An Oral History of

JAMES ARNOLD, DEVENDRA LAL, and JULIAN SHEDLOVSKY

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ARNOLD: —I thought about how to proceed, which would be that I would open out with a sort of account of the pre-period, the period before we were all together in La Jolla. And then Julian could start asking questions, making comments, whatever you wish, and we would all—

SHEDLOVSKY: So, you want me to ask questions?

ARNOLD: Yeah. And make comments, both. It's a conversation. But it would be helpful if you would stimulate us a little, if you can. Knowing Lal and myself, I don't think it's really terribly difficult to get us to talk. So I think it should work fine. Okay. Well. If that's all right, then I'll just simply begin.

SHEDLOVSKY: We're on, so—

ARNOLD: I want to—this is Jim Arnold, at the start of another of our oral history sessions. I have with me Devendra Lal and Julian Shedlovsky, who participated to some degree in the events—or completely in the events that I'm going to describe, and we're going to deal with. I want to begin, as I said a moment ago, with a brief but I hope clear description of how the group of people came together and focused on the meteorite research, which preoccupied us for—in that period, in which I think we did some very interesting things.

The story begins back at Princeton University, which was the place that I was on the faculty before I came to UCSD. I had two graduate students at that time, at Princeton—Julian here was one of them, the other was John Merrill. Contemporaries, both very good. And what's important for our present purpose is that Julian was working on a problem having to do with the isotope manganese-53, a radioactive isotope which had been discovered shortly before, and which at that time was known to have a long half-life.

That was about all that was known, I think, when we started your thesis, wasn't it? During this period, a very important event took place, when Mastake Honda came to join us in my laboratory for what turned out to be a stay of something like six years in Princeton and La Jolla,

and totally transformed, increased the capability of our group to do some of the things that we will describe. Throughout his career, after that, he was one of the really remarkable laboratory nuclear chemists, and he had also many other talents, and transformed the capabilities of my group.

Now, Julian spent I think something like two years trying to measure manganese-53 in what are called deep sea spherules. Manganese—

SHEDLOVSKY: Nodules.

ARNOLD: Nodules, right, thank you. Which are oxides of manganese that deposit in the bottom of the sea. And it was a good idea, but it didn't work in the—with the capabilities of the time. It became clear that we were not able to measure this isotope with sufficient sensitivity in that source. I remember being quite embarrassed, because that's not a nice thing to happen to a student. So one of the constructive results of my embarrassment was that Julian and I sat down together, and had a discussion of about ten different topics, or some large number, which he might choose to get a degree in a reasonable length of time, and in doing something interesting.

It was a natural thing, I think, for him, to pick the topic of measuring manganese-53 in a meteorite. The argument being, if I recall correctly—and Julian please correct me if I don't—that he still wanted to discover manganese-53 in nature, and this looked to both him and me like a sure thing. It looked as if it was something you really could do.

And so this started us for the first time in my group looking at meteorites. Before that, we'd been looking at deep sea sediments and things like that. Am I stating it correctly, more or less?

SHEDLOVSKY: Well, I have a little different take on it.

ARNOLD: Please.

SHEDLOVSKY: When I joined you as a graduate student, you suggested the idea of looking for manganese-53 in manganese nodules, in the ocean bottom. And the problem that was being investigated at that time was there was an apparent discrepancy in the amount of tritium in the natural in the natural Earth's atmosphere—forgetting about bomb production—compared with estimated calculations of production rates from cosmic rays.

And there appeared to be an excess of tritium, naturally occurring, by perhaps an order of magnitude. And so, Jim Arnold proposed to me that a possible—If this tritium discrepancy is real, then one has to try to understand, where does it arise from? And one could rule out by some calculations that it could not result from bombardment of meteoritic dust coming to the Earth. The reason there being that the exposure times of meteoritic dust would be very, very long—perhaps ten to the ninth years. And that the ratio of tritium to helium-3 that would be produced should be a very small number.

And that there should be relatively a lot of helium-3 in the Earth's atmosphere. And that doesn't occur. So that was—rule that out. And a similar kind of argument could rule out bombardment of cosmic proportion elements—carbon, nitrogen, oxygen being very low compared to the amount in the Earth's atmosphere. And that the tritium hydrogen ratio should be much, much smaller than what is actually measured, something like ten to the minus 14th.

So, the idea was there is a possible accretion of manganese-53 from solar flare material, coming to the Earth. And manganese could be made, this isotope could be made in the solar corona by lower energy proton excitations on iron-56. And the advantage of—then that material could come to the Earth. The advantage of looking at an isotope like manganese-53 is it would not be formed in the Earth's atmosphere, because the heaviest isotope is argon, and this is larger mass.

There is iron in the solar corona, and there are low energy protons. The long half-life. So that manganese-53, if it could be an interesting tracer, it might help in understanding dating of ocean sediments, similar to what was being done with beryllium-10 by Dr. Honda and John Merrill. And so, I had tried for a couple of years, as you stated, Jim, to find manganese-53, and only got an ambiguous result.

And I think it was—I think I proposed that the place to look for it would be in iron meteorites.

ARNOLD: That's possible. That's the only real discrepancy between your comments and mine, though. You remind me of the origin of this question, which I had forgotten.

SHEDLOVSKY: Oh, and I wanted to say one other thing. That all this began around 1956, 1957, just so we put a time marker on it.

ARNOLD: Good. Thank you. We moved to La Jolla in the fall of '58. Now, back up a little bit with one more very important step toward what happened in La Jolla. I had been looking at the

isotopes of beryllium in nature. The radioactive isotopes, beryllium-10 you've already referred to, beryllium-7. And it turned out that quite unknown to me, in Bombay, Bernard Peters had decided to pursue the same problem. And somewhere along in that process, thanks to Professor [Subrahmanyan] Chandrasekhar, that fact was called to my attention.

And so I started a correspondence with Peters, and we shared our information about our progress, and so on. And it was very pleasant, to me, because he was a very brilliant scientist, to have that contact. Now, at the very end of our stay in Princeton, having built to the place where Julian was ready to go, looking for manganese-53 in iron meteorites, he went to the closest large collection of meteorites, about which we knew very little at the time, was in New York at the Natural History Museum in New York City.

And the curator was Brian Mason, someone we got to know very well indeed later. And Julian went to New York City, and met with Brian Mason. And came back with a piece of an iron meteorite called Williamstown. And a fist-sized piece, let's say approximately 100 grams. I don't know.

SHEDLOVSKY: 400 grams, about. Cut a slice.

ARNOLD: That's right, a slice. And here was a new problem for Honda and me and Julian. And we gathered around this piece. And we decided that we could not only measure manganese53, but perhaps some other isotopes. And we started attacking it, Julian, and Masa, and Dr. Honda, started attacking it with strong acids. Iron will dissolve in aqua regia, mixture of horrible acids, rather quickly. And that was done. And those were the first experiments, which we carried out with meteorites. Something in which we continued just about for the rest of my career.

Then we packed up and moved to La Jolla before that experiment was really complete.

LAL: So, all of you dissolved the entire Williamstown?

ARNOLD: Well, basically.

LAL: In Princeton?

ARNOLD: There may have been—I forgot whether we kept a spare piece or not, but. You're the best authority.

SHEDLOVSKY: Actually, we got three samples. And I think a very important thing—Jim suggested—Jim instructed me that we need to get as good counting precision as possible. So we need to have large samples. Another group at Carnegie Tech, under Truman Kohman's laboratory, had already measured some beryllium-10 and aluminum-26. But they were on small samples, and the counting statistics were quite low.

So, Jim said if I'm going to measure—try to measure these radio isotopes, be sure to get large samples, so to improve the counting statistics. So one of the samples we got was the Odessa meteorite, which we purchased from Ward Scientific in Rochester, New York, 5.5 kilograms. And that got everyone's attention when I started dissolving that. Because there were liters and liters of aqua regia and fumes and O2 fumes going up the fume hood.

LAL: That happened in La Jolla.

SHEDLOVSKY: That was in Princeton.

LAL: Was it? I had this impression that most of the solutions, green, blue, red, were all in La Jolla.

ARNOLD: Well, they were—later.

SHEDLOVSKY: The preliminary chemistry was done in Princeton. Then Jim made this interesting offer to me. He said I have good news and bad news for you, Julian. The bad news is, I'm leaving Princeton and going to do chemistry at the University of California in La Jolla. That's the bad news. The good news is, you can mail me samples, and we'll count them in La Jolla, and purify them again, radio-chemically, purify them, and send them back out for the counting, and then write your thesis.

The good news is, you can come to La Jolla with me, and make the measurements in La Jolla. So it took me about one minute to decide to go to La Jolla *[laughter]*.

LAL: Very good.

ARNOLD: So along come the trucks, the moving trucks. Packed up my laboratory. Of course, another moving truck packed up our family's possessions. And something in mid-October or so of 1958, we made the big trek, and you and your family made the trek, and we came to La Jolla,

and found the lab wasn't ready. Which is, by the way, the story of my life and everybody's life. I'm now in my mid-seventies and I've never seen a lab finished on time.

And I don't think I'll live to see one. And so here we were, Julian and Masa and I, and we had time on our hands, and we were joined very soon by a new arrival from India. There'd been some correspondence. Meanwhile I think Harmon Craig had gotten into the act in some way. But at any rate, arrangements had been made by that time. Lal had his degree. And arrangements had been made by that time for him to join us in La Jolla.

And Lal of course was a physicist. Peters was a physicist. Neither of them seems to have been afraid of chemistry. But perhaps they were—well, I withdraw the statement. I was starting to say perhaps they were a little less heroic than we were, a little less afraid of large-scale stuff. But I don't think that's true. I think they were probably just as brave. And Lal of course also came back. Peters was a cosmic ray physicist of international fame, and Lal came armed with all that expertise.

And also with a considerable amount of useful background and skill in building detectors. So there we were. And I think you arrived before the lab was finished. I don't remember for sure.

LAL: I arrived in November 1958.

ARNOLD: November, okay. So then the lab was still in the process.

LAL: Yes.

ARNOLD: I want to stop, before we go further, and remember one incident that must be burned in your memory, Julian, even more deeply than mine. And I think you probably know the one I mean. Here we were, every day the lab was going to reach some point tomorrow. This would be finished, this will be finished, and every day nothing happened. So. Julian was of course very impatient, and I can't say I was terribly patient myself.

The hoods were set up. And one day the hoods were running. And the hoods are the places you go where all the noxious gases can be sucked up.

SHEDLOVSKY: The fume hoods.

ARNOLD: Fume hoods, yes, that's right. And the fumes were—could be taken away. And there was electricity in the lab, so we could have hot plates and heat things to make chemical

reactions. And the only thing we really needed to have were drains, because we had all these very troublesome acids, and we had to clean them up and get rid of them. Julian, maybe you should tell the story at this point.

SHEDLOVSKY: Well, there was—there were large volumes of strong acid iron solutions that I wanted to get rid of. Because I was not measuring—not going to measure anything in the iron. Anyway, there were these large volumes of quite acidic solutions to dispose of. And so one evening, working in the lab, in the fume hood, there was a drain in the back of the fume hood, and I poured the solution down the drain and ran water. About an hour later, I discovered this iron chloride solution leeching out onto the floor from under the chemical bench.

And the problem was the drain in the fume hood had not been hooked up. So, I was—

ARNOLD: It's really a little worse than that, Julian, in my version. The drain was hooked up. But the workman came and removed it to do something *[laughter]*. And we didn't know that.

SHEDLOVSKY: In essence, I was pouring this solution on the floor, unbeknownst to myself. Behind the drawers of the laboratory bench. And many, many bundles of paper towels later, I had wiped up most of the material. But I was totally disgusted.

ARNOLD: I was going to say, I think that was a record never surpassed of disgust in a graduate student of mine. I've had many graduate students since, but—

SHEDLOVSKY: That's right. They had hooked the drain up, and then it had been disconnected.

ARNOLD: Yes, that was the real crusher. Because—well—

LAL: In the initial stages, you had a lot of frustrations, eh? The [*inaudible*] nodule, and then this acid, and—

SHEDLOVSKY: That's the life of a graduate student. That's what science is sometimes. You keep plugging away.

ARNOLD: Right. Well. Now, at any rate, in November, we are all together. And the measurements that Julian needed to make—we had a counting system that was transported, a shield, a low-level counting shield, a lot of—big hunk of iron that surrounded our apparatus and

kept the background low in our chamber. That was all set up and running. And now the actual measurements could begin. And Julian was proceeding with that.

And meanwhile, Lal and Honda and I were growing acquainted with each other. And because the laboratory had been slow in completion, and we still I think were working with very limited facilities, the three of us, while Julian was completing his thesis work, were starting to discuss what a program could be in this field. And it was just very exciting. We could see endless possibilities for studying the history of cosmic rays, finding out whether they had changed in intensity in the Earth over time, and also the history of the meteorites themselves, which was quite fascinating as we got to know more about this subject.

And so when things settled down more, and my memory of the chronology is vague, but at some point, Julian, you actually completed the key measurements that you had to make for your thesis. Could you give me some sort of date for that?

SHEDLOVSKY: Yes. Well, as you said, we got to La Jolla in October of '58. And just as a little sidelight, I was staying in a motel with my family. And the rent was nominal. It was enough that a graduate student could afford. But the proprietor or manager of the hotel told me on the first of June, of the following year, the rent would increase fivefold. And so that was June of '59. And so I had in the back of my mind, I had to complete all of the laboratory work by the first of June, within eight months.

ARNOLD: One might explain that that was because the summer season was the tourist season.

SHEDLOVSKY: Exactly.

ARNOLD: Right. Students couldn't pay much rent, but the tourists from Texas could pay rent.

SHEDLOVSKY: Exactly.

ARNOLD: When did you actually leave?

SHEDLOVSKY: I think it was right around the first of June, of '59.

ARNOLD: Then you were here for a lot of the excitement later. We realized we had no very strong theoretical structure for the work we were doing at that time. Lal had it potentially, but we'd never done the work of producing a model. But we knew that if we compared a short lived radio activity with a long lived radio activity, we could short-circuit a lot of that. And there's an

isotope manganese-54 with a one-year half-life, to compare with manganese53, by then we knew was a few million years, or one or two million years.

And so, we had a number of comparisons like that, beryllium-7, beryllium-10, chlorine-36, argon-36, and so on. And so, we mapped out an ambitious program, which depended only on one thing, that we didn't have—namely, a freshly fallen meteorite. Because you can't measure a one-year isotope in a meteorite that fell 100 years ago. Okay, then there came news of a meteorite that fell in the Soviet Union, as it was then, called Arus.

And there was lots of it. It was an iron meteorite. And that is the simplest kind of chemistry, so we were not prepared to look beyond irons yet. And I even knew by that time I was getting to know my way around a little bit, that the owner—to effect, the controller of this material, at a museum in Moscow, was a gentleman that I had never met, named E.L. Krenov. And I wrote him a letter, essentially pleading with him for a kilogram of this freshly fallen object, so for the reasons Julian already has given, we could get started on it.

Meanwhile, Lal was mapping out a program, Honda was mapping out the chemistry. How are we going to separate 14 isotopes of whatever it is, 10 elements or something in the kind, from a chunk of iron? And Lal was mapping out the detector problems. Because each isotope has different characteristics, and we again needed to maximize the signal. So Lal was sketching out detectors that were suited to each one, different kinds of detectors.

All right. I never got an answer from Krenov. I've never had a letter from Krenov. But at some point, maybe one of you can help me, or both of you, what the date was. But at some point, we got a notification from customs, in Los Angeles, that there was a package for us. I don't remember. None of us remember. Too bad. But it was in this period. And probably—

LAL: It came in about '59?

ARNOLD: Probably was '59, early '59.

LAL: I think so.

ARNOLD: And so, we urgently took the steps necessary to get the package, and, sure enough, there was a chunk of iron, a slab of iron, weighing 1.1 kilograms, if I remember. So he had sent us more than I had asked for. And boy, the campaign started in real earnest.

SHEDLOVSKY: Lal, what kind of detectors had to be utilized for making all of these different radiochemical measurements?

LAL: I think there is a little confusion here. Most of the detectors which we—new ones which were introduced, were during the Apollo time. And at this time we had the low-level beta counters. And –

ARNOLD: We had the x-ray counters.

LAL: The x-ray counters. And that's all.

ARNOLD: Yeah, but there were different beta counters for –

LAL: Right, right. Different beta counters. But I want to ask you, Jim, when you got the Arus sample, did you ever worry about whether the sample was from the outside or the inside? Because iron meteorites are so big. And some of these samples could be completely shielded from cosmic ray radiation.

ARNOLD: We knew a little bit about that. Because these early meteorites that Julian got, and that we bought from this company, Wards, included some which were from very large meteorites. And I remember particularly—again, maybe we shouldn't worry too much about the chronology, because it'll take us too long. But there was a meteorite, Toluca, which was our—kept us honest. Because after we had run some others, this was a very large iron meteorite. And there was nothing in it.

Because it was not so much it was old, there were no rare gases in it either, because this piece was buried so deep that the cosmic rays hardly, by our sensitivity at the time, could reach it. On the other hand, did we worry about it? I think the truth is not very much.

LAL: Just took a chance.

ARNOLD: Just took a chance. But besides that, again, of course we were a little innocent and lucky, I think the recovered mass—we can look now, but it may have been 100 kilograms or so, which is not too big—

LAL: Not too big, yes, yes.

ARNOLD: On the other hand, the recovered mass, 99 percent of it could have been somewhere else. So the question is a good one, but we did just go for it. And at that time, being able to collect a freshly fallen meteorite was a brand-new thing. I mean nobody had done that before. That was sheer luck on our part that the meteorite fell, and that Krenov was obliging. And so we did attack it. And, fortunately, the situation was pretty favorable.

And we ended up at the end of this campaign—again, I won't bother with the chronology—with a publication on the Arus meteorite, with, if memory serves, and I think it does, 14 different radioactive species, which had been measured, thanks to Honda's virtuosity, taking these things apart, and Lal putting together the right counters, and of course everybody pitched in, and we were all doing things to push it forward.

The critical moment for me was at a scientific meeting. I knew there were other people doing things like this. And, in particular, a very dear friend of mine, then and now, Tony Turkovich, had been—I had many interactions with his older brother John, who was at Princeton, a professor at Princeton who had helped me from the time I was a freshman until that moment when I came back to Princeton.

And Tony was his younger brother. Both brilliant scientists. And Tony and I shared an apartment in Chicago. And he had all the same ideas that we had. I mean he was just—thought of everything. And he had had a bit of luck that we didn't have. Because we were in touch. The luck was he had told one of his students about this project, and it would be nice to have a freshly fallen meteorite and all that. And the student went home for a weekend to his hometown, and he mentioned it to his parents.

And they said oh, a meteorite fell here last week. That was the meteorite Hamlet. And Tony had Hamlet, which was a stone meteorite, I must say, before we had Arus. And I was still in some awe of Tony Turkovich. So, I expected, in a friendly way—we were certainly friends—I expected real, real competition. And so I got to the meeting. And I made an extra copy of the manuscript that we were going to submit for him. And the table with the 14 things.

And I handed it to him, expecting to receive in exchange something quite comparable, or perhaps indeed better in some way. And to my astonishment, he started making apologies for the fact that they had not been able to get nearly as far. Tony himself was a brilliant laboratory chemist, and I'm sure could have done things, but probably not as well as Honda. And, in any

303 case, he had not managed to get that done. He did have a couple of small results to show. And
304 I thought, wow, we're the top.

305 **LAL:** Yes, absolutely. Arus's work was the first, which everybody will remember. And did Tony
306 come up with a paper on Hamlet soon afterwards?

307 **ARNOLD:** No. In fact, it was years afterwards.

308 **LAL:** Right, right. That's what I remember.

309 **ARNOLD:** And I think to be perfectly frank, he had many ideas, and many projects. After he saw
310 our paper, he didn't see any particular point in making it a major project. So he did a few other
311 things, and there is a paper in the literature, but it is much later, and not as extensive. So all of a
312 sudden, here we were, suddenly we had done something memorable. And I can remember
313 relating this to UCSD, that telling Roger Revelle about this. And he was just delighted. I mean
314 he was just so pleased, because, I guess, he thought, well, my bet on this guy seems to be
315 paying off. Something like that.

316 **SHEDLOVSKY:** Jim, do you want to say something about the kinds of chemical techniques
317 that Honda utilized to separate all these?

318 **ARNOLD:** That's a good idea. I think we can say something in general without getting into too
319 much detail. The key—Honda was a master of any of the laboratory techniques necessary. But
320 the one which at that time was certainly new to me, and which he was a past master of, was ion
321 exchange. There were two—there's a technique that had been developed a few years earlier.
322 Which I had never learned, because it was too new, and the Chicago experts that trained me
323 hadn't required it yet.

324 And sometimes anion resin, sometimes cation resins, I don't think in something of this sort I
325 should go into any particular detail. But the key element was the idea of a column. You would
326 take your solution, once it had been not a solution which was full of these violent acids you're
327 talking, but later, a purified solution, which was much gentler. And pour it into the top of a
328 column, a glass tube usually, or a plastic tube, with some sort of glass wool on the bottom to
329 hold the resin particles.

330 And then things would flow through. And if the ions were colored, you would see color bands on
331 the column. This is how it was developed originally. Say green for nickel, kind of other colors for

other elements. You'd watch them move through. Each element would be separated in that way. Some would move through faster, some would move through slower. This was one absolutely key technique, and Honda had already written a book in Japanese in not only the use of columns, but their manufactory in NATO's own resins. Typical of him. And so, he just used that very, very effectively.

LAL: And, Jim, if I remember, he had developed these techniques in connection with the fallout.

ARNOLD: That's also correct. But that wasn't the only thing. He used it for a number of things, but fallout was one of them. And I even have the impression, which I think he's verified from me, that in an earlier year, when he and I didn't know each other, and I was writing articles about fallout, for the Bulletin of the Atomic Sciences, and all this was classified secret in the United States—I had written to Japanese scientists to get their information. Because the Japanese were not bound by this secrecy.

And he was one of the people that I didn't write. I didn't know him. But one of the people I did write to, asked him, and sent me—he sent some paper. I don't think I ever located the paper afterwards, but there was this former tie. So you're quite right.

LAL: And is that how you came in contact with Honda?

ARNOLD: No. Let me say that a little more, how I did. Honda had decided he was stuck in a rut in La Jolla, in Tokyo. He was an assistant professor. He'd been an assistant professor for some time. And assistant professors in Tokyo those days were like postdocs in the United States. That is to say, the postdoc expects to be working for the professor, and he expects to be working on a project the professor has thought of and wants to do.

An assistant professor is supposed to be somebody proving he can do it on his own in the United States. But Honda was a Tokyo assistant professor. And he decided to go to somewhere else for some study and so on, and to get into the field and try to break out of that mold a little bit. And he chose very sensibly the Physikalisches Institute in Bern, which had been founded by another famous scientist, Fritz Houtermans. And he went there as—to work with Houtermans and Geiss and the other people that we later knew well.

But as you remember, Lal, Houtermans was not a person who appreciated chemists. He thought that chemists were not good for very much. So he had one of the best chemists in the

world, and he was not paying any attention to him. So that's how this whole thing happened, was that Honda, who had heard of Tony Turkovich, but had not yet heard of me, wrote to Tony, at Chicago, and tried to get an appointment. And I was leaving Chicago for Princeton at that time.

And Tony came to me and said here's this interesting letter, and I don't have any money right now, do you? So all I had to do was read his list of publications and see that he'd published papers on beryllium. I went a-ha. I was in the middle of the beryllium isotopes at that time. So I made him an offer, and he came. And that was how it began. And that was—he went, did a lot of things. He developed this Honda column idea for –

LAL: He came to Princeton in '57?

ARNOLD: I think may have been late '56.

SHEDLOVSKY: '56 I think.

LAL: '56?

ARNOLD: Yes.

SHEDLOVSKY: I think so. Lal, you mentioned Honda using ion exchange columns in fallout. Let me tell you a pointed story he told me one night when we were working in the laboratory. He was—he wanted to use some radiochemical tracers to measure ion exchange properties. How strongly different chemicals are absorbed onto cation or anion exchange columns. And as a result of WWII, Japan was prohibited from having radiochemical material.

ARNOLD: They destroyed a cyclotron.

SHEDLOVSKY: Cyclotron was destroyed. So, Honda went to Hiroshima, dug up material, purified—

ARNOLD: That sounds like—

SHEDLOVSKY: —strontium-90, cesium-137, and other—

LAL: I had not heard of this story. *[Crosstalk]*

ARNOLD: I had not heard of it either.

SHEDLOVSKY: —and then used that to calibrate his different exchange resins.

ARNOLD: Wow.

LAL: Goodness, this is fabulous.

SHEDLOVSKY: That was typical Masa.

ARNOLD: Wasn't it?

LAL: You know, a similar thing, when we wanted beryllium 7, we used to clean it off the floor, and use it as beryllium 7 for calibrating the sodium iodide crystal.

ARNOLD: Naturally.

LAL: Just natural beryllium 7. This is—

ARNOLD: It was rather easy to measure, so—

LAL: He went to Hiroshima, and this is super.

ARNOLD: That's a great story.

SHEDLOVSKY: Thanks.

ARNOLD: So, okay. And these—the other techniques that were used, first of all the dissolution we've already described. Which worked for—comparatively easily for iron meteorites. The stone meteorites later, which had a much more complex chemistry, we had to use more complex techniques. But hydrofluoric acid in particular, is the key there. But at any rate, it was still the first stages were violent. And I think I'll want to come back when we get to Bruderheim, and talk a little more about that example.

The later stages were specific to the individual element. An element like manganese, for example, is an element that has a good many different valence states, a good many different compounds, which say get manganese with two chlorine atoms, with three chlorine atoms, and so on. Five. Permanganate ion, you get several different kinds of manganese chemistry with different colors, and all the rest.

And so the purification for elements like manganese and chromium and iron, which have different valence states, often involves changing the valence state let's say up, and then again down, or from anion to cation, so that other elements behave differently, and you can clean out the other elements. One of the things we have to remind ourselves of in this connection, which is brought out by this wonderful story about Masa, is that fallout was everywhere in these times.

And so we had to be particularly careful in our purifications not to introduce some spurious radioactivity. It really wasn't until the treaties in the early 1960s banning nuclear tests above ground that life began slowly to get easier. Again, it's a tribute to Honda. As far as I know, not a single one of those measurements was seriously wrong, was seriously impacted by—

SHEDLOVSKY: Lal, you could tell us how long the materials, radioactive fallout would stay in the stratosphere and mix down? Wasn't some of that work done in India?

LAL: Yes, little bit. But most of the work was of course done in the United States on the fallout of strontium and other things. About one year it stay, a mean residence time of strontium, if it is put anywhere in the stratosphere, it comes down in one year. Even if it is put in the equatorial stratosphere.

ARNOLD: Well, one over E comes down, so yeah. It's still—since there was a lot of it, even—

SHEDLOVSKY: So, it still was a number of years after testing had stopped that that material would still be present.

ARNOLD: Yeah, that's true.

LAL: But this is one thing that I had forgotten. This was one big concern. Because everything was low level counting. And so, one could easily get contaminated, not only by potassium and other metal, uranium and other things, but the fallout. I now remember, this was a big concern during those days.

SHEDLOVSKY: Just for clarification, low level counting really means high sensitivity.

LAL: Right.

ARNOLD: Counting very low levels—so this was a name invented by Bill Libby. Not perhaps the ideal name, but it means you're counting—the way I always liked to put it in the early years is the samples are less radioactive than we are. Because we have potassium in our bodies, and

we have carbon-14 in our bodies, and we—when I'd show visiting dignitaries around, I would always make the point that this heavy iron shield was not to keep the radioactivity in, but to keep our radioactivity out.

LAL: Actually, this shield which you have brought, it has some interesting history, doesn't it? Is this from a gun battle, or a—

ARNOLD: I'll tell you that story. My friend, Tom Sugihara [?], who was a student of Libby's, one of the many students of Libby's that I became friends with, had moved to Clark University in Massachusetts. And he had gotten from the NSF in those days money for his research. No, no. It was one of the military agencies. And that's part of the story. Then he said to the—whichever military agency. There were several. He said how about getting us a cannon for a radioactive shield? And they did.

LAL: Oh, it was a piece of a cannon?

ARNOLD: Yes. And when I made the measurements, discovery measurements on beryllium-10, in the deep-sea sediments, I didn't have a counting system of my own at that time. And I borrowed his. And one weekend, I went up there. And made the measurements in his system. So, when we got going with Julian's project, I went to the office of ordinance research in North Carolina, which was the first group, very small group that I'm very grateful to—gave me my first research grants on my own—and I said how about treating me the same way?

How about ordinance research, ordinance, cannon? Get me a piece of a cannon. And they reported back that essentially all the surplus cannons had been disposed of, and I guess the rest had been melted for scrap, or they were souvenirs for somebody. They failed. So. What to do? And what I did was call around to see who could possibly do this. And I had the good luck to call some receptive person in the Bethlehem Steel Company, who was the nearest steel mill to Princeton, which is where I was.

And he agreed to fabricate—he agreed that Bethlehem Steel would fabricate it, and he was even nice enough to contribute it. It didn't cost us anything. So sure enough, that old shield, which still exists somewhere, I think, up on the hill up here in Mount Soledad, still there, because Bob Finkel, much later, used that shield.

That shield with sort of cork ends made of iron—I'm using my hands, which won't show on the tape, but perhaps if you picture a plug, something like a foot in diameter, and a foot long, probably a little less than a foot in diameter.

[END OF PART ONE, BEGIN PART TWO]

ARNOLD: That was part of the canon, yes.

LAL: Yeah, sorry.

SHEDLOVSKY: Can I take you back to Princeton for a moment? You had a contract that was providing some of the resources for conducting this experiment, these radiochemical measurements, the beryllium-10 measurements. And that was funded by the Air Force Cambridge Research Lab?

ARNOLD: No, it was funded by the Office of Ordinance Research. I ultimately had some money from the Air Force Cambridge. You know Ed Martell. Another student of Libby was there at that time. Later became—went to Inkar, where you were for years. And he was helpful in that way. But originally, the original support came from this small organization. I don't think they had more than $30 million or $40 million to distribute around the country. And I used it for this purpose, and also to develop the liquid scintillation system further, which I had done it at Chicago, with university funds, originally.

But then that was—later, of course, we shifted to the NSF. And in those glorious days, Julian, I may say for the tape, later became an official of the National Science Foundation. So, he saw it from the inside for many years. But in those glorious days, if you had a reasonable sounding proposal, you got funded. And so—

LAL: It was quite easy.

ARNOLD: Quite easy. And so, when—in this Arnold-Honda-Lal period I'm talking about, funding just wasn't a problem. We told them what we needed. We tried to be careful. We didn't want to waste money. I've always been like that. But the problem of research funds was not a problem. Basically.

LAL: There was one question I had. I remember you had difficulty in publishing your beryllium-7 paper.

ARNOLD: Yes.

LAL: Got to some classification problems.

ARNOLD: Yes.

LAL: Then these long-lived nucleoids, you never had any problems.

ARNOLD: No.

LAL: So, the military, Air Force, military, all these people, they are very smart. They know that long lived nucleoids, are not useful, that's fine.

ARNOLD: Well, they were certainly told by me and by anybody else they were consulted. No, I had no further classification problems at all with our laboratory work. We had sometimes referee problems or things like that. That's the nature of the system. But on the whole, the work was very well received. Maybe—Julian, or Lal, you may have more other topics. I was thinking of moving on, perhaps, to the stone meteorites.

SHEDLOVSKY: Could I—

LAL: Yeah, he had a question.

ARNOLD: Oh, I have one other topic.

SHEDLOVSKY: I just wanted to give an amusing incident, with the AFCRL contract that you had. And one day, Martell, who was the contract monitor, came down with somebody from the business office. And the person from the—this is Princeton. And Jim and Martell were talking. And the businessperson wanted to—had labels to put on all the capital equipment. And one of the items was a platinum dish of—

ARNOLD: Correction. That was in La Jolla.

SHEDLOVSKY: No.

ARNOLD: Yes, sir. We're about to get into a fight.

LAL: Good, good.

ARNOLD: Yes, it was in La Jolla. I can picture it to this day in that lab.

SHEDLOVSKY: Anyway.

LAL: What is the story?

ARNOLD: Go ahead.

SHEDLOVSKY: Platinum dish is a very valuable piece of equipment, especially for stone meteorites.

ARNOLD: Yeah, of course, it's very proper to be accounting carefully for platinum dishes.

SHEDLOVSKY: But this was a dish of approximately 500 grams, or a kilogram. So –

ARNOLD: Yeah, I think it was—

SHEDLOVSKY: -a significant amount of platinum. And the monitor, this businessperson, wanted to put a label, the numbers, that this property belongs to Air Force Cambridge Research Lab.

LAL: I can see where the problem is.

SHEDLOVSKY: And Masa and I said to him, it's crazy to put this label on the platinum dish, because we heat it to 800 degrees Centigrade. It's going to burn off. It's going to destroy the platinum where it's adhered. So, he was perplexed. So, I suggested that he just put the label on the cardboard box. And he thought that was wonderful. Which he did.

LAL: Excellent.

ARNOLD: You just have things like that. There's one more, before we move onto iron meteorites, there's one more really—

SHEDLOVSKY: Stone meteorites.

ARNOLD: Stone meteorites. Before we move on to stone meteorites, one incident that really needs to be recounted. There were other people getting into this trade. And one of them, who did very well in the years that he lived, was Oliver Schaeffer, whom I had known first at Harvard.

We were both postdocs together there. And then in George Kostiakowski's lab, and then later he was at Stony Brook. Or no, he was at –

SHEDLOVSKY: Brookhaven.

ARNOLD: Brookhaven. You're right. He was at Brookhaven. And he had a student named Esther Sprenkel. And he was starting her on a thesis which was quite parallel to yours. And so, they were in fact—because they, too, must have gone to Brian Mason. But at any rate, they had a piece of Williamstown, among several meteorites that they looked at. And they measured four or five radio isotopes in that piece. And they got results, which for all but one, agreed with ours. Now, in this case, we had a problem. Because we had not measured the isotope chlorine-36.

LAL: That's what I was thinking.

ARNOLD: Chlorine-36 has a half-life of 300,000 years. And it was the shortest lived on the long-lived isotopes. And we couldn't—this was our first experiment. We hadn't gotten smart yet. And so, the horrible brew of acids that we dissolved the Williamstown meteorite in, was aqua regia, which has lots of HCL, chlorine, acid, hydrochloric acid in it. So, we couldn't measure chlorine-36, because we had drowned it in this mass of HCL.

Then we got smarter after that, and you mentioned Odessa. And a couple of others that we—I don't remember whether we did Canyon Diablo at that time, but there were several. And we learned, particularly in Arus, and that there were—that the amount of chlorine-36 for a small meteorite—and, again, I'm using my hands, which isn't very helpful for the tape. But a meteorite the size of somebody's head or the size of a cardboard bigger box, maybe packing case, would have about 15 to 20 disintegrations per minute, per kilogram, which was the unit we used, because we were measuring kilogram samples at that time.

That was the range. And Esther Sprenkel had reported five. So, when we wrote up the paper, with all the four names on it, which summarized our iron work, we made some remarks to the effect that quite possibly there was some error in Sprenkel's measurements, because all the others were in this line. And Oliver called me up, and was a little miffed at me, with good reason, as it turned out. He said we hadn't measured it, how are we sure that it was wrong? Which was a fair question.

And he said if I get you another piece of Williamstown, will you measure it again? There was only one possible answer for that. So, I said yes, and we did, and we got five, or six. And we added a note in proof to that paper. The proofs came back. And we're sitting around saying why these others are 15, and this one is five. And I remembered two things at that point. One is that I had consulted a number of metallurgists about how long an iron meteorite would last on the surface of—in a temperate zone, like in the US. And the answer was, oh, 1,000 years or so. It'll just dissolve or disappear or something.

Anyway, they assured me that it couldn't survive for a very long time. The other was something that Harrison Brown had told me—Harrison Brown is a very brilliant scientist, who is mostly forgotten these days, but he had a lot of good ideas. And Harrison Brown had noticed that the collections in the museums contained lots of iron meteorites from Kentucky. But no iron meteorites, or very few, from Ohio and Illinois.

South of the Ohio River, they were plentiful, and north of the Ohio River, they were not. And he speculated that the reason must be that the ice sheets came down and ground everything up and disturbed everything. And of course, they made the Ohio River Valley. And then on the south side, there was no ice sheet, and so these things just lasted. And when that memory came back to me, it was the a-ha. That meteorite has been down there for more than a half-life of carbon-14, of chlorine-36, and we calculated about 500,000 years.

And that was one of our major discoveries, and it wasn't—it was absolutely driven, we were driven to make that discovery by Oliver Schaeffer. And I think more than once apologized to Oliver for my slur on him and Sprenkel. And for not, so to say, not giving him a chance to discover this for himself, by looking at other meteorites.

LAL: So that became the first determination of terrestrial—?

ARNOLD: This is the first determination of a terrestrial age of a meteorite, and, as I say, it was—we had to be dragged to do it. We didn't—it's not just me. Lal, I'm involving you and Honda as well. We were all unable to think that thought.

SHEDLOVSKY: Terrestrial age where age is long compared to the Hamlet that fell last week.

ARNOLD: Oh, yeah. Or the Arus that we had. And by now, I don't want to get into all the later history, but by now, there are meteorites that have been collected, which have terrestrial ages laying on the surface of the Earth for 5 million years or more.

LAL: Before you go to the stone meteorites, may I add something here?

ARNOLD: Please.

LAL: Because at the same time, when all this work on the chemistry of iron meteorites and cosmic radiation was going on, we were also thinking of what was happening on the Earth. And Honda was extremely active, and working on meteorites, as well as all the terrestrial problems. And we once went to Berkeley to make recession measurements. And all this happened between just in '59, including the year of '59.

ARNOLD: Well, you left when?

LAL: I left in June or May, '60. In the year of '59—

ARNOLD: —[*inaudible*] many things together.

LAL: Right. We did many things together with Honda and you. And that included going to Berkeley, measuring cross-section for tritium, beryllium-10, and all that. And also, making direct exposures of water, and argon, at Aqua Lake and Mount Evans.

ARNOLD: Let's fill in on that a little bit. The idea was to go to high altitudes, where the cosmic rays are more intense, because they haven't been absorbed, and to compare—to make measurements there, and also make some measurements at sea level, so as to have a comparison. And you and Honda—I was never as adventurous for field trips as you guys. And in fact, this is a tradition in my laboratory that's lasted throughout my career. Everybody always loved field trips. Except me.

I always wanted to—I was lacking in imagination, so I always wanted to stay in the lab and get lots of new results. But I of course had to relent on a number of occasions. And yes, it is important that that work was done. That also eventually fitted into what became the Arnold-Honda-Lal paper, as we put everything that we knew together, including the absorption in the atmosphere. You were—

LAL: And then you see the other thing which happened was that we looked at the marine sponge.

ARNOLD: Yes.

LAL: And discovered silicon-32 in that.

ARNOLD: Yes, that was your baby.

LAL: Right, but see, what happened was then [David Regier] Schink joined you. And he did his PhD with you on doing silicon-32 in the ocean water. And he developed a very ingenious water sampler, which he used to send samples of 60 tons of water and bring it to the surface. And then with the hose, get the water to the shipboard. And then used large columns, again, Honda's influence. So, there are many things which are going on, and all happened around '59, in the same period.

ARNOLD: Well, Schink's thesis was completed in '64, so there's a little more stretch to that.

SHEDLOVSKY: Just for clarification, Jim. The measurement was not measuring cosmic rays directly at the high altitudes, but measuring the cosmic ray product—

ARNOLD: Yes, that's right, indeed. And that was what we wanted, because—

SHEDLOVSKY: What kind of targets did you use?

ARNOLD: Well, water, as he says.

LAL: And argon.

ARNOLD: And argon. But the—water for the beryllium isotopes.

LAL: Then later on, you invited Ron to –

ARNOLD: Ron—

LAL: Then he exposed hydrochloric acid, and salinity [crosstalk], mercury, also some things he did.

ARNOLD: That's right.

SHEDLOVSKY: But back to stone meteorites.

ARNOLD: Okay. So. Now we had this success under our belt, and we continued to make more iron meteorite measurements, as opportunities arose. We got Sikhote-Alin meteorite from the Soviet Union, again. And we began to do many things. We should perhaps take more detail on that, get out of here. But the idea of a stone meteorite was very attractive. Because with many other elements, we could get more isotopes, and we could get higher production of isotopes, like, say, aluminum-26, which is not made very much from iron, but which is made enormously effectively from silicon and aluminum.

So, we wanted to extend our work. And of course, meteorites did fall from time to time. And we were ready. But the chemistry was much more complex. And the first meteorite to come through fell in Canada. By now, we're not—as I put it virgins anymore. We know our way around in the field. And Ian Halliday, at Winnipeg, was known to me, and he reported this fall in Western Canada. And so, we asked him for a kilogram, or some similarly large amount.

LAL: You did not say the name of the meteorite, you only said Canada.

ARNOLD: The name of the meteorite was Breuterheim. And meteorites are named, for the record, after the nearest post office, officially, but they have odd names sometimes. Apparently, there was some place named Breuterheim near where it fell. And he sent us a large sample. And Masa, again, by now, was ready to tackle that. And there was a particular adventure, which I want to relate. Lal, you were not present, and Julian, you were not present. So, I'm now the sole witness present.

The thought occurred to us that we had this nice big sample. And we could not only measure the radioactive elements, which were in solid form, but that for instance argon has interesting radioactive species. And so, it might be nice, if possible, to measure gasses as well. Okay. So, I went to Harmon Craig, who had a big vacuum system in his lab, doing various things. And discussed with him the idea of dissolving this meteorite in his vacuum system. Oh, I'm getting it wrong.

It wasn't this—spoiling a good story, it's a shame. This was an iron meteorite, actually. And I'm having some trouble. It might have been Sikhote-Alin, I don't know which one it was. No, it was a short-lived iron. And it's not the same period. So, let's cancel that one out, and go on. Well,

no. It's a Honda story. I just changed my mind. I'll tell it anyway. The story is this. We have an iron meteorite in this glass flask attached to the vacuum system. Everything is closed.

And we have a way of introducing the acid, which is by now only nitric acid, into the bulb, which contains the iron. And it's working. And we're all standing around, watching, as Masa is at the controls, of course. And I had a technician at that time, she really was a very good technician, despite this blooper that she did. And we ran out of nitric acid, and I sent her back for some more. To my lab. And she came back with a bottle of—plastic bottle, with some acid in it all right. It was an odd thing. It was kind of hot.

And it turned out that in her haste and excitement, she had put sulfuric acid into that bottle instead of nitric acid. So, when the reaction changed character, and we could see that something was wrong, the discussion occurred, and quite soon, she realized her mistake, and she—

LAL: Brown fumes would have come out.

ARNOLD: She of course was devastated. I don't remember what symptom, but these acids behave differently. We're chemists, and we saw—

LAL: Sulfuric [*inaudible*] releases nitrous—

ARNOLD: Right. And so now oh my god, you know, this is a wonderful way to begin making an analysis. You've introduced something into Honda's scheme, which is going to propagate everywhere through. Terrifying. And there was sort of a dead silence. Masa closed his eyes, and none of us dared say anything. We knew he was concentrating, so we all just kept quiet. And he opened his eyes, and five minutes later, I believe, maybe shorter, but it seemed like an hour, and he said, it's all right.

He had redone the damn scheme. And he just carried on. And there were no problems. Let's go on now, to iron. I think it's quarter after 6:00. I'm watching the clock a little. Breuterheim was a very exciting meteorite, again. It showed us quite a few new things. Some new isotopes. Some, as I mentioned, the example of aluminum-26, sodium-22, a number that were much more easily measured in Breuterheim. And so, we had another set of white long table of isotopes now from several iron meteorites we had, and from a stone meteorite.

And the triumph, again, was Masa's, to carry that out successfully on the first try. That was always characteristic of him. He could use a model, he could try his chemistry on a piece of rock somewhere, or maybe on an old meteorite that we had bought. But every one of these things, including the lunar samples, worked the first time. Just without any problems. Then, the question was, all right, we have all these exciting results. What are you going to do with them? Are you finished? You going to let other people write the papers which explain what it means, and how much the cosmic rays have changed, and haven't changed?

That's no fun. Meanwhile, Lal is back at the Tata Institute of Fundamental Research, in Bombay, and he's now a young faculty member. I may mention that Honda, when he did go back in '64 or whatever it was, went back as a full professor to the University of Tokyo, so that I felt that we had done some good there. And Lal, I may say, also moved up quite rapidly. At any rate, we agreed, by correspondence, that we would write a paper. And so, we started writing a paper. I think Lal probably made the original outline. I can't remember the details. And they don't matter.

But at any rate, we started producing manuscripts. We were doing a lot of calculations in La Jolla, on depth effects and things of that sort. We had equations that Lal had given us. And we were going back and forth. And the problem—these were—it's amazing in this time of email and that sort of thing, to think it took two weeks for a letter to go from one place to the other and back again. San Diego to Bombay and back again was on average two weeks. And the manuscript, we completed a manuscript. In fact, we completed several manuscripts.

But we never completed the manuscript. And you can defend yourself, Lal, but my particular claim is the reason we never completed the manuscript was that Lal always had another idea every two weeks. And so, he would think it'd just be much improved if we do this. And you want to say anything? *[laughs]* No?

LAL: No comments.

ARNOLD: No comment. And well, we might have contributed to it a little ourselves, but he was far away, it was convenient for us to blame him. Meanwhile, a much nicer thing had happened. He had invited me to come to Bombay on sabbatical, which I later did, with my family. Small children. And so, I had another motive in going there. If you look at India from the United States, and you've never been in India, you can conjure up horrors about disease and sanitation and all that sort of thing. And so, I wanted to check that out.

So, I went to India. Bombay. Stayed with Lal and his wife, Aruna. Wonderful wife, Aruna. And we had a great time together. And we finished the paper. And then I used all my—which I say any remaining authority I had over this gentleman, to say no changes, this goes to the journal, and of course, we polished it a little, I'm sure. Masa must have corrected a few things. He was a full participant in all these discussions. It was not—I remember Lal coming to me once and saying what kind of a person is this? He not only does all these things in the lab, but he understands and participates in all these things.

LAL: Absolutely, yes.

ARNOLD: He had one idea, I should mention. There's one paper which has only his name on it. In which the idea was that there's a natural radioactive isotope of potassium, potassium in our bodies has radioactive potassium-40, with a very long half-life, 700 million years. And the—what Honda had figured out was that an iron meteorite, iron metal is very incompatible with potassium. And so iron meteorites have really tiny traces of potassium in them. And the potassium made by cosmic rays from the iron is actually most of it.

So, we did a very simple character separation of potassium, from an iron sample. And showed that the total potassium present was very small, but the radioactive potassium was detectable. And published that. Now, he was outdone by a man famous for just this one thing. Bosaga and Mainz managed to do it mass-spectrometrically, which was a much more precise technique.

LAL: But much later.

ARNOLD: But later. And so, Honda was the first person to show that potassium -0 was made by cosmic rays in the iron meteorites, and neither Lal or I had anything to do with that besides patting him on the back. Okay. So. The paper—the Arnold-Honda-Lal paper was published quite soon thereafter. And it was the only paper that I had published in my life, had my name on it, which had had initials, you know. We were here in La Jolla, and the most famous paper of that kind that I can think of called B-squared-F-H. And Jeff and Margaret Burbidge, Willy Fowler and Fred Hoyle, were the authors.

AHL was referred to by a number of other people as AHL. And has I think held up one embarrassing mistake—one of my calculations I made a slip in writing down the formula we used for the depth effect, which John Reynolds caught later. I was embarrassed about it. But other than that, one embarrassment, that paper also has—has weathered well. And Lal had put

a number of ideas together for it. And in particular, this idea of using nuclear emulsions, basically photographic emulsion, in which you can record the passage of cosmic rays, the work of the Bristol Group in England, Powell and Company, and many other sources of information, to make this synthesis possible.

So as far as I'm concerned, we've roamed ahead a good deal. But nonetheless, to me, that is the end of this what shall I say, pioneering period for the history of the cosmic rays and the history of the meteorites. Many things have happened after that. We have already a tape which Lal participated in, which, for the Apollo period, where we picked up the same tools, and found many things, too. Don't need to repeat any of that.

LAL: And that period also became famous. I was just talking to Doug, and I told him Shedlovsky is coming. So, he's the S in the Shrelldalff. So that's a very famous period, also. People know about this.

ARNOLD: Well, I might at least explain that on the tape, for any—when we—we had a much larger group for Apollo. We had, again, Lal and Honda, Shedlovsky and myself. These four people were the senior people. And then I counted them last night. Five graduate students and two postdocs. So, there was a group of 11 people. And at that time, it's not so novel now, the idea of expecting anybody to remember 11 names was preposterous. And so, but 11 of us all worked hard, and of course, a couple of others who weren't named. So, I got the idea somewhere of making an acronym.

NASA is full of acronyms. And we had 11. Took the 11 first letters, and there were only two consonants. There was my name and John Evans' name. And there were two vowels and nine consonants. And we rearranged them in various ways. But it was Tony Delaney who was much the most gifted of us. There's always somebody who's good at everything in that group, and Tony's gift was that sort of thing. And he invented this acronym, Shrelldalff, S-H-R-E-L-L-D-A-L-F-F.

And I took it home and tried it on my, by then teenage kids. They said oh that's the name of a Tolkien dwarf. And I said, fine, fine. So that was that. And Julian was indeed the S in Shrelldalff. And Julian absolutely deserved to be the first letter of the alphabet there, for his management of everything, so that it ran like clockwork. And his participation as well.

SHEDLOVSKY: So, this meteoritic work began in the late '50s.

ARNOLD: Yes.

SHEDLOVSKY: And, fortunately, you had close to a decade of experience before lunar material became available.

ARNOLD: Indeed.

SHEDLOVSKY: So, the timing really seemed to work out.

LAL: Very well.

SHEDLOVSKY: Very well. So that by the time lunar material became available, there were lots of techniques for measuring lots of different radio isotopes.

ARNOLD: And our techniques had become more sensitive, not like today, when we do these things on milligram samples, or some smaller. But at least we were talking about samples of 100 grams instead of kilograms. And so yes. It was—so all of that was brought back together.

LAL: A number of coincidences which—for example, you kept getting fresh fallen meteorites, one after the other. And you first did iron, and then stone. Everything was just at the right time. And then as Julian says, the moon samples became available. And this has been very nice. And I also felt the same in the work on terrestrial cosmic radiation work for [*inaudible*] samples of filtered air from the stratosphere. And just because they were bomb fallout, this technique was developed by the Air Force.

So, you could get air samples. And then you want to do work in the oceans. And there was a big Joseph program which allowed filtration of water from all the oceans. So, I think most of this work on cosmic radiation and applications in extraterrestrial, terrestrial samples, somehow it was a game of luck throughout the thing that things happen at the right time. There were people who were interested in doing these measurements. And there were samples which were available to us. Any of these samples are so expensive to get, if these facilities didn't exist.

ARNOLD: Well, there is—and I do not say this with any vanity about myself, but I've been mentioning Honda a number of times, and you, Lal, and others—what is it they say, luck favors the prepared mind. What was unique about us, those things were available to everybody, but what was unique about us was the team. We had people who were good at everything that was

needed. If it had been any two of us, to come back to the central three, and the three are central in my view, it would not have been the same.

LAL: Right.

ARNOLD: Something good could have been done by Honda and me only. Something good could have been done by you and Honda only. But I think it really took all of us. And to me, that was the big luck. Because it didn't have to be so. There was an element of accident in all of it. Once we had been together, it was always very much in my mind—I remember years later, Price Russ, we were—we happened to be at a meeting together. And somebody was taking pictures of us, the three.

And Price said something to me about this. And I said, yes, this—I'd forgotten it was the fourth or fifth time we'd actually been in the same place, at the same time. And he said, yeah, you seem to be enjoying this very much or something of this sort. And I said yes, every time we have one of these, I begin to plan the next one. And that is true to this day. And if I could have possibly persuaded Dr. Honda to be here today, he would've been. But I failed. Okay. Have we perhaps done it?

LAL: Thank you, Jim. Because I had forgotten many things, which are so exciting.

ARNOLD: Well, all of us have. Julian corrected me, and extended remarks on one key point, and I think I was correct in correcting him on another point *[laughter]*. But you're free to hold your own opinion. Okay.

LAL: Thanks very much.

SHEDLOVSKY: Thank you.

ARNOLD: Well, thank you, Brad, for bringing us all together.

[END OF PART TWO, END OF INTERVIEW]