Jack Connerney

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Interviewer

Q: Okay. We're being recorded now. I'm Erik Conway. I'm talking with Jack Connerney, formerly of Goddard Space Flight Center, and I think now you work for a contractor there?

Connerney: Now I'm with the Space Research Corporation. Like many others, I ducked out of civil service when I approached the forty-year cliff. The way the system works is if you have more than about forty years of service, your pension benefit doesn't increase, but you're still paying into it every pay period, so oftentimes when that cliff looms, people make an arrangement to transition into private sector, and that's what I did.

Q: But keep doing what you were doing before.

Connerney: Yeah, basically. So, I mean, it's not a bad arrangement. There are some freedoms in the private sector that as a civil servant one did not enjoy, some of the crazy travel restrictions and things. I mean, I don't denigrate my service at NASA over the years. It was a wonderful opportunity, obviously, to be involved in things that you couldn't be involved in anywhere else or many other places, but we certainly weren't compensated, I think, as well as my JPL colleagues or some of the other institutions. So it worked out great for me.

Q: It sounds like you enjoyed your career.

Connerney: Yeah.

Q: So, first, Jack, tell me where you were born and how you were educated.

Connerney: I was born in Mattapan, Massachusetts, near Boston, many years ago. I went to public schools in Braintree, Massachusetts, which is the southern end of the Beltway loop there, and I went to Cornell University, got a scholarship there that was intended for, I think, the children of Irishmen; it was called the McMullen Scholarship. So I did my undergraduate work at Cornell and I worked in the Surface Physics Lab up there for a year. Then I did my graduate work at Cornell, as well, in the Applied Engineering Physics Department, worked with Art Kuckes, a refugee from the Plasma Physics Lab at Princeton who wanted to get into geophysics, and I think he recruited me because I was pretty good in laboratories and he needed to have a student that could build stuff.

That ultimately led me to Goddard Space Flight Center, because we did an EM induction experiment in the Earth's crust where we were trying to measure the electrical conductivity at depth in the Earth's crust. Prior to that time, almost all of the work that was done was magnetotellurics, which involves measuring the surface electric field and magnetic field, and they were getting large conductivities in the lower crust that my committee chairman, Art Kuckes, didn't believe. He thought it was a problem with the electrical field measurement. You can imagine you stick a few electrodes in the ground and you have galvanic issues with the electrode working with the soils, and you might stick the electrodes in a highly conductive patch that shorts out the electric field, and the rest of the crust beneath it might be very resistant. So he came up with a way to basically repeat that same measurement but using the gradient of the magnetic field, substituting that to the electric field. So you can work that out mathematically. It works if you take Bz -over- dBx/dx plus dBy/dy [$B_Z/(dBx/dx + dBy/dy)$]; you get a measure of the conductivity using the horizontal gradient of the magnetic field instead of the E field measurement. You can get the same information that you get out of the magnetotelluric formulation of E-over-H. But that was my project.

We did a natural source experiment, using the source variations of the magnetic field that you just measure in the environment. We also did a controlled source experiment where we put a loop of wire up in the Adirondack Mountains and got an old Navy generator and put 20 kilowatts through it at frequencies ranging from—I guess we went from about 50 Hertz down to 20-second periods, long periods, of course, for the deep penetration. And for my thesis work, I was able to measure the electrical conductivity of the crust well beyond 20 kilometers depth, and lo and behold, we found a very highly conductive layer in the lower crust at about 20 kilometers, and it's an ironclad measurement. So my committee chairman turned out to be wrong.

But to do that experiment, I had to learn how to build magnetometers. Well, we had them spread out in the Adirondacks as far as about 100 kilometers away from our loop source, and we retrieved that signal, part of the primary signal and the induced signal 100 kilometers away, basically by measuring over a long period of time and doing synchronous detection. So we had these oven-controlled crystal oscillator clocks that we carried around to synchronize the $19.99 data acquisition systems that we built using cheap Radio Shack cassette recorders. The early days of that experiment was kind of interesting.

It was actually an interesting thesis project, too, because one of my colleagues, Tony Nekut and I got to live up in the Adirondacks for months and months at a time. We wintered over up there, and it's kind of a remote area, and they're very curious about what you're doing up there. They thought we were prospecting for oil companies. They had all kinds of conspiracy theories going. So I went and gave them some talks at the local VFW. Once you've wintered over up there, they sort of treat you differently. You achieve a level of status that none of the tourists can claim. So I used to get—like I couldn't get a speeding ticket. I'd have to run this big loop. It was maybe—I don't know. We had four instruments spread 100 kilometers in either direction, so it took a couple hours to run that loop, and, of course, I ran it at 80 or so miles an hour.

I did get a speeding ticket, and it was funny, I had to go see the judge in the back of the country store. He's sitting around a hot stove with a bunch of his buddies smoking cigars and whatnot. The judge looks at the ticket and he goes, "Yeah, I'm going to give you an unsafe tire."

So I start complaining that there's nothing wrong with my tire, and the guy elbowed me and he says, "He's fixing your ticket. Shut up." [laughter] Anyway, it was fun.

So part of that is I went down to Goddard Space Flight Center because my committee chairman, Art Kuckes, knew a guy down at Goddard that was doing magnetic field work. Turned out to be Mario Acuña, who was affectionately known as the grandfather of fluxgate magnetometry. Fluxgate started to develop, of course, during the Second World War, basically military uses, but Mario really sort of perfected the ring-core fluxgate magnetometer. Flew them on early spacecraft, flew them on Magsat, for example, the first real highly accurate vector field measurement made in space. Learned a little bit doing that, learned how to improve fluxgates by that experience. Post Magsat, the fundamental instrument hasn't changed very much. You know, improvements in some of the electronics, but fundamentally they're all evolved from the improvements he made after Magsat.

So I worked down at Goddard for a summer learning how to build magnetometers, and I guess I visited there a couple of times for calibration purposes and things. So they said, “Hey, when you’re done with your thesis, we’d like you to come down here.” They suggested an NRC postdoc at the time.

So I came down as a National Research Council, National Academy of Sciences postdoc back in 1979 when I graduated. In 1980, they hired me on the scientific staff. I had been there ever since through 2016. Got involved early on, had the Pioneer 11 encounter, got involved in that, and then with Voyager, who had already launched but I’d not yet gotten there. At one point, I was added as a co-I on Voyager. I sort of grew up on Voyager and drifted away from my geophysics background towards more planetary physics, space physics.

So, like many of the co-investigators and instrument providers on Juno, we all cut our teeth under the magnificent seven in the Voyager era, and I think, to be honest, I think that’s why Juno has been the success it’s been. If you look across, you know—so I was the lead for the magnetometer. I sort of worked under Norm Ness and Mario Acuna on a number of missions, Norm Ness, of course, one of the PIs on Voyager, one of the early post-Van Allen era prima donnas in the space business. The other one, of course, was Tom Krimigis at APL, so Barry Mauk at APL grew up under his tutelage. Same thing, Bill Kurth up at Iowa, he was one of Jim Warwick’s workers during the Voyager era. Fran Bagenal, not associated with one instrument on Juno per se, but she worked the plasma instrument on Voyager under Herb Bridge at MIT, another one of the Voyager PIs. So the tentacles of Voyager basically wrapped around the entire Juno team.

Q: What did you learn from Voyager? How would you put it in a couple of minutes?

Connerney: Well, for one thing, we learn how to do the role of a project scientist from Ed Stone, and just the excitement and the rush of having a new encounter basically every so often. Back in those days, we were working with very primitive assets compared to what we have today. But many of us also learned how to build or program instruments or data systems. We knew how flight instruments were built and how they went through environmental testing and how to properly architect a data analysis system. I mean, many of us did different roles, but by the time Juno came around, we had thirty years, forty years of experience in doing just this.

You look at the way NASA—here I'm going to get political. You look at the way NASA selects PIs these days, you look across the board and a lot of them that are selected have *zero* flight experiment experience, and to me, that's not a formula for instant success.

But anyway, it's one of the advantages of the PI-class, Scout-class Discovery, New Frontier-type missions is that you sort of organically select the people that you know how to do the job, that you can trust to come in on time, on budget with your hardware. Of course, at Goddard we—me in particular, I've adopted the Goddard philosophy of always coming in basically on time and on budget and doing whatever it takes to make that happen, because the worst thing in a project is a financial surprise that a project manager can't solve. Typically, they can't solve them themselves. They have to go back to Headquarters. They take a beating. If you're one of these PI-class missions, you approach the 15 percent with an automatic cancellation hearing.

So if you're a project manager, it's great to have an instrument provider and experiment lead that you can depend on to come in on schedule and on budget and not be a problem. So when you collect people, the PI and whoever he's working with, collect people to do these various flight hardware investigations, that's kind of paramount consideration, and I think that's why a lot of the New Frontiers and Discovery, well, some of the older ones anyway, have come in, unlike directed missions, have come in on their original budgets.

Q: So the experience of the instrument PIs and of maybe the mission PIs is crucial, you think, to the Discovery programs and New Frontiers programs' occasional successes?

Connerney: Well, just look at the history. I mean, I won't mention any specific names, but some of the more recent attempts have not lived up to the promise of the New Frontier Discovery class. I mean, we're doing essentially a directed mission complement of science, and certainly Juno discoveries have been up there with any of the directed missions, at a fraction, a small fraction of the cost of Cassini or of Galileo or now Clipper or—what's that other one, the quadracopter?

Q: Oh, yes. Dragonfly.

Connerney: Yeah. I mean, the numbers I heard on that recently are just staggering, mindboggling. In all fairness, when that was selected, it was obvious that that was not a New Frontier mission.

Q: Yeah. Well, that's what I thought, too, but I'm JPL so I won't comment more.

We talked about Voyager and its contributions. So let's talk about formulating the Juno mission. So tell me your role.

Connerney: Well, tell me when do you think Juno mission was formulated?

Q: Scott and I were talking about early to mid-1990s. Especially there was a key moment after a Saturn that Cassini did flying by Jupiter, which would make it about 2000.

Connerney: Yeah, so actually the mission originated in 1985.

Q: Eighty-five? Okay.

Connerney: 1985. There was a study group chaired by George Siscoe, then at BU, Boston University, I think. In 1985, he was to form a study group to look into a mission to Jupiter, and they came up with what was called Jupiter Polar Orbiter. Jupiter Polar Orbiter was a study of everything Juno is, basically, and a few more things.

Actually, the reason it didn't go anywhere was because the community was split into two camps. One camp very much wanted to have an orbit just over the atmosphere, close-in orbit. Like Juno's. I was in that camp. Because I'm mapping the magnetic field, I wanted to envelope Jupiter in an envelope of close-in magnetic field observations, which from potential field theory says any closed surface about the object is all you need, and so that's also good for gravity. It's also good for atmospheric studies. You can do a lot with the close-in measurements.

The other camp wanted to study Io intensively and to pass by Io many, many times. But they came up with an orbit that had a perijove just above the cloud tops of Jupiter and an apoapsis, apojove, out by Io. You can study both. Well, turned out it's too hard to get there. Takes too much fuel to drop the apojove to get it in as far as Io at 6 RJ. Our apojove was 100, or a little over 100, Jovian radii. We were originally going to burn that down and have an apojove of around 40 or something, but that would have consumed all the fuel, and it was just a losing battle trying to get in close with that. So because you had two competing camps, and there was no solution to that problem, so that a mission did not evolve from that.

So that Jupiter Polar Orbiter study really formed the basis of our JPO Discovery proposal that went in at the same time that Scott Bolton's JASSI proposal went in the Discovery round. He was PI of the JASSI proposal, which was a flyby at that time. I was the PI, using APL as the institution for what we called Jupiter Polar Orbiter. We took the old name from Siscoe's study. The mission plan that we're flying on Juno is basically the same mission plan I had on Jupiter Polar Orbiter, and that was inspired by the JPL mission that preceded us in the Discovery round, Ed Smith's Inside Jupiter proposal.

That inspired the community to get together, and the consensus was, if we are going to go to Jupiter, Inside Jupiter is not the mission we want to do. It was too narrow. They were going to fly a scalar magnetometer at 60-degree inclination. We understood that wouldn't cut the auroral zone field lines. Of course, magnetic field magnitude measurements are very bad in terms of trying to characterize the internal field, because as George Backus showed in the case of the Earth magnetic field measurements many, many years ago, the so-called Backus Effect, if you just measure the field magnitude, there is a whole host of a series of spherical harmonic coefficients that are not constrained by field magnitude measurements, and it's because it's that part of the field that is perpendicular to the—it's a perpendicular effect. You're insensitive to any field that is perpendicular to the measured vector field, so that leads to a series of harmonics that you can't constrain very well. So anyway, there were fundamental flaws like that with Ed Smith's proposal, but in the broader community, it was just considered to be way too narrow. They were going to fly one or two instruments, I think it was. It was really very limited.

So the community sort of organically got together and we had some meetings. At various scientific meetings, we had splinter meetings, you know, "What should we do about this?" Ed Smith's proposal didn't win, but it got to Step Two or something, and it was a shock, really, for the community. So we got together and we discussed how a mission like that should be done, and a lot of it came from the Jupiter Polar Orbiter study experience, because a lot of the participants were the same. I mean, I was in that study back in '85. Fran Bagenal was in that study in '85. Hunter Waite was in that study, I remember. So people that are now associated with Juno hark back to that era.

So that's what led to the proposal I did with APL. I was a co-I on Scott's proposal and he was a co-I on my proposal. We were good friends at the time as well.

Then in the Discovery round, both of those proposals were deemed to be infeasible, either by virtue of technology, but in large part, it was a financial issue, that $600 million or whatever was deemed to be inadequate for either mission. So when the New Frontiers program was developed in order to support outer-planet missions, Scott and I got together and we figured that the likelihood of success would be enhanced if we combined our teams. So instead of doing the JASSI flyby, we wound up with the Jupiter Polar Orbiter mission plan. We had a very strict limit on the number of co-Is because we were sensitive at the time to Headquarters bemoaning the cost of supporting so many co-Is. So we said, "Okay, we're only going to have one co-I per instrument team." You could have other people that would not be designated as co-Is, of course. So we didn't have the marching armies that the directed missions, for example, have.

I think the Cassini infrared team under Mike Flasar. my colleague at Goddard. Their infrared team, one instrument team, is about the size of our entire Juno team with a dozen instruments. So anyway, so we had one co-I from each team. We had Barry Mauk for JEDI, the energetic particle instrument, we had Randy Gladstone for the Ultraviolet spectrometer, we had myself for the magnetic field investigation, [DJ] McComas for JADE, the low-energy particle instrument, Bill Kurth for the WAVES instrument. So it was intentionally constrained in terms of its size, the team was. We've since added a few people, and we've since turned [off] some of the engineering instruments and design instruments like the SRU and the advanced solar compass, but, yeah, we didn't have many interdisciplinary people. We had a small handful, people like Dave Stevenson and some specialists like that.

Q: How did you choose instruments and providers and PIs? How did you decide who was going to be on the team?

Connerney: Well, I mean, that's it. We knew what science we wanted to do and we had a good idea of what measurements we needed, and we knew who the most dependable and most experienced providers of the instruments were, and so we just collected them. Many of them were part and parcel of these couple of meetings we had in response to Ed Smith's Inside Jupiter proposal saying, "How do we do this mission properly? How do we get some real science out of it? What's the best way to do this?" I mean, that benefits really from having a lot of different heads at the table, and I'm sure Ed Smith didn't have that. He probably just had a small immediate group at JPL and a couple of co-Is, consultants, and that was probably it. So this was more a growth of a broader range of scientists in the community coming up with the science and necessary measurements.

Then we took a combination of the instruments that I had selected for JPO, the high-energy particle instrument, Barry Mauk at APL, obviously my own instrument, the magnetometry. We dropped an infrared instrument that I had coming from the IR group at Goddard. The PI associated with the instrument would have been Dennis Reuter. Dennis Reuter had come up with an IR instrument, but we instead went with a UV instrument from SwRI and we dropped the UV instrument that I had that was going to be built at APL. Any institution like JPL that is doing a mission wants to have one or two of their own instruments on the payload, obviously, which is how Ed Stone got his instrument, I think, on Voyager. Well, he said, "I'm not going to do this. I'm not flying an instrument." [laughs] So there you go. We got another instrument.

I also had WAVES from Iowa on the JPO proposal. He wound up on Juno. So we did a mix. We took some of the JASSI payload instruments and we took some of the JPL instruments. We had to drop a couple because of limited resources. Fortunately, we got the Italians to come in and contribute telecom components and they contributed also the JIRAM, the infrared instrument, not quite the same design as what we had on JPO, but still a hugely valuable contribution. So that was it, really, but there was really no confusion over where do we go for the instruments, because like I said, we've been in the business for thirty years. We knew who to go to. We knew who to depend on to get the job done. We didn't have to sort through a bunch of résumés, that's for sure.

Q: What was the singular key scientific question you were planning Juno to explore?

Connerney: Well, there are kind of a few prongs to that, but Inside Jupiter sort of inspired a lot of thought about how do we learn how Jupiter was put together and evolved, right? So we wanted to probe the inside, the interior of Jupiter, which would give us a handle on the composition and evolution of the planet, and so from the magnetic field point of view, I wanted to do the best job we could possibly do on mapping the magnetic field. There are two ways to learn a lot about the interior from the magnetic field. One is, you can try to determine the radius of the dynamo, the outer radius of the dynamo region, using the higher degree harmonics of the field; and the other way uses a measure of the secular variation of the field. You can actually use the technique due to—I think it was Ray Hide, many years ago (1978), where you can find a radius at which the change in the unsigned magnetic flux is minimized, and that will tell you also the dynamo radius.

You know the dynamo radius and you know enough about the interior composition and state, you can—well, for example, with the field model that we produced at the end of the prime mission, we identified that radius by the spectrum of the field, called the Lowes' spectrum, how much of the field is contributed by harmonics of increasing degree. For the Earth, that dropoff is linear with degree from degree 3 through 14 or so, and then it goes flat because of all of the magnetic crustal magnetization, sort of a random noise component, and that turns into a flat spectrum.

So the linear part of that spectrum tells you what the radius of the dynamo is. So at Jupiter, we actually know that radius better than we know it in the case of the Earth, because we measure it to higher degree, and that is a reflection of the fact that it's a gaseous planet. It doesn't have all this crustal magnetization, so that would be an obscuring haze in the case of the Earth, below which it's very difficult, impossible to look. So we know the field now accurately through about degree 18, and we're continuing to improve that. As the mission evolves, we get more orbits.

So the idea is that the dynamo exists in a region that is below the transition to metallic hydrogen, but it's stabilized by the exosolution of helium, called helium rain, that comes out of solution in metallic hydrogen and drops toward the center of the planet, and that quashes any convection in that part of the interior. So we believe that the 0.807 RJ radius of the dynamo that we found via the Lowes' spectrum indicates where the helium rain dissolves back into the metallic hydrogen because of the pressure-temperature state of the interior.

So I think we've actually learned not only about the state of the materials in the core of Jupiter and where that transition occurs, we also learned some high-pressure physics, because the theorists that came up with the exosolution of helium from metallic hydrogen didn't quite know at which pressure that transition occurs and which pressure the helium rain dissolves again into the metallic hydrogen. So I think they are actually taking a cue from our determination where we think that occurs on Jupiter, in Jupiter's interior.

So for me, that was a large part of our contribution to the probing of the interior. Of course, gravity was the other major contributor there. Now, there's a certain non-uniqueness in interpretation of the gravity measurements that one can address with knowledge of the behavior of materials with pressure, and there's still some discussion ongoing about whether the core, the heavy element distribution in the planet is in this fuzzy core configuration or whether it is kind of a mysterious enhancement of heavier elements in the outer envelope. It's kind of counterintuitive, but people are working on it towards trying to improve the interpretations of gravity observations. So that's one emphasis, the interior part.

We also sort of adopted many of the science objectives of JPO in terms of the exploration of the magnetosphere. You know, we lived through a period in early exploration where spacecraft stayed in the equatorial plane of a planet, and that's because they wanted to hobnob with the satellites, which is all well and good, but you have a distinct two-dimensional knowledge of a three-dimensional object, and so Juno's first mission really to probe three dimensions extensively, and that was entirely necessary to do any potential field work, and that includes both magnetic field and gravity, and it also includes probing these regions that we've been able to see with infrared and ultraviolet emissions, but we were never able to go through them and measure fields and particles, waves in situ.

So that was a second or third area, a discipline area that we were able to cover, and the other being, of course, the atmosphere and atmospheric composition, the depth of some of the flows and the depth—well, the composition and distribution of ammonia and water in the outer part of the planet, and, of course, that's still very active.

We were surprised, when we got the first observations, that the atmosphere was not as well mixed as we thought it was. It complicates analysis a little bit, but they're recovering from that shock and continue to improve their analyses. So it's really three disciplinary areas: atmosphere, 3D magnetosphere, and interior studies.

Q: As you say, it's essentially a flagship-class mission with lots of instruments with a New Frontiers budget. We already talked a little bit about cost discipline, making sure people are reliable providers, but talk a little bit about the management arrangements you had to put together. It seems fairly complex to me to have a PI from SwRI, JPL as the manager, instruments from Goddard and elsewhere. Talk about the management relations that you had to set up.

Connerney: I mean, any project is going to have, hopefully, instruments distributed throughout the country, if not the globe, so you always have that. Now of course, this was formulated when Scott was at JPL, and it was after our first step success, but not ultimate selection that Scott went to Southwest Research Institute. I was a little worried about that at the time, thinking that it might impact the selection of the program, but, actually, I think it turned out rather well because Scott had a great depth of experience at JPL, so he knew pretty much where everything was buried out there and how JPL operates, and from a practical "I lived it" in a sense, and not from a management chart or something, and so being external to JPL, I think, gave him a little more autonomy in directing the mission, and, of course, JPL has been very accommodating all the way.

But you also have a major spacecraft provider in Lockheed Martin, another institution that you need to manage, and so it really had the same or similar management structure that any project has with the PI being at an external institution. If the PI is strong and has the depth of knowledge of JPL and how it operates, I think it's an advantage. If the PI is simply somebody at an academic institution a thousand miles away and isn't making the day-to-day decisions, but leaving that to the implementing institution, I think you can run into some issues.

So I think Scott and I, and to some extent Steve, the project scientist, a lot of these decisions were ultimately made closely between us. Scott, of course, being the ultimate PI, had ultimate responsibility. He turned out to be, I think, an extraordinary manager in the sense that he's one manager that sees an issue through everybody's eyes, not just his own, and I think that's a tremendous advantage in working with different institutions. I mean, he wasn't going to create an issue or a problem for Lockheed Martin and say, "Okay, you deal with it." He's more like going to collaborate with them to find a solution that is acceptable and beneficial to all parties, and they realize that, and I think that's very effective management. Making everybody part of the solution, I think, is going to work nine times out of ten.

Q: What would you say were the major developmental challenges Juno had?

Connerney: Well, it's been a long time. We certainly had our share of challenges. Some last-minute stuff before launch, I know. Then, of course, I think the most impactful thing we had was the indication after our orbit insertion burn that the second burn we were going to do to go down to, I think it was originally an eleven-day orbit, and then we opted for fourteen-day orbit. That was the most impactful because it stretched our mission out from a one-year prime mission to must have been five years, I guess, if not more, on orbit. That was the most impactful, and it was a difficult decision, obviously, both for Headquarters and for us, but it didn't take us long, I think, Juno management, project manager at the time at JPL, along with Scott and I, to come to the conclusion that we've got an otherwise working spacecraft in an orbit that essentially gives us the same measurements. Our focus, our concentration, was always on the perijove observations. Why would we risk it? Why would we accept any appreciable risk of losing a billion-dollar spacecraft by changing this orbit just so that we could get it done quicker? It didn't make much sense to us.

So, fortunately we were able to convince Zurbuchen, who had just come on as NASA Associate Administrator of Science down there, and that was kind of touch-and-go because Zurbuchen, I think, sort of felt he was being maneuvered into stretching our mission out, but Scott was able to make a pretty compelling case and got the project office down at Marshall to buy in on it, JPL bought in on it. I think even the chief engineer at Headquarters ultimately bought in on it. So Zurbuchen sort of let us go on it. That had to have been the most impactful decision point we reached.

There were a host of minor things. After we had gotten our instruments to the Cape, for example, in terms of the magnetometer, we found out we had some data from the lab that indicated that in some of our instruments the thermal heaters had improperly bonded wires to the resistive elements that we used. We were able to access externally one of the instruments on the end of the boom with—I forget what the technique was now, but some imaging where you send particles, like X-ray imaging sort of, and so we determined that at least one of them was properly bonded. And since we had two, we figured, well, if the other one isn't, we'll have to go without it. So we sort of scrambled to do that at the last minute.

Of course, the magnetometer investigation was dual hardware redundant, so we always had the possibility of getting the measurements we needed with one or the other of those instruments, but typically we fly two different distances from the spacecraft so that we can check, make sure the spacecraft isn't the source of any magnetic field we're measuring.

Juno, by the way, came out to be very clean magnetically. We can't really detect the spacecraft with our two fluxgates. We've had a lot of experiences in the past where there's less success with the magnetic field mapping prior to flight, and a lot of times you really can't adequately map spacecraft fields on the ground in a flight-like configuration because, you know, the solar panels are over here and the spacecraft is over there and you're in an environment where you can't measure the magnetic field anyway. It's very noisy. So some of the things we can do in the clean room and other things you simply can't.

We did—oh, no, that was MAVEN. I'm thinking of another mission. We flew about the same time. We often do find things that need to be corrected. For example, on MAVEN, they had inductive fuel tank heaters that were supposed to be wired up so that one cancels the other. Instead, somebody flipped it and we had double the magnetic signature of one of them instead of cancelling. But that's why we do test in the clean room on spacecraft, and we found it, we fixed it.

Q: So you don't recall that there were any difficulties, really, delivering the magnetometer? I ask because nothing that has to do with Juno, but recently a number of missions have had troubles getting magnetometers delivered.

Connerney: Oh, I know, and they all came and talked to us and we said, "No problem." I told Headquarters, when they had the Clipper problem, I said, "No problem. We can produce a magnetometer for you, get it through environmental testing. It's an instrument that we're flying on Juno and we know that it's good to 100K rad TID."

Just to get us to—well, this is getting political. Back in the day when I first heard about the problems they were having, because I had also proposed, obviously, for these missions and did not get selected—Carol Raymond got selected instead—to provide this investigation with UCLA magnetometers, and at the time, JPL was—I've got to be careful I don't mix up missions. JPL was going to provide a helium magnetometer. Was it Clipper? It had to have been Clipper. I think originally JPL was going to provide a helium magnetometer. We originally were going to fly one of those on Juno, for political reasons, really. Ed Smith was going to fly that on Inside Jupiter. So originally we were going to share the payload with them, payload duties, but it turns out JPL couldn't build that instrument. The expertise for that instrument had always been this company in Richardson Texas called Polatomic. Polatomic makes plenty of money building these for Earth field observations for the Navy. They have a nice little business there, and they don't want to lose any money doing development projects. Juno turned out to be a development project, because (a), flight instruments are all development projects. You can't take them off the production line like they do for the Navy, and (b), we needed an order of magnitude more dynamic range than their instruments for the Navy. So it turns out that we dragged them into the PDR at JPL, and they really weren't interested in producing it, and JPL realized they couldn't do it themselves, and the Engineering Division at JPL wouldn't accept the responsibility. So Scott says, "Fine. We'll get rid of it. I'll put another fluxgate on there for the dual magnetometer technique." So we planned on and built two magnetometers.

So when they got selected to fly on Clipper, I had a discussion with the then-Director of Planetary down at Headquarters, who came from Goddard, Lori Glaze. But I said, "Hey, look. These people can't build these instruments anymore. We just had to deselect them on Juno. Polatomics doesn't want to build flight instruments and JPL can't build them, so they're going to show up at PDR, and you're going to have to make an accommodation." So I said, "Look, as a backup, why don't you just send Goddard enough money to basically buy parts and we'll just warehouse the parts, so that if late in the game you decide you need another solution, we'll have the parts. But if we don't have the parts, of course, you're screwed. So it won't cost you peanuts. We'll buy the parts and we'll just hold on to them. And if it turns out two years before launch you have to do something else, we can get you out of trouble."

Well, she declined to do that, but I told her, I said, "this instrument is not going to fly."

Anyway, so it turns out I was right, and they wound up with UCLA magnetometers. I did not appreciate at the time that UCLA was going to have the same problem, that they were going to run into issues, and not just for that mission, but for I think it was Psyche at the same time also, couldn't get any performance.

So Psyche came to us. They eventually had my colleagues at DTU provide the instrument. Dave McComas also had issues with his mission. [Interstellar Mapping and Acceleration Probe]. He came to us originally. Everybody said, "What can you do for us?"

Then we said to all of them—Bob McDowell was taking the lead on the McComas mission, and we told them all the same thing, says, "As long as we have enough time, we'll provide the instruments for you. No problem." Some of them would be different instruments, a MAVEN-type instrument, not a—the Juno instrument is like the Rolls-Royce of the industry, but you don't need that for most missions. Juno, we measure the vector field with basically 100-part-per-million absolute vector accuracy. Nobody in solar physics is *ever* going to understand what happens in the solar wind to better than 1 percent. [laughs] So you don't need 100-part-per-million accuracy. They're much simpler instruments to build and to calibrate. So smaller instruments like the MAVEN instrument would be more than overkill for those things.

So we, of course, respond with whatever matches the measurement needed, the particular mission. But for various reasons, each of those missions, Psyche and McComas and, naturally, I think, probably Clipper all went in different directions.

There is some concern at Headquarters. I think they didn't want to give Goddard too much business, and they have actually been trying to develop now alternative magnetometer suppliers out at Iowa and I think up in New Hampshire with the SwRI contingent up there.

[interruption]

Q: Thank you for your time.

Connerney: It's been fun recounting the history as best as I can recall it.

Q: Take care.

[End of interview]