

# DISCOVERY 30<sup>TH</sup> ANNIVERSARY ORAL HISTORY PROJECT

## EDITED ORAL HISTORY TRANSCRIPT

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INTERVIEWED BY ERIK M. CONWAY  
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CONWAY: All right, Scott, I'm glad we finally managed to connect for this. What I'm going to do is I'm going to especially focus in this interview on the proposal process, and then we'll talk about the mission as it happened. But first, I'm going to start out asking you for a brief kind of biographical sketch of yourself. So please tell me, Scott, when were you born and how are you educated?

BOLTON: I was born in Newark, New Jersey, and my education went from—I grew up in Michigan mostly, and then went to the University of Michigan as an undergrad, studying aerospace engineering, and I did a lot of physics and astronomy in that with a lot of electives, as well as business and art and things like that.

Then went to graduate school, started at Caltech [California Institute of Technology, Pasadena] and then transferred and finished at [University of California] Berkeley because the advisor I was working with left and went up to Berkeley. So I ended up following him up and then switching advisors anyway, but ended up getting my final degree, Ph.D., from UC Berkeley, but I was at Caltech for a while.

Then I started at JPL [NASA's Jet Propulsion Laboratory, Pasadena, California] right after my undergraduate degree from Michigan, so I came out to JPL and started as an undergrad in aerospace engineering, working on mission design at JPL. It was in a section called 312 back then. And then found myself liking the science more, so I started taking classes at Caltech to go

back into physics, and eventually planetary science and astrophysics, and moved over from Division 31 at JPL to Division 32 once I started as a student there, and then just went back and forth. After leaving Caltech and going to Berkeley, I would go back and forth every six months or so, moving back to JPL and working for a while, and then going back up to Berkeley and doing more studies and things like that until I finished.

CONWAY: That's great they allowed you to do it. The place was much more flexible, apparently. That would be very hard to do now.

BOLTON: It was hard to do then. In fact, when I started to take the classes at Caltech, they said, "Well, you have a degree in aerospace engineering, so you can't really start in grad school in physics. You need to take these classes and do things to catch up."

They didn't really have a program for me to go to school there, but they created one. I went to the director's office and I went to different places and said that I wanted to go to grad school at Caltech, and so they created something that would allow them to pay the tuition for me as a JPL student, but they required that you first get accepted into the program before they would do that. Since Caltech said, "Well, if you're going to switch degrees, you need to catch up first," so they allowed me to catch up, and then I started to take undergraduate classes, and Caltech said, "Don't do that. We think you should catch up using graduate school classes."

So I ended up starting with graduate school E&M, electricity and magnetism, and classical mechanics and quantum mechanics, and then after those three classes, then they said, "Okay, we're not going to give you a degree unless you go full-time at Caltech. You can't go part-time." Because I was going part-time while I worked at JPL. They did have a bus that went

back and forth to campus, so I would take that. But basically, they were forcing me to go full-time in order to get the degree.

Around that time, I was looking at different advisors. Ed [Edward C.] Stone. A guy named Kip [S.] Thorne was doing gravitational physics. I was interested in that. And then a guy named Hal [Harold] Zirin was doing solar physics, because I was getting involved with the development of a telescope to look at the Sun. Then there was a guy at JPL that was doing some stuff with me, and I decided I wanted to work with him. Then he left, mainly because he was the project scientist to what became the Parker Solar Probe, but this was a long time ago, way before that. That mission kept getting delayed, and he finally lost patience and left and went to Lawrence Berkeley Lab, so I went and applied there. I applied to their Stanford—Caltech, of course, I was already there.

I had some backups in case I couldn't get in, and those were like University of Colorado [Boulder] and maybe San Diego, I think it was, and then I think I had Princeton [University, New Jersey], Stanford [University, California], Caltech, and Berkeley. I got into all of the schools that were hard to get into, and I didn't get into the backups, which was kind of strange. So I got accepted and decided—I went up to visit Berkeley and I liked it and the advisor was there, so I ended up going there.

Then to JPL's credit, they continued to cover the tuition back and forth for UC Berkeley, and I maintained my job and eventually wrote a proposal, while I was a student, as the PI [principal investigator] to investigate Voyager data at Uranus. I think we flew by Uranus in 1986. So this was around the time I was going up to Berkeley.

At that time, there was some effort at NASA to fund students and postdocs and early career people, so my proposal stood out. I was analyzing Voyager data from the planetary radio

astronomy instrument, and it got accepted. So then I had my own funding to go back and forth to Berkeley. It was kind of interesting. I was very lucky and fortunate, and it was just, like you say, times change and there were periods where it was really hard to get money and then there are periods where it's a little easier. Right now I think things are shifting toward early career in NASA. It's a little easier to get started on things like that, but for a little while after I did it, it was hard to do. I went through a little window. And I'll never forget it, because I didn't think I would get accepted.

I had a sense of humor, and my proposal to analyze the Uranus data was "Low-Frequency Emissions From Uranus," and when I went to go submit it, JPL was reviewing it and they said, "You need to change the title."

I said, "No, I'm not doing it." And it was low-frequency radio emissions, but I just called it low-frequency emissions. Of course, there were comments in the reviews, but generally people thought it was comical and humorous, and I ended up winning it. So when I got the NASA letter saying, "Your proposal, 'Low-Frequency Emissions from Uranus,' is accepted and funded," I saved that letter.

CONWAY: That's a great story. But what I meant to ask you was, you never told me what your Ph.D. was in.

BOLTON: Oh. So the Ph.D. is in, I think, technically astronomy and astrophysics. I don't really know what the exact title is. I think it might just be astrophysics. The thesis, which isn't really in the degree name, but the dissertation was "Time Variability of Jovian Synchrotron Emission." So it was on Jupiter, and in particular the high-energy radiation belts of Jupiter.

CONWAY: So it sounds like most of your career, then, has been in outer-planets things, which makes sense.

BOLTON: Yeah, I had some stuff while I was at JPL where one of my first tasks was to work on a Haley's Comet mission, which didn't happen, but I did do some comet work there and then got involved with the Venus—what became Magellan, but at the time when I first started on it, it was called VOIR, Venus Orbiter Imaging Radar, which is French “to see,” right? So it was a novel name.

Then I worked on what was called Star Probe, which turned into Solar Probe, and that was about the Sun. I built a mirror at a JPL lab to take photos, be able to image in x-rays, which was hard to do because it doesn't reflect well. So it was a multilayer mirror that I made in a lab along with somebody. The person that went to Berkeley was at JPL when I was doing that. Then we were using a technique that a professor named Troy [W.] Barbee at Stanford had developed to literally layer multiple layers on a mirror that were like one molecule, one atom thick. We would do it in a vacuum chamber, and you added a bunch of different reflections from these different layers, which would have different dielectric constants and things, and they would reflect, and you could add up to get 20, 30 percent reflection, which was enough to make an image. So that was trying to focus an image of the Sun in x-rays.

Then I worked on Deep Space 1, which was like a technology demonstration, and that was kind of cool helping to build the plasma instrument.

Then when I came back, while I was at Berkeley doing my dissertation, I started to give talks at conferences. One of them was attended by the people that were doing magnetospheric

studies on Galileo. I didn't know the teams, but they had built a couple of the instruments. So they offered me a job, when I came back to JPL, that I could be what was at that time called the science coordinator. Now it's called investigation scientist, but it's the same idea. There's a scientist at JPL that represents an instrument team where the PI is not at JPL, and they represent them in flagship missions or any other mission, I guess.

So I came back to JPL in about 1989 and joined the Galileo team working on the magnetosphere instruments, the plasma instrument and the plasma wave instrument. That was a bit of luck as well, because the reason that opportunity opened up was the Galileo launch was delayed because of a shuttle problem, right? The shuttle had had an accident. So it was supposed to launch in '86 when it would have been on its way. Maybe I would have been able to join, but the opportunity was that it was sitting there and was going to launch in '89, I guess it was. I came back just in time, basically, to start on that project, and then stayed with that and hopped from there to Cassini, and then from Cassini to Juno.

CONWAY: What were you doing on Cassini?

BOLTON: Same idea. I had been invited to join the Cassini team back when I was on Galileo, but I didn't want to give up Galileo and I didn't think I had time to do both. But then as Galileo was winding down, different teams had approached me mostly from the magnetospheric side of things, so I joined an energetic particle team that was run by a guy named Tom [Stamatios M.] Krimigis, and also a plasma instrument that the head was a guy named Dave [David T.] Young from Southwest Research Institute. So that's what got me first involved with Southwest Research Institute. But I was also involved with at the time the plasma wave instrument, the PI was Don

[Donald A.] Gurnett out of University of Iowa, and I had worked with him on Galileo. So I was affiliated with all three of those teams.

Then one of my jobs on Cassini over time became—I was advocating that we should turn on the instruments and do a calibration and a scientific study of Jupiter as we passed it, and there was no budget to do that, so we were advocating it, myself and “Candy” [Candice] Hansen, who I knew from Voyager. She was an imaging person in ultraviolet. Anyway, so we were advocating it. Basically, it’s one of those stories where be careful what you’re advocating, because they eventually said, “Okay, and now you guys organize it.”

So we became the leads to try to figure out what Cassini could do as we flew by Jupiter, and so we held workshops. Do you want to do a calibration? Do you want to do science that’s unique? The calibrations, unique calibrations were highest on the priority if you needed it to accomplish your Saturn objectives. Then we thought we had a good chance of getting that done.

Then to my surprise, the radar team did not suggest anything. Charles Elachi was the head of that team. They had a radiometer mode where it was a radar instrument, but they could do a listen-only mode, and it worked at 2-centimeter wavelength. So I proposed it myself and said, “Why don’t you guys do this, and you could map the synchrotron emission as we fly by Jupiter at 1 centimeters. We can’t see that from the Earth because the atmosphere confuses us. Cassini, because of its proximity and the radar instruments’ beam, you could map this out and you’ll see higher energy electrons than we can see from Earth. So that will tell us something.”

So that ended up getting accepted. It was a little bit like the other thing where the radar team came in and said, “Okay, we’ll do that, but you do it. You work on that.” So I wasn’t on that team, but I ended up leading that. It was the first paper from that team, we got that data.

And that experiment also led to the idea behind Juno, because there were some scientists on Cassini, two of them that actually started and were responsible for Cassini getting funded, named Toby [Tobias] Owen and Daniel Gautier. Toby Owen was at the University of Hawaii, I think, at the time, although he might have been at Stony Brook [University, New York]. I'm not sure the timing when he went to Hawaii. And then Daniel Gautier was in the University of Paris or Observatory of Paris in France.

They both came up to me and said, "If you make this map at 2 centimeters, can you use that to constrain the temperature of the atmosphere at 20-centimeter wavelength?" Which is a different radio frequency, right, or radio wavelength. And the problem at 20 centimeters is that the synchrotron emission dominates, and you can't measure the atmosphere very well. So they knew I was going to try to get this high-energy radiation in the middle of and be able to eliminate the atmosphere, so they thought, well, maybe you can bootstrap that.

So I looked at that and came back to them and said, "I don't think I can do that with a lot of precision in this way, but I think I have an idea of how to make that measurement." I said, "Why do you want that so bad? Why do you think that's important?"

They said, "Well, if you could make that measurement and figure out the atmosphere temperature like that at 20 centimeters, then you could constrain how Jupiter was formed."

And I said, "Well, that sounds very profound." I said, "Let me understand how that works exactly."

It was tied to the Galileo Probe measurements and how much water was in Jupiter and things like that. So I came back and said, "Okay, I figured it out. We can't do it with Cassini, but if you could get a spacecraft over the pole of Jupiter that had these frequencies of radio antennas on them, I could make that measurement for you, but it would take a whole mission, I think."

And they said, “We think this is the basis of an entire mission.”

And I said, “Oh, that’s crazy. I don’t think so. But maybe something would happen that you could do that.”

And then I started learning more about these two gentlemen. When I realized that they were responsible for basically selling Cassini, I thought, “Well, maybe they know something.” So I said, “Okay, let’s see if we can form a mission.” And, you know, the story jumps quite a lot and skipping steps, but eventually that led to Juno, and Juno *is* that mission. I mean, we do a lot more than just that, but that was the idea of where it came from.

CONWAY: Toby, at least, was very well-connected.

BOLTON: Yeah. So you knew Toby.

CONWAY: I know *of* him. I never met him.

BOLTON: He was on Juno, obviously. He and Daniel helped me create the project, and we started off with a Discovery mission proposal.

CONWAY: Talk about that.

BOLTON: There was no New Frontiers yet.

CONWAY: Yeah. So talk about then that kind of initial effort to make it a Discovery Program-level mission, which at the time you're talking about '99ish, maybe?

BOLTON: 2000, something right around then. You can look up when Cassini flew by Jupiter [December 30, 2000]. That's when it kind of started.

Prior to that, I had played around with a Discovery mission working with Terrence [V.] Johnson and Dennis Matson, trying to say, "Well, maybe we could make a mission that would bounce between IO and Europa." Of course, the radiation was going to kill you, but they both liked it, but we never really got anywhere with it and it didn't get proposed.

But this other one, we started to put together a fly-by mission, because it had to fit in Discovery. There was an orbiter being studied that I had kind of helped with a little bit, but I wasn't really involved, that Ed [Edward J.] Smith, who was a scientist at JPL, a very prominent scientist, he, I think, built the magnetometers for Pioneer and was very well-established, and he had put together a Discovery mission called Inside Jupiter. It had been proposed prior, I think in the late nineties, to one of the Discovery calls, and managed to get into Step Two, but didn't win. It was something that orbited Jupiter very, very close. I think there were like five-day orbits. It was almost crazy. And they were going to map out the magnetic field and the gravity field. That was the primary science that they were doing.

I was trying to convince them to put on a radio antenna and some high-energy particle instruments so that we could look at the radiation belts, but I wasn't successful at convincing them of that. That mission went ahead and got somewhere in the sense that it got to Step Two. So Jupiter was on the radar screen, so to speak, for Discovery Program, and JPL was considered pretty successful that you even got into Step Two. So that was a major effort.

So when I came along with a different Jupiter mission, JPL was willing to look at it, but we weren't really at the top rung of the ladder because Inside Jupiter had more heritage. But we put together what was called JASSI [Jupiter: Atmospheric Sounding And Sensing of the Interior]. It was a fly-by right over the pole of Jupiter. So you went one shot like a polar—you started on the south and went over out the north, I think, but I don't remember for sure. You made a map with this radiometer instrument that we call MWR [Microwave Radiometer] in Juno. It was an early version of that. Then you had a magnetometer and some particle instruments and a simple imager.

The goal was to get the gravity field and the magnetic field and to measure this atmospheric temperature to get the composition and how much water was in Jupiter, which is something that the Galileo Probe puzzled. The results from the Galileo Probe were very puzzling, because it had very low water abundance, and yet it showed an enrichment in all the other heavy elements and the noble gases. The general idea was that Jupiter was enriched through icy planetesimals, so how could the water be depleted and everything else be enriched if ice was the thing that carried the enrichment in? That was a puzzle. So getting the water in another way more globally, more deep without a probe was enticing and somewhat promising.

So I spent a lot of time on that proposal and sent it in, and it did not do well in its first review. The resolution from the review boards was that this was an easy mission to do and that the measurement was straightforward and simple and easy to get, but that the science was not important enough.

CONWAY: Oh, no.

BOLTON: And it was really surprising, because the team felt it was the opposite. The measurement was sketchy because we weren't sure we could do it and nobody had ever built an instrument quite like this before, but the science was *really* profound. So they had it completely backwards. JPL was not very supportive when we got that negative response.

So the next Discovery round was coming along, but it wasn't announced yet, and people on the team were saying, "We need to try this again and we need to look at how to reword our proposal so that we can get this point across."

There was a Decadal Survey also being done around that time, and there was this question that maybe there would be this New Frontiers mission AO [announcement of opportunity] that would come out. New Horizons had already gone through something similar, which was considered to be the first New Frontiers, but its AO was not like a New Frontiers AO. It was "We want to go to Jupiter." In fact, I was on proposals for those, all of the proposals for Pluto, but I ended up not joining the team, even though I helped out with all of those, including Alan Stern and Larry [Laurence A.] Soderblom had one, and I think it was a guy from Colorado, Larry [W.] Esposito, I think, was the third proposal.

Anyway, so we thought New Frontiers might get started, so I said, "Okay, maybe let's take a look at a possible JASSI-like mission." And because it had more money and more resources to be able to do a bigger mission than Discovery, I reached out to the Inside Jupiter team and the JASSI team and said, "Why don't we join forces in one proposal and we'll do what Inside Jupiter wants *and* we'll do this microwave and then we'll have both."

So they went back and forth, but eventually Ed Smith said, "Yes, let's do that," and all the team was pretty supportive.

We still didn't know what it looked like. I said, "We should do a study just in case the AO for New Frontiers comes out first, and if it doesn't, we'll do the separate Discovery and then when the AO for New Frontiers is out, we'll try that later."

So everybody kind of got behind it and I started to do meetings, but Charles Elachi was a little cautious, and rightly so, because he said, "Well, I've got a young early career person, Scott, leading this, but I've got Ed Smith, who's unbelievably senior and can probably win this and has already done well. We should have him as the PI, not Scott."

So he went to Ed Smith, and Ed said, "I don't want to do it." He goes, "I'm getting too close to retirement. I think Scott should be the one to do it, and I'm behind him."

So that helped a lot, although Charles was still saying, "Well, you're not very well-known and you have really important people like Toby Owen and others that are on the team. Maybe they should be the PIs." And so he did a little workshop to try to figure out who should be the PI.

But pretty much all the team, none of them wanted it, and they wanted me to do it because I had come up with, they thought, the important instrument. So Charles got behind that, and that's how we ended up moving forward, and I merged these two teams.

But there's another piece of the story that might be important before you ask too many questions, which is—so I was a little skeptical because of the fact that JASSI was not well-reviewed, and the idea that the science of understanding how much water is on Jupiter was considered low priority.

The Decadal Study was going on, and so the people that were running it reached out to me and said, "We hear you're going to put together this. Would you come and give a talk about what the mission would be doing?"

And I said, “Yes, I’ll give that talk on one condition. I’ll come and give the talk, but I want you to understand that if the Decadal Survey for planetary and outer planets does not deem this measurement important, I will not be re-proposing it. So I’m not going to tell you the answer of what you should have. I just want you to know that if you come out with ‘it’s not important,’ that this mission will disappear, and if you think it’s important, it’s critical that you say that, or the mission has no chance.”

So there were two people that were co-chairing the meeting. It was at Caltech, the one I went to, and it was a Reta [F.] Beebe and Andy [Andrew P.] Ingersoll. Andy Ingersoll was a co-investigator on Inside Jupiter with Ed Smith and didn’t think too much of JASSI. He wasn’t invited in, but he didn’t think too much of it, and I didn’t realize that. But Reta Beebe was a huge fan and realized it.

So they got me to come in, and Reta said, “Look, you’re a little bit too connected to this because you have a lot riding on it. Let’s ask Sam [Samuel] Gulkis to give the talk of what your instrument would do.” I used to work for Sam. In fact, I was in his group. He was sort of the father of radio astronomy at JPL. So he was going to give the main talk.

So I said, “Fine.” And I gave him all the stuff.

And Reta said, “You can be in the room, but you shouldn’t say anything because you’re advocating for your own mission and we don’t want that.”

I said, “Okay.”

So I went in there, and then Sam started to give the talk, and he was an excellent scientist. In fact, most of what I learned, I learned from him.

Andy was very critical of the entire concept and said, “I don’t think this can be done. I don’t think you’ll be able to interpret it. It’s all model-dependent.”

And in the middle of Andy kind of criticizing and giving Sam a hard time, Reta turned to me and said, “Okay, Scott, you take over the presentation.”

And I said, “I thought I was not allowed to talk.”

And she said, “We’ve changed the rules. You answer Andy.”

So I turned to Andy and I said, “Can you name one spacecraft measurement investigation that is not model-dependent for its interpretation?”

And he sat there and thought and he said, “I guess not.”

And I said, “So let me explain where the models come from and why I think this can be done and why we’re where we are.”

So I went through, and at the end of it, he turned to me and he said, “Okay, you convinced me. Can I join your team? I’m ready to give up Inside Jupiter. I’m going to join your team instead.”

I said, “Wow! I must be on to something if he’s willing to switch like that.” Now, that was before we knew that we were going to go New Frontiers. But that was the beginning of it.

So that spread, because that was the whole Decadal Survey, and they came out with a very positive thing. Of course, what they showed in the Decadal Survey was a Juno-like mission go into orbit, map out the magnetic field and gravity field like Inside Jupiter had, get all the microwave that JASSI had, and study the aurora, which I also had in JASSI. Then they said, “And drop three deep probes as well, to get the water in two different ways.”

So when we started to study Juno, it became clear that you couldn’t do all that inside the budget. The three probes were very expensive. They had to go to 100 bars pressure.

So I went to JPL and just said, “I don’t think you can do all of this and be believable. It’ll be billions of dollars. It’s not going to be sellable.”

And they didn't really agree. They wanted me to put a probe in, but as PI, you could make these decisions, and so I said, "We're not going to do a probe. We're going to just do the microwave." And that's how we went forward.

CONWAY: Wow. So that one meeting at Caltech that the Decadal Survey folks asked you to do, that kind of turned you all around.

BOLTON: It did. That was what enabled it. And, of course, it wasn't just that meeting, but the fact that it got written up as a high-priority mission that should be one of the few selectable New Frontiers missions. So, as you know, the Decadal Survey identifies candidate missions for a New Frontiers Program, unlike Discovery, which is just completely open, and so you had to get on that list. The fact that they added that to the list is what really made the difference, and the fact that NASA's AO said you don't have to do exactly what the Decadal Survey does, you have to do a mission that gets most of the science objectives and make a case. So I felt that we could make the case for the science without the probes, and I think the team all agreed that the probes would put us way over budget.

I did look at trying to add very simple probes. In fact, that's where the idea of a shallow probe came from. So nowadays, if you look at the list of New Frontiers missions, there's a Saturn probe listed and it's shallow. It just goes to 5 or 10 bars. And that came from a study that I did while I was doing Juno, as I was looking for, well, what's the least expensive probe that we would need?

Of course, they wanted you to go to 100 bars because the Galileo Probe went to 22 bars and the water abundance had not leveled off and it obviously didn't go deep enough. A lot of

people thought that was because it went into this anomalous region, sort of the Sahara Desert of Jupiter, but nobody really knew whether that was right or wrong. So you couldn't really drop a 5-bar probe in, and you could make an argument we're going to go into a different region and see if it's the same, but that's pretty expensive.

But what I did is I looked at, you know, you would like to do a probe at Saturn, Uranus, and Neptune and compare the composition with Jupiter to tell the story of the solar system, and the water on those other giant planets is rather deep, even deeper than 100 bars, probably, and so it wasn't clear that you could do it. So what I did was an analysis to show that at 5 bars, you could get all the noble gases and most of the heavy elements, except ones that were condensing, like ammonia and water, and that you could compare that to the Galileo Probe at Jupiter and that would be sufficient. You didn't really need the water completely. You need the water for Jupiter because you're trying to figure out what does the enrichment, but once you understand that, maybe you don't need it for Saturn, Uranus, and Neptune in order to understand the story of the solar system.

So I had put together this idea and went to Toby Owen and Daniel Gauthier to try to formulate the story that I was trying to tell, and they both joined in and agreed with me, and so we wrote a paper and gave it at an international probe conference, that you didn't need to go deep, that you only needed to go to 5 bars. And we were booed off the stage.

As I was leaving, Sushil Atreya and another guy named Tristan Guillot both said, "We think you might be right." So they wrote comparison papers at that thing, and over time, that paper became sort of a landmark paper, and the whole idea of a probe shifted and the shallow probe was accepted. So now you see that that's what's being advocated. Even at Uranus and Neptune, you don't need to go super deep. And I think Juno has shown that for different reasons,

that it's not worth going deep unless you manage to go *really* deep, and that's because we found that the atmosphere of Jupiter in ammonia and probably water are not well-mixed below the condensation levels, so even if you had dropped three deep probes into Jupiter, you probably would have got three different answers, because things that condense, like water and ammonia, you think once it goes down to the temperature where everything's gas, that it ought to be well-mixed, but nature doesn't work that way. Nature is more complex than the simple models we like to use.

The example that I give for this is that here on Earth we have hail, and if a really simple model existed, you would not have hail, because the hail comes down, lands on the ground on your driveway, on the street, in your lawn, and it's chunks of ice and it's way too warm for ice to exist, so it lands and then it melts or evaporates rather quickly. So something like that could be happening at Jupiter or all the giant planets. So if that's true, you're depositing ammonia and water down deep, and so things aren't well-mixed until it takes a while for them to rise back up. So there are some papers out from Juno that talk about this. They call it mush balls, but it's basically mushy hail that's a water and ammonia mixture.

So you have to be very careful interpreting probes, and I'm not in the majority here, but even in the noble gases and non-condensables, you could have effects due to weathering that you don't anticipate, but that doesn't mean you don't make the measurement. We've got to make the measurements anyway. An example of that is the helium on Jupiter, which is a noble gas, is depleted in the upper atmosphere, and it's because they believe it's getting rained down by neon, and so there's an interaction there. That was predicted by Dave Stevenson and others early on, and then the Galileo Probe kind of confirmed that.

So nature's more complex than then we give it credit for, and it's not surprising. We can hardly predict the weather here on the Earth very accurately.

CONWAY: And we have lots more data than any of the other planets.

BOLTON: Absolutely. So nature's complex. The same thing with the magnetosphere. We fly lots of spacecraft around the Earth, but it's hard to explain all of the aurora and everything that's happening, so it's not surprising that Jupiter's complex, too, even more complex.

CONWAY: Thank you. Let's see. You've mentioned Juno's origin in two missions, JASSI and—and now I've already forgotten the other.

BOLTON: Inside Jupiter.

CONWAY: And Inside Jupiter. When you were formulating the Juno proposal, though, did you bring anything else into it? I'm trying to get at assembling the rest of the science.

BOLTON: Absolutely. We put together a very robust magnetospheric program and auroral study, because we were going to go over the poles of Jupiter and we're really close, so you could really study the aurora.

So Juno has a full set of instruments that are intended and designed to study the magnetosphere, so low-energy charge particles, high-energy charge particles, plasma wave magnetometers, and an infrared camera to study the aurora and ultraviolet, both, and the

ultraviolet is a spectrograph and imager, and the infrared is an imager and a spectrometer. So we use it for lots of things besides the aurora, but it has a very, very, very powerful auroral payload on it.

And it was also designed to be simple, but also to be able to get all the measurements that we would like on all of the different disciplines. So you have the interior structure, right, that is mostly gravity and magnetic field, then you have the deep atmosphere and dynamics and composition, which is the microwave and the gravity field a little bit as well, and then you have all the aurora. And Juno's a spinner, and so we spin around, and so we designed it for the fields and particles instruments to have electron and ion sensors, multiple ones, so that when you're spinning, you could get very high-resolution pitch angle and distribution measurements. And everything's designed so that everybody takes turns looking between the solar arrays, so to speak, as you spin across, and so you don't have a big complex scan platform.

So a lot of what Juno, when it was designed, was to not repeat the complexities that drove the costs on Cassini and Galileo. Since I was heavily involved in both of those, one of my jobs on Cassini became to run the working group to decide what the Titan orbiter measurements would be like, and there was a big scan platform and the radar instrument pointed in a different direction, so it was a nightmare to try to figure out, and nobody could work together very well. You had to decide the pointing, and one science was enabled and another science wasn't. So I wanted to avoid all of that.

Galileo tried to avoid that. It was a spacecraft that was called a dual spinner, and what that meant was that part of the platform spun, the ones that were making the fields and particles measurements, and then the upper part of the spacecraft didn't spin and was a scan platform for imaging. That was very clever and really innovative, but also turned out to be very complex, and

there were problems with it, where literally the spinning platform was what would literally be called de-spin. It was spinning in the opposite direction, so to speak, to make it—it's hard to explain.

But anyway, that had problems with it, where they kept going into safe modes and had electrical shorts across the spin versus the non-spin because you have to run power across that interface somehow, and so they had these brushes that would maintain conductivity, but sometimes they'd short. And I got pulled into trying to help them analyze how that was. Even though I was a scientist on it, I had a good background, thanks to Sam Gulkis, in statistical studies, and so they asked me to do—they couldn't figure out the cause, and so they came and asked if I could do a statistical study to try to find links that were—why were these safe modes happening when they did, what did it link to, anything, any parameter whatsoever.

And they gave me access to a very powerful supercomputer, and I ran all these and came up with nothing. So I didn't help much. I eliminated certain things, but we never figured it out.

But anyway, so I was afraid of that kind of complexity. I think JPL was in general. So I looked for a simpler solution on Juno and worked out imaging cameras in visible, infrared, and UV that could all work, even though we were spinning. So these are compromises you make in order to make things simple.

And, of course, the end result was that we were within the New Frontiers budget and were arguably very complex, like a flagship in disguise. And yet we're a tiny fraction of the cost of something like [Europa] Clipper or Cassini. So there are ways. It's not just the spacecraft design, but it's the synergy between the measurement objectives, the measurement design, the spacecraft design, the orbit design, all of that. If you can fold those in at the right level, you can find synergistic solutions that can be efficient. You also need to—there's some advantages to PI

missions in the management structure that can allow quick decisions that are science focused and that really consider the science and the traceability matrix and just make a decision very quickly, which is critical to maintaining schedule, which is critical to maintaining budget.

CONWAY: Yeah, one of my questions was, I was interested in how you approached the low-cost requirements, the cost-cap requirements in New Frontiers, and so part of your story is simplification. But what else is there?

BOLTON: Well, there's also the synergy of the engineering science and design and orbit navigation, but you also have an infrastructure where you can meet with the right people to make decisions efficiently and quickly without having more meetings than you need.

Another piece of it is NASA has to agree—and JPL—to stay out of your way, not to require huge reviews and different things, because those all take time. They have to have to trust you and set up a schedule that basically says “Okay, if you're able to satisfy your Level One requirements and you're still in budget, we're not going to bug you.” If you show that there's risks that all the sudden rise and you can't deal with them and you need more money, then of course you're going to be open to large reviews and decisions that might be out of your control. JPL and NASA kind of encourages that kind of thinking, and I think that's a big part of it.

We spent a lot of time in the proposal looking for trade space options that we could carry out, should we run into a problem. We kept orbit trade space open. We kept instrument measurement trade space open, different things in development that had to do with the technologies, even solar power, things like that.

We had designed when we needed to make that decision by in order to get the savings, because if you wait too long, you don't save any money. You've got to maintain the schedule. You have to understand that. So not only was that put into the proposal, but it was integrated into the traceability matrix, so that when a decision would need to be made, I could very quickly assess the impact to the science and whether or not it was worthwhile doing or possible to do without a major effect or as a minor. So it enables you to make decisions very quickly, and I believe that that method works. That may be unique to PI-class missions, because having a science team and a scientist at the top level of that decision tree is very important, because there are no discussions without us in there. It's not that we're smarter, it's just that the decisions need to be driven by that and not by some technology that engineers assume we need.

I had the ability—and I surprised Lockheed and JPL a few times—where I just changed my requirements midstream. I would look at it and say, “I'm not going to do that.” The first time I did that, I remember Lockheed saying, “Well, you can't change that. It's in the requirements. You would need a waiver.”

I said, “No, I'm going to change the requirements instead. I don't want to bother with a waiver. I'm just going to change these right now. I didn't know that this requirement was going to cost this much. Now that I know that I don't need it anymore.” And they were shocked. I couldn't do that with something called Level Ones, which is a contract with NASA, but I can do that with the other requirements.

So if you set it up right, you have a lot of flexibility there, and I think that's an enabling feature that can allow you to make a change and stay on schedule when otherwise you might not have been able to. And it's very hard to do that in a flagship where you have so many people

working and so many different levels. If you look at even Europa Clipper's traceability matrix, it's not designed in a way that you can enable that kind of decision very easily.

They're at some risk because they lock themselves into an orbit, even, thinking that they can't change it, when you don't know what you're going to get when you get there. I had the orbit flexible and had looked at different orbits, primarily because I wasn't sure what the radiation environment was going to be like. It turned out we had to change our orbit because there was a symptom in the rocket engine, in the fuel lines that made us worry that it wouldn't work the way it was supposed to. But the fact that the orbit was flexible allowed me to quickly assess whether that could be done. And, of course, we ended up doing that. That kind of flexibility was very, very powerful.

CONWAY: So it sounds like that kind of traceability matrix with the timing of decisions that had to be made already built into it didn't catch on.

BOLTON: I don't know if it didn't catch on. Our traceability matrix was rather unique. It was sideways to what everybody else was doing. I just kind of turned it on its side. It was much more elaborate than most of them that you see. If you just look at the traceability matrix, it doesn't go down into a trade space. It doesn't include that, but what it does include is a science goal, a question, a science objective, a measurement objective, a measurement requirement, a spacecraft requirement, a navigation requirement, a data analysis requirement, and eventually leading to a data product. And because all of those things were well-defined, I could see what the spacecraft requirement or the navigation requirement or the measurement requirement had on any given question or any given objective. So if I was going to have to give up on something, I could see

right away, “Okay, I’m going to compromise this requirement right here. What is that going to do?”

And another piece of what we folded into the Juno proposal is, I’m a very big fan of what’s called science margin. So when you’re writing a proposal, NASA requires you to carry cost and schedule margin and reserves, and typically during Phase B and C and D, those could be very high, 30 percent or higher, and you need that. I’m not necessarily disagreeing with that.

But what I also carried with science margin—and I advertise it, I understood where it was, so my measurements were a little more than I needed, let’s say, to accomplish things, and so that gave me some flexibility. And if you understand that, if I couldn’t get the measurement exactly the way I wanted it, or if I didn’t get all of them, could I still accomplish the science objective and the Level One requirement? And we had a lot of that, and so as we went through things, if I had to drop something or compromise something in order to save money, I could still maintain my Level Ones because I had what I called science margin. And, of course, if you could get all the science margin, then you got more science, which was great. But you never knew. Part of that was I could lose an entire instrument and still accomplish my Level Ones. So you have to work that.

And the other part that you have to work is there’s something called a minimum mission. There’s the baseline mission and then the minimum mission that you define, and that minimum mission needs to be defined very well and carefully thought through, not that you’re going to propose to carry that out—nobody wants that—but what you learn as you create that—and there may be different branches for that minimum mission—that really teaches you what you might need to do during development, during Phases C and D, and even E, as it turns out.

So our minimum mission was rather complex. It had different facets to it, but what it was really defined as, in NASA terms, is if you go below this, you're ready to recommend termination, because you're basically saying it's not worth doing.

CONWAY: Right. You won't accomplish your primary mission objectives, so why bother spending the money.

BOLTON: Right. Well, you're not going to accomplish what you even think is the minimum. And I think they look at that, but you have to define that right. It could be that a review board doesn't agree with you that the minimum mission is worth it or that you have it defined correctly. We got a lot of strengths.

One of the strange things about Juno was once we submitted it, then NASA came out with the Discovery call right after, and they said they were going to do it before we knew that it was coming. So Charles Elachi turned to me and said, "Put in a Discovery version proposal, just in case you don't win the New Frontiers."

CONWAY: Good grief.

BOLTON: So he convinced me to do that, and so I didn't want to bother the team, because we were exhausted, but what I said was, with a very small group of people I would take the Juno New Frontiers proposal and edit it down to what fits into Discovery, and I would just do that pretty much by myself with a few helpers.

And we did that, and it was a fly-by. It went back to the fly-by. It didn't end up doing Inside Jupiter, because everybody felt the microwave is too important. So we ended up submitting that, and I had a table in that proposal that basically said, "Here's Juno and here's JASSI," so that somebody could see what's in and what's not very quickly and easily, because I couldn't hide the fact that everybody knew Juno was submitted, and I think maybe we won the first round, even, by then. I don't remember the timeline exactly.

Then it turned out that they didn't make any Discovery selection on that round. Did we talk about this when we saw each other? I might have told you this story.

CONWAY: No.

BOLTON: No? Oh, I know. I told it to David [W.] Brown, maybe, because he's right trying to write the Discovery history.

So there was this round where no Discovery selection was made. Susan Niebur gave me my debrief and she basically said, "JASSI is the first proposal we've ever had that went through a review and had no weaknesses, no minor or major weaknesses. We've never seen that."

And I said, "Well, that was a flaw in your review, because I can tell you there are some weaknesses in it."

But they said, "Well, people really thought of it, and we knew you were going to win Juno, so there was no way that we could select this. You can't have two missions."

And I said, "Yeah, I understand."

So there was a review by NASA by a guy named [N.] Scott Pace. He was the AA [Associate Administrator] for Program Analysis and Evaluation and later Deputy Chief of Staff

for NASA. He was doing a review of why they made no selection, whether that was the right answer, and what went into those decisions.

So they asked me to come and testify to this thing or do this thing, and basically one question is, “Do you think NASA could select a Discovery mission to go into second round if there was only one and it didn’t have any competitors?” There were people at NASA that felt, no, you needed to have a competitor.

My answer to that was, yes, that’s possible, because in the second round, Step Two, you’re really competing against yourself. The question is, “can your engineering, under further scrutiny and further investment, prove to be affordable and still maintain the science.” And I said that’s really what Step Two is about, is a closer look, and can you actually accomplish the science for this cost, and do we fully understand the risks associated with whatever needs to be developed. So I said, “I think that they could do that.”

And then there was other pieces of it, which is that CONTOUR [Comet Nucleus Tour] had been proposed along with JASSI on that round, and it did not make it to the second round. Only JASSI was the only mission that was going to be advanced to Step Two. And I couldn’t comment on that, other than I didn’t understand how that could happen, since CONTOUR had already won and been launched and lost. How could another review find that it’s not great? So I said I don’t understand how that worked, but I wasn’t privy to that. I haven’t read their review.

But I did talk to Joe [Joseph] Veverka, who was the PI of it, and he was perplexed as well. I felt bad for him—he was a friend of mine, and still is—that he kind of got pushed aside there for whatever reason.

And then there was another guy from Goddard who had done a Venus mission, and he also was interviewed, but I’m not sure why he couldn’t get into the Step Two.

But, anyway, they ended up not technically having a Step Two. They just gave me a briefing saying I would have been in it. But I won Juno, and obviously that would have been my preferred choice. But it was a little strange.

So Scott Pace was running it. So you've heard of him.

CONWAY: Yes. I think he was Office of Policy and Plans, and I believe he's at George Washington [University] now.

BOLTON: He might be.

CONWAY: GWU. They have a Space Policy Program.

BOLTON: I haven't spoken to him for a really long time.

So that wasn't a story about Juno, but it was tied to it, the fact that Charles had gotten me to send in this other proposal. And the interesting thing is the first JASSI proposal got slammed. The second one, no weaknesses. So obviously the education of our team, plus the Decadal results, plus going out into the community and advertising what we could do really worked.

CONWAY: Yeah. Well, thank you. We are over-time.

BOLTON: Sorry. I hope you can edit it down to what you want.

CONWAY: Oh, no. Comprehensive is great. Actually, it's not unusual for PIs to take two or three hours, so I'm used to it, and like I said, comprehensive is great because it's overall better to just really completely understand these missions. So thank you for your time.

BOLTON: I appreciate what you're doing. Thank you.

CONWAY: All right. We'll speak again. Have a great day.

[End of interview]