility between two or more NASA centers, and you have more than just an instrument being delivered from one of the centers to the lead center. In this case, I think it was the hardest part of the flight system, the spacecraft was being built from Ames, and the responsibility was evenly divided between the two centers, and that was not a smooth experience. I think if you look historically back at NASA where the Hubble [Space] Telescope was split between Goddard and the Marshall Space Flight Center [Huntsville, Alabama], there were issues there, friction between the two centers, and we had a similar system like that.

I think the other contributing factor to the difficulty the project had was that Ball Aerospace, which has its history in scientific instrumentation, only got into doing spacecraft and missions later on. Because no one had ever built anything like this before, the full scope or the full magnitude of the technical challenge was not understood very well. Until Ball got into the thick of the engineering, they didn't realize the resources that would really be needed to do this mission. There was a component of underestimating how difficult it was going to be to pull this off technically. I think that the Ball Corporation was having trouble executing their plan. Then there was a budget rescission that threw our branch into the works and created a lot of chaos, and the divided management structure between Ames and JPL, those all contributed to difficulty in the 2005 time frame.

CONWAY: Okay, so you come into the project with Leslie and some others. What do you do to restructure it, that complex set of contracts? I presume you had to have [NASA] Headquarters involved because of the interface between the two centers?

FANSON: Well, we didn't have NASA involved beyond what you would expect. Kepler was part of the Discovery Program Office, which was run out of the Marshall Space Flight Center, so of course, we had interactions with the program office. Another curiosity here is that Kepler is really a telescope mission, so it looks like an astrophysics machine, but it was in the Discovery Program, which is typically solar system exploration. It was an odd fit where the Astrophysics Division at NASA was responsible, in some sense, for the science of this mission. Exoplanets didn't really have a home at NASA, it wasn't really astrophysics, it wasn't solar system, but astrophysics did not have a large, competed mission opportunity. They had the explorers, but they didn't have anything big enough to handle something like the Kepler Mission. So Kepler got proposed to Discovery, and it was managed out of that program office.

The relationship between Ames and JPL was a direct one, and here, we had a little bit of friction with the PI, with Bill Borucki. Bill had been, as I mentioned, at Ames for a long time from the 1960s, and what he observed was that missions would be formulated at NASA Ames and get started at NASA Ames, but then NASA would move them to JPL. Let's see, what was the first infrared mission?

CONWAY: IRAS [Infrared Astronomical Satellite].

FANSON: IRAS, the Infrared Astronomy Satellite, was started at Ames and got moved to JPL. Then the Space Infrared Telescope Facility that Mike [Michael W.] Werner was associated with for so much of his career, again, a mission that had been formulated and started at Ames and got transferred to JPL. That's when Mike Werner moved from Silicon Valley down to Pasadena. I think that Bill Borucki was suspicious that the same thing would happen to the Kepler mission, and so that's part of the reason, perhaps, that he preferred to team with Goddard rather than JPL. But what we discovered was that there was a degree of, I'll call it, paranoia about JPL and not trusting JPL in this partnership that had been brokered between the centers. That was definitely part of the difficulty of a smooth, unified command and control scheme, if you will, for managing the mission. We had a direct relationship with Ames, but it was somewhat a troubled relationship.

Then as we were going through the replanning process, you asked, how do you replan when you're in a situation like that? We put together a team to work with Ball, since Ball was the "doing" part of this for the most part. They had to design and build the flight system. We had to work with them in an iterative fashion to come up with a forward plan that they believed they could execute, that we believed could be executed—a credible, executable, forward plan. I remember we went through about a dozen iterations of that planning and then we would convene a review with NASA.

There were some important personnel changes that occurred while we were doing that multi-month-long replan. One change occurred at Ames, where I think [G. Scott] Hubbard was the center director at the time that I arrived on the Kepler Mission, but then [S.] Pete Worden came in and became the Ames Center Director. Pete's kind of a swashbuckling character, a larger-than-life figure. He swooped in, brought some Air Force people with him, and of course, they didn't know anything about the mission, so we had to bring Pete up to speed, get him and his people engaged.

Another change that occurred is I think that Wes [Wesley T.] Huntress had been the Science Mission Director and Associate Administrator at NASA, but then Alan Stern came in into that role. I might have that wrong, and it could be there was another person intervening or intermediate between Wes Huntress and Alan Stern. But Alan came in with an attitude that we're not going to put up with these cost overruns anymore on these NASA missions, and we're going to insist on efficient, effective management. He was looking, surveying the portfolio that he had at NASA, and Kepler stood out as a problem at that moment. It was clearly not going to be able to finish on the path that it was on, and so Alan ultimately became the person that had to be briefed and had to make a decision on whether Kepler was going to go forward or not.

I can remember, we gave a briefing to Alan and his group from NASA that indicated the result of the replan and how much additional resources we felt we need to finish. Alan sat in the review for two days, didn't have anything to say. At the end, he thanked us for doing the presentations and the work to prepare the plan, but he said, basically, "You've got 30 days to come back with a plan that doesn't require any more money, or I'm going to cancel this mission." So that put us all back to the drawing board, and I think it was 30 days we had to put together a revised plan.

At that point, that's when Leslie was working with the Center Directors at JPL and Ames as well as the executive leadership at Ball Corporation on what changes we could make that would enable us to go forward without asking for additional money but would still be able to finish with an acceptable risk posture. The interesting thing to me about Alan Stern's approach to this was that he was open to changes that needed to be made to allow things to be done at lower cost. So things that I didn't think NASA would ever agree to do, he just waved his hand and said, "Make it so."

We came back in a month's time with a revised plan that involved Ball making some compromises. Ball put some of its fee that it was guaranteed to earn under the contract. If the mission had gone to completion, they agreed to put that back into the project reserve with an opportunity to earn that back as we went forward. JPL offered what JPL could do to try to make offsets to the mission to reduce costs, and Pete Worden did the same at Ames. The story was acceptable to Alan Stern, and there were other changes that we asked to make, and we went forward after that.

I have to give a lot of credit to Leslie Livesay. She very deftly handled the astro-political aspects of dealing between the centers and with NASA management. But it didn't mean that it was an easy job to finish at that point going forward because there were still surprises on how difficult it was to build the Kepler photometer that could meet the performance. It was still a difficult road ahead, but at least, we got the green light from NASA to proceed.

CONWAY: From the articles that you and Leslie and a couple of others wrote, I guess there were replans in 2006 and in 2007. So you're talking about the 2006 one, or did you combine those two things, just so I know which we're talking about?

FANSON: I think the meetings that I've described with Alan Stern were in the summer of 2006, and there were probably adjustments that had to be made in 2007 as well as we went forward. Some of these are probably programmatically sensitive to talk about, so I don't know that we would want to go into that and record it, or you'd have to put a blackout on it for 50 years or something. But there were some adjustments that had to be made in personnel and organization structure that occurred a little bit later.

Of course, the centers made good faith commitments to certain offsets and so on, which I think in practice, it was found very difficult for the centers to actually do that, to actually implement those things. I'm sure that there were some adjustments as well in 2007 and 2008 as we approached the launch.

CONWAY: One obvious personnel switch was that Borucki ceases to be the PI, and it's no longer a PI-led mission. Can you talk about how that came about, or is that one of those things that's too sensitive to record?

FANSON: Well, what I will say is that when we approached Alan with the changes that we felt needed to be made to allow us to implement it at low cost, one of those was that the PI would report to the project manager as more of what a project scientist would be on a directed mission. Whereas typically in a PI-mode mission, the PI is the person in charge that makes the key decisions. I mentioned earlier that I thought Bill was perhaps a little inexperienced and a little suspicious of JPL, but there needed to be strong leadership at the lead center.

JPL was the lead implementing center, and that was one of the changes that we made. There were other adjustments that were really forced later on by the science team. I think the science team felt that it was difficult to work with Bill. While I was project manager, there was continuing narrowing of Bill's role and responsibility. This speaks to the need to select a principal investigator that has the right set of skills to work in that position on a spaceflight mission.

The rationale for having a scientist lead a PI-mode mission is very well justified. You want someone that can deliver an investigation, that can make the tough decisions on what's the minimal impact to the science if you have to make some compromises for cost or schedule. But the person also needs to have enough experience to feel confident delegating project management responsibilities to the project manager and working within that system. I think Bill had never been the PI of a spaceflight mission before or possibly even an instrument, so I think Bill was a little bit out of his element there, and that created some implementation difficulty for us.

Alan agreed that he would reclassify Bill's role, and he did that on the spot. Alan waved his hand and said, "If that's what you want done." I remember in the corridor after the big briefing, the second big briefing we did, Alan Stern basically pointed to Bill and said, "You now report to her now," pointing to Leslie. So he was very decisive about it, very direct, quick to make decisions, and just get on to the next challenge.

CONWAY: Okay, fair enough, and thank you. Let's talk about your next challenge, the photometer since that seems to have been where your largest headaches were. Tell me what were the challenges in completing it?

FANSON: So to give you some idea of how difficult the measurement is that Kepler was performing, at least at that time, we were trying to detect, have a sensitivity to be able to detect an Earth-sized planet orbiting a Sun-like star from somewhere away in the galaxy. That was a part-per-million-level measurement, and we did not have the ability to simulate those conditions to actually test the photometer on the ground to that level of sensitivity. We couldn't, for example, construct a stable enough light source to be able to make that kind of a measurement on the ground. So, we had what I would call a piece-wise verification and validation program where we could do some things by analysis.

What they did up at Ames was they constructed a laboratory test setup where they could simulate this level of precision on a very small scale to verify certain aspects of this measurement. Then we would do analysis on other things, and we would test at the flight system level, and we would put that story together piece by piece that, in an end-to-end sense. Then you could get some confidence that it would actually work in flight. But it was a very difficult measurement. It was difficult to measure or to build something at that level of measurement sensitivity and then actually to verify it on the ground.

There were many other more mundane challenges for the photometer. One of them was that the focal plane was very large, particularly for that time for spaceflight focal plane. I think Ball discovered that it was just much more difficult to manufacture, package, and assemble the focal plane. There were some compromises that were made in installing focal plane modules that would engage blind-mate connectors. Basically, if you go to the JPL design principles, it says don't use blind-mate connectors, they're not reliable enough. In order to be able to package it and assemble it, that's what Ball resorted to, but then there comes the additional challenge of ensuring that things are reliable, and accessible, and so on.

We had difficulties with the manufacture of the composite structural components. So when you look at the Kepler Mission, there's a solar panel that shades the photometer from the Sun to give it a more stable thermal environment. The body of the photometer is a tapered cylinder that supports the primary mirror at one end and this large corrector plate, this lens at the front. The best composites manufacturers in the country were contracted to manufacture those components, and we just discovered that they had a great deal of difficulty manufacturing the composite structures. Part of that I think they attributed to not having enough involvement from a manufacturing perspective when they were designed by Ball. But part of it was they just, themselves, underestimated how difficult it would be to manufacture those structural components.

Another challenge we had was with the primary mirror. In order to get the reflectance that we needed for Kepler, we wanted to put a protected silver coating, reflective coating on the glass primary mirror. Silver is a much more difficult coating to apply to a mirror, and there were not very many companies that had the ability to do that. There was one company that did this for military missions, and it was a very highly classified environment, and they basically said, "Well, if you ship your mirror in a crate to our shipping dock, we'll phone you when it's coated, and you can come pick it up, but we can't tell you anything about it. You can't come in here and watch us work." We found that a little less than satisfying process.

We had the mirror up at a company in Northern California that had successfully coated some sample mirrors with a reflective silver coating. But their schedule was being dominated by a defense project with a so-called DX [Defense Priorities and Allocations System] rating, which gives them a national priority in the manufacturing world. It was unclear if they were ever going to be able to get to our mirror and get it coated in a time that would work for us, and of course, science missions are the lowest priority in the national scheme.

We then went searching for a company that could do this, and we found a company near San Diego that had the ability to do a protected silver coating, but they had never done anything on the size scale for the primary mirror for Kepler. So we basically rolled up our sleeves and worked with them to recapitalize their facility and get to the point where they could coat the primary mirror for Kepler. So it was successful, but it took a lot more time and effort than we had expected.

At any rate there were there just many stories like that, as there are with any space mission probably, of the technical challenges you discover and have to overcome all the way from technology through design challenges, to the supply chain. And whether you have access to the people who can do something or you don't have access, you have to construct some other way to get the job done. I could go on, but there are many examples.

CONWAY: One question I like to ask, and may be rough on your memory, but when you came into the project, you must have had some ideas of what you thought your chief risks would be versus what did they turn out to be. What did you think they were going to be before you embarked in the project?

FANSON: This is a little hard for me to reconstruct from 2005. I think my concern was for the ability to build the focal plane assembly and the readout electronics. Often, that's the highest technology that you're flying, and surprisingly, the detector arrays themselves were really not the problem. The problems we wound up having were the more mundane things that you wouldn't expect to have a problem with, like building composite structures for example or putting a reflective coating on a glass optic. We had to learn as we went forward where the problems are, and one of the things I've learned over my career is that oftentimes, the problems occur where you're not focusing your attention. You tend to focus your attention on the hard problems, and often that successfully manages the hard challenges, and it's the more mundane things where you're not focusing your attention that become the problems for you. That was certainly part of the story with Kepler.

CONWAY: Yes, I've heard all sorts of variations of that. That it's the thing you weren't looking at that bites you, but you can't look at everything all the time.

FANSON: That's it's difficult to be everywhere all the time. Another thing that surprised me, maybe it shouldn't have surprised me, so I'm taking that experience into my current assignment, is that when you work at the level of measurement sensitivity that we needed for Kepler, lots of things that would be ignorable on other missions matter to you now. The behavior of the focal plane arrays and the signal chain electronics, the crosstalk, the nonlinearity, the noise that is injected into the system, whether it's conducted interference or radiated interference.

What we discovered is when we started testing the system was that there were a lot of nonideal behaviors, which were nonlinear and time varying and had to be calibrated out in the data processing tools built on the ground. Some of these things we didn't actually measure until we were in orbit, until we were in space, and we had the stability of the space environment we were seeing at the level of sensitivity that mattered to us. So it was a real challenge, and this was the responsibility of the of the team at NASA Ames. There was a lot more effort required, and it took a lot longer to develop the tools that could calibrate out these effects so that you could extract the light curves ultimately from the Kepler data that were used for exoplanet discoveries. It was a process of building modules and tools and doing successive processing of the data. You go back and reprocess the data and reprocess the data, cleaning out these effects and calibrating out these nonideal behaviors. Because some of them were nonlinear and some of them were time varying, it was a real challenge. There was a real serious menagerie of these creatures that had to be dealt with. That was a surprise, but perhaps in retrospect shouldn't have been, recognizing that we were working on the limits of sensitivity of the instrumentation.

CONWAY: So it sounds like the commissioning of the telescope phase took longer, unless that's not part of it, unless that's a different phase of the project?

FANSON: Well, much of this had to do with the post-processing on the ground. There was a period of time of taking data, trying to understand what these characteristics were, and then building the tools to calibrate them. You could collect the data, but you'd have to go back and reprocess the data, and basically clean the data of these artifacts and these nonideal characteristics, calibrate that all out.

CONWAY: So then that actually went on for, at least, part of the science phase?

FANSON: It went on for the science phase, and I think there were subsequent releases of the data that involved reprocessing of the data, going back to the beginning. In each successive release, the quality of the data was higher.

CONWAY: Okay, we have about 10 minutes left, so tell me who the other key team members were?

FANSON: Oh gosh, well, at JPL, I've already mentioned Leslie Livesay. Peg [Margaret A.] Frerking became the payload instrument manager, and it was her job to deal with some of the difficulties with the photometer. Riley Duren was our project engineer working alongside our project systems engineer. Brian Cooke was our project systems engineer. We had many other people on the team that had important roles at Ball Aerospace. Monte Henderson was the program manager when I arrived. Eventually John Troeltzsch was promoted to be the program manager at Ball, a very capable individual, he still works at Ball, pleasure to work with him. I'd have to bring up the roster to give you the full list of key people, but those are people whose name come readily to mind.

CONWAY: Okay, and talk about communications within the team, with the contractors, even Headquarters. It sounds like you didn't actually have that much beyond your Alan Stern moments, et cetera. How was it done and what kinds of frequencies?

FANSON: Well, we had a management team, which is a typical management structure for a project that would involve the leadership from each of the key players: JPL, Ames, and Ball were the three main players for Kepler, and we would meet as a team on a weekly basis. We had a very strong email infrastructure at that time, so we're in some sort of an electronic realm in that era. I mentioned Ball and JPL, but key players at Ames included Roger Hunter and Charlie Sobeck, who became the project managers during the operations phase. Ames led the operations of the system.

We had people at IPAC [Infrared Processing and Analysis Center] that were involved in doing the catalog, archiving, and making the data available to the community.

We would identify the issues and figure out how we were going to tackle them as a team, and we communicated on a daily basis by email, by phone, and we would have frequent face-to-face meetings. The "humanity" is very much enhanced by having face-to-face meetings, and so we did that where we could.

I didn't mention some of the changes that we had made to be able to implement at lower cost. But Ball Corporation also essentially reorganized, and they brought in a team of people from Ball who were working in the commercial space sector where they would work under fixed price contracts. Within the Ball Corporation, they had their own tailored, streamlined way of getting work done. They cut out a lot of the process complexity, you might call it red tape-type of issues. Cary [W.] Ludtke and Alan Frohbieter became the team that led the effort at Ball, which helped a great deal with efficiency and streamlining of the management approach. They were also key players in the implementation.

CONWAY: Okay, that's good, thank you. My classic final question is what haven't we talked about, about Kepler that's important? In other words, what didn't I know to ask you about?

FANSON: One of the interesting things to me about the Kepler project is that it was very noble work in the sense that as long as intelligent creatures on the Earth have been looking at the stars, humans have been wondering, "What are the stars?" and "Are there other places like the Earth in the universe?" There were a succession of revolutions starting with the realization by Copernicus that the Earth revolved around the Sun and not the other way around.

The Earth was not the center of the universe. Then the realization that the solar system was in a rather obscure part of the Galaxy. Harlow Shapley was the first to actually make an estimate of the size of the Galaxy, and Edwin Hubble realized that there were other galaxies apart from the Milky Way. So there's been this succession of revolutions in astronomy and our understanding of the universe. We didn't know whether there were other planets orbiting other stars basically until 1995. The first planet orbiting a normal star was discovered in 1995, and it was difficult to discover exoplanets because it's a difficult measurement in most cases.

Kepler was designed to do something very interesting. It was designed to do a survey of a large slice, a representative slice, of the Galaxy with a large number of stars, like 250,000 stars or so, and basically do a census of what kinds of planets orbit what kinds of stars in the galaxy. The likelihood that a planetary system will happen to be aligned with your line of sight from the Earth so that these transits can occur and you can measure the light fluctuation from the transit, that's only a few percent likelihood that a planetary system be aligned like that. Most of the stars that you're looking at, you're not going to see anything, so you have to look at a large number of stars if you're going to get a statistically significant sampling of actually finding planets around stars.

The Kepler Mission was designed to do this survey, and it was designed in such a way that if we did not find planets, that it would place very tight constraints on the likelihood that those planets actually existed. So before we launched Kepler, we did not have any idea what kind of planets orbited what kinds of stars. We had poor statistics, a few discoveries that had been made.

By the time we ended Kepler, we knew that rocky planets like the Earth are fairly common, that there are many planets that orbit in the habitable zone. We can actually make predictions of how many planets there would be in the galaxy of different types. We discovered that the most common type of planet was a super Earth, that giant planets like Jupiter and Saturn in the outer solar systems was not typical, that our solar system is not typical for solar systems in the Galaxy. So these things were, in some sense, like a completion of the Copernican revolution where not only were we aware that the planets of our solar system orbit the Sun, but now, we can discover planetary systems orbiting other stars in the Galaxy.

In some sense, this was the culmination, the closing of an arc, the answering of a question that has come down from the millennium in the human history of the planet Earth. We live in a very privileged time to be able to make the first scientific measurements and say something about the prevalence of planets around other stars in the Galaxy. That's a pretty satisfying piece of work to do in your career.

CONWAY: Yes, I can imagine.

FANSON: Yes, there aren't very many moments like that where you make the first kind of census of a type of object like plantes that might be habitable.

CONWAY: Yes, I've tried—there's two different Ames research center historians now—in getting Ames to write a history of the mission, and I've gotten nowhere. They're just not going to do it.

FANSON: Well, that's unfortunate because Ames is the home for where this idea came from. I'll give you another piece of information. So I mentioned that NASA had these studies early on about how to go about detecting exoplanets from space, and that Bill was the advocate for this transit method, this photometric method. There were several studies, but I was involved in one study as a facilitator. People in the community just did not believe that such a measurement could be made.

This is partly why it took NASA four or five times before they would select this mission to go forward. It's just the skepticism that the measurement could be made. But Bill persevered, and this is another example of where you have to persevere with a vision in order to be successful. I think people were astonished at how successful the Kepler Mission was, and that you could make these measurements at the sensitivity required.

We all owe a huge debt of gratitude to Bill Borucki. I have an enormous amount of admiration for him. He stuck it out when people told him that it couldn't work, he just kept his nose down and kept working, solving one problem after another. It's a great story for Ames that they were the incubator for this concept and for this mission which was such a stunning success.

CONWAY: Yes, and on that note, I think we've got to sign off.

FANSON: Okay. Always a pleasure to talk to you, Erik.

CONWAY: Yes, thanks very much.

FANSON: You're welcome, thank you.

[End of interview]