DISCOVERY 30TH ANNIVERSARY ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

WILLIAM J. BORUCKI
INTERVIEWED BY ERIK M. CONWAY
MARCH 20, 2024

CONWAY: This is Erik Conway. Today is the 20th of March 2024, and I am talking to William Borucki, now retired from the Kepler mission, initiator of the idea, and PI [principal investigator]. So please tell me what your education was.

BORUCKI: I'm William Borucki. I'm a retired scientist at NASA Ames [Research Center, Moffett Field, California], and I'm here to answer your questions. Your first question [that you sent] was: "According to your bio as a physics student originally, how did you come to work at Ames?"

My answer: Since the time I was a boy, I've always been interested in space exploration. My interest was fired by reading science fiction, and when I left college, I applied *only* to NASA research centers. When I received multiple offers from NASA, I followed my father's advice, "Go west."

I joined the Hypersonic Free Flight Group at the NASA Ames Research Center to do research related to the Apollo heat shield. The physics and the chemistry involved in the reentry fit interest and my educational background.

CONWAY: What interested you in aerophysics?

BORUCKI: The challenge of obtaining spectra of hot plasma covering the heat shield of a model traveling in a laboratory ballistic range at super-orbital speeds was something I simply couldn't resist. The measurement of the spectra needed to be completed in two-tenths of a millionth of a second. That tiny exposure time is *billions* of times shorter than what I saw in the research labs at the University of Wisconsin-Madison where I trained. I thought of this problem as a wonderful challenge.

CONWAY: Then you left that and shifted to chemical modeling, around 1972. What was your motivation then?

BORUCKI: In 1971 all of the personnel in the Vehicle Environment Division were fired after NASA had successfully landed the astronauts on the Moon and returned them safely to Earth. Upper management felt that there was no longer any need to continue research on heat shields.

After searching for another research position at Ames, I chose the Theoretical Studies Branch in the Space Science Division. I joined an existing research team that was developing mathematical models of the chemistry occurring in Earth's upper atmosphere.

I would also have available time to pursue my own interests. I enjoyed working with a knowledgeable and motivated group to quantify the effects of pollutants on the Earth's atmosphere. Mathematical modeling of the Earth's atmosphere was different from the experimental work on heat shields that I had done previously, but it was still an opportunity to use physics, chemistry, and mathematics to solve important problems. I was in.

CONWAY: I have a similar question about the lightning work that you did in the seventies and eighties. What was interesting about planetary lightning?

BORUCKI: When I was a boy, my father and I chased lightning storms to enjoy the spectacle. When I was in college, I and the young lady that I later married compiled observations by people who had seen ball lightning. Later at Ames, I developed a computer program that put the survey answers into multidimensional space and then sorted through that space to find associations, i.e., to identify several different types of ball lightning. The program was also used to find associations among star types.

CONWAY: So you focused on ball lightning, but in planetary lightning—and my comment here was that it suggests to me that there's some sort of interest in exobiology questions underlying that. Is that true?

BORUCKI: A major purpose of space exploration is the search for life. Lightning activity has been found on other planets. That activity might have given rise to prebiological molecules and the fixation of nitrogen that are necessary for life. Finding prebiological molecules in the atmospheres of other planets is an impetus to look more seriously at the planet to determine if the transition from prebiological molecules to life has occurred. My experiments with simulated lightning in planetary atmospheres showed that prebiological molecules should be expected in other planetary atmospheres. Because exobiology is an important part of the search for life in our galaxy, it is natural for me to be interested in it.

CONWAY: Fair enough. Fair enough. Then you mentioned space missions for lightning observations. So what were these? What data were you using?

BORUCKI: To understand the role of electrical activity in planetary atmospheres, I led several different research efforts. The research included lightning on Earth, Venus, Jupiter, and Titan. Prior to the rise of oxygen-producing bacteria, lightning activity could have been the source of fixed nitrogen needed for life on Earth. I published a paper on my lab experiments and calculations for the rate of nitrogen fixation by lightning activity in the Earth's atmosphere that was widely cited.

For Venus, I searched for lightning activity with the Venus Pioneer Orbiter spacecraft. For centuries, observers have reported seeing light on the dark side of Venus. They called it the Ashen Light. Russian entry probes into Venus's atmosphere reported radio signals that were consistent with lightning activity. I used the star sensor aboard the Pioneer Venus Orbiter to show that the Ashen Light was actually a chemically driven air glow at the top of the atmosphere, i.e., it was *not* evidence for lightning activity.

For Jupiter, I used the Viking observations of Jupiter to show that strong bands of lightning activity were occurring along zone regions at three different latitudes and that the flashes were as bright as the Earth's brightest flashes, i.e., "super bolts".

I also proposed an experiment for a NASA mission to Jupiter that would determine the distance and direction to lightning flashes and thereby identify the cloud layer that was hosting the lightning activity. On Jupiter, different cloud layers have different compositions. Consequently, the amount of prebiological molecules that are produced by lightning activity depend on which cloud hosts the lightning. My proposal was given high marks, but a similar experiment was proposed and funded by a German research group.

CONWAY: Next question, then. So you started to investigate ways to detect exoplanets in the early 1980s. Why exoplanets?

BORUCKI: A critical factor in the search for life in our galaxy is the occurrence frequency of small rocky planets in the habitable zone of solar-like stars. At the time, it was thought there might not be any such planets or that they might be extremely rare. If so, there might not be any life and we might be alone in our galaxy. If the opposite were true, then life might be plentiful and just waiting for us to contact it.

In the early eighties, NASA Ames began sponsoring seminars on possible life on Mars and other planetary systems. Consequently, it was now feasible for me to propose to accomplish the first critical step, i.e., the determination of the occurrence frequency of Earth-like planets in the habitable zone.

CONWAY: My next question is that you became a champion of the transit method. What were the other possibilities in the 1980s?

BORUCKI: I proposed using the transit technique as the most promising method to find small planets in the habitable zone because the amplitudes of those signals are proportional to the ratio of the area of the planet to the area of the star, rather than the much smaller ratio of the planet-to-star masses that the radial velocity techniques sensed. Further, the transit technique is the most sensitive to innermost planets with short orbital periods, rather than the outer planets like Jupiter that have long orbital periods.

Other research groups promoted searching for giant planets with the radial velocity technique because that technique seemed to be the most practical. However, I had worked with the techniques of photometry and spectrometry on the Apollo Program, and I'd worked with very capable groups of scientists and engineers who could and did accomplish the nearly impossible, i.e., to send astronauts to the Moon and to return them safely. Based on my calculations and my experience, I was sure I could develop the transit technique sufficiently to find Earth-size planets in the habitable zone of stars like our Sun.

CONWAY: I found a 2016 article by you in which you mention a whole series of workshops starting in 1984 that NASA funded to improve photometers. This is just the beginning of the transition in astronomy generally from film to solid state. So what do you remember as the state of the art then in photometry?

BORUCKI: I used the Ames Director's Discretionary Fund to fund those workshops specifically for the development of a photometer that had the precision needed to find Earth-size planets. When we look at previous techniques to measure the brightness of stars, photometry had progressed from visual estimates to measurements of the size and darkness of star images on photographic plates. That was followed by the use of photomultiplier tubes such as the 1P21 attached to a telescope. It measured the brightness of one star (and the sky background) at a time in several different color filters, and then the telescope was moved to a comparison star and then to a standard star for comparisons. Data was recorded on a paper chart and then calculations were made to compensate for the effects of the Earth's atmosphere on the comparisons to the standards. Observing several stars per night was all that could be expected, even when the weather and the atmosphere were favorable. The most recent research used CCD detectors to simultaneously observe the relative brightness of many dozens or hundreds of faint stars in a small region of sky.

Instead, I proposed to observe 100,000 relatively bright stars simultaneously over a large field-of-view and to automate the data analysis.

CONWAY: I guess to a degree you've already answered this next question. Was this all organized under the umbrella of the NASA astrophysics program, and do you happen to remember who the program officials were then? But it sounds like it was actually Ames funding this.

BORUCKI: My answer is, no, the NASA astrophysics program did *not* fund this work. Earlier work was funded by Ames and by the Origins program led by Anne [L.] Kinney. Some of the program officials I worked with over many years included Ed [Edward J.] Weiler, Susan [T.] Niebur, Paul [L.] Hertz, Anne Kinney, Colleen [N.] Hartman, Carl [B.] Pilcher, Ken [Kenneth W.] Ledbetter, David [B.] Jarrett, Harley Thronson, Philippe Crane, Guenther [H.] Riegler, Jay [T.] Bergstralh, and Mark [P.] Saunders. I'm sure I forgot many, and I apologize for that. They were very informative and helpful.

CONWAY: Thank you. Thank you. Now, the next thing in your story was going to NIST [National Institute of Standards and Technology] to develop some prototypes and then to the Lick Observatory [Mount Hamilton, California] for testing. So tell me how all that was arranged.

BORUCKI: Some of my proposals to the Ames Director's Fund were funded to pay for the workshops. Small grants funded my work at the University of California Lick Observatory. There were also small grants from [NASA] Headquarters. Some of my colleagues at Ames were also associated with Lick Observatory. My arrangements were made through their auspices. I also felt that the directors at the Lick Observatory were very positive about working with people with new ideas. I did not have any difficulty getting time to use the small telescopes that I requested. Contracts were let to Russell Schaefer at NIST (formerly National Bureau of Standards) to build prototype photometers to determine if solid state detectors could provide the photometric precision to detect Earth-size planets. Our test results were encouraging enough to continue with the transit detection approach.

CONWAY: I'm not trying to dig up dirt. I'm just, as I said, always interested in who did what and who funds what. So what did you learn from that series of tests?

BORUCKI: I learned that the atmosphere's changing transmission properties affected the photometric precision of the measurements so severely that it would be impossible to reliably detect Earth-size planets in the habitable zone of solar-like stars. Therefore it would be necessary to build a space-based photometer to be successful. Finding larger planets like Jupiter through the Earth's atmosphere seemed feasible, but not small planets in the habitable zone of stars like the Sun. I also learned that small size and high precision of solid-state detectors and their amplifiers would make a space born photometer possible.

CONWAY: An important finding, then. Your first three proposals' concept studies after the '92 meeting and the first two Discovery missions all resulted in review panels essentially saying that they didn't believe you could achieve the required stability and performance, and the '96 review called for another ground investigation, also done at Lick, the Vulcan photometer. Did the Discovery Program then pay for this next stage of work?

BORUCKI: No. NASA Ames funded the development of the Vulcan photometer that was constructed for tests at the Lick Observatory. The program called for some sort of demonstration that high precision photometry on a large group of stars could be done simultaneously and that the data reduction could be automated.

In 1997, my proposal to the Ames Director's Discretionary Fund was funded at $50k to develop a minimal system. Consequently, we borrowed most of the equipment and built some of the equipment in house. In particular, the CCD [charge-coupled device] detector was borrowed from a colleague Zoran Nikov at the Rochester Institute of Technology. It was replaced after our Headquarters proposal was funded in 1998. Three different objectives for the telescope were tried: a World War II-era Aero Ektar lens, a surplus Aerojet Delft lens previously used in a custom-made spectrometer, and a Canon F/2.8 300-millimeter telephoto lens.

The telescope mount was a modified amateur telescope mount that our team installed in a small telescope dome that was constructed in 1892 at the Lick Observatory and seldom used. I found volunteers to paint and seal the roof of the control room. They also removed the old development tanks for photographic plates. To prevent rain leaking on the equipment from the leaky dome, We used a garbage can lid with a rope tied to the dome shutter that covered the telescope with its electronics when the telescope was not in use. Later, the rain cover was replaced with an aluminum sheet that rode on tracks that our in-house machinist made. The device for the CCD flat-field correction used a paint bucket with some plastic diffuser plates purchased from a hardware store.

In 1998 and later years, the project was funded both by the Ames Director's Discretionary Fund and by Headquarters for a total of about $200,000 a year. Progress was much faster and results were published.

CONWAY: So my next question, when was it built and operated? It sounds like over many years.

BORUCKI: I have a letter from Remington Stone, who was the director of Mt. Hamilton operations in 1997, giving us permission to use and to modify the Crocker dome that was built in 1892. My records show that the first observations were dated October 1997. Observations ceased in 2002 because the Kepler mission was well on its way to being implemented. Funding from the year 2000 to 2002 was received in response to our proposal to Headquarters dated 6/17/1999, i.e., prior to the acceptance of the year 2000 Discovery proposal.

CONWAY: What did you learn from your experiences with this Vulcan photometer?

BORUCKI: I built a team to address the many issues we faced, and we built the software algorithms needed to analyze thousands of stars simultaneously. We demonstrated that we could detect an exoplanet with our tiny telescope, published our results showing the capability of doing photometry and automated analysis of the data, demonstrated the ability to use a radio link between an automated photometer and a ground station at Ames, and we published our successful results.

CONWAY: What was the community reception to your demonstration? I presume you talked at meetings about it, for example.

BORUCKI: Several other research groups also built analogous systems and ultimately detected many large exoplanets and published their results. Our results and that of our colleagues meant that the astronomical community began to recognize the practicality of the transit approach.

CONWAY: You re-proposed, or re-re-proposed, your spaceborne mission in 1998 and you were rejected again. This time the review committee didn't believe the system precision was achievable. Of course, you'd just done these Vulcan tests, so what was the review panel asking for on top of what you had already done?

BORUCKI: The observations with the Vulcan telescopes were meant to prove the practicality of a new method of doing automated high precision wide-field photometry, the simultaneous observations of many thousands of stars, and automated data reduction. Those results were revolutionary, but because the observations were made through the Earth's atmosphere, the measurements could not attain the precision necessary to find Earth-size planets in the habitable zone of solar-like stars, i.e., to find other Earths. This fact was understood before the construction of the Vulcan system. The equipment was not designed to demonstrate the extremely high precision that a large telescope in space could accomplish. Consequently, we needed to take the next step and prove that a photometer could be built that had the required precision.

CONWAY: So then that next step is the Kepler technology demonstration. Tell me about that. What did it include?

BORUCKI: An in-house test bed facility using a prototype photometer was developed to perform end-to-end laboratory measurements that demonstrated the feasibility of differential photometry to routinely obtain ten-part-per-million photometric precision under realistic operating conditions. The test facility included a realistic star field with a galaxy-like brightness distribution of stars, a mechanism to generate transits of a selection of stars, fast optics with realistic point-spread function, and employed a readily-available back-illuminated CCD detector. Spacecraft motion that would be expected on orbit was simulated using piezoelectric transducers. Data acquisition and processing used the same methods as planned for the space missions, and these results were sent to NASA Headquarters for review and were published in journal papers.

CONWAY: Next question's easy. Who funded it?

BORUCKI: NASA Headquarters funded the proposal that we submitted for $330,000. An additional $500,000 was borrowed from NASA Ames.

CONWAY: I gather you then also had an oversight group from NASA for this, so who was on it?

BORUCKI: John [C.] Geary, who was from the Smithsonian Astrophysical Observatory, Timothy [M.] Brown from the NCAR High Altitude Observatory, and Steve [B.] Howell from the Planetary Science Institute.

CONWAY: Thank you. You finally proposed successfully in 2001. What do you think changed to enable your selection?

BORUCKI: I think that the change occurred because our team had answered all the technical questions satisfactorily and had shown a reasonable approach to answering programmatic and financial questions. Another factor was the need for the Kepler results to define the requirements for the future missions such as the Terrestrial Planet Finder that was actively being studied.

CONWAY: And I gather from a previous interview you did with Susan Niebur that you'd also been working with Ball [Aerospace] through much of this time. Why Ball?

BORUCKI: Ball was not part of any work that I did prior to the Discovery proposals, i.e., in the period 1983 through 1991. Dave [David G.] Koch and all other members of the Kepler team, including Ball, had not been part of my research prior to the preparation of the 1992 Discovery proposal. Previous to that time, I worked with student help. Their names are listed as coauthors on the papers that I presented at conferences and published describing our work. In preparation for our first Discovery proposal, it was necessary to have an industrial partner. I chose Dave Koch as my deputy. He had worked with Harold Reitsema at Ball on previous missions and was favorably impressed with their capabilities. Harold and Ball joined for the first proposal and for all subsequent proposals.

CONWAY: And they self-funded all this work since 1992-ish? I don't really know exactly how the money flow in proposals work.

BORUCKI: Ball was funded by Ames for various studies of the CCD performance, but they self-funded their portion of each Discovery proposal.

CONWAY: Thank you. Do you recall who the Discovery program scientist was at Headquarters at the time?

BORUCKI: I believe that Susan M. Niebur was the Program Scientist at the time of the Kepler Mission selection.

CONWAY: Then I had a second part to that question. I was curious about how Kepler, an astronomy mission, was chosen for Discovery, which is meant to be a planetary mission.

BORUCKI: That is a very interesting story. Personnel at the Goddard Space Flight Center [Greenbelt, Maryland] informed me that our Ames proposal would be considered illegal because it didn't involve planets in our solar system, which was a subject of the draft AO, Announcement of Opportunity. Ames management talked with Headquarters officials about that situation. Ed Weiler the Associate Administrator at HQ was incensed that it excluded exoplanets. The AO was reworked to explicitly allow exoplanet proposals so we didn't need to go to court.

CONWAY: Well, that's interesting in and of itself, but that answers the question. Discovery Program changed its own rules for the mission. Very interesting. Thank you.

The strongest message I took out of Kepler, which was fascinating to me, was that the goal was really statistical in nature. By staring at a large number of stars, it would tell us the probability of planets forming, not *just* that there were exoplanets, which was known by 2001. So where did the ideas of gathering statistics on planets, as opposed to just looking for a few of them, come from?

BORUCKI: The practicality of searching for life in our galaxy depends on the occurrence frequency of habitable planets, not just the existence of one or two planets in the whole galaxy. The first title of our 1992 and our 1994 Discovery proposals was not "The Kepler Mission," but, instead, "The FRESIP Mission." FRESIP is an acronym: Frequency of Earth-Sized Inner Planets. The proposal title made it explicit that our objective was always to get some measure of the occurrence frequency even if that answer was an upper limit. Had you heard about FRESIP?

CONWAY: I had not seen that used, although I may have read it in one of your articles and just brain-slipped over it, because it doesn't trip the tongue as well as "Kepler" does. But that explains it. It was always in your proposals.

BORUCKI: It was even the name of the first two proposals: "Frequency of Earth-Sized Inner Planets." We're interested in occurrence frequency. We're interested in Earth-sized planets and they need to be close to their star, not out at the distance of Jupiter.

CONWAY: Right. Understood. My point was that the concept was there at the beginning.

BORUCKI: Yes, it was always there.

CONWAY: Thank you. You initially proposed Ames to be the management of the mission, but NASA changed the rules after you submitted. How did you choose, or did you choose, JPL [Jet Propulsion Laboratory, Pasadena, California] as the implementing center versus, I guess, Goddard was the other competitor?

BORUCKI: The Ames director urged us to choose JPL, so we chose JPL because we needed the support of Ames management.

CONWAY: That was simple and to the point. So now I have to find out who they were. Initially, I guess you intended Larry Webster from Ames to be the project manager. He became the deputy when JPL became the development manager and Chet [Chester] Sasaki became the project manager. What was working with Chet like in that early couple of years?

BORUCKI: After selection of the mission, JPL joined the team and assumed responsibility for the spacecraft development. Ames was responsible for the instrument—that is, the telescope and photometer development. Larry Webster was the Kepler instrument manager from 1999 to 2006, when JPL took over management of *both* the spacecraft and the instrument.

To answer the question about Chet, Chet was a very gracious person. I enjoyed working with him, even though he would never show me the status of our finances. Ultimately, both Chet and I recognized that Ball was falling seriously behind in the amount of work being accomplished according to the schedule and the budget. Chet reported that situation to JPL and was replaced.

CONWAY: Thanks for the correction.

BORUCKI: It's an important correction.

CONWAY: Oh, yes. I knew that the photometer was being managed by Ames throughout, and I didn't get Larry Webster's title right. My apologies.

So then Chet's replaced and there's another reorganization in 2006 that resulted in a new management team being assigned. I know that NASA imposed a budget cut in 2005 that delayed the work and raised costs and also forced you to undertake some de-scopes. But then it happens again in April 2006, there's a re-plan and reshuffling. So the first question is: What's been accomplished by that point in 2006?

BORUCKI: A summary in 2005 of the CDR by Chet Sasaki, the JPL mission manager at that time, lists the accomplishments to that point. "One, CCD detectors have been delivered. Two, polishing of the primary mirror is complete. Three, surface finishing of the Schmidt corrector is in progress. Four, field-flattening lenses are being delivered. Five, all procurements have been placed. Six, much of the hardware has been delivered, including the RAD 750 computer that will run the electronics aboard the spacecraft."

A statement in the Kepler Implementation Plan dated October 16, 2006, by the new JPL mission manager, Leslie Livesay, summarizes the accomplishments as of that October: "Project re-plan complete. Management tools and processes in place. Experienced business management team in place."

The specific changes since April CDR are listed as "One, launch date moved to November 1, 2008. Funding profile changed to reflect new mission profile and revised baseline. Two, JPL post-commissioning support added, L plus 90 days of JPL management. Three, project management organization changed. JPL assumes the lead for mission design."

At the end of her presentation, she states that "Kepler is in good shape and moving into fabrication."

CONWAY: Thank you. I haven't seen those documents. I doubt I ever will, but possibly.

BORUCKI: You should have all those documents. There's a huge amount of important material in those documents, what was done, how things were done, what kind of difficulties were faced, and what caused various delays and overbudgets. I'm surprised that as a historian you don't have a whole set of documents. Without the documents, I couldn't answer your questions.

CONWAY: Thanks. Then let's talk about the organizational changes that were made in that year. Chet gets assigned and Livesay gets assigned, and what changes about your job?

BORUCKI: I'm going to answer what organizational changes were made.

CONWAY: Okay.

BORUCKI: The July 2006 CDR for Programmatic Status states "The project completed a major restructuring and re-plan in July 2006. One, Level 1 requirements are unchanged. Two, a new management team is in place. Three, the organizational structure has been changed to integrate the flight segment contracts at JPL and Ames. Four, processes are in place to manage the remaining work. Personnel added: deputy project managers for flight segment and ground segment". "Created a chief engineer position." I was surprised that the new JPL manager had added several JPL personnel and their costs to a mission that was being attacked for being overbudget.

CONWAY: Thank you. So those are the organizational changes made in 2006, and then 2007, there's another review, and this time the mission's reclassified as strategic and you're reassigned as leader of the Science Team. The approximate cause was continuing overruns on Kepler.

BORUCKI: It's a good question. You said, "What do you recall as the causes of the overruns?" My answer is: The mission was overbudget the day it was accepted, because Headquarters required changes such as changing to full-cost accounting. That added many millions of dollars. Adding JPL management with its rejection of the "better, faster, cheaper" management approach, that was another $50 million. And the mission was delayed several times, including a one-year delay at startup.

Sometime after the startup of Discovery missions 9 and 10, I attended a Headquarters-sponsored meeting of the Discovery PIs and their project managers. When I made my presentation, I told the audience that Headquarters had changed the mission management, the mission costs, and its budget, and added delays to the mission completion schedule, yet the AO states that the PI was supposed to have complete control of the mission and be responsible for keeping it within budget. In practice, the NASA and JPL managers had complete control of everything except the science. The audience of PIs and project managers laughed and gave me a knowing smile.

CONWAY: Thank you. That was good, and it's what I expected too. I knew about all the Headquarters-imposed changes.

BORUCKI: You couldn't possibly know all the changes. They were endless.

CONWAY: What's hard for me to estimate in retrospect is the dollar amount, but whenever you change management structure like that, you're going to cause costs.

BORUCKI: The National Academy of Sciences was asked at one time to do a study of the reasons that missions go over budget. Their answer was that Headquarters generally makes changes in scope and causes delays in the funding. Those were two of the major reasons. Glitches in the development was another. The Kepler Mission was significantly affected by all three problems.

Now, did I always object? No. It really didn't make any difference when I objected. Furthermore many of the changes improved the mission. When you go from "better, faster, cheaper" to a much more rigorous development of a mission like JPL did, you get a much more reliable mission.

CONWAY: Great, great. Thank you. I'm glad you decided you wanted to answer that question.

BORUCKI: A good question and thoughtful questions.

CONWAY: So that was the cause of the overruns. Tell me how you found out about your removal as PI. Just tell me that story.

BORUCKI: At a meeting at Headquarters, the associate director, Alan Stern, stated that because the mission was overbudget, it would become a strategic mission and that I would be the science team PI, *not* the mission PI, and that JPL managers would take charge of the mission.

CONWAY: Did you try to fight that?

BORUCKI: No. It was obvious that the Kepler mission was greatly overbudget and behind the original schedule. Changing the mission to a strategic mission put it into an appropriate funding organization. There was no actual change in mission management, i.e., it was always managed by JPL and Ames. As mentioned earlier, the PI had no control over the budget, schedule, or program management. With some exceptions, the PI's only control was over the science team, and that didn't change.

CONWAY: So from your perspective, nothing really changed. Okay. I see.

And then the next question is about assembling the science team. When you were putting together your science team, what were you looking for in terms of specialties?

BORUCKI: The Kepler mission had a need for a wide variety of researchers. Exoplanet specialists like William [D.] Cochran from UT [University of Texas]-Austin, and Geoff [Geoffrey W.] Marcy from University of California-Berkeley, who were using ground-based telescopes to search for and find planets with radial velocity techniques, were invited to become co-investigators to do the follow-up observations that were necessary to exclude false-positive events caused by binary stars.

Dave [David W.] Latham, the astrophysics observer at Harvard, and Tim Brown at NOAO [National Optical Astronomy Observatory] Solar Observatory, were recruited to classify the stars in the field of view, to distinguish solar-like stars in the appropriate brightness range from the millions of other stars. Dave Koch from Ames and Harold [J.] Reitsema from Ball Aerospace had experience developing and operating instruments aboard spacecraft. Ed [Edward W.] Dunham from Lowell Observatory and Tim Brown had experience with developing and operating small ground-based telescopes to find exoplanets with the transit technique. Jon [M.] Jenkins developed the statistical methods used to detect signals from the Vulcan photometer. John Geary from the Astrophysics Observatory had experience with the development of amplifiers used with CCD detectors.

Ron [Ronald L.] Gilliland and several members of the European team of astrophysicists were experts at using the brightness variations of stars, asteroseismology to determine the size and age of stars. Gibor Basri at University of California-Berkeley and Andrea [K.] Dupree, Smithsonian Astrophysics Observatory, had expertise in analysis of the stellar variability of small stars that would regulate and control our ultimate precision. Alan [P.] Boss and Jack Lissauer were theorists who could interpret our results relative to the current theories of planet formation. Natalie Batalha joined the Mission team, made major contributions to operations of the Vulcan telescope, and later led the Kepler Science Council which provided overall guidance to the science effort. Douglas Caldwell (Kepler Instrument Scientist) shepherded the instrument through its many difficulties.

Edna DeVore from the SETI [Search for Extraterrestrial Intelligence] Institute and Alan [D.] Gould from the Lawrence Hall of Science were educators that had developed and conducted the education and outreach programs. Steve Howell specialized in identifying faint stars that were so close to our target stars that they could confuse the results. Jill [C.] Tarter from the SETI Institute used a radio telescope to check for radio signals from detected exoplanets. Not all the members of the team are mentioned here, but each member on our team had a specific expertise and a task required for mission success.

CONWAY: Ames had responsibility for post-launch mission operations. How is that organized, implemented?

BORUCKI: Originally, the agreement was for Ames to regain control of the Mission at the beginning of flight operations. JPL changed that transition to occur after 90 days from the beginning of operations. At that point, Roger Hunter at Ames assumed control. The people involved in putting Mission Operations together included Charlie [Charles K.] Sobeck, Marcie [Martha] Smith, Dwight [T.] Sanderfer, Rick [Richard S.] Thompson, Janice [E.] Voss, who was an ex-astronaut who had joined the team, and Jon [M.] Jenkins at Ames.

At Ball Aerospace, there were people who oversaw spacecraft performance and recovery from on-orbit anomalies. There was also a group of people headed by Bill Possel at the Laboratory for Space Physics at the University of Colorado who directed the communications between the ground stations and the spacecraft.

CONWAY: Thank you. Then you also had a ground-based observing effort to validate planet candidates. How was that organized?

BORUCKI: Prior to the start of the mission, Dave Latham at the Smithsonian Astrophysics Observatory made telescopic measurements of several million stars in and near Kepler's chosen field of view and at several different optical wavelengths. After he processed the data, they were passed to Tim Brown from the High-Altitude Observatory (and later, the Las Cumbres Observatory) who classified each star with respect to spectral type and luminosity class. He then generated a catalog of appropriate (not too big and not too hot) stars for observation by the Kepler Mission. Another consequence of this effort was that the follow-up team had preliminary information about the brightness, size, temperature, spectral type, luminosity class, and position of each of the stars that had a candidate planet.

Dave Latham, William Cochran, Geoff Marcy, Steve Howell, and a European team member made most of the follow-up observations when they were able to obtain time on suitable telescopes. Additional observations were made by European and Asian astronomers, especially the members of the Kepler Asteroseismic Science Consortium (KASC) headquartered in Denmark. They focused on evolved stars on the Kepler target list. More information about their observation program is described in a Memorandum of Agreement between me and the KASC.

Follow up observers used both ground-based and space-based telescopes including the Hubble Space Telescope and Spitzer [Space] Telescopes. Lists of follow-up observations and suggestions for target priorities were coordinated by Dave [David R.] Ciardi and Nick [Thomas Nicholas] Gautier, both from JPL. Nick Gautier also served as the first Kepler Project Scientist.

CONWAY: And how was that funded?

BORUCKI: The Kepler mission executed grants to U.S. observers. Foreign observers covered their own costs.

CONWAY: And who led it? Did you already tell me who led that effort?

BORUCKI: The arrangements for a follow-up program to identify false positive events were discussed in the Kepler proposal and the concept study. The program was managed by the Science Operations Center (SOC) at Ames under my direction. That responsibility was delegated to the SOC director Janice Voss and later to Michael Haas. The observers (David Latham, Geoffrey Marcy, and William Cochran, Edward Dunham, Ron Gilliland, Timothy Brown) and their institutions (Smithsonian Astronomical Observatory, University of California at Berkeley, and University of Texas at Austin, Space Telescope Science Institute, Les Cumbres Observatory, respectively) had wide latitude to pick targets and make arrangements for telescope time at major observatories.

CONWAY: Thank you. Thank you. I gather there's substantial effort post-launch put into understanding the data coming down from Kepler, improving the data processing pipeline. Please talk about what needed to be done.

BORUCKI: Okay. This is one of the most complex things that the Kepler mission team accomplished. I can describe it only in summary.

Pixel data were downloaded from the spacecraft to an operation center at the University of Colorado and then sent on to the Space Telescope Science Center in Baltimore for archiving and transmittal to the Science Operation Center at Ames for analysis. There the pixel data were converted to instrumental fluxes using the Kepler pipeline software module that calibrated pixel data, performed aperture photometry, and corrected for systematic errors. Systematic error-corrected light curves were then passed to the transiting planet search pipeline module that marked possible signatures of transiting planets. These events were labeled threshold-crossing events, TCE and were then subject to a more detailed analysis to determine the probability that they were actual planet transits rather than false-positive events.

Those TCE with sufficiently high probabilities were labeled planetary candidates and then put on a list of targets to be observed by follow-on observers using both ground-based and space-based telescopes. Their results provided additional information about the source of the transit signatures and better-defined stellar properties such as the stellar size, mass, temperature, and age. Because the planetary properties (i.e., orbital distance, and presence in the habitable zone) were derived from those of the star it orbited, stellar observations were an important aspect of the Kepler Mission.

Those candidates that passed all the tests were labeled “confirmed planets” and their identity and characteristics were published in journals as well as being placed in a public archive at JPL. Several comprehensive articles that describe the data analysis have been published, and a data handbook is available at the Mikulski Data Archive at the Space Telescope Science Institute.

CONWAY: Thank you. Ultimately, you had lots of data and exoplanet discoveries. What was your favorite result?

BORUCKI: I was really delighted to prove that most stars have planets. Many of these planets are roughly Earth-size and some of these are in the habitable zone. These results imply that life could be common in our galaxy.

CONWAY: What was the most unexpected result, in your opinion?

BORUCKI: Two results stand out. One, small rocky planets were found orbiting the earliest stars that formed before the formation of our Sun. Consequently, there may be some *very* advanced civilization in our galaxies for us to meet. It will be very interesting when we make contact with them.

Another surprising result was that many planets have sizes between that of the Earth and Neptune. These planets could be unlike any planets in our solar system and be completely different from theoretical models developed prior to the discovery of exoplanets. For example, mathematical models of planet formation by George Wetherill in 1996 predicted that no rocky planets larger than Earth could be expected except for gas or ice giant planets formed beyond the ice line. The Kepler result that was most expected was the proof that most stars have planets.

CONWAY: Science that you didn't expect is the most interesting sometimes.

Is there anything Kepler didn't accomplish that you hoped it would?

BORUCKI: A major goal for this mission was to determine if Earth-size planets in and near the habitable zone of solar-like stars were rare or common. The Kepler mission succeeded in accomplishing that goal. That success means that in the future, the development of more capable and more expensive missions can be justified now that we know for certain that most stars have planets and that small planets in and near the habitable zone are frequent. These facts are no longer matters of speculation. I am very satisfied with the many planets, planetary types, and planetary systems discovered by the Kepler mission. The great progress in stellar astrophysics was a welcome bonus.

I would have liked to have obtained a more quantitative estimate of the "FRequency of Earth-Size Inner Planets," but I am satisfied that the Kepler Mission has made a giant step in humankind's understanding of planetary systems in our galaxy.

CONWAY: I guess the last more detailed question is you told Susan Niebur a decade ago if you knew what you knew now, you'd never have started down the Kepler path. It was too much work, effort, pain, but you're proud of the results. What else wouldn't you do again? Were there other lessons you took out of the Kepler experience?

BORUCKI: I found that to accomplish breakthrough discoveries like those from the Kepler Mission it is necessary to persevere in the face of unending opposition and technical difficulties.

Although there were many difficult situations, there were also some happy and humorous events. I'll recount a few. One, before the Mission was approved by NASA HQ, the Kepler team was at Headquarters for a review by the Associate Director Ed Weiler and his staff. Each of the Kepler team members answered their questions about critical topics such as the mission cost, schedule, program development, and operations. When it was my turn to answer questions about the science program, Weiler's staff asked several questions about the expected science results, the transit technique, the data analysis, and the elimination of false positives. After each question was asked and before I could speak, Ed Weiler answered the question. I was shocked. It was my prerogative to answer these questions, but rather than following my impulse to tell Ed to sit down and be quiet, I realized that his knowledge of the mission science was excellent. I couldn't do any better. His enthusiasm implied that he supported the mission, understood its science, and would allow it to proceed. All I needed, from my part, was to sit down, be quiet, and smile.

CONWAY: That's a nice story.

BORUCKI: A second story regards my great pleasure when I saw the first images and the first planetary detections from Kepler. All the 84 CCD detector channels were working. The entire field of view was filled with *millions* of stars. In a few weeks, we had enough data to prove that the Mission was providing a bountiful harvest of exoplanets orbiting a variety of stars. The early data not only showed many transits by exoplanets, but also the much smaller changes in starlight that occurred as the planet moved behind its star and its reflected light was occulted. Those data proved that Kepler had sufficient precision to find Earths around other stars.

The third event that I'd like to mention was the time at a Kepler science team meeting when we were trying to decide the names of the hundreds and thousands of planets we were discovering. Most team members had no suggestions. Dave Koch suggested naming them with numerical values, using their sky coordinates. No one liked that suggestion, but no one had a better suggestion. I decided to suggest something that was so awful that everyone would jump up with a better name. I suggested that we name the planets in chronological order of discovery appended to *my* name, i.e., Borucki-1bcd, Borucki-2bcd and etc. The number would designate the star in the discovery catalog followed by a letter designating the planet or planets orbiting the star. Instantly, team members jumped up and came up with alternative names.

Finally, we were all happy with the naming convention of Kepler-1bcd, Kepler-2bcd, etc., chronologically. I really had a good laugh as to how well my suggestion had inspired the team to find an appropriate naming convention. The International Astronomical Union Committee for Naming Astronomical Objects made minimal changes and accepted our naming conventions.

CONWAY: If you don't mind, I don't know if you answered my bigger-picture question, but I'd like to give you the opportunity, since you were at Ames for so long, to tell me about how the place changed during that time.

BORUCKI: The NASA Ames Center has expanded from a state-of-the-art aeronautics center to one that is also a major player in space exploration. Ames has made breakthrough contributions to vertical take-off-and-landing aircraft such as the Osprey, and to air traffic control. Ames led some of the first missions to the Moon, Venus, and Jupiter. The Ames lunar missions have found evidence for substantial amounts of water on the Moon. The water could be used to provide oxygen for breathing, water for drinking, and hydrogen for rocket fuel to carry on further exploration of our solar system. New missions to the Moon are underway that will rove over the surface to determine the amounts and distributions of water. Ames also continues to develop Earth-orbiting micro-satellites to study the Earth and the Moon.

Previous Ames missions to Venus employed a radar system to map the mountains and valleys of its surface and flew entry probes into the atmosphere to measure its thermal and density structure. Ame's scientists have developed and deployed instruments to Mars that are identifying its minerals. Ames is also involved in using supercomputers and quantum computers to address a wide variety of important problems especially in fluid dynamics.

CONWAY: It sound like you would say it didn't change very much the topic, but didn't change its—what is the word I'm even looking for—commitment to innovation.

BORUCKI: Yes, Ames has always been identified as center for innovation.

CONWAY: So what didn't change?

BORUCKI: A constant theme at Ames has always been innovation. Examples are the blunt body shape designed for manned space missions to protect astronauts during the high-speed entry from Earth orbit and from the Moon. An important research enterprise that continues is the design of a nation-wide network to track and schedule air traffic to maximize safety and efficiency. That effort has expanded to enable remotely piloted operations to transport people and goods within urban areas.

CONWAY: And it sounds like they were willing to spend funds internally to promote good ideas. I mean, your Kepler story is a classic case of that.

Anything left you'd like to say before we go? My famous last question is what didn't I ask but I should have.

BORUCKI: I thought your questions were insightful and pertinent. There are many more interesting and informative stories in the comprehensive records that are available for all aspects of the Kepler Mission.

CONWAY: Thanks for this. Like I said, I will get this transcribed and turned around for you to look at, but it sounds like you've already got it all written down so it should be easier for you to compare and fix whatever my errors are in the transcript. Thank you so much for your time.

BORUCKI: Goodbye.

[End of interview]