An Oral History of

JAMES ARNOLD, ALBERT METZGER, and LAURENCE PETERSON

On October 26, 2000

ARNOLD: This is Jim Arnold, and I have with me Al Metzger and Larry Peterson who shared with me and some others—Jack Trombka, in particular—the experience of the gamma ray spectrometer experiments on Apollo 15 and 16. This was a time of great stress and great happiness because the experiments were successful. Our plan now is to start with the prehistory, the time before this opportunity opened up, and Al Metzger is going to lead that part of the discussion. Or Al and Larry will lead that part of the discussion because they were the major players at that point.

METZGER: After the Ranger efforts—which were covered in a previous interview—there was a period when it was thought and we hoped there would be a Ranger follow-on program, which would include an opportunity for the gamma ray experiment. That didn't materialize after a period of 18 months or so—let's reconcile to that. And the next firm opportunity that appeared—and this came quite suddenly—was the unmanned Lunar Orbiter program, which was initially designed for scientific purposes and then, with the onset of the Apollo program, became diverted as an effort to locate landing sites for Apollo. And, at first, there was no thought of there being anything but cameras on board, but they had an unexpected degree of success: the first, the second, the third were all successful. Five were planned, and I guess it was by about the time that the third went successfully, that there was some thought about doing something other than taking pictures for Apollo. They had had all that they needed. And so, they probably approached other people but they did approach us and asked what we could do by way of providing a gamma ray spectrometer. Well, we wanted to do it, we did our best to satisfy them. But what it came down to finally was a decision at headquarters. And it was a one-person decision—

ARNOLD: Oran Nicks.

METZGER: —Oran Nicks, thank you.

ARNOLD: Associate administrator at that time.

METZGER: And what it entailed was a delay in the launch of the last orbiter by as I recall, perhaps six months—maybe not more than four, at least four and no more than six months—at an additional cost of several million dollars—two or three million dollars. And the decision was made not to do that and so instead of having a polar orbiting gamma ray spectrometer experiment in 1960 something, 69 maybe or even 68.

ARNOLD: It would be earlier.

PETERSON: Oh, earlier than that.

METZGER: Oh, alright. It didn't happen until 1998 or 99.

ARNOLD: Yeah, there about.

METZGER: A thirty-year exchange for a $3 billion saving.

ARNOLD: Yeah, what I remember from that experience is that when this news came that this item had been crossed out of the NASA budget, I had two engineers from the Boeing Corporation in my lab. We were working together to see how we could build the instrument fast enough and do everything else fast enough. And somebody from the Boeing people in Washington called to give us the word that this was not going to happen. And I said to the two engineers, “Well, at least today we should try to write up and wrap up and you know, really, at least close this thing out. So, if we get another opportunity, we have everything recorded.” And they agreed. And within an hour there was a phone call from Seattle saying, “Get on an airplane—the next airplane—and come home.” And so, they got on the next airplane and went home and we never did write up the experiments. That was the atmosphere at that time.

METZGER: The one plus aspect of this was that it did present front and center the loss of the opportunity and the result of not being prepared and it did provide us with a good talking point and some sympathetic ears to give us the resources so that we could begin to do a flight and that was certainly important when Apollo came because that also turned out to be a last minute call: “Hurry up and how soon can you do it?”

ARNOLD: Yes.

METZGER: What we had done in the interim was—well, in part there was this activity that we were talking about just before we started in developing, quote the NASA pulse height analyzer

unquote. There were a couple of people at JPL who had done a system of this type—maybe for other instruments—but I know with the Alpha Scattering experiment in mind, that was not—it was not their instrument that went on Surveyor for the Turkevich experiment. That was done by the University of Chicago. But still they had a chance at a time before that decision was made to begin thinking about and developing a pulse height analyzer which they continue to do. So, NASA went to all the recognized experts in the field which was a purely commercial field at that time—there were three or four in number—and then, additionally, was good enough to include at least two young upstarts from JPL, Marshall and [Timothy] Harrington to participate. And that competition—playoff—was held I think in Oak Ridge.

ARNOLD: Well, perhaps yes—

METZGER: And the upshot was that the young guys who from the beginning had thought of this in terms of a space instrument, which the other people had not—I mean they tried to, but that was not their culture initially, as it was Harrington and Marshalls'—they were put to shade for use as a space instrument and after that there was no question that we could go to them for the electronics for our analyzer.

ARNOLD: I remember the other people involved talking a great game right up until this moment. In fact, they may have been the ones who proposed the direct comparison of the two instruments. And then it turned out what they brought was not an instrument but just a piece of an instrument. They just didn't—hadn't gone beyond that. So, it was gratifying.

METZGER: Yeah. But there were at least two and I think, three entries, in addition—to whatever degree of completion—in addition to what Harrington and Marshall had to offer.

ARNOLD: OK.

METZGER: But we then—perhaps just after, perhaps it started about that time—undertook a study contract with them where they began designing the electronics. And we then at JPL, but with the impetus of some work that had been done down at UCSD, I'm just remembering—and was that out of your group, Larry?

PETERSON: Yeah

METZGER: —had a design for a FOSS switch system.

PETERSON: Yes, that's correct. My involvement in this came not so much from the lunar and the geochemistry side, but from the gamma ray and X-ray astronomy side. And we'd been doing a number of, primarily, balloon experiments to study various configurations, detectors, and backgrounds and so forth. And one of the types of detectors we were studying was essentially just three by three-inch sodium iodide cone [?] with an anti-coincidence shield around it, and in various configurations. So, Jim Arnold, who recognized the importance of this—we sort of formed some kind of a first an informal, then a more formal collaboration.

ARNOLD: Right.

PETERSON: And, and we started working with JPL on these detectors. And I remember we built a number of different sorts of engineering type of models which you flew on balloons, and then we also got interested in the background properties—that is, the production of gamma rays in various materials. And I remember under Jim's suggestion that we flew these crystals on balloons to high altitudes like thirty or forty kilometers with the detector buried in an iron block which was about a foot on a side and another one which we buried in some material called gunite, I guess, which is very similar to olivine and did a number of experiments like this. I think the culmination of it all was that there was a fellow from Los Alamos named Bob Shook, who came to work with Jim about this time and he sort of put together a detector which had the right kind of a configuration of properties to be eventually put on a space mission. And it was supposed to be an improved version over what had been done on the Ranger.

METZGER: Yes, and Bob Shook was the one who had built the detectors for the Ranger system.

PETERSON: That's right.

METZGER: And I misspoke a minute ago when I referred to the new design—the one that came from you, Larry—as FOSS switch. The Ranger design was a FOSS switch. This one had separate central detector and anti-coincidence detector.

ARNOLD: Right, yes.

PETERSON: But I had flown a similar type of a sodium iodide plastic FOSS switch on the OSO 1—Orbiting Solar Observatory—back in about 1962 or something like that so there was a little background here, too.

METZGER: So, we took that dual detector design—two detectors, two photomultiplier tubes—and began adapting it for use in the lunar instrument and I can remember one meeting—being able to come with that detector. And just, it was unmistakable that having something to put on the table was convincing.

ARNOLD: Yes. When the—Marshall and Harrington at some point left JPL to join kind of your – Josias, wasn't it? – at this company.

METZGER: Josias and one other person.

ARNOLD: I'd forgotten the—

PETERSON: Lawrence—

METZGER: Yeah, Jim Lawrence.

PETERSON: Jim Lawrence, yeah, they formed the Analog Technology Corporation.

ARNOLD: Analog Technology, yes.

METZGER: Or ATC as we referred to it.

ARNOLD: Yes, right. So, this was going on through this period. Is that enough, or should I – if not I can—

METZGER: Well, just this one point to say that in terms of our interest in—if not actually work with the Apollo program—that really began as an outshoot of this three-way association in the form of a proposal to put an instrument on an earth orbiting manned laboratory. These preceded the Apollo lunar missions. So, we submitted that proposal. It wasn't accepted, but at least it provided some continuity for us and some practice in proposal writing. [*Laugh*]

ARNOLD: [*Laugh*] Which was a young art at that time.

PETERSON: Not only was the proposal not accepted, but the program was eventually canceled.

METZGER: Ah, that's why. [*Laugh*]

ARNOLD: Ok, well, maybe it's time to move on then to Apollo. I was very much involved in—oh, but before we do, it was before the Apollo began that Jack Trombka, thanks to you, Al, I believe, was brought into the partnership, as well. He certainly played an important part from then on. I had been deeply involved in Apollo since 1964 since the early planning days because of my interest in return samples, which became an active part—we've talked about, I've talked about in another session. But when Apollo 11 came along and I, with others, descended on Houston for the splashdown period, the party which followed the successful returned of the astronauts and the samples, and a period of about six weeks, when we were busy with allocating the samples and doing many, many other things. There came a point at which the turmoil—the smoke and sometimes flames which accompanied the effort to get the science really organized to make use of the Apollo opportunity—there came a moment at which a change in the management structure took place. And a manager named Tony Calio took over full responsibility for the science part of Apollo at the Johnson Space Center at Houston and began to think about, or was encouraged to think about, the possibility of putting—in addition to returning samples for the scientists—of putting scientific experiments on the orbiter. There had always been the plan of putting scientific experiments on the lander and that began with a package on Apollo 11 and continued through the whole Apollo sequence. But the idea of putting a – instruments—not an instrument but instruments on the command module to take data from orbit surfaced and began to be very seriously discussed for the second block for Apollo 15 or 16 and 17. The other missions were coming on pretty short centers so there was no chance of say, getting an instrument ready for Apollo 12. Or after that for the ill-fated Apollo 13 or so on. There needed to be a little bit of room.

And one day on a later trip after the first hot, intensive six weeks, Tony Calio asked me to come to his office and told me that if I was willing I would get on a plane with him and fly to Washington where there was—Johnson was seeking active approval for a package of experiments to be flown on the command module. I think cameras had always been thought of as part of that, but there were three other instruments picked. And I don't think there was there was a formal proposal process, a review process at all. It was pretty much a list of experiments where the question was simply "Can you be ready?" And that sounded wonderful. So, I got on the airplane with Tony Calio and flew to Washington. We talked to the administrator, I believe—we talked to Tom Paine and we discussed – there were questions asked about our readiness. The time was short. Apollo 15 had to be – the instrument had to be delivered. It was a much longer interval of course, but the instrument had to be delivered within eleven months of the

decision to start. We were encouraged. The main thing we did was to put on the table again, this instrument that we had built—it a breadboard, it was not a flight instrument but it was a breadboard, a full breadboard—and say “Yes, here's the instrument, the electronic design is complete, we know just what we want to do. We can have the analog company take the responsibility for the actual construction. If we can have the Jet Propulsion Laboratory as the agent for us and taking care of the administrative details, handing out the money and all that.” Then when asked I'm sure I consulted you gentlemen, but I at any rate took a deep breath and said “Yes, we can do it.”

And we were told to go ahead. And this immediately started, of course, a very intensive process. It was not only—and I'm not going to leave this out—but was not only getting the instrument built, but we had to do a thorough survey of the radioactive sources onboard the spacecraft already. The—what shall I say—the frightening example was that a great many of the parts of the various command modules were made out of magnesium thorium alloy, which was not cheerful at all. And there was a very limited ability to substitute parts that left out the thorium but NASA was willing to try. There had to be the whole operation of procuring and testing a forty-foot boom. We insisted that the boom was required to separate the instrument from this very massive spacecraft which otherwise would probably have given us almost an equal signal to the one that we obtained from the moon. You have a correction in mind?

METZGER: I was trying to remember, what was the length of that boom?

ARNOLD: I think forty feet.

METZGER: I think it was twenty-five.

ARNOLD: All right, this will be carried as an ambiguous thing. The articles that described it I'm sure were complete with that detail.

METZGER: Yes, there is a document written about the—

ARNOLD: And there are plenty—I think you have plenty of slides, Brad, of the process and so this is an answerable question.

PETERSON: Excuse me, I think eight meters sticks in my mind for some reason.

ARNOLD: Well eight meters is twenty-five feet with an experimental error. So, I'm outnumbered and probably you guys are correct. Okay. Anyway, it was certainly a long boom and a retractable boom, something which years later they were unable to make. But at any rate, what the atmosphere—

METZGER: I'd like to put a footnote on the retractability. By providing us the retractability feature, in return, they said, "Yes, but we're going to have the ability to slice it off and cast it away if it presents a problem." Well, fortunately, that never occurred with us on our two flights. Fortunately, because we wanted the return data between the Moon and the Earth that was important for background, but there was another instrument on board, the mass spectrometer, which also used a boom and a boom which was retractable with the same removable feature. And that one was cut off and cast adrift on one of the two missions.

ARNOLD: Well, let me let me go back a bit to the to the run up. I remember one experience where—oh, I hope you remember the name, Al, maybe both of you, remember the name of the administrator at JPL who was managing and defending us because I am blocking out at the moment.

METZGER: You probably mean Bill Collier.

ARNOLD: Yes, Bill Collier. There was a meeting between the officials at JSC and Bill Collier, ironing out the details of the contract which governed this process, and we sat there. There was a manager at Houston, who I got along well enough with personally, but who seemed at this particular meeting to be defending Johnson turf with some fierceness about, you know, everything is going to be managed, is going to be controlled here. By the end of the day I had a large worry as to how this difficulty of two separate organizations pulling in different directions was going to affect us. At the end of the day Bill Collier said, "Well, by the authority that I have in my position, I have signed the contract today with the Analog Technology Corp. and I've done this and I've done this," and it was clear that that was going to be the decision because if the manager at Houston tried to reverse that decision the resulting dogfight would have made it—we would have been unable to meet the schedule. Bill Collier on that occasion showed himself our friend and I think he deserves an acknowledgement here.

METZGER: Yes. Bill was a very capable administrator, I have the highest respect for the way he helped us. Because he, like hardly any other person I've ever met, knew how to manage lightly.

ARNOLD: That's true.

METZGER: He knew the occasions on which he was even there—so to speak, you know—imposed himself in all the things that I was involved in because I was the task manager at JPL. There were very few. And I just took that as an expression of confidence and on those few occasions it was always positive and always effective.

PETERSON: The thing that impressed me—and I was not in too many of these meetings but I was in on some of them—was a sort of difference in culture between the Houston management, the manned space flight management, and an unmanned program, the JPL management, and there were just numerous little conflicting wrinkles all the time.

ARNOLD: All the time.

METZGER: We were actually breaking fresh ground in this respect. I was trying to remember—I was thinking about this yesterday—what JPL's involvement was in any other aspect of Apollo because my recollection is there was some JPL participation in one of the surface instrument packages. I can't recall. But on the other hand, if there was, it couldn't have been much. We were really the first exposure JPL had—that manned program—and as both of you have indicated, it really was a clash in the beginning of the two cultures, and I don't remember that exchange that you related between yourself and Bill after that day's meeting, but I certainly remember that meeting. That first meeting, we went down, there was a large delegation led by Bill Collier to negotiate—

ARNOLD: Yeah. Well, that was that was the meeting I'm talking about. And Bill said that in the meeting at the end when we were getting ready to adjourn and essentially rendered moot all the all the claims that were that were being made.

METZGER: That was one long, wearing meeting, let me tell you.

ARNOLD: It certainly was. That was why I was so concerned.

METZGER: And yet, as time went on both sides learned that they could trust one another. And it became a very easy, very amicable relationship.

ARNOLD: It was amazing. I was certainly getting ready to say that whereas there had been—well, there had been confusion during Apollo 11 for reasons I think I've spoken of when I

was talking about the lunar sample experience. But once the people at Houston signed up to this thing, they were willing to go to some extraordinary lengths at times. Certainly, the matter of this retractable boom was no mean, not a small effect on their program. But there were other things, as well. As we surveyed the materials—and a great deal of that was done by Jack Trombka but others participated—the fact was turned up that there was a big lens, a pan camera lens. My gestures with my hands don't convey on the tape very well but it was something like a twenty some odd inch lens weighing quite a lot of kilograms. And there was—I don't remember—but an amazingly large percent of thorium in that lens and for some reason one of the two lenses—Apollo 15 or Apollo 16, I think it was 15—had remarkably little gamma radiation. The gamma rays all come from the daughters. And it must have been a different batch in some way or another, but one of them was just full of thorium lines. And they had to make a heavy metal cap for the lens so that whenever the lens was not in use for photography (which was a very important part of the mission from their point of view) it was not only closed up, but this heavy—whatever it was. It wasn't lead, but it was some other heavy metal gamma ray shield was covering the lens. And that was, again a major thing to design and build and all that sort of thing. And they just went ahead and did it.

METZGER: My recollection is that they made it of tungsten.

ARNOLD: Yes, that I think tungsten is correct. So, the model shifted—and we're quite agreeing on this—from a situation in which we were concerned about the Houston management's limited contact with the instrumental science and in which we had all the cooperation in the world and things were going very well. What happened then that I feel worth recalling—maybe one of you would like to go back a step—was that Al was carrying the major part of the burden on working with the Analog Technology people on the instrument. You were running out there essentially daily, I think. And I was concentrating more on the science side, estimates. Bob Reedy was around as a postdoc and he became more and more a full member of the team as time progressed. And we were calculating things and so on.

METZGER: To the point where you formally requested his addition as a member of the team.

ARNOLD: Indeed, and he's been involved in such matters ever since. The question of how the instrument would operate, the interpretation of the data, and so on was a collective thing. Larry Peterson was our expert on what we could do, which was finite, about gamma ray sources in the sky. That and the return flight—some very significant measurements were made that we'll

get to. And Al was certainly fully involved in the calculations. This was the side of the house that I—and plus the politics, the inevitable questions that arose that required dealing with high level people, was pretty much my province. And we proceeded toward flight and the instrument was indeed put together and it was indeed tested and passed the tests. I don't remember whether there were any crises in that. You would be more able to say.

METZGER: Well, suppose I say a few words about the instrument development as it transpired. The electronics were provided by Analog Technology Cooperation. And I did run down there very frequently, and several of the other people who work with me at JPL even more frequently, but fortunately it wasn't too difficult to do so because they were located in Pasadena, about ten miles away from JPL. I was functioning in this as the task manager. At the beginning of the program, JPL had said to me, "Well, if you want JPL to agree to do this program, you will have to be the task manager." And whether that was a bluff or not, I don't know. But, you know, doing it had some interest for me to see how this all would take shape and was not all that dissimilar to how I functioned with Ranger, as well. So, we went from there. The detector was done in house at JPL.

ARNOLD: Right.

METZGER: Then delivered—after it been fully qualified at JPL—to ATC, who were designing, fabricating, and assembling the electronics. Then they incorporated the detector in with the rest of the instrument. Did the acceptance tests at that level then delivered the entire instrument to JPL where it underwent its environmental tests.

ARNOLD: Okay.

METZGER: After that to MSC. Now, we did have to submit a proposal—JPL had submitted a proposal. I can't recall whether we submitted a proposal as a science team or not. You were saying that was perhaps informal—

ARNOLD: We were a science team, but I think the proposals were kind of after the event.

PETERSON: Yeah, I think that's right.

METZGER: And the proposal is dated the 25th of April 1969. And it undertakes to deliver the first of three flight instruments on January the first 1970.

PETERSON: [*Laughs*]

METZGER: However, it does say a complete engineering prototype now exists. I wouldn't have thought so as far as the electronics were concerned. The detector yes, I remember we had put a detector through qualification level testing, but I didn't recall that the electronics were quite that far advanced. There was a design and ATC had fabricated something in the pre-Apollo period. But for sure, taken all in all, it's an extraordinarily short development time. And it had to depend on the fact that we were in a relatively advanced state of readiness—

ARNOLD: Indeed.

METZGER: —when got the green light because we did meet the delivery dates. And as far as instrument crises were concerned, really not. Don Shrader who was the contracting officer for the experiment said afterwards that this was probably the smoothest running flight program of any he knew of at JPL up to that time.

ARNOLD: Well, that's quite a compliment.

PETERSON: That’s very good.

METZGER: The most serious decision I can recall—but that's probably because it became my decision—was in selecting the detector for Apollo 16. On Apollo 15, we had a detector that performed just at the level of specification called for. I have in the file a letter from you that apparently was required by JPL providing a waiver not for the Apollo 15 instrument but for the prototype where the resolution was about 10% poorer than the flight specs called for and in it you expressed—you grabbed the waiver and you expressed the hope that when we get to the flight instrument that will meet specification. Apollo 15 did, not by much, just sort of barely. For Apollo 16 there were two detectors available from Harshaw [?]: one that had been very well tested, had a very solid pedigree test wise, and a resolution of a little over 8%; the other had a resolution of 7.4, 7.5%, which was very good by those standards, and it's by far and away the best of those that we had gotten from Harshaw [?] for the program. But test wise its history wasn't as reassuring. I made the decision one evening when I found myself in Las Vegas. I walked up and down that boulevard for an hour mulling it over and then I made my decision.

ARNOLD: Which was?

METZGER: Guess.

ARNOLD: [*Laughs*] 7.4

PETERSON: [*Laughs*]

METZGER: That's what we got on Apollo 16. [*Laughs*]

ARNOLD: Right. At any rate, then my thoughts begin to turn to the preparations for a flight. And in particular the drill that we went through together on the flight simulation drills. Every scientist—of course one of the secrets of Houston being able to accomplish the extraordinary success of the Apollo program was that everybody, all the mission controllers, everybody who had a say in—a voice—in actually managing the flight itself, had to go through repeated—one might almost say continua l—tests by a team of engineers who had nothing to do but to imagine failure modes of one kind or another and to cook up software which portrayed them and tested the ability of the team to respond to such challenges, to correctly identify what the problem was, to correctly identify what the correction was, and so on. And we had a milder version of the same thing. All of the scientists who had experiments had a milder version of the same thing where we had to appear and go through such a scenario and do the right thing. Not for the whole mission, but just for the much more limited part of it that we were responsible for.

And there were two aspects to that. One was the aspect of sitting in mission control with these things going on. And I remember my own part in that. I did what I think any typical professor would do in preparing for something like this, who was also teaching classes and doing lunar sample stuff, and this and that and the other thing. That is to say, I read the manual on the plane going to Houston to take these tests. And the result was that I blew these calls in a little bit of an embarrassing way. And one of the things that was very clear about the simulations was that if you made a mistake, the best thing you could do was immediately call attention to the fact and explain just what you had done wrong. And I knew some scientists who never made it into Mission Control because they absolutely could not bring themselves to do that. And while even for me—and I thought, you know, that wasn't going to be a problem—but it turned out it was a sort of business of taking a deep breath, you know, and really doing it. Yes. Do I want to say right out loud that I did this damn fool thing? Well, if you didn't, then the test manager pointed his finger at you and explained in excruciating detail what you had done wrong. And that was quite a sufficient penalty. So, I think I climbed that curve quite intensively and rapidly.

And we underwent two such tests before Apollo 15. And the incident I remember most is that after the second round, or as the second round was winding down, Tony Calio came into the

room and he looked at me and he said, “Jim, your lips are white”. And I said, “You're right. And the reason they're white is we've had two tests and you've jettisoned our boom both times.” And it wasn't our fault as I remember it, it may be a biased view. In either case, it was that the task people had written something that would cause a real flexing of the main crew’s thing and they had not picked up the particular fault. So, the boom had to be jettisoned, I may say that once we got into real flight situations on Apollo 15 and 16, oh, it was a picnic! But by comparison, though, we had a few worries of our own. Al you might comment on—

METZGER: Well, I just remember so clearly those drills. I'd never encountered anything like that before. I was just extremely impressed by the seriousness, the severity, and just the absolute professionalism of the people on that team.

ARNOLD: Oh my, yes. Yeah, I think it was a real wakeup call as to, as to what they had had to go through, beginning with all the whole Apollo series from the beginning. Just all the respect in the world for people who had gone through that process and we're now thoroughly at home with it. So, okay, we got in, we passed the test, it seems, well enough. And now, the time comes for Apollo 15 to actually launch, unless there's anything else one of you—

PETERSON: Well I thought—oh go ahead.

METZGER: Well, just one quick mention of the fact that we haven't said anything about North American Aviation. But in fact, JPL did deal directly in a number of respects, with North American in the development and particularly in the integration of the instrument into the spacecraft but of course, always under the umbrella of the general MSC supervision.

PETERSON: The thing I remember was that we had to work out a timeline for all our operations.

ARNOLD: Right.

PETERSON: And that was—and we'd worked that out ahead of time with a lot of negotiations. And when we got down there, there had been some changes, which I don't remember specifically, they had made for operational reasons. And we had to deal with a—they had a specific person who was responsible for the timeline down to great detail. I can't remember his name now but he was a real character anyhow.

ARNOLD: He was the first longhaired engineer I'd ever seen in my life.

PETERSON: But he was a tough guy, anyhow. And a tough guy with a smile I think is the way to describe him. But we did have to make some changes in there. It was a—they agreed to them all right, but they had to be well explained before there's going be any changes made.

ARNOLD: Yep. And, okay, and now we're gearing up and we are actually inside Mission Control. Well, the night before the launch, we had been down in Orlando, Florida. You were there, too, weren't you, Larry?

PETERSON: No, I was not there.

ARNOLD: You were?

METZGER: No, not for 15.

ARNOLD: Okay.

METZGER: I was there for 16.

ARNOLD: Well, well. So, I was the only representative of the team. Jack Trombka was there. And of course, [Iryna] Adler who had—for the X ray experiment. Trombka was on that, too. And so, we listened to the audio—the television programs—describing Apollo 13 with all the trials and tribulations.

METZGER: Oh, Jim! They used it for a training exercise. I had forgotten that.

ARNOLD: We helped—I helped train the Apollo 13 astronauts. I remember that vividly. I don't remember—

METZGER: Yes, some part of it.

ARNOLD: —being at Houston. Maybe you were there and I wasn't because I don't remember being at Houston during 13.

METZGER: Well, that’s possible, yeah, yeah.

PETERSON: But I remember after one of these training sessions, Gene Kranz himself came into the room and made some comments, particularly to you about how the team had operated. And I had the impression he was sort of pleased with what we'd done at that time.

ARNOLD: Yeah.

PETERSON: But it was the first time we'd met Gene. Or at least that I had met Gene Kranz.

ARNOLD: That's right. Now Gene Kranz wasn't there during the test. That was managed by others. I think that is correct.

PETERSON: Oh.

ARNOLD: Anyway, we were leading up to the 15 launch—I didn't go to the 16 launch—which was quite an experience. What shall I say—cars on the base as far as the eye could see. And it was a great deal of tension—the day before we made a tour [?]. Family was there and there it went. It was most impressive. And then we went back and started settling down to the tasks at hand and got to Houston and got into Mission Control, where we had a room on the second floor in the back. That was part of the Mission Control apparatus. But under carefully controlled conditions we were allowed to speak to our immediate superior, who was a flight controller in the main team room.

METZGER: The “we” in this case, excuse me Jim, is for all of the experiments of the mission.

PETERSON: Yeah.

ARNOLD: All the experiments were in that room together. And we interacted with people in various ways. Harold [?]—I've forgotten his last name was the mission controller that had to manage us. Never mind. And we were—it should be explained here, that during the actual landing when our gamma ray instrument was flying over the moon, Al Metzger and I worked each 12-hour shifts. I was the old guy, so I got the attractive 12-hour shift from noon to midnight. And Al was the young guy, so he got the unattractive shift from midnight to noon. And we had a single hotel room because there was no point in having two, which became a tradition with us later. And so that was a part. You had your meals in Mission Control which were not very good, but they were right there. You took meal breaks and any other kind of break when the spacecraft was behind the moon and there was no communication. Then there were unlikely to be any crises that demanded your attention.

And so, we were there as the game began. And the first thing that happened, which certainly attracted our attention was that there started being intermittent interruptions in the power supply

to the bus that was supplying the power to our instrument. And that was a little scary. That was on my shift I think not yours, Al, you're looking puzzled.

METZGER: Yeah, I don't remember that.

ARNOLD: Daytime shift. And about an hour or two later, Eugene Kranz came into the instrument room, and he had a switch in his hand, an armored switch. And he explained to us that they thought that this fault, this intermittent interruption of power, was due to an armored switch having had—the wire inside the insulation was twisted copper wire—and he thought what had happened was that one copper wire had somehow escaped the packaging and when there was vibration it was touching and shorting the end, causing the breaker to break—to kick out. And he said if this is true, then that's good news because we can work around it. We have a backup way of connecting it and so there's not—we can we can rest easy. But I wonder if any of you gentlemen have any other thoughts about what could produce this symptom I've described? Well, I thought of that as pure morale booster because we were so ignorant about the hardware on the mission, but it was rather typical of the way that he behaved. He was trying to reassure us by keeping us busy and also by just appearing and answering and addressing our concerns. But what I will go on to say is that when the command module got back and six weeks later we got an anomaly report, the X rays showed a wire outside. And wow was I impressed! They were more than 100,000 miles from the earth and they figured out what was happening.

Well, relatively, as I remember—and again, subject to correction—on Apollo 15 the arrival at the moon and the burn to put it into the proper orbit went uneventfully when they came out from behind the moon. It was at the expected time, and everything seemed to be basically okay. So that was an enormous relief. And in due course, the boom was extended and our data gathering began. Normally, on my shift, the crew inside consisted of myself and Bob Reedy, who sat together at the console and did various things including plotting data by hand. And that went along. The computers at Houston on Apollo 15—the mainframes—were totally unavailable for our use; they were entirely devoted to servicing the mission. And so, we simply were told that we'd get our data in due course and plus whatever we could squeeze out of—so we were writing down numbers off the monitor and things like that. Also, Larry Finan [?], who was the engineer—

PETERSON: Oh yes.

ARNOLD: —who worked with me at that time on this and was sitting in another building and we could communicate with him whenever we wanted. And meanwhile we were accumulating data and we were generally reassured by what we were seeing. The indications were that the instrument was functioning the way it should. We were getting things like total counts, and we discovered that in some regions of the moon, there were much more, many more events than there were in other regions and it quickly became apparent that the highlands were counting more than the—I'm sorry, the other way around. The maria were counting more than the highlands which turned out to be because potassium and thorium are quite a lot higher in the maria. So, all that was reassuring, but it was also of course frustrating that you couldn't get any significant distance beyond that information. You just had raw data, and the data reduction process we knew was going to be laborious. So meanwhile, we were turning over the terminals and I would brief Al at midnight and he would brief me at noon.

METZGER: Except let's not give the idea that it was quite that automatic a change over. We didn't just greet and say farewell to one another at 12 midnight there would have to be some overlapping periods—

ARNOLD: Oh, yes.

METZGER: —so that we were talking about stretches that were about 13 hours a session.

ARNOLD: Oh, yeah, I think that's a wise remark. We were also talking at length to our friends on the X ray mission. And of course, Jack was one of ours too, and also Paul Gorenstein on the alpha experiment.

METZGER: And we had a work area in a trailer—

ARNOLD: Yes.

METZGER: —some distance away. And I recall we requisitioned a bicycle through Ernie Schonfeld.

ARNOLD: Yes. Yes, yes. I'd forgotten.

METZGER: We used the bicycle to carry information back and forth—

ARNOLD: Yes, I'd forgotten that.

METZGER: —in those days. And there were people besides Larry and Bob there. At least on 16, Dave Gilman put in—

ARNOLD: Yes.

METZGER: —an appearance, and Dick Parker from JPL.

PETERSON: I think that I did not come down until the middle of the orbital phase on Apollo 15.

ARNOLD: Could be.

METZGER: Yeah, because you were there for the transfers phase.

PETERSON: —because for the retraction—for the transfers phase.

ARNOLD: Right.

PETERSON: But I think I remember something about the Apollo 15 in that the detector began to show some kind of a gain change or something like that.

METZGER: I can't remember our being aware of that during the mission. We may have been and I've forgotten about it but you were—

ARNOLD: Oh, we were, we were. Yeah, the gain, the drift overall, from beginning to end of the mission, I don't remember a breakdown, was something like 40% degradation. So, we had to step up and it was much, much less on Apollo 16.

METZGER: Yeah, it was more like 20 to 25%. But right, we had to be aware of it. And we did increase—we had to command for the high voltage supply and we did step it up.

ARNOLD: Yes, that's right. We did step up one, yeah.

PETERSON: The thing I remember is that in the experience that we'd had, there had been lots of many different examples of gain changing. I remember a call from you, Al, at midnight at my home, trying to have a discussion about what to do about the gain change.

ARNOLD: Can I ask for a break? I need to make a pit stop.

METZGER: Are we still in the middle of Apollo 15?

ARNOLD: Yes, I wanted to perhaps pick up at this point. We mentioned the things going well, that didn't mean that there were no issues to be dealt with. The timeline, which Al referred to earlier, was something about the size of a telephone book. And it was extremely detailed. And there were not at all rare occasions when something caused a change of plan. And if it was at all a serious change of plan, it seemed that this young man whose name we should remember but don't, who had written the timeline would be called in. And there would be a discussion with people concerned and now and then that included ourselves. And the remarkable thing besides the fact that he seemed to have the timeline entirely in his head was the efficiency with which the plans could be modified—the back to the nominal was the slogan. The whole idea was that you would you would do something to deal with the problem at hand, with the goal of being back on the page as quickly as possible. And so that was very interesting to watch.

There was a particular example which took my fancy and I think yours, Al. We had been told that our boom must not be extended at a time when the pan cameras were photographing the lunar surface. Because if that happened the boom and our instrument would extend into the field of view with the camera and perhaps cause a loss of information on a particular photograph that was taken and it might be proved critical in some way. The group and the, what I'll call the scientific instrument group, and the field geologist image group, didn't quite see eye to eye about things of this kind. And I remember a time, and since it was at night it might even have been when both of us were present, Al. But there was more than one occasion judging by results, but there was one particular occasion when there was such a departure and we, whoever we were, realized that nobody had noticed that the boom was going to be—had not been retracted when some change meant that some photographs would be taken then, rather than another time. And I guess we were all worn down and a little bit sleepy, and our friends in the photographic group missed this. And so, we smiled quietly to ourselves and we got our pictures you know, like the pictures of your baby or your graduating senior. We have those pictures now showing our instrument. The one I always use, it almost appears as if the instrument is poking its nose into a lunar crater. I think there were four of these all together. So that was fun. And it was also, again, remarkable that this combination of flexibility and planning in detail worked as well as it did. You should speak about interesting events on the night shift if any. Night and morning shift.

METZGER: I'm sorry I don't remember more. There were—I do remember one occasion when Gene Kranz himself came by and invited a couple of us, of whom I was one, into the

control room. That is, the main control room where the real interactions were operated from, and proceeded to give us a lecture tour on how it was all set up and that was memorable.

ARNOLD: Wonderful! There was a VIP viewing port in the main—adjacent to the main—with a glass wall between it and the main control room and I sat in that from time to time when I had the liberty to do so. But I never got into the control room itself. Well, let's go on then to the trip back again.

PETERSON: But I think that there was a closed-circuit TV in which you could see what was going on in the control room.

ARNOLD: Yes, that's true.

PETERSON: So, we could sit in our isolated little room and look up at the screen and of course we didn't have control of where the camera was pointing, but nevertheless you could pretty well—you could see people walking around and discussing this and that.

ARNOLD: Now they were all—you were also able to see the astronauts on the surface.

METZGER: I was about to say that.

PETERSON: That’s true. And they were more thrilling.

ARNOLD: And that was—that was of course a very, very tempting thing was to get distracted by the by what you were seeing on the screen about their activities. Well, let me then perhaps push on to the end of the lunar part of the mission and there was—then there came the time to come home. We watched with great interest and I'm sure accelerating heartbeats the rendezvous take place, which it did duly, and everything—people climbed back in and sent the LEM down to crash into the moon, recording earthquake – moonquakes—and then started the mission coming home, where Larry played an active role. And what I remember most from that, was that the instruments started acting up immediately afterwards.

METZGER: [*Laughs*] Defend yourself, Larry.

PETERSON: Yeah, right. [*Laughs*]

ARNOLD: And giving us spectra which we could not interpret. And this of course, was very distressing to us indeed because we needed that information, we felt—and I think rightly—in

order to interpret our gamma ray, our lunar gamma ray data. We really wanted that background information. And we couldn't get it and we started phoning around the country. We started using all the facilities that they made available to us. And mostly we got from every, all the engineers concerned, “Oh, no, that can't be right.” Which rather than—because I was assured by somebody, it's probably as well not to remember who—that what we were describing was absolutely impossible. And that wasn't very useful. And so, I think Al and I were very much preoccupied with this problem, and Jack too, and Bob. But there was nothing very much that we could get out of our engineering support team to assist us in any way as to whether there was any way to deal with the problem. And then there was this wonderful incident. I'll be jumping a little ahead of the—I think of the observations made on the on the celestial sources when the LEM command pilot—oh my goodness. Al—

METZGER: Worden.

ARNOLD: Worden had to climb down to the service module, had to make an extracurricular vehicle – EVA—to collect some film canisters and substitute other film canisters in the instrument panel and somebody asked me whether I wanted him to do anything and I suggested that he kick the instrument. And anyway, when it turned back on it was working. And of course, we relayed this information joyfully to him and then talked about how this was the first space… He said later that he had not in fact kicked the instrument. What he had done, no doubt, was shake it in the process of taking out this big heavy film canister. And so that was probably what did it. Months later, as I remember it, our friends at Analog were able to track down and duplicate the problem in some obscure part of the design. But at any rate that, since all turned out well it was a delightful incident to remember. And it didn't take us but more than about 10 minutes to get the data we needed to fix the—to do our job as far as the data processing for the lunar data and be pretty much ready to turn the whole thing over to Larry, with whatever help we could give, for the astronomy part of this.

PETERSON: I remember during the trans—the translunar phase, being on these twelve-hour shifts, and we were just wondering, I think it was Al that was taking the other part of it. I don't know whether you were or not.

ARNOLD: Oh, I got a free ride, did I?

PETERSON: Well, you were there a lot.

ARNOLD: Yeah. Okay.

PETERSON: But after we had just plotted up some, there was enough data coming by so we could plot a crude spectrum. And I remember plotting up some of the crude spectra, like you did during the lunar phase, during the translunar phase. And of course, there was two interests, two pieces of information we wanted out of the crude spectra. One was the basic background for interpreting the lunar gamma rays, but the second was much more important to my heart at that time. That was trying to determine what the spectrum of certain—the total spectrum—of cosmic gamma rays was because there's a lot of the data on the moon was lunar gamma rays. And when the boom was retracted inward, it was spacecraft gamma rays, and someplace in between where the gamma rays which we thought were mostly celestial of some kind, and the translunar operation had been planned so that the spacecraft would slowly rotate around—

ARNOLD: Barbeque mode, yes.

PETERSON: Excuse me?

ARNOLD: Barbeque mode: to make the, make the temperature uniform.

PETERSON: The barbeque mode, yes.

ARNOLD: Well we wanted to take advantage of that because even though the detector was basically isotropic, the spacecraft itself did make occultations and there was some angular response. So, one of the greatest students, Dave Gilman [?], later on analyzed that data and made a crude map of the sky with 20, 30-degree resolution from the gamma rays.

METZGER: And with that in mind, we utilized the capability of the boom to position itself at length intermediate between full retraction and full extension—

ARNOLD: Yes, that's correct. That’s right.

METZGER: After we had gotten the data we needed to correct the Lunar data with the boom at its full extended position, then we retracted the boom partially to obtain this occultation data.

PETERSON: Yeah, we have this wonderful plot of the one over r squared effect when you're close to the spacecraft which became constant later on.

ARNOLD: Yes, right.

PETERSON: I remember that was does a pretty exciting piece of data at the time.

ARNOLD: [*Laughs*] Yes it was.

PETERSON: And it was good enough so it could actually make the plot up—plot the crude data up during the orbital phase.

ARNOLD: Mmmhhm, you could see it. Well, we're getting close to the end of the excitement I guess.

METZGER: Well, let’s see. Apollo 16, I think because of the experience we'd had on 15 turned out to be a quieter experience for the instrument.

PETERSON: Yeah.

ARNOLD: Well, for the instrument but not for the spacecraft, not for the mission.

METZGER: Right, yeah.

ARNOLD: Yeah. There was one incident that's—you're right, that so far as the instrument is concerned—well, we had a little bit of access to the computers at that time. It was somewhat helpful, slightly helpful. But as you say, it was mostly experience. But the thing I most remember vividly about Apollo 16 was that when they went around the moon and the burn to put them in orbit around the moon was made behind the moon, they did not emerge at the time when they were supposed to. They emerged earlier because they had not made the burn and they had not made the burn because there was a red light. There was one there was one backup system that was showing that it was not working properly. Or it was the red light, which I think it turned out to be. And they wanted to check with Houston control before authorizing a landing on the moon. And of course, if they didn't authorize the landing on the moon, they were going to come straight home and that was not going to be very good for our experiments, so we were very deeply involved but we were of course also concerned in other ways.

And at that time—this was an, again, a daytime thing because they came and politely invited all of us to leave the backup room because they wanted that room for a meeting. And the group of managers and—right, I suppose Bob Galbraith [?] was there. It was certainly—we haven't mentioned the—oh I've got a block on everybody's name. The Associate Administrator, Rocco Petrone—I got one—who was the one guy who always came by every day to ask us what was

new and what was going on among the senior administrators. But at any rate, he was a very good person to have. But on that particular occasion all these people gathered and there was a discussion, and we were standing in the hall waiting for the discussion to be complete. And when we heard shouting I can still remember this very queasy feeling in my stomach; my God these guys are yelling at each other, what's going to happen? What—is this incredible organization, you know, beginning to show signs of strain? Well, very shortly thereafter, they all came out and they had a decision made to land. And we took—I don't remember who "we" were at the time, but whoever was on duty at the time, we took one of the engineers who had been present, one of the company engineers, to lunch in the Mission Control lunch room, and pumped him about what had been going on. And what turned out was that one of the astronauts present had encountered a similar problem on one of the—let's say Apollo 8 or 9—one of the earlier missions, and it had turned out to be benign. And he described it. And that was a critical element in their deciding that they would take a chance on this. And of course, it worked. But my sense is certainly the same as yours, Al. Certainly on the way back we didn't have the same difficulties we had the first time. And so, there was less drama in Apollo 16 except for that particular moment.

We certainly left with the feeling that the system was getting—which was already very good—was getting even better as they had more experience. I remember in this period taking a break and going—since everything was going so well—and going to the room where the geologists were. And the geologists were—there were maybe 15, 20 geologists in that room—looking on a large television screen—what was large at that time—at the astronauts on the surface. And Jim Lovell, no less, was the capcom for this group—special group—who were able to talk directly without the usual intermediaries to the astronauts on the surface doing the runs around the lunar surface. And it was simply amazing to see people on the surface saying, "Hey, would you like one of this sample here?" And the other one saying, "Well, no. Why don't you take that one over there?" And sometimes when there were issues—I didn't spend a lot of time doing this, but I probably spent all told an hour and somewhere between an hour or two—and it was just enthralling to just imagine what life would be like in a situation where you really had this sort of stuff going on all the time. So that you could have an interaction between the people on the planet or whatever it was that you were investigating and the people back on Earth. That was, that was very exciting.

Other than that, I think all I can think of to say at this point was that it was a marvelous adventure. That it was very successful scientifically, ultimately. Although it took us years to milk

the data for all that it was worth. Indeed, that process was not complete until Phil Davis arrived and gave it the final polish. All sorts of things were done as we learned more and more. It was a kind of bootstrap operation, the data analysis, because we had to establish what backgrounds we were going to use. There were a lot of cases where we had to essentially iterate either at mostly at U. S. Geologic Survey or at Goddard Space Flight Center where on more than one occasion, I think we had the use of Goddard's main computer for a day. Entirely ours for data processing. So those are another part of the chapter scientifically, very satisfying, I must say both at the time and in retrospect, and very successful. Though I think, if you look at what our friends could do on the Lunar Prospector, you see that it's a different world today.

METZGER: Any comments you'd like to make about the interactions with the press during the missions, because I know you were involved, as many others were, with press conferences.

ARNOLD: Yeah, this was rather a picnic, both 15 and 16, compared to Apollo 11. When we talked about Apollo 11, I must have told about the extraordinary interest we had. There were hundreds of media people and their extraordinary interest in picking up bits and pieces of what we weren't supposed to talk about, which is what the samples were showing—until the sample distribution list was prepared and the secrecy was lifted. On Apollo 15 and 16 everything ran much more smoothly. The number of media people was down quite a bit anyway, and some of them by that time were our friends. I mean, they were people who had earned our respect. So, all I remember about it is that it went—almost all I remember about it was that it went rather smoothly. For one thing, of course, the quarantine was over. And this meant that both John Young for Apollo 16 and David Scott on Apollo 15, were available to the media, as well as ourselves, and sometimes together. So, what I mostly remember was that in general it was a lot of fun. And that we, of course, weren't able to satisfy their curiosity very well. We had to keep repeating that, you know, we will have to do the calculations later and you should look forward to when we get ready to give talks in Houston and when we get ready to publish papers in the literature. Because all we can tell you right now, there are such tidbits as, well there are—just as the as the images show there are definitely—and the samples show because this was already after Apollo 14, which went to the west. That there were three kinds of material on the moon, the highlands, the maria, and the creek [?] material, which is always classified as part of highlands, although it's really chemically very different. So, we could tell them those things just from the raw counts just from what you could see on the on the panels and by now with primitive plotting equipment. But we were hardly in a position to say anything more. One thing we could get after about Apollo that it was reassuring to us also, immediately, was that Apollo 16, of course, went

over some of the same areas that Apollo 15 had gone over. And yes, they gave us the same data. So that was reassuring. But all the discoveries, you know, that when we were making color maps later at Flagstaff and other places when we had the La Jolla Consortium, all those things, what turned out to be the edge of the big backside basin that we touched on Apollo 15. We didn't know that at the time and we didn't know it at all until Galileo came along and took images of the backside and showed us what we had seen.

METZGER: What is it we didn't know?

ARNOLD: We didn't know that there was a large basin on the backside of the moon, which had a mixed composition, as far as I'm aware.

METZGER: Maybe not the latter point. But we did see enough in the Van de Graff region—it was sufficiently extended so that we did surmise the likelihood that we were just on the edge of something that was larger.

ARNOLD: Well, I remember mainly we were using a term like Van de Graff for that region, which was a nearby crater and a rather unusual one. I don't remember being conscious, if you say so you're surely right.

METZGER: It was the region that we picked the name because it was a feature in the region that was easy to, you know, describe the region.

ARNOLD: Right. But at least in my case, there was no great sense that this was necessarily anything like the enormous extent that it proved to be.

METZGER: Oh, not as enormous, true.

ARNOLD: Okay. Okay. Well, we're perhaps splitting hairs here.

METZGER: Something I thought of while you were talking about the interactions—it is out of order now but perhaps worth mentioning—that before both missions the experimenters had an opportunity to meet with the astronauts.

ARNOLD: Yes.

METZGER: And discuss the experiments with them and then answer questions.

PETERSON: Yes, indeed.

ARNOLD: Good, good. That should be talked about. The astronauts were undergoing all this training. And, of course, we wanted them to know something about our experiment. And so, we had, as you say, sessions with the three astronauts lasting I think in both cases, something like an hour. And in their busy schedule, and these people were undergoing training like 16 hours a day for a year. And they would march in and we would brief them really briefly and then they would ask questions and we would tell them, you know, what we thought were most important things and so on. The only one who was really, really, really concerned was of course the command pilot because they really liked us because this gave them something to do. The other two were polite and interested, but not as deeply involved. The one I remember actually more is John Young than David Scott, though I, in later interactions, I got to know both of them pretty well. I think this was stimulated by the fact that the schedule had slipped a little bit, and they were an hour behind schedule at this point. But I started through my briefing, and I was naturally slipping into what I will call sales mode, you know, telling them what a great experiment this was. And John Young, who later became just the friendliest guy you could ever imagine, said to me, "You don't need to wave the flag." And that stopped me in a hurry. But it was exciting. There was one occasion—oh, it was Apollo 13. You guys weren't in on that. I was remembering having dinner with the astronauts, or was it 15? I'm not sure. Were you with me on that? No.

METZGER: No.

ARNOLD: Okay, well, then it must have been Apollo 13 as I remember that after the training session, we were out in an Arizona lava field. Lee Silver, who was running a test, and the main crew and the backup crew, which included John Young and the Apollo 16 crew, had dinner with us and that was a very pleasant occasion. We listened to the gossip and answered questions. Well, maybe we've—going, going I think we've had our session and used our time. Thank you for making it possible when we're three hours late.

BRAD WESTBROOK: You're welcome.

[END OF INTERVIEW]