An Oral History of

JAMES ARNOLD, ALBERT METZGER, and ERNEST ANDERSON

On July 19, 2000

ARNOLD: I'm going to start out with what I'll call the pre-history: how I got involved and how we managed to get the project going which is a sort of amazing thing in itself—as we've been agreeing—how little fuss and muss there was. And then I think we'll take that to some point or other where, what Ernie and his colleagues did, you know, got involved in Los Alamos and then how Al joined JPL and us almost simultaneously. And then we'll just talk. So, that's going to be the sequence. I brought along a couple of—a paper and an unpublished thing that we can add to the— Well, look at that reproduction! [*Laughs*] It's still legible, I think.

WESTBROOK: It's fading.

ARNOLD: It is fading. That was— Those were before the days of Xerox.

WESTBROOK: You might want to photocopy that before it becomes like [*inaudible*]—

ARNOLD: Well, the thought occurred to me but I'll just turn this over to you and you can do what you like.

WESTBROOK: Okay.

ARNOLD: Okay, well, so anytime you're— are we ready or, Al, do you want— I notice you were getting organized here.

METZGER: Ready.

ANDERSON: We're ready.

ARNOLD: Okay. This is Jim Arnold and I have with me today Al Metzger and Ernie Anderson, two colleagues who were involved in the subject which we're dealing with, which is the first part of our activity in the effort to map the moon in radioactivity, and thus determine the composition of the lunar surface overflown. The second part we're going to do later, which was the Apollo mission, which was successful. This is the Ranger program, which was not, but which was a pioneering effort and the interest attaches to the fact that it was one of the first extraterrestrial objects— the first extraterrestrial object targeted. And the first effort to explore it.

METZGER: Excuse me, this is Albert Metzger. I would put in there that even though the experiment was unsuccessful in terms of its lunar objective, it did make the first observation of the interstellar intergalactic gamma ray flux in its energy range that was ever made.

ARNOLD: That's true. And my other piece of paper here is our Nature article that we wrote. Let me see, that's Albert Metzger, Ernest Anderson, Marvin Van Dilla, and James Arnold have their names attached to it. Okay, let me go back then to the start of the space age. In 1957, in late 1957, Sputnik flew— Sputnik 1. There were banner headlines in the New York Times. There aren't many times when they go across all the columns. And my interest in space can be judged by the fact— at least, I judge it by the fact— that in retrospect, the day after that announcement, I was trying to reach the Vanguard program, which was the US effort to put a spacecraft into orbit as it stood at that time. The developments continued without any active intervention on my part until a year later when we had put up our first orbiting spacecraft, when Jim Van Allen had discovered the radiation belts and so on, and when I moved from Princeton University to UCSD as it was beginning to form.

The first launch of the gamma ray studies developed in the following way. The Jet Propulsion Laboratory in Pasadena, which was already the designated center for any possible such adventures, had started a seminar series. And in what Al tells me is either the second or the third of those seminars, my friend, Harmon Craig, scheduled me— before my arrival and without asking me— to give a talk. So, I had to think of something to say. And I thought of the experiment which is the subject of today's program: an experiment to use the gamma rays emitted from the surface of the moon to get information about its composition, its surface composition. And so, I thought, well, I'll amuse these people for 15 minutes and that will be the end of it. Well, I gave the talk. There were questions. And Harold Urey rose to his feet in the front row, sitting next to the director of the Jet Propulsion Laboratory, Dr. Pickering. And he said, “This is the best experiment for lunar studies I have ever heard of yet in space.” And turning to Pickering, he said, “You've got to see to it that Jim Arnold gets to do it.” So, I was trapped.

The history of the idea goes back further. It's a fairly obvious one, as may be judged from the fact that I became aware of four independent inventions of it in the United States, as well as relying on the statement by Yuri Surkov that this was invented quite independently in the Soviet

Union. I'm perfectly prepared to believe that. The first priority belongs to someone I had known for a good many years named Irving Glifford [?] who sent me much later, a memo which he had— in 1957 already— addressed to his bosses at Ford Aerospace Company, proposing this experiment. And which, however, had never been published in any scientific journal, and therefore it certainly never reached me. One of the later ones still preceded me, and this was a proposal by someone at the Texas Instrument Company, then a quite new outfit but a very rapidly growing one. And they proposed something very similar and even wrote to NASA, suggesting that they be allowed to carry it out. We got into the act thereafter. And it became "we" when I got in touch with Ernie Anderson here, knowing how strong Los Alamos was in gamma ray spectroscopy and that Ernie was connected with the people who were doing that sort of work and looking for assistance in the actual production of a space-worthy instrument and the carrying out of the experiment. I was in touch, also, with the Jet Propulsion Laboratory management and someone whom I got to know later named Connie [Conrad] Josias— who later founded a company that built an instrument for us on Apollo— was assigned to me for a while until Albert Metzger arrived. He tells me late in 1960.

METZGER: '59.

ARNOLD: '59, thank you for the correction. And was almost immediately assigned to this project and to work with me and that— it's now July 2000 and Al and I have been working together since then. Ernie and I, of course go much further back. I—

METZGER: I was about to ask— you go back originally to Chicago, right?

ARNOLD: Oh, yes, to 1948. So, I tend to continue to be on speaking terms with my collaborators for rather long periods of time, which I take pleasure in. At any rate, discussions began with the Los Alamos people first and Ernie recruited two other people who are important in the story. One is Marvin Van Dilla, with whom he was working on gamma ray spectroscopy applied in the health physics division, I guess, that you belonged to at that time. And the other was Bob Shook, whose name occurs also in our carbon-14 tape. He was the technician on that project and he was the builder— the actual hands on constructor— of the gamma ray instruments that were used on the actual Ranger experiments. Once Al had arrived and joined the project— we might say by early 1960— the team was essentially together, the key members of the team were essentially together, and a serious effort commenced leading to the first launch of a Ranger spacecraft containing our instrument, an instrument— a seismometer—

which a fellow named Frank Press, later famous in other ways, was the principal investigator of, and a television camera, basically, were on that spacecraft. And I'm going to turn this over to Al in particular at this point, who has a catalogue of some of the interesting documents he has in between us. Al?

METZGER: Alright, well, as Jim just said, I came to JPL in December of '59. And the best recollection I have is of first coming down to La Jolla to meet Jim early in 1960 sometime in the January to March period. My two main recollections there are the beauty of the California coast as we came down in what was then a JPL-provided helicopter and the unsurpassed location of Jim's laboratory situated as it was within one short flying leap from the beach.

ARNOLD: Yes. That was my recruiting laboratory. [*Laughs*]

METZGER: [*Laughs*]

ANDERSON: No one who visited him there could understand how he ever got any work done with the ocean and the beach just outside his door.

ARNOLD: Most productive period of my life.

ANDERSON: [*Laughs*]

METZGER: I have among the materials I brought down letters that preceded my arrival which relate to correspondence between the people at Los Alamos and NASA, but there's also an item quite unforgettable in the light of how things are done today which involve the formal— if it was— submission of the proposal by Jim on behalf of himself, Ernie, and Marv Van Dilla to get approval for the experiment. Jim, do you want to mention the labor involved in that effort?

ARNOLD: Well, the very great contrast with today: Al as my authority for the statement that the document was a page and three quarters long. And was, as you say, signed. There were some very modest requests for money, I'm sure, by modern standards but we got a go-ahead. Let me say one other thing: the Texas Instrument group, as I say, was handicapped by the fact that they just weren't familiar with the moon and I had Harold Urey, the world authority next door to me and always ready to correct me and frequently had to. So, we had an edge in that department and the capability between Los Alamos and JPL was unmatched. I don't remember any formal announcement of proposals, announcement of opportunity; I don't remember any formal screening, though there probably was some minimal thing of that sort. So, we were off and running in short order.

METZGER: The instrument divided itself into three main parts. There was the detector system as the responsibility of Los Alamos, which began in house with yourself, Ernie, and Marv and Bob Shook, and Bob doing most of the hands-on work. And then that shifted over to a private firm that Bob moved to, with an agreement that was worked out very smoothly. And I can recall Bob saying how accommodating— was it David Packard or Lyle Packard?

ARNOLD: Lyle Packard.

ANDERSON: Lyle.

METZGER: Lyle Packard— was in making that shift. It really was seamless. At which point then there was a contract entered into between Packard and JPL. The pulse height analyzer, which we obtained from one of the firms building these instruments for laboratory use in what certainly was the first space adaptation of a pulse height analyzer, and where JPL and Goddard found themselves interested in the same kind of device. And since, Jim, you were much involved in making the arrangements in that, you should discuss how that came about.

ARNOLD: Well, Al reminds me that, of course, I had known a number of the cosmic ray people, being involved in— being a user of cosmic rays in other aspects of my research. Frank McDonald at Goddard was one of the leaders. And it seems that what was the state of the art at that time, which was a 32-channel analyzer— I don't think this is the kind of part [?] where we should go into great technical detail— but it could distinguish 32 energy levels of the gamma ray spectrum and characteristic lines associated with elements in different slots of that assortment. McDonald had a contract with this company to build these things and we wanted one and he wanted plenty. So, the outcome was that he got the first one which was an advantage to us because he was no doubt a very critical customer. And we got one of the early ones. And, in fact, we got more than one, but the first one was the critical.

METZGER: And the third part of the instrument was the high voltage power supply for the detector. And that JPL built in-house based on a design provided by, essentially, a consultant, who was quite expert in the field and built a very small, usable supply for the program.

I'm looking at a letter here that was written by you, Jim, to Al Hibbs in April of 1960, which describes the status of things at that that early date and at a time when, apparently, it wasn't even known as the Ranger program because it's referred to as Lunar Impact Vehicle and you are requesting space on that Lunar Impact Vehicle and referring to earlier memoranda to NASA and to Hibbs, mentioning who the people were who would be involved, asking for some funds, and then talking about that 32-channel analyzer. And I have attached to it— you were asking me before what the cost price was, and I gave you an estimate that actually was not too close. There is the record.

ARNOLD: Alright, what it says here is 32-channel analyzer, $18,675, and the ground support equipment, $6,575. In the Mars Observer project, which was the last one that Al and I worked together on, there were three more zeros at the end of numbers like that. That shows what the progress has— but not necessarily forward progress. However, it must be said we were just learning how to build space instruments and so was everybody else. I can remember, Al, when I was up at JPL, you know, getting told what this instrument had to survive in the way of the launch environment, and so on, that somebody did a little example as follows: there was an instrument— a model instrument of some sort— maybe eight inches cubed, something about that size, and it was hooked up to an oscilloscope which could register the way the instrument worked. That was getting a pulse every now and then. And this person had the instrument sitting on the table and then he knocked it on the floor. And as he said, not only did the instrument continue working, but the trace did not jump. And, that was to give you an idea of the kind of behavior that had to be expected of an instrument that we built. And I can assure you, I had no instruments in my laboratory that would survive even the drop on the floor much less the other. So, we were learning by doing and we succeeded in learning.

METZGER: JPL knew how to build flight survivable equipment, at least as far as the state of the art was in those days. But it was a relatively confined expertise and no commercial electronics company, such as RIDL [Radiation Instrumentation Development Lab], who did the analyzer, had that knowledge. The way that problem was dealt with was that RIDL did essentially the design and the layout, knowing how the components ought to be arranged, and then the task of packaging this— designed to flight qualified level— was given to another firm in the Los Angeles area. So that made it a three-way collaboration: RIDL, there was this other firm— AMP [?], and JPL. Now, what I find interesting is that I don't think Los Alamos had any knowledge of how— any prior expertise [*inaudible*] in building flight instruments—

ANDERSON: No.

METZGER: —and yet you and Bob at Packard did and did it successfully.

ANDERSON: We had instruments which suffered very violent treatments such as an atomic bomb explosion, but their data terminated abruptly the minute the shockwave reached the instrument. So, to have a shock and then continue making measurements was unusual.

ARNOLD: Yeah. The other constraint of course, was the limitation not only of mass—because our capabilities were limited to lift things into orbit— but also power and size. So, this was new to all of us and we learned by doing. The instruments that were built— designed at Los Alamos and built by Shook in one or another environment— worked.

Okay, so here we were, getting ready. And the Rangers 1 and 2 were being developed at the same time. They were deep space vehicles, is that right?

METZGER: Yes, they had field and particle instruments— a good many of them. There were about ten instruments.

ARNOLD: I see.

METZGER: Same complement on both 1 and 2. But the idea was to take advantage of these test flights.

ARNOLD: Okay. As we came up toward Ranger 3, we were approaching 1962.

METZGER: Yes.

ARNOLD: And we had three scheduled flights out of what was supposed to be, and indeed eventually was, a rather long series.

METZGER: Perhaps you should say something about the plan sequence and what the instruments were designed to do and—

ARNOLD: Okay, this would be a good time to do that. Right. The idea of the experiment was this. The spacecraft launches and goes into Earth orbit, then goes out of Earth orbit on the way to the moon. This was the first of many calculations to guarantee as best possible that the spacecraft and the moon arrive at the same place at the same time. The way that the spacecraft addresses the moon was simplicity itself. Namely, it was designed to impact the moon on the front face, where data could be recorded up until the actual crash and telemetered back to Earth safely before the spacecraft instrument and all were destroyed. The gamma rays that we were seeking would not impact our— would not reach our instrument in appreciable numbers until the spacecraft was rather close. One of the things that had to be considered was the fact that the instrument registered gamma rays in two ways. One which was the radioactive decay of certain chemical elements and in particular potassium, thorium, and, to some degree, uranium. And these would give rise to— the emission of gamma rays of known energy would allow us to identify those particular lines in the spectrum.

But a much richer source of lines was the cosmic radiation caused by the cosmic rays impacting directly on the solid surface of the moon. On Earth, the cosmic rays penetrate the thick atmosphere and very few of them reach unaltered to the Earth's surface because of the heavy blanket of air above. On the moon, there is no such blanket. And so, the full energy of these very high energy particles goes into the surface of the planet— of the moon, in this case— and gives rise to nuclear reactions which generate lines again characteristic of iron, silicon, oxygen, and other elements which make up the stony crust of the moon.

Now comes a problem. The spacecraft is also made up of matter and the spacecraft itself is made of aluminum and various other materials which under the same bombardment will emit their characteristic gamma rays, thus making it difficult to disentangle what is due to the moon that we're interested in, and what is due to the spacecraft that we know all about and are not interested in at all. The technique that was used— and is still, in many cases used— to overcome this problem or to make it manageable is to extend a boom— or rod, essentially— carrying the instrument away from the mass of the spacecraft itself. You can picture the spacecraft as weighing— oh, let's just grab a number out of the air— 100 kilograms or something of that kind. And if it's close to the detector, the detector will tell you about it. We had a six-foot boom— roughly two meters, for those raised in metric— which could be extended away from the spacecraft and which made the spacecraft a much poorer source, a much weaker source, of gamma rays. Just what we wanted. So, what Al has shown me here— and I think we can probably spare one for the record, or make a copy for the record— is a mock up, a model, of what the spacecraft looked like including the instrument in a spherical container with the boom extended.

METZGER: The detector was in that sphere. The electronics were back on the main frame.

ARNOLD: Right, and there's a cable connecting the two.

METZGER: And the gamma ray spectrometer was one of three instruments in the spacecraft, the other two being a camera, and then a balsa globe [?], which enclosed a seismometer which was intended to survive a hard landing, being detached just before the balance of the spacecraft impacted, and roll around on the surface, survive, and then hopefully begin sending data.

ARNOLD: Presumably bounced rather high, at first, but at any rate, that was the idea. And now, as we prepared for this first experiment, the intensity of the activity rose. I think before we get to the actual Ranger 3, you had a list, Al, of documents that you had collected from the old days out of your files. I thought it would be interesting to read some highlights from that list.

METZGER: Well, these list the correspondence, not so much documents as specifications. I may have those but I didn't find them.

ARNOLD: You've described a few of them already.

METZGER: Right. But they tended to be letters or attached to letters or enclosures with letters. But it shows the way the arrangements were made to initiate the understandings between Los Alamos and then JPL and then for the procurement of the analyzer as you've described, Jim. And then there's a lot of correspondence having to do with, you know, the details of the detector as it began to be fabricated and the results, the scheduling. I mean, I could try to find something typical. But if you want to give me a minute, I'll find the letter which sort of summarized the status of things as it stood in November. There was— ah, yes, here. On November 30th of 1960, Bob Shook— I believe he was probably still at Los Alamos at that time— wrote to say that he had completed a mockup of the detector and the first working unit was almost done and would be shipped in about a week.

While I look for that, maybe Ernie would say something about this picture that's always been dear to my heart, which of course illustrated a prime objective of the experiment: namely, to prove or disprove the Green Cheese Theory.

ANDERSON: [*Laughs*] That would be the first thing that would occur to most people when you say, "What's the moon made of?" They would recall that, "Oh, yes, the theory was that it was green cheese." We had a very ingenious photographer on the staff, who came around to the lab to just take some background pictures of what was going on. And he was quite intrigued by this dichotomy between the moon as a rocky body and the moon as being green cheese. And he said, "It's obvious that the best authority on cheese is a mouse." And since we were a biological lab we had lots of mice around. So, he suggested, “Why don't we bring in one of these experts, and we will let him decide what the possibilities might be.” So, he made a photographic montage of the surface of the moon in the background, a chunk of meteorite in the foreground— since meteorite were the only extraterrestrial material that were physically accessible— and he got one of our mice to sit upon a slice of Swiss cheese which was placed next to the meteorite. And the mouse obligingly, or by the skills of the photographer, inspected the meteorite very carefully, and obviously preferred the cheese.

But the mouse was not, at that point, able to look at the moon directly. This was a purely Gedankenexperiment, as the scientists might say. But we also prepared a series of graphs showing the expected gamma ray spectra that would come out of various things. The actual measured spectrum of a Sedona [?] meteorite showed quite a series of lines, and some from cosmic ray induced activity, some from natural radioactivity. The cheese was essentially non-radioactive; its spectrum was absolutely flat. And then our photographer pointed out that if, by any chance, there were mice living on the moon, these mice would have potassium-40 in them. So, he had us make another artifactual graph, which shows in comparison with the cheese only a spectrum of the cheese plus the mice, so that here was a chance to not only decide whether the moon had activities like potassium and radium on it, but whether there was some additional potassium activity which could be ascribed to the presence of mice on the moon. That was not taken very seriously, but it did indicate that we tried to keep our sense of humor as we dealt with these esoteric problems.

METZGER: Since we're now talking about the prospective science results of the experiment, this would be the appropriate place for you to mention the selenographer’s lexicography.

ARNOLD: Yes. I mentioned just a little bit ago the fact that we had two sources of gamma rays, and I, indeed, could have divided it a little further as in the document that Al refers to. There are two kinds of ways that gamma rays are produced by cosmic rays. So, I listed those separately and we had three categories, and this hardly— this faint, light lavender colored document here— documents my calculations as they were probably around 1960 or 61. This was not a published document so it does not have an official date, but somewhere in there, I made the first attempt to calculate something that we now calculate by huge software packages that are maintained at Los Alamos and elsewhere. This was all done with slide rules and very crude knowledge of the intensity of the various lines as they stood around 1960. But at least they were a guide to tell us what the likelihoods were.

This may also be a place to make a small confession. The look with the usual benefit of hindsight, had our experiments succeeded. Because we only spent a relatively short time close enough to the moon to get a good signal. The estimate I remember— I haven't checked it – was twenty minutes. As the spacecraft approaches the moon and the moon becomes big in the field of view, the gamma rays begin to record. And in twenty minutes, with a nice sized crystal as we had— it was a good instrument for its time— nonetheless, the statistical accuracy, the number of events that would enter the crystal and be analyzed was not extremely large. And so, in all probability, we would have been able to set limits and say, for example, that there was not more than— to snatch a number out of the air— 5% potassium in the rocks. But I'm afraid that today I would have to say that we could not have done anything very quantitative. Still, when you know nothing, when your ignorance is complete, even the smallest shred of information— if you divide your present knowledge by your past knowledge you get a satisfactory infinity, so it might have been alright.

METZGER: Shall we go back to status?

ARNOLD: Okay, Al, have I given you time to discover here?

METZGER: Yes. Alright, so a letter from you, Ernie, dated October 2nd of 61 to me and I'll just paraphrase that reports that you're shipping moon spec— your appellation for the detectors— moon spec number SN-9 on shipping memo number so-and-so. The first unit in the second PO from Packard. So that means that eight units had been built and, I guess, probably all had been shipped. We might think about the difficulties, why so many had to be made, when I get finished with this. But it summarizes measurements on prior numbers seven, eight and nine; reports on measurements that were being done at Packard and at Los Alamos on output and reject threshold and the margins for those; and that tubes had been received, two additional tubes; and the boards were being prepared at Packard, and would be ready on October 6th; and, here, we have requested your— that is JPL— quality control man to be there for inspection. JPL performed that function on the detectors as well as, of course, the electronics. And, also you say, “Marv and I will be able to visit JPL on October 13th and 14th as you suggest. Have you checked with Arnold?” So, that indicates it was to be a meeting of the full team.

ARNOLD: I presume took place.

METZGER: Right. Okay, well, I'm trying to remember as best I can the difficulties. I think it was a combination— some units just decided to lay down and quit during testing. Other units, I think now, there were difficulties with two or three on the sealing because these units were hermetically sealed.

ANDERSON: Yes.

METZGER: And that was a non-trivial requirement.

ANDERSON: Yeah, there were two main vulnerabilities that these instruments had. One was the possibility that the violent shaking of the rocket launching might cause an electrical connection to fail or something inside to break physically. As any of the astronauts who rode these things well knew, they are not a smooth means of transportation. With a rocket engine vibrating the whole system and producing accelerations of many times gravity, the whole unit— the detecting unit, the sodium iodide crystal— had to be immersed and supported by a foam packing that would absorb the shocks. If this system failed and the shocks caused something to break inside, the unit was lost. The other one was that the photomultiplier tube— the light detecting device that converted the light pulses which the gamma rays made as they pass through the crystal— converted them to electrical signals. This photomultiplier rig which was essentially an amplifier with a very, very high gain had to be powered by a high voltage— a kilovolt or so, perhaps. Well, in a perfect vacuum, the voltage is no problem. In a high pressure, such as the normal Earth atmosphere, the problem is not too serious. But as you get it in between— if the pressure within the detector begins to drop because there's a slight leak in the system and you have a vacuum of space around it— you pass through the region where electrical discharge takes place, and this would wreak havoc with the electronics and the photomultiplier itself.

So, when the system was tested in the very rigorous machines that JPL used, either of these two things could happen. There could be a mechanical failure induced by the vibration, or as turned out to be the case, some of our hermetic seals were not hermetic, and they were very slight seals. But on the other hand, over a period of time, the inside pressure would leak out, and the electrical discharge then would destroy the system. So, the two principal things that JPL was testing for in this destructive phase was sensitivity to the vibration of the rocket, and a pressure leak that permitted the gas inside of the detector to leak out into the vacuum of space. And the leakage problem was quite serious, especially at first, because we had rather naively assumed that when a skillful arc welder welded the stainless steel container that it would be vacuum tight. Well, the vacuum requirements here are very, very extreme; this thing is sitting in space for a number of days in essentially a perfect vacuum outside, and even a very slight leak would be enough to drop the pressure to the danger point. So, the reason for making the multiple units was, of course, you always want spares. And the spares were to take care of losses during the dynamic testing and also to provide for the fact that some of the units might have defects which had escaped our notice at Los Alamos.

METZGER: And this experiment was to be carried on three separate launches.

ANDERSON: On three separate occasions, right.

METZGER: 3, 4, and 5.

ARNOLD: Yes.

METZGER: While talking about requirements, let's mention another that we had to pay a lot of attention to. I think somewhat belatedly— not at the beginning of the program, but partway through— we had the sterilization requirement. And I'm not sure now whether the hermetic sealing sort of saved our situation, or was in part, at least, a reason why we did not have to sterilize the detector. It sounds quite strange nowadays, but at that point there was some concern about populating the moon with microbes from Earth! [*Laughs*]

ARNOLD: Yes. [*Laughs*]

METZGER: But in any event, the entire spacecraft, with just a few selected exceptions of which our detector was one, was compelled to undergo heating at 125 degrees centigrade for twenty-four hours.

ANDERSON: Which would utterly destroy the photomultiplier tubes.

METZGER: Yeah, not to mention the FOSS switch. [*Laughs*] The plastic.

ANDERSON: Actually, the entire detector was not free from the sterilization requirement. The thing we talked them out of was sterilizing the sodium iodide that was grown into a crystal at several thousand degrees centigrade. They were persuaded that if it had any contamination, the only part alive was on the surface. And therefore, it was required that the components be chemically sterilized since they couldn't be sterilized by heat. The agent chosen was ethylene oxide, and we built a large tent, a gas tight tent with gloves reaching in through arm holes and all the components that would go into the detector were [*inaudible*] the skilled technician then would take all the components that were going to be built into the detector unit and assemble them in the sterilizing atmosphere. The unit was then closed sufficiently to prevent any rapid ingress of microorganisms and was then arc welded to seal it shut and prevent anything else from getting inside. So, there was a surface chemical sterilization carried out on the components of the detector to make sure that when this thing was broken open and scattered around the surface of the moon it didn't introduce some earthly biological organisms— microorganisms.

ARNOLD: We've had a more complex sequence of events involving Mars. I think that's a case where contamination of an extraterrestrial object might have a slightly greater than zero probability. But I think on the moon we— and again in retrospect— we can be pretty confident that not much has survived there.

METZGER: I see a sentence in a letter from me to Bob Shook on December 21st of 61 saying the scoreboard on eight units, including the rebuilt system from Packard, shows four still operating and four which have failed during environmental testing. So, that was where the mortality ensued.

ANDERSON: Yes.

ARNOLD: Okay. Other items on your list that might be mentioned?

METZGER: Yes. In the problem department, there is this letter from Ernie to me and a copy to you, Jim, which I had quite forgotten about, dated February the 5th of 62 reporting that you, Ernie, had read about material being used on the rocket which was to boost the range of spacecraft containing some natural uranium and perhaps thorium, and wondering if whether any of the materials of the Ranger system might have a similar content, which triggered off events culminated in our surveying the spacecraft. And because we hadn't had the thought earlier, sorry to say, by the time we had access to the spacecraft it was now down at the Cape— this was Ranger 3— for systems testing prior to launch. And a technician was there working with me. I applied for access to the spacecraft and was given that access without any hindrance of any kind— no formality— one night when everyone was at home enjoying their evening. And we went in with laboratory equipment and just moved around, got right as close to the spacecraft as we wanted to, you know, maybe a fraction of an inch between us and what was easily accessible so that we would have the most sensitivity and just went around systematically.

Spent five or six hours, as I recall, surveying it, and found to— well, I can't recall whether it was surprise or not surprise, but certainly our great interest— that there were two notable sources of interference. And, as I recall, one was somewhere on the main frame and the other was up in the radar system at the top for sensing the approach and it caused a bit of a stir back at JPL when we reported this.

ARNOLD: Do you know whether any remedial action was taken?

METZGER: Again, as I recall, there was— one of the two was modified. Even at that late date they did something— perhaps put a bit of shielding on one, and we just agreed we could manage despite the other one. And we did have the advantage of the boom that you were describing before, Jim, to mitigate that problem.

ARNOLD: Right. Okay, well, are we ready then to move on to Ranger 3, or would you – or is there some more there?

METZGER: That's fine.

ARNOLD: Okay, well, this was our baptism. Al, you were probably— being the JPL person— closer to events certainly than I was. I never made it to the Cape until very much later. What was your situation?

METZGER: I was at the Cape for the system testing, during which the survey for radioactive materials on the Ranger spacecraft took place and saw the launch, then came back. The operational profile was such that we knew enough about the radiation belts at that time. They had been discovered by James Van Allen's original experiment on the first U.S. satellite. And I think there had been additional data— one or two Pioneer launches had gotten more detail.

ARNOLD: I think so.

METZGER: So, it was our planned purpose to stay off until we were through, and then turn on and acquire data on route to the moon for two main reasons. One as a background which would be subtracted from whatever we were fortunate enough to observe during final approach to the moon, but secondarily, and we were aware of the potential for interesting science in terms of the possible presence of an interstellar gamma ray flux. This had been discussed in the theory not long before the Ranger launches, principally by Phil Morrison at MIT.

ARNOLD: Oh, right. Yes, I had forgotten that.

METZGER: But it was a surprise when— and I am quite sure it was on Ranger 3— the first data we got, it was anything but quiet. I'm not— I have the information in the file, but I don't know exactly what letter. Maybe when someone else is talking I can determine it. But either on 3 or on 5, for both of which we obtained some data, instead of being relatively quiet when we turned on, the counting rates were very high, very high. And then they proceeded to diminish with time until we were down to more or less what was a lot [more] reasonable expectation in cislunar space. And in contrast, on the other launch where the systems were functioning initially and we obtained data, we didn't see any such activity. It just illustrates the fact that comparatively little— much less— was known about the extent and orientation and variations in the radiation belts in those days. So, we made our decision as to when we would turn on based on the best information available to us up to the time of launch. And it turned out, that wasn't the case, that the belts— the outer belt— did extend further at that time than we expected.

ARNOLD: I see. Okay, but now, then, you passed that point.

METZGER: Ranger 3 operated for forty hours, as I recall. The problem was— and that took it well beyond the moon. No, that doesn't sound right. It seems to me it had to be a three-day trip.

ARNOLD: That's correct.

METZGER: Well, this is now a memory problem. But we had forty hours of data. I might be confusing it with Ranger 5 where we had forty hours of data. So, we must have had more data than that from Ranger 3. In any event, I remember that the problem with Ranger 3 was navigational. They were unable to put it on the prescribed trajectory and impact the moon. And our system operated all the time the spacecraft was operating. We had intelligent data. But the best we could infer from it is that the moon was not composed of 100% potassium.

ANDERSON: [*Laughs*]

ARNOLD: [*Laughs*] Yes, I remember making those calculations and announcing the result with my tongue firmly in my cheek. Okay, so then we regrouped. And perhaps I should tell the story of the Ranger 4 as it impacted me. It happened that I was— as not infrequently was the case— had to fly to Washington for a committee meeting during and immediately after the Ranger 4 launch. So, I was on this aircraft, on the way, and appropriately on tenterhooks, of course, as to the outcome. Got in, took a taxi to my hotel. There was waiting for me a telegram from Al. Apparently, he sent it to others as well. But I can easily quote through the entire text. The text said, quote, “Nothing. Al.” Do you want to— well, here is a copy, okay, in case my memory is questioned. [*inaudible*]

ANDERSON: [*Laughs*]

METZGER: [*Laughs*] Well, they did say, “Be succinct.”

ARNOLD: [*Laughs*] Alright, you might want to fill us in and be a little more loquacious on this occasion and tell us how you learned and what you learned or when you learned.

METZGER: Well, first I’ll give my best recollection on Ranger 3. It was at some point close to the end of the mission, being in a trailer and you were not there, Jim, simply because you were off on some prearranged trip. So, I was sitting in in your place in the presence of the investigator team for the photographic observations— the TV system— which consisted of Harold Urey, Gerard Kuiper—

ARNOLD: —and Eugene Shoemaker.

METZGER: —as intermediary peacemaker. [*Laughs*]

ARNOLD: Yes. Three people, no pair of whom were on speaking terms with each other. That’s what made it fun. Yes, okay.

ANDERSON: [*Laughs*]

METZGER: On Ranger 4, it was known very, very early that it was a bust because they couldn't make it come on. Something failed in the electronics and there it was, just a dead body hurtling. And this time, unlike 3, it was going to the moon. So, unfortunately, if they'd had the two problems on the one launch, we would have had one very successful mission. But Ranger 4 was silent and I can remember feeling compelled and I yielded to the compulsion of going to JPL at the time it was going to impact. And just going there, and there was the flight director— maybe he was the program manager at that point, someone known well to you, Jim. Jim Burke.

ARNOLD: Yes. Another old friend.

METZGER: And looking wistfully at him, you know. [*Laughs*] And just, nothing.

ARNOLD: Alright. So, we rallied our forces and we realized we had one more scheduled flight. Of course, considering the importance we attached to this experiment— we knew that there were later Rangers. Well, anyway, let's—

METZGER: I have the recollection that they didn't plan— well, they must have. But I wasn't very much aware of a later Ranger program until rather late in the— they called it Block 2. Block 1 was the firing of the first two and ours was Block 2: 3, 4, and 5. But I did notice in the file that we were talking about a possible Lunar Orbiter even before the launch of Ranger 3.

ARNOLD: Oh, really?

METZGER: There’s a letter here before— sometime in 61. [*inaudible*]

ARNOLD: That would— we surely must have realized by then that that would be a far superior way to do the experiment. Whether we could get NASA to realize it was another matter.

METZGER: And I don't have anything really specific to Ranger 3 in my letter file that sort of stands out. I do have a letter that I wrote to Van Allen at the beginning of 64 after we had our data from 3, 4, and 5 and saw the contrast in the three five profile asking him what he could explain this and account for.

ARNOLD: He would have been the authority, alright. Still around.

METZGER: Yep. And then beginning, I think, though, before the launch of Ranger 5, we get into a lot of work involving planning: discussions with you, Jim, and then design development discussions with you, Ernie— and Marv and Bob— on redesigns of a system for what was called Block 4.

ARNOLD: Right.

METZGER: I hadn't appreciated how much work we had done on that, because it unfortunately didn't develop into anything.

ARNOLD: Okay, what was the date of Ranger 5? Do you have that handy? I don't remember, but I think these things were four or five months apart, is that—

METZGER: Yes, yes.

ARNOLD: - or something of that sort. In those days, everything was much simpler. It was hard enough and an extraordinary achievement on Apollo to have those missions going on centers [?], something of that sort. In fact, we kept trying to slow it up so that we could be ready. But in the early days, they just were fired. Okay, now Ranger 5.

METZGER: Ah, yes, Ranger 5 went in October of 62, I believe—

ARNOLD: Okay.

METZGER: —because I have copies of a couple of cables from you regarding the results of Ranger 5—

ARNOLD: I was in India at that time. Yes, yes, that's right.

METZGER: —including one with the classic punctuation of "Stop." The "stops" every so often. [*Laughs*]

ARNOLD: Oh, yes. The old-fashioned, yes.

METZGER: The old-fashioned way. And then a comment from you on the failure of Ranger 5. And then there's some correspondence— you might remember and say something about this— where you, in conjunction with other investigators like Frank Press, were taking a dim view of the commitment of future launches to 300 pounds of photographic equipment.

ARNOLD: Yes. Well, you see, here we were. We had these three flights and we had a result, as Al says and a publication— Nature— that recorded that. But so far as the moon is concerned, we had not advanced the information one whit. The seismometer did not record anything. And the cameras saw nothing, as well. Now, of course, that situation couldn't be allowed to continue. And what you're reminding us was called Block 4 began to take shape and the rumors soon became certainty that the only thing that was going to be on Block 4— there been a reorganization and some heads had fallen and so on, all of which perfectly human and understandable. But it was rather disturbing and called for this sort of protest letter that Al describes. A protest letter that, perhaps, I even knew at the time would be ineffectual, but just making sure that people knew how unhappy we were. That in fact, the later Rangers were to carry a television camera only— a one-instrument spacecraft. I was very much reminded, being a constant reader of the comic strip Pogo at that time, of a remark that occurs there: "When you starve with the tiger, the tiger starves last."

And that was our situation. We were constrained by the fact that, of course, the publicly perceived presence of any successful flight with close up images of the moon, the images that would count would be the cameras, because anybody can understand looking at a picture. Whereas what we wanted to do— and of course, what they interpret as the seismometer, as well— the results would be enshrined in dry scientific papers talking about things that the general public wouldn't understand. And, okay, that's the price you pay for doing this sort of thing. So, we did protest and we did do this and that, and we tried to get others— I'm sure I recruited Harold Urey to write a letter and so on. But the decision had been made. And so, we sat down to nurse our wounds and to write up this paper on the space results that Al has referred to, earlier. I think in retrospect, it's the— okay, do you have? Al is giving me a letter I wrote to Homer Newell whom I knew very well by that time, who was an associate director, and words like this: "The decision to commit the immense weight of 300 pounds to a single experiment for the fifth, sixth and seventh time, appears to me to be quite wrong… At Tuesday's meeting, six experimenters made a presentation. Each of their proposals was inconsistent to some degree with the ground rules laid down. This illustrates the difficulty in putting in instruments as an afterthought after the mission…"

Well, okay for the record. And as I say, no useful response. Newell was a good person and the upset has healed since then— clearly healed long before. But we did not expect the great difficulty that resulted in getting anything like this done again before Apollo. And indeed, when Apollo began, it started— I think I can anticipate this far— it started much in the same way. That is to say the purpose of the experiment— the purpose of the Apollo mission, which Mr. Kennedy had stated in his famous speech— was to get the astronauts to— or at least an astronaut or astronauts— to the surface of the moon, and bring them home again safely. And in the early days, the idea of putting instruments on the Orbiter, which by now we had realized was the place we really wanted them, had no reception at all. On the other hand, fortunately, that program was large enough and long lasting enough that once that main result had been achieved, NASA was then receptive to doing things very fast that had to be done very fast to help with science. And the science of Apollo, both the return of the samples and the flying of our instrument and other instruments on the later Apollo missions, are a very warm memory which will take up in our next.

We all had our experience to show for it when we were finished, and that made us much better able to tackle a second round. And indeed, so far as Al and I are concerned, at least, to continue to participate in later rounds and write many proposals to participate more and on the basis of that experience to do a much better job. I think that is perhaps the lasting positive residue of the Ranger series as far as I'm concerned. I think all of us emerged with a— we were no longer innocents; we were professionals. And we had had an experience of blood, sweat and tears from which, ultimately, as in Mr. Churchill's example, we were able to emerge, to a considerable degree, successfully. At least those are the lessons I draw. Gentlemen, any other last words?

METZGER: Well, a minor amendment to something you said. I think 6, 7, 8, and 9 were Block 3, and that's the Block you were objecting to— the idea of deferring another chance at doing the more diverse science until after number 9.

ARNOLD: Okay, I was off one. That's correct.

METZGER: That's when Block 4 was to take place. We were officially invited and accepted on Block 4, and financially encouraged to continue. But, as so often happens in these cases, you get too far down the line, they shut the door before you get admitted. There never was a Block 4 and we had to wait several years before the next opportunity, which will be the next installment.

ARNOLD: Right. That's completely correct, and that made it all the more disturbing and again, not at all a unique event when we thought, "Okay, we've got to wait it out." And it turned out that the wait meant never, as far as the Ranger program was concerned. Maybe I will throw in the fact that— one more bit before we get to Apollo. A series, and one of the best series of lunar spacecraft between Ranger and Apollo, was the Lunar Orbiter series, which was conducted not by the Jet Propulsion Laboratory but by Langley. They mapped the lunar surface in considerable detail from orbit. They did five missions, all of which were successful. They were done at a low price. It was an exemplary series. I got to know Jim Martin, who was the manager of that program at that time. Another person who was very much— that did great things later. There was a spare; there was a Lunar Orbiter 6. And we went to bat and we put in a proposal saying, "Hey, all right, you've got all your five done. Why don't we put a gamma ray spectrometer and any other instruments that may be suitable on this Lunar Orbiter 6? We know it's a worthy operating system and we will promise you a very high yield for the money expended." Which I think was $8 million. That happened to arrive on a bad day on an associate administrator's desk. I think it was Oran Nicks who decided "Ah, $8 million— we've got more pressing needs," and drew a line through it. And the experiment was carried out on Lunar

Prospector a couple years ago, which gives you an idea. It cost us about thirty years to get a complete lunar coverage and all that sort of thing. That is another example of a near miss. These war stories begin to sound like the fisherman stories about the ones which got away. But that's the way history developed. Ernie, anything more? Al?

ANDERSON: No, I have nothing further to add.

METZGER: We didn't say anything about our submittal of the scientific results, and their being rejected initially. [*Laughs*]

ARNOLD: Oh, well, alright. Give that little footnote.

METZGER: We submitted our observations from the Ranger 3 experiment to the Physical Review, which properly rejected our submission, even though we were reporting on the discovery of an interstellar flux, and, in a sense, not the founding theoretical paper but I submit the founding experimental paper for the field of gamma ray astronomy, which is today a rather flourishing one. [*Laughs*]

ARNOLD: Well, one of the—

METZGER: So, we had it resubmitted elsewhere.

ARNOLD: Right. Well, this parallels the history of carbon-14, to go back to that one, where Bill Libby's first full paper [*inaudible*] the subject was rejected by Physical Review on the grounds that it was of insufficient general interest.

METZGER: Well, at least they're consistent. [*Laughs*]

ARNOLD: Yes. [*Laughs*] Moral is: publish your papers in some other journal. Okay, well thank you, all. Over and out.

[END OF INTERVIEW]