DISCOVERY PROGRAM ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

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NIEBUR: Today is August the 27th, 2009. I am sitting in Tom Krimigis's office at the Applied Physics Laboratory [APL]. We're going to do an oral history for the Discovery Program oral history project. My name is Susan Niebur, and I'll be doing the interview.

So, Tom, can you tell me a little bit about your background?

KRIMIGIS: Well, I was a student of James Van Allen at the University of Iowa a few years after he had discovered the Van Allen belts. It was a very exciting place to be, so I got involved in hardware. About a year into my graduate career, Van Allen came to me and said, "How would you like to be a Co-Investigator on the Mariner mission to Mars?" I said, "Wow."

I had some experimental background because I was a student assistant at the University of Minnesota. As an undergrad under Jack [John] Winkler, I was building ion chambers that we were flying on balloons at that time to look at so-called solar cosmic ray, solar particle events.

NIEBUR: Is that the lab that Bob [Robert O.] Pepin then came into?

KRIMIGIS: There were several labs. It was a different one from Bob Pepin's. But this one was Winkler and Ed [Edward R.] Nye, mostly. In fact, Van Allen had come to give a talk at

Minnesota, and he came down to the lab and he stopped by my bench as I was working, [building an ion chamber], and he asked me if I had any plans for grad school.

I said, "Well, I thought I might go."

He said, "Why don't you apply to Iowa?" So, that's how I ended up in Iowa City. And within a year or so, I built one of the first solid-state detectors that were ever flown in space and put it on Mariner 3 and 4. In those days, we used to make two of each, and we had Mariner 3 and Mariner 3. Mariner 3 didn't make it, but Mariner 4 did, and that was the first spacecraft to encounter Mars. The pictures were rather disappointing at the time, because, despite all the hoopla about Mars, as we all know now, it turned out to be pretty dry and pockmarked by craters, and so on.

Our objective was to see whether there were radiation belts at Mars, and needless to say we didn't find any, which was the first wake-up call that, well, maybe not every planet has a magnetic field. But we did discover things on the way, like we observed for the first time that the sun was putting out electrons in small flares.

NIEBUR: I didn't realize that.

KRIMIGIS: That was a discovery paper, Van Allen and Krimigis, 1965.

NIEBUR: Wow. That is exciting.

KRIMIGIS: Yeah. Then, after I graduated, I was asked to stay on as assistant professor, and I did Injun V [satellite]. Before that, I had built a detector for Injun IV. Now, these were University of

Iowa satellites. This is in the early '60s. There were entire satellites that were built at the university department in the tradition of Explorer 1 and 2 and 3, which is what Van Allen had done to prepare detectors, but now we were building the entire spacecraft.

And so, I grew up in this kind of an atmosphere where everything was possible, but you did these things very quickly and without a lot of paperwork. You tested a lot, and then you flew. We had Injun III and Injun IV. We built detectors for Injun IV, and with those we discovered that the radiation belts had Helium nuclei as well as protons and electrons (Krimigis and Van Allen, 1967).

And then, Injun V, but by that time I was getting a bit anxious to do different things and build my own team, and I had a standing offer from the Applied Physics Laboratory. I came here in the fall of 1968 and joined the small instrumentation group that they had, and began to be involved in more planetary, Earth orbit, and interplanetary projects. At that time, I was a principal investigator [PI] on the IMPs, [Interplanetary Monitoring Platform], and so I built the detectors for those, and we got a lot of science out of those.

NIEBUR: I was still using IMP-8 data at my dissertation.

KRIMIGIS: I didn't know that.

NIEBUR: Yes. I came from galactic cosmic rays, so I'm fascinated by what you've been saying.

KRIMIGIS: Well, you probably used mostly the [John A.] Simpson instrument.

NIEBUR: Yes, just a little bit.

KRIMIGIS: We continued the so-called monitoring program with data from the CPME [Charged Particles Measurements Experiment] for which I was PI. It worked like a charm all the way to the end, until IMP-8 was abandoned, after some 30 years.

Soon after getting to APL, I decided to propose for the then outer planets Grand Tour, and we assembled a team. George [M.] Gloeckler was a member of the team. He and [William] Ian Axford and Charlie [Chang-Yun] Fan from [University of] Arizona. Some of these people you probably wouldn't know. Charlie has just passed away recently; some others are mostly retired. Others on the team were Lou [Louis J.] Lanzerotti [Bell Labs], and Tom [Thomas P.] Armstrong [Kansas University].

NIEBUR: Yes, him I don't know. Gloeckler, of course.

KRIMIGIS: Yes, and Carl [O.] Bostrom from here who was in the Space Department, and he had hired me. We went up against John Simpson, Peter Meyer, Jim Van Allen, Frank [B.] McDonald, and Robbie [Rochus E.] Vogt from Caltech [California Institute of Technology]. Each of course had a different take on things, but it turned out that our instrument was selected for Voyager program, the name selected for the Grand Tour.

Simpson and Van Allen and Peter Meyer were left out, and the other team was Robbie Vogt that teamed up with Frank McDonald and Ed [Edward C.] Stone. So, those were the two

principal particle instruments for Voyager. Needless to say, as you know, Voyager has been terrific venture, as you saw last year.

NIEBUR: Absolutely, the cover of Nature [magazine].

KRIMIGIS: Last year, this place [APL] had the cover of Nature and the cover of Science, with MESSENGER [Mercury Surface, Space Environment, Geochemistry and Ranging], in the same week.

NIEBUR: Was it really?

KRIMIGIS: Yes, yes.

NIEBUR: Wow. Now, I knew about the Science coup. I think I must have missed the Nature cover. What issue is that, just for the record?

KRIMIGIS: This was July [3rd 2008].

NIEBUR: Volume 454, [Issue 7200]. Okay.

KRIMIGIS: Yes. And they also did a News Feature—they had an overview article in there.

NIEBUR: Nice.

KRIMIGIS: And we did some interviews. Basically, what they wanted to know is what had been most memorable and they called three of the former PIs who were there—yes, I'm still the PI.

NIEBUR: Oh, how nice.

KRIMIGIS: And asked us—John [W.] Belcher of MIT [Massachusetts Institute of Technology], who succeeded Herb [Herbert S.] Bridge who was the original PI [he passed away], and Norman [F.] Ness. They asked us to reminisce about various things.

NIEBUR: How nice. Voyager's done some amazing work.

KRIMIGIS: Yes, and it still does. We're delighted with what it's doing. And in many ways, it's influenced a lot of things. Do you know about IBEX [satellite]?

NIEBUR: Yes.

KRIMIGIS: Well, there's going to be a special issue for Science on IBEX.

NIEBUR: Really?

KRIMIGIS: But, in that, there will be a paper from our Ion and Neutral Camera [INCA] on Cassini [spacecraft]. We essentially have a picture of the sky in energetic neutral atoms. We just sent the paper into Science on Monday.

NIEBUR: Wonderful.

KRIMIGIS: And it's quite a revelation. Of course, Voyager provides the normalization point by being there for imaging the heliosphere. And now George Gloeckler and I will talk next week at ISSI [International Space Science Institute] in Berne [Switzerland], where they have a workshop on the suprathermal particle slope that George Gloeckler and Len [Lennard A.] Fisk predict in this model and seems to be observed everywhere. It has a lot to do with what's happening beyond the termination shock. Anyway, I told you a lot more than you cared to know.

NIEBUR: Not at all. Actually, that was wonderful. So, Voyager. You were a Co-I on a Voyager instrument.

KRIMIGIS: No. I was a PI.

NIEBUR: You were a PI—I'm sorry, a PI on a Voyager instrument.

KRIMIGIS: The PI on Cassini and various other programs.

NIEBUR: Right. I apologize. I misspoke.

So, what happened after Voyager? Did you go directly from that into other flight projects?

KRIMIGIS: Oh, yes. Well, I was PI on AMPTE, the Active Magnetosphere Particle Tracer Explorers, where essentially we released some lithium outside the magnetosphere trying to trace where the solar wind was coming in.

NIEBUR: Wow, interesting.

KRIMIGIS: We never did find out, but we established some upper limits. It was a joint program between the U.S. and Germany, Max Planck Institutes, Gerhard Haerendel in Garching, Germany. And the British, in the end, took part in that, and that's where the first pick-up ions were detected, by the way.

NIEBUR: Okay. Well, there you go.

KRIMIGIS: And so we did that, and it was a terrifically successful scientific program, and by the way, it was, I think, the first, if not the first, the second, mission where NASA selected the single PI for an entire mission on the basis of a proposal that the science investigators pulled together. We proposed that. George Gloeckler was part of that. He did a very nice instrument for charge and mass composition. I'm saying this because it's connected to Discovery [program], in many ways. What happened is that we were able to do that mission, on cost and on schedule, and it worked like a charm for five years. I think that the spacecraft was eventually killed by radiation.

NIEBUR: Now, was that in the old Explorer model?

KRIMIGIS: Yes. It was the Explorer program. And it turned out to be, the way we proposed it, a three-spacecraft mission – a US spacecraft, the German spacecraft, and the German spacecraft had a British sub-satellite on top of it.

So, it was a small team. We had a wonderful time, and the science was really terrific that came out of it. The first measurements of the Earth's ring current, for example, were made, and the composition and what it consisted of for the first time. And the pressure, the plasma pressure versus the magnetic pressure and all that stuff that had never been done before.

NIEBUR: Were the instruments built here, or were they built other places?

KRIMIGIS: Well, one instrument was built here. One was built at the University of Maryland. One was built at Lockheed. And in the end, we also managed to get in a plasma wave instrument out of TRW with Fred [Frederick L.] Scarf, someone you probably didn't know.

NIEBUR: I do know the name, actually.

KRIMIGIS: You know the name? He was a wonderful scientist, a very good friend of mine. And it's a shame that he passed away.

So, I did that, and that was in the 1980s. And of course, I proposed as a Co-PI with Don [Donald J.] Williams for Galileo, and we built an instrument for that. Also on Ulysses with Lou Lanzerotti as PI, and we built the instrument here.

NIEBUR: And then Cassini.

KRIMIGIS: And then Cassini, right.

NIEBUR: And Cassini's huge. I mean, Cassini is like one end of the spacecraft spectrum, and then these little Discovery, like NEAR [Near Earth Asteroid Rendezvous], end up being at the other end.

KRIMIGIS: Well, the decade of the '80s was a real disaster for the planetary community, because the only thing that was going at the time was Galileo. And it slipped, and there were all sorts of problems, and it was going to be flown on the [space] shuttle, and then it wasn't, and problems went on and on.

So, nothing much was happening, and that's when the Explorer program was shining, because it was possible to really do something for a few tens of millions of dollars and really get good science, focused, not very extensive, not very broad, but quite focused.

So, that was AMPTE, and that was my experience with the AMPTE spacecraft. And after we did that, both George and I and Glenn Mason said, "Hey, you know, we ought to do a job on charge composition for solar radiation from L1 [Lagrange point], from plasma to cosmic rays, in the same manner."

So, Glenn hosted a meeting at he University of Maryland, George and Glenn, and invited John Simpson and Ed Stone, and also Len [Leonard F.] Burlaga from [NASA's] Goddard [Space Flight Center, Greenbelt, Maryland]. We cooked up the idea of having a so-called AMPTE follow-on at the time, but that would be an interplanetary Explorer at the L-1 point, and it would make all these measurements.

At the time, we said, "Well, who's going to be the PI?" Needless to say, once you're a PI on something like AMPTE, you don't want to do anything like that for a while. Ed Stone was—I wouldn't say forced—but persuaded to take the lead for ACE [Advanced Composition Explorer]. And of course, he always does a wonderful job and, as you know, he had been the project scientist on Voyager all these years. He did a terrific job.

So, we did it. We persuaded NASA. They had sent out a letter, a request for proposal concepts. We went in with that, and we were selected, and we did the study. We actually started in '85 or '86, but they sat on it for a while, and then they stopped, and then they went, and eventually they gave us some funding, and we did a study in '89. We had briefed it to Goddard, who were the Explorer program office—program manager, I guess—and we had just briefed that after we had finished the study.

I had, in the meantime, been asked to be a member of the Solar System Exploration Committee working a strategic plan, which was convened in the summer of '89 in New Hampshire. I think that I narrated in that article [Journal of the Astronautical Sciences, 1995]. It was there, and it was such an illuminating experience of sorts, because the community was down in the dumps. We had just had the whole decade with just Galileo, and it was still on the ground. There was the start on the so-called Planetary Observer [program], which had been part of a

previous strategic plan, and it was supposed to be very cheap and so on and so forth. And that was the time when people were finding out that it wasn't going to be cheap, and it had slipped.

There was this small working group with Bob [Robert A.] Brown from Space Telescope Science Institute; he was the leader to see, "is there really a concept for low-cost planetary missions?" We in the group persuaded ourselves that there was, and so Bob came and made the presentation to plenary.

I was being polite in the article; I said the presentation was met with widespread skepticism. In fact, I remember Don [Donald M.] Hunten, who as you know, is a rather outspoken fellow, and he said, "How? We've tried that low-cost concept before, and it's called Planetary Observer. Why don't you guys go away?" And some other people, I think justifiably, were expressing grave doubts.

That's when I said, "Look, the Planetary Observer is the wrong paradigm. Why don't you look at the Explorers? They've been going on for decades now. They do focused science in a very cost-constrained environment, and they do a good job."

He said, "Okay. How about giving us a presentation tomorrow morning?"

What was I going to do, you know? So, I called my secretary at APL, and I said, "Fax me the viewgraphs we presented to Goddard on ACE."

NIEBUR: Really? See, I had no idea they were that clearly connected.

KRIMIGIS: Yes. Yes. In fact, it was that same week we had presented the end of the study on ACE to Goddard. So, she faxed me copies of the viewgraphs on the spot. The next morning, there I was. I stood up there to make the presentation. Joe [Joseph] Veverka was the chair of the

plenary at that time, and so I described the program and the spacecraft and the instruments. Nine instruments. I think it was Veverka who said, "All right, Krimigis," he said, "What's the cost?"

I said, "Why?" I said, "Joe, you guys have a lot of experience. What do you think a program like that would cost? It has propulsion. It has a lot of instruments. It has this and that."

He says, "$400 million."

I said, "Well, you know, you're close, except you have one zero too many."

He says, "That's not doable."

I said, "Yeah, it's going to be. Ed Stone is the ACE PI and we just finished AMPTE on cost and schedule." At the time, ACE was being estimated at $70 or $80 million. It was $40, 45 million for the spacecraft and about $30 or so million for the instruments. And, by the way, we brought it in fairly close to the cost, except that it sort of started much later—it's more money than that in real-year dollars, but not much.

So people were taken aback because they—I personally was very surprised—the planetary community didn't know much about the Explorers.

NIEBUR: You know they don't crosstalk.

KRIMIGIS: There is no crosstalk. I mean, it was just amazing to me. They said, "Well, you know, maybe the small group should go ahead and continue to study that." And then, Geoff [Geoffrey A.] Briggs, then head of Planetary, came up afterwards and said, "Hey, how about sending me an unsolicited proposal to do a study at APL?"

So needless to say, we did the study, and then the politics came into play. The politics were really amazing at that, because, as you know, up until that time, [NASA's] JPL [Jet Propulsion Laboratory, Pasadena, California] was the laboratory for planetary programs. Nobody else was in play. And so, Geoff said, "Well, we have to also ask JPL to study this."

We said, "Well, fine, yeah, all this." In the interim, Geoff moved on and Wes [Wesley T.] Huntress came, and I describe in this article how the committee under Brown essentially looked at the whole spectrum of proposals but didn't make any specific decisions except to decide that the name of the program would be called Discovery.

When Wes came, he said, "Well, you know, I want this thing to focus on a mission. And Veverka, you be the chair and see what you can do." They gave specific instructions to do a study at APL for NEAR, and JPL for any other thing, including NEAR. After the studies were completed, we had this meeting in Pasadena where Wes had gotten together a team of senior managers. It was, I think, chaired by Jim [James S.] Martin. I don't know if you've heard of Jim Martin.

NIEBUR: I did. I have.

KRIMIGIS: You know, Jim was quite a character, a very strong personality. He was the Viking program manager in the '70s. We had done the study at APL, and we thought we could do it for $110 million. Tom [Thomas M.] Coughlin, who was the study manager, made the presentation. Bob [Robert W.] Farquhar by that time had left the government and joined the laboratory and did the mission design. By that time, I was the Space Department head and I had put the team

together, with Andy [Andrew F.] Cheng as the project scientist and Rob [Robert E.] Gold as the instruments manager.

NIEBUR: Now, why Andy? He was very young at the time, wasn't he?

KRIMIGIS: He was, yes.

NIEBUR: What did you notice in him that you thought he should lead a project for you?

KRIMIGIS: Well, he was a very smart guy, and I had hired him into this department. He used to be at Rutgers [University], and he was assistant professor there. He had graduated from Columbia [University] with Mal [Malven A.] Ruderman as his advisor. And he had shown up at the Jupiter flyby of Voyager 1. I think he might have been a post-doc of Lou Lanzerotti for a while. Lou is a member of our Voyager team and had invited him. And he came to JPL. I was so impressed with how smart he was and how quick he was in catching some of the physics. He listened to all of this stuff, and then he goes away, and a couple months later he comes up with this ApJ [Astrophysical Journal] paper that, without actually analyzing any data, just taking the data we had published in Science from all the teams, did a terrific synthesis of the magnetosphere and the sources from Io [moon of Jupiter].

I read that paper, and he wrote so well and the physics were so clear, and only in the end I realized it didn't have a single figure in it, not a single figure. So, I recruited him, and it turned out that his wife was a doctor, and also was in between jobs looking to where she was going to go next, and so she decided to come to NIH [National Institutes of Health]. And the rest is

history, as they say. But then we worked together, and I knew that he had the breadth of knowledge. Andy, as you know, can be quite comfortable talking about black holes and also planetary surfaces.

So, anyway, that's why I picked Andy to be the project scientist, and he did a very good job. I must add here that there were doubts expressed by the late Henry [C.] Brinton—then program scientist for Discovery at Headquarters—who came to see me specifically about Andy. He thought Andy was too soft; he said, "You know some of the scientists on this team are like you—real barracudas—and will eat him alive." I thanked Henry for the "compliment" (never thought of myself as a barracuda) and assured him that Andy had the science knowledge and intrinsic toughness to deal with the personalities on the science team.

NIEBUR: Excellent choice.

KRIMIGIS: In fact, I remember we briefed NASA Headquarters afterwards. Wes Huntress took me aside, and says, "Where the hell'd you find him?" [laughter] So, that was his response.

NIEBUR: Excellent. That's a good response. Excellent. Okay. So, you're putting the team together.

KRIMIGIS: Yes. And so, we go to Pasadena, at the Hilton, or wherever the meeting was with the committee, and the APL team and the JPL team was asked to make their presentations. The APL team made their presentation and Coughlin and Andy and Farquhar had the estimate of

$112 million, and they had the pedigree of AMPTE and other projects where the costs were in family—AMPTE was $30 million, mind you, for the mission.

Then, the JPL team came on, and it was a remarkable presentation. I'm trying to recall the name of the fellow who was leading the study, John—I'll remember it. Anyway, he said, "Look," he said to the senior managers who were sitting there, he said, "Let us tell you one thing at the outset. There ain't no planetary mission you can do for $150 million [the NASA committee's bogey for launch plus 30 days, exclusive of the rocket and mission operations]. We're going to explain all this, but we want you to know the bottom line right at the outset." So, people were kind of very curious about this approach.

What they laid out was a study whereby you start out with a bus that essentially has no instruments on, and then you kind of shoot it up and see whether it would survive, and that was $150 million. And then, you do a second one, but now you would put on a camera and see whether you could stabilize it enough to really get any images. That would be another $150 million. Then, the third thing would be the evolution of that, which would have a camera, and, I think, a spectrometer on it, two instruments or something like that, for another $150 million, and try to orbit an asteroid. So, it would take 10 years and $450 million to do anything worthwhile to go to an asteroid.

Needless to say, people were absolutely stunned at that response, and I remember Jim Martin getting red in the face, and he says, "You people think we are stupid, don't you?" I mean, he was absolutely raging.

NIEBUR: Wow. I can't even imagine the gall, that someone would come up and say that.

KRIMIGIS: Yes. That was it, you know? Needless to say, the next day, Ed Stone, who had just become the JPL Director, heard about the fiasco, came back to the committee, and said, "Well, you know, we were not clear. There were some misunderstandings," and he is going to direct the team to really take a different approach, and that was the conclusion of that meeting. Afterwards the committee of senior managers recommended unanimously that Headquarters go to APL and have this mission done.

Okay, that's when the real politics came into play. I alluded to that in that write-up [S. M. Krimigis and J. Veverka, Foreword: Genesis of Discovery. J. Astronaut. Sci. 43 (1995), pp. 345–347]. I get this call from Len Fisk, who was the Associate Administrator, "Look," he said, "It's going to be very difficult to really assign this mission to APL, because the first Discovery mission must be done at JPL. What we are going to do is we're going to award you the NEAR mission, and we will take the MESUR [Mars Environmental Survey] /Pathfinder study that was being done at Ames, transfer it to JPL, and that will be the first Discovery mission. And then, NEAR can proceed."

NIEBUR: And how did that go over back here at APL? I mean, you were excited to get the NEAR, I would imagine, but as number two after you did so well at Pasadena? I always wondered how it went down back here.

KRIMIGIS: Well, it didn't go down very well with me, to say the least. The team was disappointed, and so I was very upset about the whole thing. Len sent the official letter with all of this to me, and after things sort of settled down a little bit, it was time for the Congress to write their appropriations for FY '94. And they were aware. In fact, Congress had been pushing

to have NASA do a low-cost planetary mission, and the only thing that NASA had was this study. And NASA decided to put it in the budget.

At that time, Dan [Daniel S.] Goldin had now become NASA Administrator, and, as you know, they had a falling out with Fisk, and they proposed to have a new start for Discovery, but without specifying what kind, or which mission it would be. They said, both. They said MESUR/ Pathfinder and NEAR.

So, I had to ask Farquhar to see what kind of launch dates were possible for near-earth asteroids, because, initially, we had an asteroid [Nereus] that would have a launch, I think, in '98 or thereabouts. I don't remember. So, he looks at it, comes to me, and says, "You know, I got something. If we do an Earth flyby, we can go to Eros [asteroid], and it would launch in February of '96. Now, MESUR/Pathfinder was supposed to launch in December of '96.

So, we studied that, and it was a very nice mission. Eros was a much more interesting asteroid than Nereus—I think that was the original target.

NIEBUR: I have it somewhere, yes.

KRIMIGIS: So I said, "Well, we're going to tell Headquarters about this," and we did. In the meantime, in the Congress, there were these power plays. The House wrote into the bill that they appropriate $50 million, but it would be only for MESUR/Pathfinder. Some people were at work with this House committee, which made us exceedingly unhappy. And then, the Senate committee, with Senator [Barbara] Mikulski wrote in the bill two things: a) that the $150 million ceiling had to be respected; and b) NEAR must also start. That was it.

NIEBUR: Good.

KRIMIGIS: The subtext on that was that the Mars Pathfinder mission, that used to be called MESUR/Pathfinder mission, needed the $150 million plus a $40 million rover that would be supplied by the Astronautics division of NASA. So, the total cost would actually be $190. So, by the Senate saying that there had to be at $150 million for the total NASA spending, it excluded MESUR/Pathfinder.

NIEBUR: Tricky.

KRIMIGIS: Yes. Some people thought that it might be a good negotiating tactic, and that both missions could go under these conditions. Needless to say, people became very alarmed, and in the end, there was money available for both missions in the House-Senate Conference Committee report.

NIEBUR: Very nicely done.

KRIMIGIS: Yes, ma'am. And we had a kickoff in December of ’93 for NEAR.

NIEBUR: Yes, because it was FY '94.

KRIMIGIS: Did I say 94, pardon me, '93, because by December of '95, we shipped to the Cape [Canaveral, Florida].

NIEBUR: So incredible.

KRIMIGIS: So, two years. As I said, 24 months from the kickoff to the time we actually shipped. One of the issues that came up was that of contingency. In this place, we worked with agreements between the program office and the supervisors who did the technical work in their groups, and we kind of signed internal contracts where we would say, "Okay, you are doing the C&DH [command and data handling] system. Here are the specifications. Tell me how much it would cost." The group supervisor would sign a contract with the program manager that it was going to cost say, $2 million; with details, mind you, and people and resources and everything, it was all there.

We would sum things up. Sometimes the program manager would second-guess the group supervisor, depending on the history of the groups, and he knew that the group that was doing the mechanical work, for example, sometimes was low on their estimates, whereas the group doing the data acquisition, or the star tracker, would always overestimate. So the program manager would make these kinds of adjustments.

So, Wes [Huntress] comes to me and said, "Okay, what about contingency?"

I said, "We don't work with contingency."

"What the hell do you mean?"

I said, "Well, you know, we make these estimates, and everybody is expected to live within those costs."

And he says, "Well, you know, we can't go on the program without contingency."

I said, "Look, if you want to have contingency in the program, you do it, and don't you dare tell us anything about that."

NIEBUR: Because to tell you about it—

KRIMIGIS: And that has been our philosophy. If you identify contingency, everybody thinks it's theirs, and then they go ahead and plan on spending it.

NIEBUR: Of course. Yes.

KRIMIGIS: That's the way life is. That's the way people are. So, it was funny, but then I think a year later we had the first meeting on low-cost planetary missions here, and it was sponsored by the International Academy of Astronautics. We had, like, 300 people at the Kossiakoff Center auditorium here, and Dan Goldin was going to be the dinner speaker.

We sat there at the table, and then he got up, made his remarks, and in his inimitable way said, “Well, you know, we’re going to do the Discovery Program, and wouldn’t you know it, we said the limit is $150 million, and everybody came in at $150 million.” I sat there dumbfounded because I knew we had estimated it for $112 million.

I said, "Well, you know, not everybody has."

He says, “That’s what I know.”

NIEBUR: Oh, how interesting.

KRIMIGIS: Then, it finally occurred to me that that was the contingency that they were holding at Headquarters. So, I didn't say a whole lot at the time. In the meantime, we had a couple of situations where, as you know, I think SAIC [Science Applications International Corporation] was the official advisor and cost estimator for the Planetary Division. Wes sent them out to see how we were doing within six or eight months of the beginning of the program. They went back and gave him a report, and they said, "Well, this program is going to cost at least two and a half times what these people said."

NIEBUR: Wow.

KRIMIGIS: And so, Wes, to his credit, thanked them and said, "Okay, thank you, I'll take that under advisement". In the meantime, we had a wonderful program manager from NASA Headquarters. Betsy [Elizabeth] Beyer. That was the other part of the agreement I made with Wes, that no NASA center would be involved in this program. Betsy Beyer was the Headquarters program manager, and she would come out here and sit with Tom Coughlin, and they would go over everything. She had true visibility on everything we were doing. She was giving reports to Wes and saying that these guys are doing the work, and it's good.

Now several months had passed, and I don't know whether this was a contractual obligation, but Headquarters sent out another SAIC team to check on the cost and schedule. Of course there had been considerable work done already. So, they went back with another report, and they said, "Well, okay, we might have overestimated the overrun the first time, but it's going to be only at least 50 percent higher than what these guys have estimated ."

So, Wes thanked them again and so on, and then, of course, we shipped to the Cape and got ready to launch. We hadn't used up the money we said we needed. People had worked very hard, lots of hours, and these contracts really held between the technical groups and the project office. We had all the required reviews, but those days, the reviews were the CoDR/PDR/CDR [conceptual design review/preliminary design review/critical design review], and pre-ship and launch readiness. It wasn't a review every other day, as is happening now, by some other group that felt obligated to write all kinds of unfunded mandates on what everybody ought to do.

NIEBUR: Well, we'll get to that part, and I do want to get to that part.

KRIMIGIS: So we launched, and the spacecraft was working fine and all this, and so I thought that it would be appropriate, when it was launch plus 30 days, nominal—

NIEBUR: The end of C/D.

KRIMIGIS: Right; that we should have some sort of a reception and thank the staff. We invited Dan Goldin and Senator Mikulski, and that's when we gave back the ceremonial check for $3.6 million.

NIEBUR: Famous pictures.

KRIMIGIS: That was the point. Dan Goldin used to be very unhappy with me, but after that we couldn't do anything wrong.

NIEBUR: Why was he unhappy with you?

KRIMIGIS: Well, he used to regularly complain about things, and Farquhar would make some disclosure on our plans or sort of say some blurb to Space News, and they would call me up and say blah, blah, blah.

NIEBUR: I see.

KRIMIGIS: I think it came as a revelation to Dan Goldin that we hadn't spent $150 million. Up to that time, that was what was being bookkept. Subsequent to that, what happened is that Wes was able to fund the Stardust mission with $35 million that hadn't been used by NEAR, and that program was able to get a faster start. By the way, I got a lot of abuse from those who saw that check, that picture, mostly from some of our center colleagues who were upset—they said that we broke the mold—nobody ever sends money back to NASA.

NIEBUR: Oh, so it wasn't that they were doubting that you had done it for cost.

KRIMIGIS: Oh, no.

NIEBUR: It was that they didn't think you should.

KRIMIGIS: We shouldn't really advertise that fact, was the point.

NIEBUR: How interesting.

KRIMIGIS: Yeah. Well, Susan, you can't appreciate the atmospherics that were in play during the '80s and the early '90s before all this happened. I mean, every contract—the entire NASA program—was overrunning by factors, and that was sort of the normal procedure, and the contractors were very happy with that. When we said, "Oh, you could certainly do it for $150 million," most people laughed, said, "Ha!" So then, it happened, and all of a sudden people said, "Oh, yeah. This can work."

NIEBUR: Did you maintain that cost savings going through operations?

KRIMIGIS: Oh, yes, we did. We had, as you know, a mishap on the first attempt at orbiting Eros. Something went wrong between the spacecraft and the attitude control computers, and we lost a lot of gas and we had to go one more orbit around the Sun, so another year of operations was added. But it was basically at the same rate, so to speak, so that, in the end, the whole program, when you put all the mission ops, launch vehicle, and the science and everything, I think it cost $220 million in real-year dollars.

NIEBUR: Sure. Still a bargain.

KRIMIGIS: Oh, yeah. I thought so. And so, when people said, "Oh, yeah, you can do that," it reminded me of a story that Hannes Alfvén, the Swedish Nobel Laureate, told me once. He had

written a paper where he was saying that there are interstellar magnetic fields, and he couldn't get it published because people said, well, if they exist. [James Clerk] Maxwell had talked about that; that was when they turned his paper down for publication in Nature. So he published it in Tellus, the Swedish physics journal, and did not receive a lot of attention.

He told me that after the war, Enrico Fermi invited him to Chicago to give a colloquium. And so, he went and explained about currents and so on and how he theorized that these led naturally to the existence of interstellar magnetic fields. And he said at the end, Fermi got up and he said, "Of course." And he said, "everybody in the audience said, of course" after Fermi's pronouncement. Oh, yes.

NIEBUR: Really?

KRIMIGIS: That's how the idea that magnetic fields in space do exist was actually accepted, which shows you the kind of, I would say, "Me-too-ism" that there is in the scientific community. I mean, we have this "herd" syndrome, and sometimes somebody will say something who is very important and could be very wrong, but we all say, "Oh, yeah."

So, once the principle of low cost was demonstrated for NEAR, then people said, "Of course." So, I'm not trying to overplay or over-estimate the impact of NEAR, but I think it did demonstrate something, and it changed the paradigm for planetary missions. And people began to think that you can actually do things a lot cheaper.

NIEBUR: Now, of course, after the problems in 1999, with the loss of Mars Climate Orbiter, and Mars Polar Lander—

KRIMIGIS: Yes, then things, as you know, got very uptight again, following the NIAT [NASA Integrated Action Team] report.

NIEBUR: Right. Now, did you have anything in development at that time? Where did CONTOUR [Comet Nucleus Tour] fall into the timeline?

KRIMIGIS: CONTOUR – beginning to be developed at that time. Actually CONTOUR came before the orbit malfunction of NEAR. I don't remember the exact dates.

NIEBUR: I won't hold you to it. I've talked to some CONTOUR people. I think Ed said that they were pretty much done by the time the NIAT report came out.

KRIMIGIS: Maybe. I don't remember the exact dates, but you can look them up.

NIEBUR: Oh, that's fine.

KRIMIGIS: Yeah. But, you know, in a way, CONTOUR was, you might say, it was a victim of being exceptionally cost-conscious; because once we were sure that the solid kick stage had been evaluated and found to be flyable [it was second hand from Hughes and recertified by Alliant Technologies], if we had another couple of million dollars, we might have just bought one and test-fired it. This is what I had done when I was a PI on AMPTE and we had a solid-state kick stage. We bought two of them, and we test fired one and flew the other one. It worked fine.

This time, we decided to go with just the one and not test fire. The Star 30BP [Thiokol solid rocket engine] had been evaluated and found to be good, or so we thought.

NIEBUR: Was that an easy sell, or was that a hard sell around here?

KRIMIGIS: It was discussed, but not a lot. I know that, by that time, the guy who had managed the AMPTE program, John [Jonathan] Dassoulas, who was a wonderful program manager, had just retired, and I know that if we had him around, he would have insisted that we test fire the kick stage. But everybody was very reassuring. And the NASA review teams, the excellent review teams, nobody raised a flag on that. Ultimately, it was my fault because we were running tight. When the CONTOUR was approved to go by [NASA Associate Administrator] Ed Weiler, it was approved without the spectrometer that Goddard had proposed.

NIEBUR: Oh, I didn't realize that. It was a partial selection?

KRIMIGIS: Well, it was not because of any problems with the spectrometer, but because they didn't think it would fit under the cost cap.

NIEBUR: So, did they de-scope it?

KRIMIGIS: So, what happened is that we did the study and went in for the final approval. We sat there, Veverka, Weiler, I, and Carl [B.] Pilcher, I think he was planetary division director at the time. It was a $10 million item, and the science was very important to do. Hasso [B.]

Nieman was the guy who was the Co-I building the spectrometer, and I had a lot of confidence in Hasso in doing the job.

[By now we had to have contingency officially identified, even though I didn't believe in it. It was not like this handshake agreements that Huntress and I had.] It was $10 million, and I said, "Look, Ed, you know that we are not likely to go much into the contingency." I said, "If you increase the program cost, if you put in five million, I'll guarantee you that we'll never use five million of the contingency." He said, "Okay." That's how the mass spectrometer got on board the spacecraft.

I was, of course, very conscious about keeping the cost down so that we didn't kick off the spectrometer. If things had begun to creep up, then the spectrometer would have been de-scoped.

NIEBUR: And you kept that option open for quite a while?

KRIMIGIS: It would limit me.

NIEBUR: Yes, but meaning if you actually did have an overrun at some point, the team was willing to toss it off and fly a brick?

KRIMIGIS: Yes. It would have been—

NIEBUR: As it were.

KRIMIGIS: But I tell you, nobody but nobody believed that we would have to come to that point. We went through as if it was just the thing to do, and we were going to do it. And actually, we came in pretty close to the cost, so it proved to be a good plan.

NIEBUR: Now, I've not yet heard any remarkable stories about CONTOUR's development. It seemed to be pretty smooth.

KRIMIGIS: It was. It was.

NIEBUR: Was there any major technical challenge at the time that—there was the three different—I know that one of the targets was descoped because there were originally three targets.

KRIMIGIS: Yes. I don't remember the exact details. For that, you have to probably ask Joe [Veverka].

NIEBUR: That's fine. I see Joe next month. So, what was your role on CONTOUR? We won't talk about it too much, but you were the head of the Space Department at the time.

KRIMIGIS: Well, on CONTOUR, I was head of the Space Department for several years, and I was kind of a risk-taking person, if you like. But I always viewed myself as the guardian of the science, and that's why I was insisting, for example, that the mass spectrometer on CONTOUR stay on the payload. I mean, I understood the science. I really felt that the whole philosophy of

the department was that we're here to maximize the science. We were here to be a service to the science community. And there is no other reason to do this thing if you don't do that.

So whenever people came in with proposals for new missions, that was the first thing that I would ask them to put up, was the science, how good, how original, how can we maximize it. That was the principal criterion.

NIEBUR: I see. And APL as an institution was supportive of that mindset?

KRIMIGIS: Absolutely, but I said it, and the institution supported it. So, it was—and I think the community felt it, too. I remember the incident where we had the first conceptual design review for NEAR. One of the review board members was Steve [Steven W.] Squyres.

And you know in typical reviews, there is a system engineer or a subsystems person describing their subsystem, and a person from the review board says, "Well, you really have to watch this, and I think you should change the design this way, because this is going to improve it." And so, the engineers here had the view that they were indeed here to advance the science and preserve the science and enhance it when possible. So, Steve made some suggestion, and I don't remember now what it was, in what subsystem, but the presenter said, "Well, that's a good idea. I'm going to go and look at that."

Steve came up to me afterward and he said, "You know, I can't believe this. This is something that has never happened to me before [an engineer accepting a suggestion on the spot]. I've sat on these boards, and people are so darned defensive. What have you told these guys?" So that was the one thing.

The other thing is that we pushed to land NEAR—and it was remarkable in that sense, after we—I don't know if you want to hear about the [NEAR] landing.

NIEBUR: Oh, I'd love to hear about whatever you want to talk about, actually.

KRIMIGIS: Okay.

NIEBUR: I do want to save time to get to MESSENGER.

KRIMIGIS: Okay. So, the landing idea, of course, it was Farquhar's, but the management people and the engineers don't want to hear about things like that. So, Farquhar came to me and said, "Look, at the right time, we need to raise that issue."

I said, "I'm going to do it. I'm going to Ed Weiler and explain this thing." So, after NEAR had done fairly well and we had decreased and increased the altitude of the orbit several times, I thought we had enough credibility, and I went to Ed and I said, "Look, we're going to have a little fuel left at the end of this mission, and if we don't keep maintaining the orbit, it's going to drift off. So, we have another idea. Why don't we try a controlled descent?"

"Oh," he said, "What's that?" So I showed him the charts and all this. He said, "Okay." He was persuaded that it was worth the time, and he took on the task of persuading Dan Goldin.

Now, what happened in between, however, is that Mars Climate Orbiter and—.

NIEBUR: MPL [Mars Polar Lander].

KRIMIGIS: —MPL, both crashed or were lost. And at that time, NASA was really wary of risk taking. So, here comes February 12th, and we had the people in the Kossiakoff Center auditorium, and there were some talks about Eros and the forthcoming controlled descent and all they're saying.

So, I was there giving my pitch, and then I went back to the NEAR control room. And Al [Alphonso V.] Diaz was standing at the door, and he said, "Oh," he said, "Goldin's here. He's mad as hell. You'd better go inside and talk to him."

I said, "Oh. What am I going to do? What did I do?"

So, I go and see Dan, and he says, "Your people are irresponsible."

I said, "Well, what do you mean?"

He says, "I heard one of your guys on the radio this morning, and he was saying that we're going to land the spacecraft."

I said, "What?" I bet he wouldn't say that—everybody has been told that this is going to be a controlled descent. Nobody is to talk about the landing.

He says, "Well, he was sounding very optimistic. I don't know what the exact words were. Ed," and Ed Weiler was there. [Goldin said,] "You go to the auditorium and tell the press that this is going to be a crash landing, because the last thing we need is for the headlines to say that NASA crashed another one."

So, poor Ed trots off to the auditorium and tells the press, "Hey, look, you guys have to be very realistic about this. This is going to be a crash." I had pleaded with Goldin to just wait another hour, because we were an hour from touchdown, so to speak. He would hear none of it, and he was walking up and down and fuming.

We had the laser altimeter on the spacecraft, and we had a pretty good prediction of the descent rate, with the usual light-time delay. But the laser altimeter's data that was coming back was right on the track, the predicted track. Then, of course, the spacecraft set down on Eros and the telemetry signal kept coming right through, "Bing, bing." Well, needless to say, Dan Goldin was absolutely amazed and Ed Weiler was smiling, a lot. Nobody expected that. And so, it was quite an experience.

So, it's one thing to talk about taking risks [the Discovery mission line was expected to take risks], but when the rubber hits the road, not too many people are willing to really stand up and be counted in situations like that. But now we were faced with the problem of acquiring data beyond that time, and for how long. One of the things that we had found out in orbit was that the gamma ray instrument did not really have the resolution to look at some of the gamma ray lines that we thought it was going to be able to do, but now we were sitting on the surface, and the gamma rays were right there on the ground.

It was quite clear that one of the things that we wanted to do is to continue to track the spacecraft, get the data down, operate the gamma ray instrument, and see what kind of a spectrum we were going to get. Bill [William V.] Boynton came to me and said, "Well, you know, we want to do that. What should we do?"

I said, "Well, let's go down and talk to the crew." So, we went to the control room, and we gathered the engineers there and the operators. And I said, "Look, one of the things that we want to do is to really continue to acquire data. Is that possible?" Well, so, in about half an hour, we had it all worked out, and we said, "Okay, we're going to do it. Let's do it."

And as we were leaving, Bill Boynton said, "I can't believe this."

I said, "Well, why is that?"

He says, "Because I've been on JPL projects. A decision like that is going to take at least a couple of committees, and it will be a month before anybody is willing to make a decision. And you guys sat there, and the decision was made in half an hour".

So, these are elements that come into the management aspects of things and how much it costs to do projects with streamlined decision making. I guess I don't have anything else to tell you about NEAR.

NIEBUR: That's fine. You have told me a lot. And there is also other information in the literature about NEAR, but what we don't have a lot on is CONTOUR. And of course, MESSENGER is new. But CONTOUR—and I know it's uncomfortable, but I'm not talking about the MIB [Mishap Investigation Board] part.

KRIMIGIS: Did you talk to Mary Chiu?

NIEBUR: I have been trying to get in touch with Mary Chiu.

KRIMIGIS: She was the program manager. She's the one who really sort of knows all there is to know about CONTOUR.

NIEBUR: Okay. Was she involved from proposal?

KRIMIGIS: She was involved in the proposal, and she was the program manager until the very end when Ed [Edward] Reynolds took over when she decided to retire.

NIEBUR: Would you like to add any additional perspectives from being the head of the Space Department during CONTOUR?

KRIMIGIS: You appreciate that it's been almost 10 years since all of this happened, and I didn't know what range you wanted to go over. I thought it was mostly NEAR. Otherwise, I would have reviewed some of my notes, which I kept to discuss this. But, I mean, I had a very good working relationship with Joe Veverka, who was the PI.

NIEBUR: Well, you guys went back to, what, Voyager?

KRIMIGIS: Well, Voyager, yes. And of course, we worked very closely on NEAR, as well. The guiding principle was to maximize the science. I mean, Joe and I shared that, and whatever decisions we made were in that light. That doesn't mean, of course, that we would be irresponsible in that regard, but it was a philosophy. It was an idea imbued into every engineer in this department, and scientist.

I used to hold these all-department meetings once every few months, and I would say that "science without engineering is not possible, and engineering without science is pointless." So, people were really working to get there. We didn't have this antagonistic relationship between the science and engineering sides of the house that you typically find in most places. I was very familiar with that, working with both Goddard and JPL over my career. That was something that I wanted to make sure didn't happen in this place. And people, when they have a vision and

believe in it, then they are enthusiastic and they really want to contribute. So, that was the philosophy, and it seemed to work.

NIEBUR: And people bought in.

KRIMIGIS: Yes, they did.

NIEBUR: That's really refreshing to hear.

KRIMIGIS: How else can you get people to work days and nights and weekends and seven-day weeks for months on end? One of the questions that I was asked many times, especially at the conclusion of NEAR was, "Well, aren't you going to burn out your staff?" I said no, it doesn't happen, because when people believe in what they're doing, they're happy to do it. Nobody's forced to do anything. Of course, the very long hours usually occur in the final few months of most programs.

If they know they have a deadline and it's a first-of-a-kind mission—and I always used to say, “With the things you do, you're making history”. People relate to that, and they go home and they tell their wives and children, "What I did today was significant, and here's what we're going to do." Everybody had bought on and knew about the reasons we were doing what we were doing. It wasn’t a factory job. So, it wasn't hard to really keep the pace.

NIEBUR: And when a mission gets in trouble under that kind of scenario, do people really pitch in, respond?

KRIMIGIS: Oh, absolutely they do. Back when we had the NEAR mishap and we had nearly lost the spacecraft, people would sleep here.

NIEBUR: Wow. Now, on CONTOUR, you had an external PI. Veverka was not on site. Was that a new thing that you had to negotiate in terms of the working relationship between him and the team? A large number of your Co-Is were on site. Of course, your engineers were on site here at APL, but your PI wasn't. Now, that seems to me, from your history, that that was kind of a new part of things.

KRIMIGIS: Well, yes and no, in the sense that, for instruments, we have had cases where we would team with PIs that were not in-house, and we never had any problems. It's an important point you're making, though, because the PI has to have the right mindset and be compatible with your organization. And because I was a member of a science community all these years, we didn't ask PIs that we knew we couldn't work with.

NIEBUR: What did you look for? What makes a good PI?

KRIMIGIS: Well, you're asking me kind of a Pandora's box-type question, because the PI concept has evolved.

NIEBUR: That's true.

KRIMIGIS: And before, I would have said the ideal PI is a person who knows about hardware and knows about the science. Then, if you're lucky, they also have management abilities and enough experience to make the right kinds of trades. With the missions that are developing these days, and the length of time that it takes to implement one, the cadre with that kind of experience is not that plentiful.

But, a lot of the people that were involved in these missions I knew personally, and I knew their strengths and their weakness, if you like, and was able to say, yeah, that's the person that we should work with on this particular concept. And that's how it went, which is perhaps a good point to talk about MESSENGER, because as you recall—maybe you're too young to recall—but the Mercury mission had been studied in the '80s by JPL, and it had been studied by Goddard as a two-spacecraft mission, and it always came up well above a billion in then-year dollars. I'm talking about 1990 or around that year.

It was out of the question. It never had enough priority, and you had a kind of dual constituency. You had all the people who wanted to do planetary science, and also you had the plasma people who wanted to do magnetospheric science. And certainly the magnetospheric science people didn't have the clout to have a mission of their own, and the planetary people thought it was too expensive to do, anyway, and it never came up.

Then, in the community sessions for the development of a roadmap for a "Mission to the Solar System" in the mid-'90s for initial strategic planning, I think it was in the early March 1996 time frame at Caltech, I was a participant in those. I kept being amazed about how many people were coming up and saying how important Mercury was, how it's the end member of a system, how we don't understand why it has its huge core, why it has a magnetic field; and apparently a

dynamo when other dynamos have died, and a lot about the topology as well as the atmospheric brightenings, the sodium cloud and stuff like that. That sort of thing just kept coming around.

So it hit me; why not try a Discovery mission? I came back to APL, and I got the team together on March 12, including Ralph [L. McNutt], Andy [Cheng], [Tom] Coughlin, and [Bob] Farquhar, among others. I said, "Hey, I think we really ought to consider whether a Mercury orbiter could actually fit within the Discovery budget." We began scoping the thing out. Bob Farquhar was not very much in favor of it.

NIEBUR: Oh, really?

KRIMIGIS: Yeah. He was afraid that we might compromise the proposal for CONTOUR. That was his dream. He wanted to do a CONTOUR-like mission for many, many years.

But we had also other trajectory people and went on to do the studies. I asked Ralph McNutt to spearhead the science and technical scope. The concept began to show up as a real possibility, and I talked to other people in the community, some people that I knew from the Goddard studies, like Dan [Daniel] Baker and Jim [James A.] Slavin who had been involved in the previous effort. Well, I talked to them. I talked to Joe Veverka also at the time, but Joe was very much involved with the CONTOUR proposal. There was a key person by the name of John Appleby—I don't know if you know him.

NIEBUR: I remember John, sure. He used to come visit me now and then, [and talk about] the new concepts.

KRIMIGIS: That's right. I have a lot of respect for John. He was a very effective person here.

NIEBUR: He had an excellent manner. He was very persuasive in a very low-key kind of way. I respected him.

KRIMIGIS: He was an excellent writer, explainer of ideas. So, anyway, then we caucused on whom we thought would be a good PI, and I had known Sean Solomon, but not very well. But we were, you know, hi, how are you? It was that sort of thing. I knew he was a very capable scientist, however.

He was the first choice, and so I called him up. He was very interested, and the logistics were right. That means he would be a PI who could just drive to the lab every day he had to. And, of course, he had his ideas about how to make up the science team, and we negotiated on those, and that's how the whole thing came about.

NIEBUR: But what made you think of him?

KRIMIGIS: Well, you look for people who have done relevant work and who have compatible views, and I knew him from my association, his temperament. I knew also that he didn't have a lot of hardware experience, and he knew it too, of course. But this was such a multi-disciplinary payload that it was not going to be possible to find a person who would know all aspects of this whole thing. So, if you have a good team of co-investigators, good lead investigators for the instruments, then all of that can come into play with a person who knows how to lead and form consensus and make decisions.

NIEBUR: Tom, I have to admit, I'm surprised, though. I'm not surprised today, but I was surprised I guess several months ago when I began hearing these stories of how missions formed, because I guess in my little idealized world, I had thought the PI-led missions were conceived by PIs, and how naïve that was. There's so much that has to be done and put together on an engineering side before you can even begin to talk about a concept. So, I understand why it happens now. But, in this PI-led mission paradigm, what do you really see as the role of the PI?

KRIMIGIS: Well, once you have a mission concept and everybody agrees on it, the PI really is the leader, and he/she implements it and makes all the decisions. I mean, we might have been the instigators, but once the PI is on board, it's their thing.

NIEBUR: Well, I've heard some missions, and I'm not saying they're your missions here, but I've heard some missions that say that the best thing a PI can do is stay out of the way during development. I wonder what your take is on that.

KRIMIGIS: I think that is damn arrogant for people to say. What that may mean is that the PI was totally unsuitable to begin with.

NIEBUR: That could be.

KRIMIGIS: But I have to say, the PIs that I have worked with, it has been a wonderful experience.

NIEBUR: Well, you've worked with some wonderful people. I mean, you've worked with some wonderful people and done some amazing things, so I'm not surprised, but I am glad to hear that it's worked out that way.

KRIMIGIS: Oh, it has. I mean, you can ask the PIs themselves. Ask Sean Solomon—is he in charge, or is he not? Or ask Alan Stern. Can you imagine Alan Stern not being in charge?

NIEBUR: No. No, not even on a bad day.

KRIMIGIS: Of course Alan and I have had some tight moments, but we're still good friends, and he understands and I understood. You do need people who are leaders for PIs. I think the people who say, ”PIs should get out of the way” are arrogant, and also it means that they have their own institutional goals, and they don't give a damn about the science.

NIEBUR: That could be.

KRIMIGIS: We are here to do science. If you don't have a good PI, it doesn't get done. So what's the point? Keeping people employed? I mean, they can go and work for IBM or Microsoft or whoever.

NIEBUR: Okay. Thank you. I understand that better.

KRIMIGIS: So, anyway, we knew we were biting off a lot to try to do MESSENGER for basically a quarter of the cost of what had been forecast 15 years earlier. And of course, as you know, we over-ran by some amount [I think about 15 percent] on the program. But, all things considered, not by a lot. We had some very difficult moments in that program, because, unfortunately, the syndrome of being overly cautious and totally budget minded with every sort of overrun was central to the initial concept of the Discovery Program. That's my judgment on that. We must have had, I don't know, 150 reviews of every kind on MESSENGER.

NIEBUR: A hundred and fifty?

KRIMIGIS: Yes. We had six on NEAR.

NIEBUR: Wow.

KRIMIGIS: I complained to Headquarters at one time that we had a third of the staff acting on the recommendations from the previous review; another third preparing for the next review; and the final third was actually doing work. It was really horrendous.

It came up after the terrific—how shall I say, let me find the right word—the conservatism that came out of the crashes of the Mars missions in '99 that didn't go well. And essentially, faster-better-cheaper became a bad concept, and if you couldn't document everything and had it reviewed by three or four bodies, then you didn't do your job.

Well, with those kinds of obstacles on the way, I think cost is bound to escalate in time, of course. I still consider it a big mistake, the fact that we didn't launch MESSENGER in May of

2004, that we ended up in August, and we added 18 months to the mission. It was first postponed because Orlando [Figueroa] said he didn't feel confident enough. I mean, we felt that we had crossed all the Ts and dotted all the Is, and you can't just say, well, you don't feel confident enough without having specific items you can point to.

NIEBUR: There weren't specific concerns cited?

KRIMIGIS: No, not real ones. I mean, all we did between May and August—[of course, I stepped down in April as Space Department head and was succeeded by Michael D. Griffin]—but in between that time and August, when we actually launched, what we did is redo the mission for the different trajectory. That's all.

So, that was forty million added to the cost, and I think it added more risk to the mission. Thank God the spacecraft's working well and that we still think we're not going to have a problem. But you have taken most of the mission and you increase the risk of failure to achieve your final objective.

So this conservatism that has crept into everything, with all the reviews that we have done, this place [APL] now, in my judgment, is being driven so much by the philosophy of the NASA centers that I pray it doesn't really deteriorate to the point of not being science-friendly anymore and being just like one of the centers.

That might sound arrogant, but the thing is, the reason for the existence of organizations like this is to do things differently—the program needs diversity. I think it's appropriate to have some degree of competition, if you like, from contrasting management styles or contrasting levels of documentation or whatever else there may be, to have a different paradigm and a

different business model. I find it difficult to see why organizations such as this would exist if they're exactly like all the organizations that already exist at the service of the agency.

NIEBUR: And yet, after the Mars failures in '98, '99, the changes started to come very quickly. And that's about when MESSENGER was approved, because it was proposed in '98, and then it was in concept study and selected in '99, and already Ed Weiler was standing up the red team reviews again. And then, the NIAT report came out with all those changes, and MESSENGER was asked to go back pretty quickly, actually, and look to see what changes might need to be made to be super careful.

Sitting over here on the opposite coast from where the Mars failures happened, as it were, did you—I don't want to ask a leading question, but I'm curious as to how you reacted when you were given all of these extra requirements because your competitor failed.

KRIMIGIS: Well, needless to say, we felt sort of punished, even though we were innocent. And you might say, of course, that we were not innocent after we had the failure of CONTOUR.

NIEBUR: But that hadn't happened yet.

KRIMIGIS: No, it had not, but I'm just saying. Some of that also was very disappointing because we did have several of these reviews, and they pointed out certain things that needed to be done. But they were imposed on the system, and at the same time not paid for, and also not relaxing the schedule in any way, because we had a specific deadline to launch and so on. So, these were

mandates. And that's part of the problem with the reviews upon reviews upon reviews, that there is no incentive for the review teams to somehow be mindful of the schedule and the cost.

NIEBUR: Interesting, particularly about the schedule. Yes, absolutely, I can see very much how that would slow you down. I think it was seen at the time, it was just simply a matter of people were grasping.

KRIMIGIS: Yes.

NIEBUR: People were grasping because there needed to be successes. Were the reviews productive? Did a review, for instance, catch when you guys were overrunning on cost or schedule, or was that an internal catch?

KRIMIGIS: I would say that the standard reviews didn't catch anything that we were not aware of.

NIEBUR: Because they're only going off the PowerPoint charts that you give them.

KRIMIGIS: Yes, by and large.

NIEBUR: In those cases, the value really is in the preparation for the review, not the review itself.

KRIMIGIS: Exactly. Yes. So, now, I had some sort of ad hoc review teams that were called in, and they were sort of high-level people like—well, it was Tom [A. Thomas] Young or [Noel W.] Hinners, or—I could look some of these names up. I've been away from all of this now for five years, and I'm very happy about that, in a way. We would make a presentation of our own choosing, so to speak, of what we felt were concerns. I cannot go into specifics, because I don't remember them anymore, but they would say, "Well, we think you're not paying enough attention to this particular area."

NIEBUR: I see.

KRIMIGIS: Those were very good insights that we could act upon and go and dig, and then call them back and say, "Well, what do you think now?" Those kinds of things. But these were mostly internally generated and undertaken initiatives that we felt would help us. I mean, as you know, in any organization, you can be totally internally focused and sort of ignore the dead elephant in the middle of the room, and you've got to always watch out for that. So, I always valued that kind of external advice.

In fact, when I became department head, I also established a department-wide review board that met at least once a year, chaired by Lou Lanzerotti. It did include people like Noel Hinners, Bruce [C.] Murray, Jim [James] Baker [National Oceanic and Atmospheric Administration, NOAA], Mal O'Neill, Gerhard Haerendel [from Max Planck in Germany] and others. Because you really want to get some smart people to walk in off the street and tell you, "Yeah, you're all wet right here," and it's positive to do. That was wonderful advice we got over the years.

NIEBUR: So, as Space Department head, when did you get a whiff that things were not going as planned on MESSENGER?

KRIMIGIS: Oh, I knew it at least a year and a half beforehand.

NIEBUR: Really?

KRIMIGIS: Yes.

NIEBUR: You were just trying to get it back in the box, or hoping the technical—?

KRIMIGIS: Yes, we were trying to get it back in the box. We had some notable component problems, and we had this very anomalous situation that developed because there were some people in the integration area that were former contractors at Goddard, and they were talking to [Orlando] Figueroa offline and telling him, "Well, we don't approve of this procedure, and we don't approve of that procedure."

Then, Figueroa would come to me and say, "Well, I hear such and such." And a lot of that was "my way" versus some other way. A lot of the processes that we had in place came under scrutiny, which had been used for a long time, but they were not to the liking of these three people who were talking to Headquarters, and he and [Christopher J.] Scolese were upset.

So, when you have that kind of situation—I mean, essentially, there was dissention in the ranks that was instigated by these people. I had an open-door policy, I said anybody who knows

anything that they want to talk about, come and tell me. And anything that ever came to my attention, we would investigate, but there was no end to this. It was demoralizing to the staff. Eventually, these people left the lab, but it was just like having spies from external organizations sniffing around and trying to see what was questionable in their judgment, and then telling somebody. We could never work out that kind of situation. That compounded the technical problems and the late deliveries we had as a result.

NIEBUR: I would think so. I would think so. And then, you thought you had a handle on everything. Did you have other technical challenges?

KRIMIGIS: Oh, we had technical challenges up the kazoo in the sense that we had the attitude control system that was a year late in being delivered, and we had to somehow accommodate integration without that. But these were the kinds of things that happened that were not specific to this project. As you know, it was some company that was bought by another company, and they fired the staff, and then the new staff that came in didn't know how to do the job, and we had to send people to sit by them and hold their hand. So, these kinds of things are not things that you have total control over.

NIEBUR: Right. Now, did APL send people?

KRIMIGIS: Yes, we did.

NIEBUR: And they were on site?

KRIMIGIS: Oh, yes.

NIEBUR: And did you find that effective?

KRIMIGIS: It was difficult. It was difficult, but if we hadn't done that, it would have been worse. It was quite clear that it did help. But the mission was a real challenge.

NIEBUR: Oh, absolutely. That was admitted at the very beginning. The day Ed Weiler selected it, he said, "This is a flagship-size mission in a Discovery box, and I'm so excited." When I read that press release years later, I thought, foreshadowing. So, yeah, I'm not surprised.

I'm checking my notes real quick to make sure I've asked you everything that I had intended to ask you. I appreciate this very, very much. I think I've got everything, except that there was one—I think I understand that now. There's one thing I've also been surprised to learn, and again, maybe it was because I hadn't been on a project. But, when I was at Headquarters and I'd hear, "Well, there's a problem, but don't worry, we're going to double shifts, or we're going to triple shifts, they're getting it done," I think there may have been the perception—I can't have been the only person who assumed that there were actually additional personnel involved, but typically, there weren't, right?

KRIMIGIS: There weren't additional personnel in the sense that it wasn't—

NIEBUR: —I mean, how did you—?

KRIMIGIS: —There were not people from the same team. We had people from within the laboratory that we would ask for help.

NIEBUR: Do you bring in people from related projects and how do you do that?

KRIMIGIS: Either related projects or—you know the Space Department is only one division of APL. When it comes to very specialized work, of course, you have to be careful that you have enough people to cover it. But the place is full of engineers, and they do software and they do hardware design, and they do firmware control and they do sensors and so on. So, I would go through the department head of the Technical Services Department or another department and say, "Hey, it looks like we are going to need some design help in this area, and who can you spare?" We would negotiate about getting some people ready to come over, and it was easily administratively to pay them to get that kind of help.

NIEBUR: And since they were internal, you didn't have to worry so much about the time lag in bringing people on or rolling people off.

KRIMIGIS: Oh, no.

NIEBUR: Which is a huge cost in many of our industrial partners.

KRIMIGIS: True, but here there wasn't much delay because the person was still in his/her office in another building but would be working there, and they're still getting the same paycheck, and it's just that they write down charges to another budget for the day. And so, it wasn't that difficult.

The difficult part was to find the right people and negotiate their availability. I relied on the technical people in the department to say who was the right person to bring in to do the job that we needed to do. And sometimes, if there were conflicting requirements in all this, I would go get help from the director and say, "Can we get priority for this?"

NIEBUR: Sure. But APL has that depth.

KRIMIGIS: Yes. That's, I think, one of the advantages of being part of a larger organization. Otherwise, if you have a need and you get to advertise for a position and bring somebody in and bring them up to speed and all this, by that time you're in deep yogurt, so it's not really possible. So, I think part of a sort of surge capability, to use an oft-used word, is the fact that it was possible to go around and get people to do things. And so, the personnel in the department may have been stable, but in terms of how many people we were paying, it would go up and down.

NIEBUR: Right. Right. I don't think I have anything else, so thank you very much for your time, Tom.

KRIMIGIS: Okay.

NIEBUR: I appreciate this more than you know.

KRIMIGIS: Well, you're very welcome. I'm sure you'll have a successful project, and I will be looking forward to reading your book.

NIEBUR: Thank you.

[End of Interview]