An Oral History of

JAMES ARNOLD, DEVENDRA LAL, and CANDACE KOHL

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ARNOLD: —the second oral history discussion series. The first was done by Ernie Anderson and myself some time ago. This one is being conducted with another old friend, Devendra Lal. And also, in this occasion, the interviewer— I guess we'll call you Candace— Candace Kohl, who got her PhD with me— I looked up in 1975— who has worked with both Lal and me on many occasions since then. And so, she knows a lot of secrets. And I thought I might start by giving a little background to the beginning of this story, how we came to La Jolla, to UCSD, and started doing the things we did.

Of course, I got into the work on cosmic ray produced isotopes with Willard Libby doing carbon dating and that was wonderfully stimulating and exciting. And after that was over there were a lot of things one might have done, but I didn't really want to give it up and I also didn't want to compete with him and Han Seuss and others who were doing it. So, in the course of looking around for things that I might be able to do and would find interesting to do, the next two isotopes on any fairly obvious list of likely isotopes to study made in the Earth's atmosphere were two isotopes of the element beryllium— the fourth element in the periodic table— beryllium 7 and beryllium 10. Beryllium 7 has a half-life of 53 days, very much shorter than carbon 14. And since these isotopes are made like carbon 14 in the upper atmosphere, mainly, the interesting application we had in mind, which to some degree actually happened, was to use beryllium 7 as a tracer for atmospheric movements. So, at Chicago— where I was in the early 1950s starting my own program— we set out to do that. The more interesting one was beryllium 10 with a half-life now known to be one and a half million years or 1.4— I'm not sure, what is the best half-life now?

KOHL: One and a half.

LAL: Yeah it is still one and a half – holding on that.

ARNOLD: Okay. Anyway, but that seemed harder. I think that was correct. And so, we did the beryllium 7 first and we looked for it in rainwater. And Chicago is an industrial city, the

rainwater was very dirty— grimy— but we found a roof belonging to the university with a drain pipe. And I had a very nice Iraqi technician Hussein Ali Al-Salih [?]. And he and I, whenever it started to rain, we would go off to this building with containers, and we would collect, I don't know, 20 liters or something like that of rainwater and take it back to the laboratory to separate out beryllium 7.

That turned out to be a fairly easy problem. And so, we were able to publish on it rather soon. I had some adventures in that process, but this is not that story. I'll go on. And we started then on beryllium 10 about the time I moved to Princeton in 1955. And that was my first interaction with people here at the Scripps Institution, because it was Ed Goldberg and Gustaf Arrhenius— who are still colleagues and friends— who provided me with cores from the deep sea, because that seemed to be— and was— the best place to find beryllium 10 since the sediments accumulated very slowly on the ocean bottom, and therefore each gram of sediment contained a relatively large amount of this cosmic ray produced isotope. And sometime around 1957— early 1957— I was able to extract the beryllium 7 and took it through the kindness of Tom Sugihara who was then at Clark University— former Libby student, another old friend— to his counting equipment in western Massachusetts, and counted it and everything was as it should be. And we published that.

I had two students at Princeton, two excellent students, John Merrill and Julian Shedlovsky. John's PhD was to measure the concentration of beryllium in seawater because we needed that in order to understand the geochemical cycle. We came out to La Jolla in the summer of 1957, he and I and my family, and we spent that summer working on this problem of getting 200 liters of seawater. But I need to go— and the other was Julian Shedlovsky— but I need to go back because before these two students joined me, Dr. Honda joined me, and he's an essential part of the story.

This was a very lucky period of my life. I was leaving Chicago when my friend and colleague Tony Turkevich brought me a letter from Dr. Honda, who had been spending time— had gone from his position at the University of Tokyo as an assistant professor and he had settled down in Bern, Switzerland with Houtermans and Geiss and Everhart and all that good crowd of people. But he found that although that was a wonderful place to work, Houtermans was not much interested in chemistry and chemists. And so, it was difficult for him. And he knew of Tony Turkevich so he wrote to Tony and asked whether there would be a position in Chicago. And Tony didn't have any money, so he knew I was moving and he brought me the letter and asked

if I had any interest. I looked at the letter, and I noticed that among his publications, he had done some things with beryllium, which is a rather unusual isotope. So, on that basis, I thought it would be nice to have someone in my laboratory who really has worked on beryllium. So, I wrote him and asked him to join me and he came.

I'll never forget, he arrived in New York and took the train for Princeton, but he missed the train and he landed in Trenton, which is the next city farther down. He had my telephone number, he called me. I advised him to take the bus to Princeton, it was raining. I met him at the bus station, and took him to our apartment and he spent the first night in the United States in our apartment— slept in our living room on a couch. And we then came over to my laboratory. It happened that I was very busy that week, I suppose with my teaching, but I found it a little hard to find him a project and he became impatient, reasonably enough. So, when I noticed that, I gave him an air filter to analyze which had been used by some friends of the Naval Research Lab for a large collection in Pasadena in the Rose Bowl of air both devoted to smog— of solids in the air— and to fission products. But I had the idea that sodium 22, which is another cosmic ray produced isotope in the atmosphere, would be cumulated sufficiently. And I gave him this sample and suggested that he should separate cesium 137, which is a fission product of similar chemistry, and sodium 22 salts and then we could count them with our gamma ray detectors. I gave it to him, I remember, in the morning; five o'clock in the afternoon, I see him putting some white powders into vials and labeling them. I asked him what that is and he said, oh, he's finished now.

Now, I had been trained by Nathan Sugarman in Chicago, and I thought I was pretty good and I thought Nathan Sugarman was about the best in the world. And I had calculated that this would keep him busy for a week. Let me say that was my first introduction to the fact that this man was the best laboratory radiochemist I will ever meet in my life. I could not hide my disbelief, and I think it's one of the very rare occasions where he could not hide his annoyance or anger that I was expressing doubt. You know, Japanese tend to be polite, but Masa was a very gentle man. Anyway, I went away kind of stunned. And that was the beginning of our real acquaintance. I've always been glad that I finished the beryllium 10 work— discovery— before he came, because those are my credentials as a laboratory chemist. Ever since, either he or his students, particularly Kuni Nishizumi; people that he is— even my own students, like Bob Tinkle [?]— that he has trained. I don't compete with those people in the lab.

Okay, well, now come forward to again to John Merrill's thesis. There was a very big problem in measuring beryllium in seawater, and that is that seawater is not pure water. It has three point something percent of salts. And the beryllium we knew at the start had an extremely low concentration. And the question was, if you needed to extract beryllium as we calculated from something like a 200-liter sample, how could you separate the beryllium out of this huge thing in the presence of all this other stuff? So, I gave him that problem. And that took him a little longer, I think maybe 10 days or something like that. He started doing strange things in our lab, which I now learned not to ask him any questions. And at the end he showed us what he was doing. He had invented what we always now call the Honda column and which is not an ion exchange column. He had written books on ion exchange, he made his own ion exchange resins in Japan, but the Honda column is not an ion exchange column it's a precipitation column. He took an ion resin, loaded it with iron salts or with iron or with manganese— I think those were the two that were tried. Then precipitated ferric hydroxide or manganese dioxide. And then these insoluble compounds picked up by let's call it adsorption the similarly insoluble beryllium compound— beryllium ion. And the capacity was not very large, but the specificity was and is amazing. That was the solution to our problem.

So then, now, John Merrill and I came to La Jolla in '57, we got the 200 liters of seawater, and John separated it using the Honda column— a very little thing about four cubic centimeters— and the 200 liters was put through that over a period of a few days and that retained all the beryllium, we believe. Indeed, that's been verified subsequently. And thanks to the help of Norman Nachtrieb, a former colleague in Chicago, we were able— he was able— to actually measure and see the signal, the spectroscopic signal of the beryllium in his apparatus, and John was able to finish and publish his thesis. Something times 10 to the minus 13 parts of beryllium and seawater, which was in those days an amazing achievement.

The other student was Julian Shedlovsky. In Julian's case, I did something much worse than giving him a very hard problem, I gave him an impossible problem. And I wanted him to measure manganese 53 and manganese nodules in the deep sea. And neither he nor I realized until quite a bit into his thesis, that with the equipment which we then had, which was a liquid scintillation system, it was impossible. But we did realize that after Julian had been there for more than a year— and I was terribly embarrassed, as I should have been— and gave him a list a la Libby, a list of about a dozen projects— and invited him to take his pick. And he looked at it and since he was interested in manganese— this is what drove him— he said, "Well, one of the projects was to look at cosmic ray produced isotopes in meteorites." And the particular thing that

interested him was that if you take an iron meteorite, there simply had to be a lot of manganese 53 in it. And it was already known that there was very little stable manganese in the iron. So, he did not see and I did not see how it would be possible to fail to detect manganese 53 in that by direct counting, which was all we thought of at that time.

So, I said, “Fine, go do it.” And I sent him to New York, because I'd grown up near New York, and I'd been at the American Museum of Natural History in New York many times and seen all the iron meteorites there. As a child, in fact. And Julian went to New York and talked to Brian Mason, who was the curator at that time, and came back with a slice of Williamstown. And we were— Honda and I— gathered around and we all looked at this. And Julian developed the chemistry with Honda's help to separate the manganese. And that was the beginning of our involvement— actual experimental involvement— with meteorites.

I might mention an earlier aborted project, which I had thought about the beryllium isotopes and it had occurred to me that if you could measure both the short-lived and the long-lived early isotopes in a meteorite, you would be able to test the constancy of the cosmic radiation by the ratio. That actually had led to a first talk with Ed Henderson at the Smithsonian in Washington. And the Sylacauga meteorite fell almost immediately afterwards, and I read that on the front page of the New York Times, so immediately, “Oh, wow!” But as you remember, both of you, and the people listening may or may not know, Sylacauga is the only meteorite that ever— as yet recorded— struck a human being. And the poor woman who was hit on the leg with this thought that the meteorite should belong to her. And I had great sympathy with that. But she was in a rental apartment and the owner of the apartment thought that the meteorite should belong to him and there was a lawsuit which was not compatible with a 53-day half-life. So, I had early learned that getting hold of meteorites was not a trivial problem. There's a first example of a story I didn't think of before I came in here.

Anyway, this has gone on. We went somewhat far with that. And we got some other meteorite samples which we bought from Ward's natural history establishment. And then we came to La Jolla. In the fall of '58, the Shedlovsky family and the Arnold family arrived here. We had a cottage on the hill above Scripps Institution, which is all there was, and the Urey's were two doors down from us in another little cottage which actually was normally occupied by Hans Seuss, but Hans was in Europe then. And all our laboratory equipment from Princeton had been purchased by UCSD, trucked out here. The only thing that was wrong was that the lab was not finished. I've learned since that that is a universal fact that labs are never finished when the

person expects but at the time this was a little bit disturbing. And so, we had to kind of sit around— Julian and Masa and I— and talk. And now I think it's time for Lal to tell us his adventures up to this point.

LAL: Can we stop it for a minute? Just give me half a minute to start ___ [*inaudible*]. Thank you, Jim, for giving me the chance to participate in this history session. As you mentioned, our collaboration goes back to a long time, over more than 40 years. And we have been working together closely since, except for some breaks here and there. But our collaboration has resumed many times in these last 40 years. It is generally very, very dangerous to go back and look at one's own career and I have never done that, as you know, and we, actually, we both have never done that. We have been always so busy with the future projects that we didn't do it. And I haven't done this because I'm always afraid that if one does that, maybe sometimes one doesn't come back to this, does remain in the past.

ARNOLD: Doesn't sound like you, Lal.

LAL: But I'm very happy to do this because I know without looking back how grand our whole collaboration has been both scientifically and socially. Actually, I started my research career in 1949 when I joined TFR. So, I worked at TFR for about 10 years.

ARNOLD: Tata Institute for Fundamental Research.

LAL: Yes, that's the— sorry— that's the Tata Institute of Fundamental Research. And during the 10 years, I worked in the field of cosmic radiation. And then we were working on some scientific projects, which paralleled what Jim was doing in Chicago and Princeton. And actually, some of the things which were done in India in this field of cosmogenic isotopes were actually inspired by this work which Libby, Jim, and colleagues did.

Actually, when I start thinking back to 1958— that was the time when I visited Jim, as a postdoc at La Jolla. And at that time, Jim was— Jim's lab was in the lower campus in the Scripps Institution of Oceanography. And that entire experience during that period was so pleasant and so enriching that the memories of that period itself are something which are, at least in my life, completely unparalleled. And something which I am— I reminisce sometimes about just the fact that we had this unique opportunity to visit and work with Jim and the Scripps Institution of Oceanography with a very great atmosphere which was created around that time. We had Harold Urey, and many other senior scientists who each were very active. So, that was— that

type of experience I never had again in my life. Also, as I will discuss a little later, the way science was done those days is so different from the way science is done now that that period was rather unique. And when I think about this— the past— I'm actually quite aware that next to the major influence which I had on my mode of thinking and towards developing scientific attitude by my guide, Professor Bernard Peters, you, Jim, influenced my career most— both scientifically and socially. And you continue to do so today.

ARNOLD: Vice versa.

LAL: This influence occurred in two distinct ways. One is, of course, during the joint scientific collaboration, and we had a great deal of science which we did together. And the other one was equally important. It was during our close social interactions with the Jim family— Louise, Bob, Ted, and Ken. And there are many, many stories which we often remember about this long association. On the social collaboration, I must mention that we were actually so close that actually I always felt I was part of Jim's family and I think I have heard Jim saying very much the same. So, it was a very mutual togetherness. Jim actually showed us the Arizona Canyon Diablo Crater, the Palomar Observatory, several comets we saw together. We saw the first manned Apollo lunar landing in his TV in his living room. And to go back a bit in time, actually, even Kashmir he showed us for the first time. We had not seen Kashmir.

ARNOLD: [*laughs*] ___ [*inaudible*].

LAL: And, actually, Jim showed us Kashmir. That was Aruna and myself. And that was something fantastic. So, all this has been a very pleasant experience for Aruna and I. We were very enriched by our close interaction with Jim and his family members.

ARNOLD: May I break in and give one example of this? Our youngest son, Ken, was three months old when we arrived and he was in a crib and Lal is very fond of children. All my life that I've known him, I've seen that quality. And children in general are very fond of Lal. At any rate, but when Lal approached the older two boys who were two years and four years old, they immediately became best friends but Ken used to scream every time he saw Lal. And that is probably the reason why they became such very good friends today. Because Lal took this as a challenge, and before he departed in his first long stay, they were already very good friends and now Ken is something like 40 years old and they're still very good friends.

LAL: You see, at that time, I had this conviction that children really read the inner self of a person. So, whenever Ken saw me he started crying, so I thought I must be a very bad person inside. And that's what scared me, and I tried to win him and change things over. So that was what Jim is referring to.

So, you see this Kashmir thing. I had been earlier to Kashmir in 1955— and as I will tell you later, to collect and extract beryllium 10 from molten snow— but there was no time for us to see Kashmir. Actually, the only time I really enjoyed seeing Kashmir was when Jim came and then we were with him that was really— so, there was something about Jim, the way he did things. He would talk about science, he would talk of very serious things, but he also had a lot of time for talking about other things where he relaxed you and to make you at home. Something which I have not yet learned but whenever I am with him, this is something which I always enjoy being with him. So, besides the social interaction, the most important and most rewarding realization which I had was that— when I think back— is that we had a great variety of ways, we learnt a great variety of ways, in which cosmic rays leave behind permanent records of their interaction in matter. And how these can be used to deconvolute information on the isotopic changes and then to obtain the history of matter itself.

So, actually, most of the work in the past in this field— as it happens, most of the key observations in this field have been made either by Jim in collaboration with Masa and colleagues, or by Jim in collaboration with myself and colleagues. And I think there was a short period of time— 1958 to ‘65— when actually Jim, Masa, and myself worked together very closely. So that was a period when we were very close to each other, but at all different times there were these combinations in which Jim operated with both of us and we did lots of things. And then these studies were of such a nature that once they were done, they were followed the world over on a rather large scale. And this even continues today. This was a really very rich experience in ___ [*inaudible*], and in science. And to put it in another way, I would say that Jim and his collaborators essentially stole away all the possibilities from others to make these discoveries themselves. Essentially, it was ___ [*inaudible*] to actually take away the pleasure from others to make any such discoveries, take away by doing it in this group. And this is actually not unexpected because in science you always find that if you lead groups who have the right concepts and facilities at the right time, they run away with the discoveries which have yet to be made.

And I think the whole sequence of events which brought Jim to La Jolla, brought other people to La Jolla, which created such a tremendous scientific atmosphere— Harold Urey, the Mayers, Craig, Arrhenius, Goldberg— and Roger Revelle was there who was actually telling people that oceanography was not doing biology of oceanography. Everybody at that time— especially the Scripps Institution was known as a center for biology. And they thought one of the most important things an oceanographic institute has to do is to study biological processes. But Roger Revelle said that this is not absolutely true although oceans stand in space. And they are not standing there by themselves and they are actually receptacles of everything which happens in space. And therefore, actually his philosophy was to bring all these people, including Jim. And Jim himself was spearheading several adventures bringing people who could see the broader context of science and he managed to bring very, very active people to the UCSD— what made the UCSD, at that time there was no UCSD itself.

So, this whole period was not just gym science, it was to me— I was just like being shot by hundreds of guns or something, it was a tremendous shock to see all this. I was lucky to be with Tata Institute of Fundamental Research with Homi Bhabha. He had this very high philosophy and he used to actually interact with Blackett and Cockcroft all these very pioneering persons in the field of physics, and he used to bring them to Tata Institute and expose us to these things. But the intensity at Scripps was far greater than anything that I had actually experienced, and at that time I was young. So actually, this made a very, very big difference to my life.

So, let me tell you actually then, how I came here. So, in 1957, if I remember I received an invitation, official invitation from Roger Revelle, to visit the Scripps Institution of Oceanography as a postdoc. And I was told that Jim Arnold is moving over from Princeton, and he's setting up a group to study cosmogenic effects. And that you can, if you like, you can work with him. This was very informal way of presentation. It was not that you are being invited to work with Jim, it was the freedom well again given to me, but it was just an invitation to come to Scripps Institution of Oceanography. Now, I had then heard a great deal about Jim Arnold. Actually, I read about his papers too. And there was as I said, there's you know, there's a lot common in our science.

ARNOLD: You had been working with Peters. Parallel work.

LAL: Yes. Right. So, in fact what I want to tell you, Jim, is probably— do not know how Peters got so influenced to start a geophysics group in India. In a way the existence of the geophysics

group itself owes to two facts, two main facts, one was the excitement produced by the discoveries of carbon 14 and tritium by Libby, Arnold, and his colleagues. And then there was a visit to TIFR by Harrison Brown in 1954. And he went around and told what all things were happening in Caltech and they were studying lead— the work of Patterson and others— and he actually explained that the environmental lead which you have anywhere, which you just do like this and pick up nanograms. And one of his statements, which Peters always quoted, that was just due to Harrison Brown was that there is one ___ [*inaudible*] of everything in anything. And then he had said that lead— common lead, dirty lead, contamination lead anywhere— was 4.5 billion years old. And this impressed Peters tremendously because he always used to get excited by the quintessence of things, you know, this to him was very exciting.

But you see, at that time— this is mid ‘50s— elementary particles were being discovered everywhere in the world and in our group headed by Peters, and we were using nuclear emulsions. And that to a very sophisticated form of nuclear emulsion technique would Peters had an idea and he developed at TIFR and that was a continuous emulsion block, like a cloud chamber. So generally, emulsion blocks, emulsions were mounted on a glass plate. So, the information on a cosmic ray event or the behavior of an elementary particle was partly obscured then because whenever the particle went to the glass you can see what was happening. So, Peters' idea was to do two things. Firstly, I suppose, freshly pour emulsions in which there were no background from cosmic rays even before exposure. And secondly, to put them in together to make a block of 20 by 20 centimeters by 20 centimeters and expose it at very high altitude to energetic cosmic rays. But the big problem was when you process a nuclear emulsion, the nuclear reaction is distorted and then it becomes very difficult to follow individual tracks. So, he had an idea that he will put a grid of polonium alpha particles by soaking tin nylon fibers of 20-micron diameter, and putting it on the backside of the plate and putting two emulsion pallets together. So, this way, there was a guarantee that wherever the alpha particles were there on the two sides, those sides were together in contact. Then of course, the next question was, how to mount these pallets so that you can just reproducibly just go on from one pallet to the other, and ___ [*inaudible*] will keep coming without having to make long transformations because one had to study lots of particles before any elementary particles were discovered.

So, these things were all accomplished: how to process emulsions without much distortion, then how to follow a particle right through 20 plates, if it goes through. And so, once he succeeded in this then we of course started discovering particles. And we also discovered new particles, new particle decay modes, we found that some particles were produced in association, they were not

produced singly, which was actually very remarkable because we had only 20 events [?]. But we found that in some of the events [?], two particles came out and we calculated the probability was very small. At that time Byers [?] had been working on these theories of— they were not the standard model at that time. But there were theories, and one of the theory of Byers [?] was that particles must be produced in association to conserve certain of these properties. So actually, we were the first to— with even just 20 [events?]. So, there were exciting things going on.

But Peters was still very worried because then CERN— at CERN, they started producing high energy beams and they made beams of ___ [*inaudible*]. So, he realized that all the edge which we once had at the Tata Institute of Fundamental Research was going away. You cannot remain in this for ___ [*inaudible*] five years. So, he was always looking out for certain change in the field. And I remember one day there was a— the c14 was already discovered, and this was well known and one day there appears a paper around 1954, if I remember, on tritium— the discovery of tritium by Libby. And we were going for a balloon flight to Delhi, and I was in one compartment and Peters was in the first-class compartment. So, every time the train stopped, he used to come to my compartment and I used to come out and he will keep telling what all things he is thinking in between. And he was so excited, and he said a new field of cosmic geophysics is coming up. This is really very exciting and we should think about this. And basically, this was the big realization that ___ [*inaudible*] had started coming. We didn't have any edge. And Peters did not keep on reproducing more work on elementary particles because it was a waste of time. And he was very aware when to get out from a project when it's not producing too much. But the Harrison Brown remark also clinched this.

Now, Homi Bhabha was very free about starting new projects, or if somebody came to him with a new idea he was very responsive generally. And Peters had a real complete freedom to do whatever he wanted. So, I remember that one day Peters asked me to come and discuss some things with Homi Bhabha. And he said, “We would like to start a geophysics group, a nuclear geophysics group because this is a big deal.” Bhabha said, “Just go ahead.” And then Peters was trying to tell him about how much it would cost him and he said, “Don't talk to me about this.” It was a complete freedom to do whatever you like. And then I decided to join this activity and really spend full time seriously and I stopped building nuclear emulsions. Now, I had lots of friends at that time and they all thought that I had gone cuckoo. They said that geophysics is zero physics, and you are stupid, you are doing elementary particle physics which is very fashionable physics, and you are ruining your career. This is not what you should be doing. And

then they also asked me that this idea of Peters which— he had written a paper in 1954 saying that beryllium 10 is produced and ___ [*inaudible*] items per centimeter square per second in a column and they asked me as many times that, “You think Peters has gone senile, this is very stupid that you will produce 10 to minus two particles per second or in 100 seconds one particle and, finally, you will expect to find it in the ocean sediment.” So, I said, “You should not ask me, I am working in the geophysics group. Go and ask somebody else about this.” So, this was the story and then at that time ___ [*inaudible*] and Ram both also were convinced. They were also working in the nuclear emulsion group. And they realized that they didn't have, I mean, they probably had a better chance to work in the geophysics group rather than this.

So, we had actually two jobs to do in the geophysics group. One was to develop chemical procedures for extraction of these atoms. And Peters then put us in touch with Shankar, Dr. Jagdish Shankar of Atomic Energy Commission and at Trombay. And then we started learning we had never heard about EDTA and TTA. So that's the first time we heard about this. And then we started doing all these experiments and it was very nice. We didn't have any facility, for example, EDTA you need to shake, as you know. So, I actually had a fan and I made a string attachment for the bottle used to shake it when the fan rotated and things like this. So, we did all sorts of things. And very quickly— we were in the barracks, we had no facilities at that time, the real TIFR completely air conditioned. What is called Homi Bhabha Taj Mahal now, that was not there. We were in barracks, and quite often pigeons used to come everywhere.

But we were just determined working day and night, learned how to do this chemistry. And then we built a low-level counting system. We had never heard about ___ [*inaudible*] gas and all this, but most of these techniques were actually in Chicago. So, Sugihara had developed a flow counter. So, I was very inspired about this and we tried to do this, but then he just told me that look, you should go out and look at various low-level counting laboratories and do this. But I had built a system with a flow counter which had a background of 29 counts an hour. And this counter was made by me by going to the cotton exchange market. There were some silversmiths so we made very small counters, and they did the blowing of the silver, the tubes and all that. They didn't have any high temperature flame. They actually use ordinary mustard oil flame. And if you blow it hard enough, you'll get a very nice blue flame. So, he did all the ceiling welding and we made this counter from silver. And then our glass blower put mica on the end, and we use we made these flow counters. And then the background used to be sometimes 20 sometimes 100. So, we had actually spent six months just trying to find out where these electrical interferences came from. And I very quickly learned— it’s a long story— but finally we

made sure that absolutely no spurious counts came at all. We learned that, but everything had to be learned. And when I went to Harwell [?] and other places to see their low-level counters, we actually found that they had much higher backgrounds than ours. I came back and told Peters that my going there was not very useful, but very, very convincing that we are not doing very badly.

[END OF PART ONE; BEGIN PART TWO]

LAL: The world was started thinking about this.

ARNOLD: Sure.

LAL: And so, I think that was good that you took that lead and made sure that the theoretical paper was written on this. One thing I just wanted to comment—. See, I had learned by working on the development of the cascade in the atmosphere that all attempts Monte Carlo [?] calculations or cascade calculations were hopelessly bad that one could get answers, which didn't mean anything and for example in the atmosphere we are dealing with 14 mainframe [?] parts, 1000 grams is about 14 mainframe parts and one was interested obtaining cosmic ray production waves right down to the sea level and one could be off by factors of 10 and actually some of the earlier calculations, which were done and actually Benioff had done very similar calculations beryllium-7 for the whole atmosphere.

ARNOLD: Huh.

LAL: But he was very wrong wo actually one thing which we learned that if we have to do something then one should take advantage of as much experimental data as possible, which is measured so that we are closer to the truth.

ARNOLD: Right.

LAL: The second thing was, which was not used before but Peters had started using it with Yash Pal much later but at that time when Jim came to India this was not used very much and that was idea of inelasticity that in a collision the primary particle essentially retained a certain fraction of energy and the rest of the energy was used up in that, the first collision and then this particle essentially went on and then it repeated the same history again with the same inelasticity. Now and when you're dealing with an atom meteorite even the size may 20, 30 centimeters you are dealing with actually quite a few mainframe parts and particles are coming

from all directions so actually I was quite nervous to say that we can make any calculation, which representative of the real truth and we didn't know about depth to field [?] at that time.

ARNOLD: No?

LAL: So to write a paper on the depth of fie and say something it made me stimulated but when thought about this inelasticity as using the ideas of inelasticity, which were held more or less very true by various observations of Russians and high-energy beams so then I some confidence that probably we can go and publish that and I think afterwards when Reedy [?] and Arnold published on the development of nucleonic cascade in the moon then essentially they are just an extension of the *[crosstalk]*—.

ARNOLD: Yeah.

LAL: The earlier part.

ARNOLD: Absolutely.

LAL: So, I think that was very nice, which Jim said, if, for example, if I was here probably nothing might ever happen but when I was back in India and Jim said, "I am coming and to finish the paper" *[crosstalk, Laughter]* —.

LAL: That really was a fantastic high-pressure environment.

KOHL: Mm-hmm.

LAL: And one gets to do things only when there is a high pressure *[crosstalk]*—-

ARNOLD: *[Laughs]* That's right.

LAL: And the less you do otherwise. Okay this was—. You know, Jim, I wanted to ask you, this idea of doing so many isotopes in one meteorite, there are rules for example.

ARNOLD: Yes.

LAL: Was this reproduced by any other lab, later?

ARNOLD: Oh yes but much later.

LAL: Well, who did that, actually?

ARNOLD: The *[crosstalk]*—-

LAL: I don't remember this. Anybody could have accomplished this task.

ARNOLD: Well, I think—. I have to think for a moment but I think that— well, I'll give you what I very much later, Hertzog and company had done that.

LAL: Not more than a few isotopes I think.

ARNOLD: Well *[crosstalk]* –

LAL: Herzog is very limited.

ARNOLD: Well *[crosstalk]*—-

LAL: But maybe Coleman's group did more than *[crosstalk]*—-

ARNOLD: Well, you know Goel had *[crosstalk]*—-

LAL: Yeah, Goel might have done that *[crosstalk]*—-

ARNOLD: Yeah, Goel *[crosstalk]*—-

LAL: With Coleman.

ARNOLD: Did so with Coleman.

LAL: Yeah.

ARNOLD: And Shedlovsky also you know went and did postdoc *[crosstalk]*—-

LAL: Went back and *[crosstalk]*—-

ARNOLD: With Coleman

LAL: Okay, okay.

ARNOLD: So that *[crosstalk]*—-

LAL: But this remains, what I wanted to say was I mean this— I mean this is not a session for self-association but I would say this remains one of the luminary examples of the necessity of doing several isotopes to really constrain cosmic-ray flux and to learn many things from it but this is a very difficult task which *[crosstalk]*—-

ARNOLD: Yes.

LAL: Which was accomplished at that time so smoothly.

ARNOLD: It was certainly there was nothing like it *[crosstalk]*—-

LAL: Yes.

ARNOLD: For many years and except what we did ourselves, kind of there's an old proverb of Libby's who just as Peters, you know, sort of trained you and set you up to be what you could be and Libby did the same for me and one comment that he made to me that was against many peoples' ideas was that you should always tell people what you are doing, you know? If you have any run and introduce to any colleague and you should chat, you should tell them exactly what you're doing and the remark was that if they're thinking of doing something similar and intelligent then they'd find out that you are already doing it, they'd leave it alone because why should they compete with you?

LAL: Mm-hmm.

ARNOLD: If they don't leave it alone and do it, well, you should be able to get there ahead of them because you have actually started *[crosstalk]*—-

LAL: Clearly, right.

ARNOLD: And on the other hand, if they beat you then you didn't deserve it anyway *[laughs]* and so *[crosstalk]*—-

KOHL: *[Laughs]*

LAL: That's a good—. That's a good philosophy.

ARNOLD: So, in a way the way this paper, well, I remember as you say we should cut short any you know sort of patting ourselves on the back but I remember Geise saying to me—Johannes Giesen at Bern who was one of the great people in this and related fields—. No, when he published his article in 1957, which set up essentially what you could do *[crosstalk]*—-

LAL: Mm-hmm.

ARNOLD: It was a theoretical article and he thought he was laying out a 10-year program and then he had read this paper and we had done it, you know?

LAL: Right.

ARNOLD: And so, I think one reason it was not duplicated very well is that it looked much more forbidding perhaps to people after they read our paper than it would have if they had started it themselves and *[crosstalk]*—-

LAL: Mm-hmm.

ARNOLD: It said that there's somebody around who does these things and that's their specialty and that's their—. What shall I say—their niche, we might—. I take the story as far as Bruderheim perhaps and then Candace, you. We haven't given you much of a chance, have we?

KOHL: That's all right, it's fascinating.

ARNOLD: Okay. It wasn't long after that the Bruderheim meteorite fell only at I see from my notes here about six months after the other and, okay, this one fell in Canada, Western Canada in the—. What shall I say—collecting area of Ian Halliday who was a meteoriticist in Alberta, I think, and of course I think we had already met a little but we certainly wrote him and fortunately it was a very large fall and so it was not difficult to get from him probably a kilogram. Again, I don't remember exactly the size. We could look it up here but never mind and the—. So that sample was a large specimen it says here. It was a large specimen and we started in platinum crucibles because one cannot dissolve a piece of stone in a glass beaker, the chemistry started. I say we, of course, Honda led the chemistry and always. There was never any question who was in charge of that operation and he of course had the experience behind him and he—but I think even Lal, you, did some chemistry at that time.

LAL: That's right.

ARNOLD: We were all involved and again though it was a much more challenging and laborious task with the other one behind us that was as far as I can recall, also completely successful and again a *[crosstalk]*—-

LAL: Which year was this, Jim?

ARNOLD: The date of the publication is October, '41. Oh, Hondo/Umemoto *[crosstalk]*—-

KOHL: Sixty-one.

LAL: Sixty-one.

ARNOLD: Sixty-one I'm sorry.

LAL: Or '62.

ARNOLD: And so Umemoto was a postdoc, another Japanese postdoc with Harold Urey *[crosstalk]*—-

LAL: Mm-hmm.

ARNOLD: Mostly doing stable isotopes but Honda and Umemoto were friends and he was a good chemist and so however many there were here, about another 6, 9, 12, 13, 16 isotopes *[crosstalk]*—-

LAL: Fantastic, yeah.

ARNOLD: Were taken out of that.

LAL: That too, for a stone meteorite for which chemistry was not known at that time.

ARNOLD: Of course.

LAL: And very difficult, much more complex.

ARNOLD: Much more complex.

LAL: Because ions chemistry is simple.

ARNOLD: Yeah and *[crosstalk]*—-

LAL: Simpler, I didn't mean simple.

ARNOLD: Exactly and of course, Umemoto was very much a part of that operation and I'm sure I was doing chemistry, too. The counters already existed thanks to you and by that time probably Ram was with us. I'm not sure. We can look and see if there was an acknowledgement. At any rate the—. And these are various *[crosstalk]*—-

LAL: So, these are Jim all '61, all these 3 papers here.

ARNOLD: Mm-hmm.

LAL: And now when did Candace join you?

ARNOLD: Oh, much later.

KOHL: Much later.

LAL: Much later.

ARNOLD: This young woman *[laughs] [crosstalk]*—-

LAL: Yes, right.

KOHL: I thank you.

ARNOLD: Joined us actually jumping ahead the Apollo was the next. We were going to have a session on that one.

LAL: Oh, you are after Apollo?

KOHL: *[Laughs]* Yeah.

LAL: Oh.

ARNOLD: Yes.

LAL: I thought you were before Apollo. I made a mistake.

ARNOLD: Oh.

KOHL: No.

ARNOLD: She—. There was—. there were these seven graduate students that took part in that, the largest group I have ever had and Lal, again, and Honda, again.

LAL: I'm sure.

ARNOLD: Shedlovsky again, yeah, all the old, old stars.

LAL: Okay so we should get back. We should get to Candace and so at this period essentially in one year at that time you had only Julian and was he with you *[crosstalk]*—-?

ARNOLD: Well, Julian was gone by then.

LAL: Was he with you in La Jolla, Umemoto?

ARNOLD: Julian was gone. Umemoto was with Urey.

LAL: And I see so what part did he play in this?

ARNOLD: He was an experience chemist.

LAL: Oh, he did chemistry on meteorites?

ARNOLD: He did. He did chemistry.

LAL: Okay.

ARNOLD: And the counting we all did [*crosstalk*]—-

LAL: Right.

ARNOLD: On the counters you know [*crosstalk*]—-

LAL: Okay.

ARNOLD: But actual counting thank God we were long past the days, early Libby days [*crosstalk*]—-

LAL: Right.

ARNOLD: Because the counters were quite reliable, not perfectly. There were problems but, you know 90-percent reliable or something like that rather than 10-percent reliable and so we—. We all did the counting. I have still some mementos from that time and we had—. They're mostly thin-walled counters. The betas were counted with thin-walled counters that you had designed and even some that we built ourselves and then there were the sodium iodide simulation counters so that we had that equipment. We had the shield, steel shield that I bought from Princeton and, no, we were in shape to that and the—. But the chemistry was done, I think, by all 3 of us but absolutely under the leadership of Honda, who is way beyond our skills. So that, too, as you say, that happened in quite a short time and I don't think we made any attempt in the theoretical paper to deal with the more complex case.

LAL: Right.

ARNOLD: We were well satisfied since that was a pioneering effort to be able to do something reasonable about the iron target. I also wrote a review paper in this period and also later in the *Handbuch der Physik* with the Germans publishing this exhaustive handbook of physics and they invited Lal and Peters to write an article and they invited Honda and me to write an article so we were a little separated at that moment but these two articles make very much a pair and should be read together by anyone who wants to look at that. I wonder if we should break for a minute of two or three.

LAL: Okay I also want to go. Excitement of work in the—during '58 to '60 about 1.5 years when I was there, one of the experiments, which we wanted to do was to measure precisely the production base of isotopes in atmosphere by cosmic rays. So, the idea was to expose argon in ordinary cylinders at high pressure to cosmic rays at high altitude at Mt. Evans and Echo Lake and also water for measuring the rate of production of beyrrilium-7 in oxygen. At that time the techniques were not sophisticated enough to measure beryrilium-10 directly by exposing water, which was done later in Jim's lab and very successfully with the development of the AMS technique but at that time by counting it was not possible of course to do that so that's

an experiment, which required a considerable amount of expenditure and when I say a considerable amount I don't mean much more than $50,000.00 or $60,000.00 or something, maybe much less but during those days these amounts were very large. I mean that's a good-size experiment.

ARNOLD: Right.

LAL: So, I went to Jim and asked him firstly, well, that I have discussed with Masa Honda and would like to expose material at high altitude to cosmic radiation to measure precisely the production rate of isotopes and he said, "Yes," that was a good idea and the figures look okay, and then I asked him about money. And he said, "Well, you write a letter to NSF." So, I said, "Who am I" and I said [*crosstalk*]–. [*Laughter*]

ARNOLD: Yeah. [*Laughs*]

LAL: They didn't know me and what does this mean? He said, "No, no, just go ahead and you just write, explain to them what the experiment is, you say you're working in my lab and explain and just this letter may not be more than 1.5 page so I just exactly did that and I don't *[crosstalk]*–.

ARNOLD: Those sure were the good old days.

LAL: Remember—. Exactly, I don't remember exactly but I think within less than 1.5 months I had a letter from NSF approving the entire expenditure without any change at all. They just said, "Go ahead and you'll have the money." So that was very, very fantastic, very encouraging that firstly, I mean, who am I to have asked that money? I am just a visitor to the United States and total was 1.5-page letter.

ARNOLD: It makes regret the past a great deal. *[Laughs]*

LAL: And then this of course, this experiment was done very successfully.

ARNOLD: There were some adventures of course.

LAL: Right. There are lots of stories about these but I think what I would like to tell you was that after the silicon-32 was discovered in the sponges, marine sponges, Shenk, David Shenk had joined Jim Arnold as a student and his idea was that he would like to measure the silicon-32 concentrations in sea water directly.

ARNOLD: Right.

LAL: So, he was quite ingenious. Firstly we needed about 60 tons of sea water to remove silicon from and this had to be done from different depths in the ocean so you essentially had to bring 60 tons of sea water about the ship so he had an idea that he will take a canvass bag, which will open on both hands and he will lower it so it's always flushing and then, at any depth, you just close the 2 ends, then bring the whole canvass bag right to the surface but not above the surface. Just keep the neck only above the surface and have the canvass bag just below the surface. Then there is no weight problem, it will be supported by its own buoyancy. And then he took a tube and put it into the canvass bag and pumped out all the water from the canvass bag on the ship and then he passed it through these iron-exchange columns and he had actually got his PhD thesis on this. He did about 8 or 10 samples. This was very, very dramatic but, once again, this was a very expensive experiment and, actually, he lost *[crosstalk]—.*

ARNOLD: Right.

LAL: One of the *[crosstalk]—.*

ARNOLD: Samplers.

LAL: Samplers, which was itself about $10,000.00. In those days $10,000.00 was a huge amount of money. Postdoc salary was only $6,000.00.

ARNOLD: Right.

LAL: And so, this was again very remarkable that within that short period while I was still there that one could, if a PhD student comes and he starts, he goes to the ship, he collects his samples and completes all those measurements this was really so remarkable.

ARNOLD: I have to excuse me break in and one other aspect of that, David called me and—or they radioed from the ship and they told me about this that the sampler is lost and so, okay, that's not very good news. I went to Jeff Frautschy who was then the business manager of The Scripps Institution of Oceanography and he said, "What time was it lost?" And I told him and he said, "Ahh," and he said, "I woke up this morning and I remembered that we had not insured that sampler so I went and did it 2 or 3 hours before it was lost." [*Laughter*]

LAL: Oh, I didn't know about this. *[Laughs]*

ARNOLD: Yeah.

KOHL: [*Laughs*]

ARNOLD: So, we got our money back.

LAL: Oh, that's great. That's absolutely great. So, you see these were—. The number of things which one could do, I was there only for 1.5 years and I as exposed to such a great variety of experiments and it was just a combination of things I mean Masa being there, the atmosphere being so distinct but what really surprised me, the flexibility with for example Jim had never done such a large, accelerated scale work, which he undertook and there was no strain in him.

ARNOLD: Yeah.

LAL: He was always relaxed and *[crosstalk]*—

ARNOLD: That was Libby's teaching was to be afraid of nothing, just be afraid of nothing.

LAL: And then the various things which we did, oceanography, large-scale experiments, nothing—. He was so cool about the whole thing and he never even told any one of us that, look, he wants to have this thing done within such-and-such time That you could never even imagine him to say to say to anyone. We were all completely free, the freedom with which we could do things there and actually *[crosstalk]*—

ARNOLD: Well, we—. It was—. We were coworkers and we were equals, it was not a—. We were—. You know, Masa is a few years older than I; you're a few years younger than I. I think we looked on ourselves, I looked on the three of us contemporaries and—. You know, my style I must say even with young graduate students, Candace can either agree or not, was always quite—what shall I say—informal and permissive.

KOHL: Absolutely agree.

ARNOLD: And so, it wasn't so much that but it was certainly in this case something that has persisted our entire lives up to this point that when we are together we—. The sensation is not somebody is above somebody or below somebody, we are friends and more or less contemporaries, certainly now, as we get older, a difference of age gets smaller and smaller relatively but, at any rate, it was like that and that made it very easy to be loose because I was confident if any—. If Lal proposed an experiment, it was an experiment he could do and if Honda proposed an experiment, it was an experiment he could do. I didn't have to worry about that. Oh, I mean we asked the questions. Always we debated *[crosstalk]*—-

LAL: That's right.

ARNOLD: And criticized.

LAL: That's right.

ARNOLD: It wasn't that I simply accepted everything.

LAL: That's right.

ARNOLD: Or you simply accepted everything I said but we were partners.

LAL: You know Jim during that time, the short time when I was there, so many things happened. One of the things I remember, we got—. I got invited to Duffield's [?] house.

ARNOLD: Right.

LAL: And at that time Robert Wilson was here and Dyson used to come. There was the General Dynamics was very active at that time.

ARNOLD: Yes, right.

LAL: And actually, they invited Niels Bohr to do some commemoration I do not know at what instance it was there and what was the reason for that, I *[crosstalk]*—-

ARNOLD: Bohr and Heisenberg and all sorts of *[crosstalk]*—-

LAL: So, and I know Niels Bohr was supposed to say something at that time. So, I know exactly this was narrated to me by one of the people that he had to say 1.5 sentence or 2 sentences for inaugurating the function. He prepared that 2 sentences, he was preparing it for months and rehearsed and rehearsed and several times how to say exactly each word in that sentence, this is what I learned, and you know to be told and to meet and to live in a time when Niels Bohr comes to the same city where you are there *[crosstalk]*—-

ARNOLD: Yes.

LAL: It's really—. And Freeman Dyson, the story about Wilson, which each of one you I think know, when he was asked by the Senate, at the Senate hearing, some senator asked him what will this—. He was building this big, giant synchrotron isolator at Brookhaven so he was asked what will this synchrotron do to the defense of the country? So, he said, "Absolutely nothing but it will make the country worth defending." So those were the types of things you get exposed to when you are very young and you are given complete freedom to do whatever you like. You are in a total state of shock, you know?

ARNOLD: *[Laughs]*

LAL: You know I was in a very bad state. Actually, I started towards the end of my stay in La Jolla; I started taking ____ [*inaudible*] who you do not know probably.

ARNOLD: No, I *[crosstalk]*—

LAL: I had become—but I couldn't stop. I had to do more because anything you want to do gets done.

ARNOLD: Yeah.

LAL: And everything is science. It's not just data collecting. You see the one big difference in culture in science between now and then was that those days it was basic research. These days a great deal of science is data collection, which is very important but the emphasis on basic research is so limited. For example, the—. All these new things, which one has done recently, which we'll discuss about unsuitable isotopes, NSF never had the pleasure of supporting any of those things for the first time.

ARNOLD: MM-hmm.

LAL: They will only support when you publish a paper in *Nature* or *Science* and then the reviewers know about it but the reviewers and the whole system is such that they never support anything in basic research but at that time if you went for a big data collection system maybe you would not be supported but with Fowler in charge of the NSF and the various—. You know really Fowler was the head of the NSF *[crosstalk]*—

ARNOLD: Oh, that's amazing.

LAL: For some period *[crosstalk]*—-

ARNOLD: Yes.

LAL: Of time and there was a great emphasis on doing science with an emphasis on basic research so the way I went back to India was that people are short of ideas, not money, the money was not the limiting situation and I would like to tell you something, which is a personal note, that towards the end at the same General Dynamics meeting, Homi Bhabha had come here.

ARNOLD: Yes.

LAL: He was invited to this *[crosstalk]*—-

ARNOLD: I remember very well.

LAL: And then he had heard from Roger Revelle that I was doing very well here.

ARNOLD: Yes.

LAL: And he of course had met me before and would talk me in the Tata Institute because he knew that under Peters I was doing these things for the first time so he actually had a meeting with me and he said, "Young man, when are you coming back?" So, I said, "I'm coming back—uhh—I'm just extended months but I'm coming back immediately." And then when I actually went back home and then Peters was there and he still had not gone back to Denmark and he said, "Lal, there are no housing here and housing is so expensive, why don't you tell Homi Bhabha that you must have housing to stay here?" Actually, he had told me earlier that before I come, use that as some sort of a levering principle but I never did that because I think that was not very right so that was one advice of Peters which I never took. And then when I went back I rented a house very close to Tata Institute because I wanted to come back-and-forth several times in a day and that was about 60 percent or 50 percent of my salary was going there but still I rented it for convenience so that was good but, you see, the story, which if I tell what happened afterwards, that showed that Bhabha was always aware that people need housing and that he always gave housing to people whom he thought were very good and deserved to be given some encouragement.

He built. He sold his land, which belonged to his aunt at Pedder Road. This is the Kenilworth.

ARNOLD: Ah.

LAL: Which became Kenilworth; it was 81 flats were made by the Atomic Energy Commission. Then one evening at a dinner, Homi Bhabha called me and he said, "Look that Kenilworth has been built by the Atomic Energy Commission and you choose any flat you like in that."

ARNOLD: Mm.

LAL: So, I go there and I look around and then of course I chose the best pad with the best view with a big garden on the top, on the 8th floor, with a balcony and everything. So, then I was seeing these houses. There are 190 people who had also come to see the house, which they wanted and so they said, "Lal, you are wasting your time, this belongs to Atomic Energy, you will not get this house, why do you want to look at this flat? You look at other flats especially on the ground floor" and things like that. But Homi gave me the best flat in the whole building. He just didn't care about—but you know it was I had found one thing about Homi, anybody who

asked him for things, he was not very sympathetic about it because when I would spend more time as some professor whom an invitation I had gone to join the TIFR, when he wrote a strong letter saying that he must have housing so he said, “As far as I am concerned, he can go and stay in the Taj Mahal.” [*Laughter*]

LAL: That was very helpful approach in the problem.

ARNOLD: Oh dear.

LAL: So anyway that was a very nice thing and then but Homi Bhabha made it very clear to me when he was talking about when you are coming back, he said, “Look, these people want you to come and spend time here, I would agree to it but these people have to come back, come and visit us, these professors have to visit, otherwise no one-way exchange.”

ARNOLD: Yes.

LAL: So actually, I told Jim that Homi *[crosstalk]*—-

ARNOLD: No pressure on me, as if I needed pressure.

LAL: That Homi Bhabha says that you must visit us so I trusted him to visit. Then before Price came and of course this whole visit was very lopsided because I think many of my colleagues visited Jim’s lab, you know, Bhandari [?], Masan Goel, Rama, everybody just one-by-one Jim invited. I never asked Jim to invite this person and Jim always knew whom to invite and—.

ARNOLD: Oh, just well this—. The first disagreement we have had here.

LAL: Yes.

ARNOLD: Because I always counted on your recommendations

LAL: You asked me.

ARNOLD: Yes.

LAL: But—. Afterwards you asked me the first time—. At the first instance I didn’t I didn’t come to you saying that I want Bhandari to come and work with you or never.

ARNOLD: Oh, but we were always talking.

LAL: Right.

ARNOLD: Right? And so, these I learned about those things and there was a long—. I found from experience that I could trust you to send me only good people.

LAL: So, all of the group members essentially came.

ARNOLD: Yep if you look at my history of postdocs and someone asked me once to make a list of course there is a preponderance of Indians and Japanese

LAL: Right.

ARNOLD: And there is no accident whatever.

LAL: Right.

ARNOLD: But if my friend, either my two friends speaks well of somebody then I went and made them an offer and you know from Honda I got Imamura and Nishizumi and that's a pretty good list and that's a pretty good list and from this guy I got also some—and it went into the second generation because Parma Goel sent me two people afterwards so *[crosstalk]*—

LAL: Right, right.

ARNOLD: On it went.

LAL: Yes.

ARNOLD: Candace, this seems to be a good time *[crosstalk]*—-

LAL: Yes.

ARNOLD: To ask you to ask you to speak up and ask us some questions or anything else that you liked and we've been talking your ear off.

KOHL: Well, it's absolutely fascinating. I'm privileged to be a part of this. Of course, I was not around in these days.

ARNOLD: Right.

KOHL: I came in the fall of 1969 and you know this is sort of my scientific background. I remember reading all these papers when I first came here as a baby graduate student, you know? This was my homework was to read these papers and learn something about what had been done. Just a minute of background about me, which was my undergraduate degree was from Carlton College in Chemistry and the reason that I came to UC-San Diego aside from the change in the weather from Minnesota winters to San Diego winters was because Jim wrote me

a letter saying that I would be able to work on the early material back from the moon, assuming that the moon landing in 1969 actually produced some material and that just sounded like an offer I couldn't possibly turn down and it was a wonderful experience for me to be a part of the lab in those days. You've talked about marvelous times in the early days of UCSD before there was a UCSD when it was just Scripps and I think that there were also marvelous times in those early lunar days.

ARNOLD: Yes, and we certainly do plan to talk about them in another session.

KOHL: How did this early work on the meteorites, though, and the cosmic ray-produced radio activity in that affect the lunar stuff, was there—-?

ARNOLD: Very, very much, it was absolutely driven by that. We had some ideas. Well, in particular this is something I'm not quite sure of and Lal may have a better view, the notion of solar irradiation, solar particles was somehow in the air in those earlier days but I don't remember it being—. I don't think it was a serious part of our plans until after Apollo 11 when we saw the first effects. I think that's right.

LAL: What I remember is that you had already a sanction from the committee to get a sample of about 10 grams or 20 grams but it was then I remember that one of these periods when I visited ____ [*inaudible*] then I said we need at least a depth profile from the surface and you can get a complete energy spectrum of this solar flare and you need about 100 grams so Jim was very cautious. He said, "A hundred grams of lunar sample," he got worried about that but he thought about it and saw that this was conservative and it was needed. So, he says, "Okay, I will try" and his very first request, he got it. So that's how it actually happened, according to the way I remember that.

ARNOLD: Yeah, I—. I—. *[crosstalk]*—

LAL: You had an earlier sanction for a smaller amount and you would not have got a depth profile.

ARNOLD: Yeah, the—. That I think is correct. We were not thinking in terms of depth profile. We were too much thinking in terms of our earlier experience when we got a big chunk of meteorite, typically half-a-kilo or by that time, maybe a little less, got a little better detectors but in terms of the scientific program it was an enormous commonality, one of the great things that we had on the moon, which we had never had before, leaving aside the solar cosmic rays, which I think were really basically a discovery was the fact that here finally we had a place we

knew where this sample came from. We were still in the process of learning and all through the '60s we were working on the issue of where meteorites came from. I was doing these Monte Carlo calculations and so on, stimulated by Urey and the—. So, we knew that we were one astronomical unit away from the sun and that was all clear. The benefit of that was that we didn't have to think about gradients of cosmic-ray flux with distance from the sun and especially in the early '60s we had the idea and so did John Simpson who made the actual observations that the gradient might be quite steep and that say at the distance of the Asteroid Belt it might be—the flux might be much higher than it was at the earth and in fact I think John's earlier—earliest measurements kind of looked like that. I'm not quite sure but at any rate of course he and Frank McDonald cleared all that up but that I would say if you hadn't read these papers you wouldn't have had any idea what we were doing because this is where the program was basically formulated and the opportunities that we had grew directly out of this, I would say.

KOHL: Well, certainly one of the ideas behind some of this early work was to understand something about time changes, if there had been a time change.

ARNOLD: Yes, indeed.

KOHL: A temporal change in the cosmic-ray flux and that was a question that's still under investigation.

ARNOLD: Yes, it's narrowed a lot but it's unquestionably still an open question to a degree. It's—. We've been wrong—. I've been wrong some of the time. It would've been more fun in some respects. Let's get Lal's opinion on this. It might've been more fun if we had seen variations.

LAL: Right.

ARNOLD: And in fact Vosaga [?] in his precise work on a billion-year time scale did see them but that is still ambiguous what it means so we have nothing definite but at the start of course it was possible if you were very imaginative to believe that the cosmic rays had only turned on in 1900 so that when Hesse detected them in 1912 they were brand-new but that of course it was a silly idea but really the time fluctuations beyond the record as it existed in the 20th Century were not known at all and so we were in virgin territory. I often used in later years and maybe even at the beginning the analogy, the metaphor of turning a screw into wood or something like that, we'd learned something about the constancy of the cosmic rays and that would help us understand the history of the meteorites and then we would learn something

more about the history of the meteorites and then that would help us go back and refine our study of the history of the cosmic rays and we would *[crosstalk]*—

LAL: A sort of boot-strap process.

ARNOLD: That cycle.

KOHL: Right.

ARNOLD: Yes, that's right, a boot-strap process. That's the other very good analogy.

LAL: Yeah.

ARNOLD: Yeah.

KOHL: Well it goes back to your phrase that the meteorite is a poor man's space probe.

ARNOLD: Oh, is that my phrase?

KOHL: Which is—. I believe it's your phrase. Is that?

ARNOLD: Well, I might've stolen it from somewhere, I don't know.

KOHL: I believe it's yours so *[crosstalk]*—-

ARNOLD: Okay.

KOHL: I attribute it to you. When did you move to the upper campus?

ARNOLD: Uh, '63, when we came back from India, the lab was on the upper campus.

KOHL: Uh-huh.

ARNOLD: So that is—. There was an accident, which Goel wrote me about, I remember, and they were taking our steel shield out of the new Scripps Building and it fell over. Luckily nobody was under it and I certainly heard about that and it was also a little bit adventurous so I was there was moved in. We had to put some special beams in the counting unit.

KOHL: You're talking about the Iron Pig?

ARNOLD: The Iron Pig.

KOHL: Yeah.

LAL: Mm.

KOHL: Which weighs—-?

ARNOLD: Three tons.

KOHL: [*Laughs*] Right.

ARNOLD: And I remember the rigger, the workmen who—expert workmen—skilled workmen whose job it was to bring that 3-ton object up into the lab put it on the big elevator in the back of Mayer Hall, pressed the button to come up to the top floor and walked out of the elevator *[laughs]* and climbed the stairs. I found out later, the elevator said it had a 6,000-pound rating and he didn't wanna be 6,001. [*Laughter*]

ARNOLD: And when we walked along—. When he walked along that balcony into finally around and into our lab, we were told later by the Buildings and Grounds people that we should never have done that but fortunately all was well.

LAL: Where is it now, that whole pig?

ARNOLD: That's as far as I know—. That's a good question. [*Laughter*]

ARNOLD: I think it's with Kuni but I'm not sure.

KOHL: If it is it's storage. I don't think the Iron Pig went with Kuni.

LAL: No, but he doesn't do low-level counting.

KOHL: He took some bricks.

LAL: Ah.

ARNOLD: That's right, he took lead bricks.

LAL: Okay.

ARNOLD: Okay I don't know. That's—. I've probably forgotten. I probably knew.

KOHL: Ah, I'm gonna have to make a note to ask them.

ARNOLD: Yes, Candace, you need to do some research.

LAL: That's a historical piece.

ARNOLD: Yes.

LAL: We should keep it.

KOHL: It certainly is.

LAL: We should not lose that.

KOHL: You need that for your realia collection.

LAL: Right, right.

ARNOLD: I –

[END OF PART TWO; BEGIN PART THREE]

LAL: Why don't you ask the Harold Urey story? Then it would be nice.

KOHL: All right.

LAL: So, whenever you start.

KOHL: We're on?

LAL: Oh, you're on.

KOHL: Oh, okay.

LAL: Yeah, okay.

KOHL: Well, yeah, I'd love to hear the Harold Urey story.

ARNOLD: Well, there are lots of Harold Urey stories but there's one that sticks in all our minds and I think I'm going to tell very briefly a little bit of the early version and then jump immediately to the time, the one dramatic moment that Lal and I shared. No, by golly, I'm gonna go back a step further. When I came to La Jolla, Urey arrived at the same time. We came in the fall of '58 and I found to my surprise there were two research laboratories the same size and one of them was my lab. We haven't described it at as yet but it was a 1,200 square-foot lab.

The whole facing the ocean, the whole west wall was glass with sliding doors and we had in fact to get the blinds because the sun in the late afternoon in the winter especially just drove you crazy and so I had this beautiful view and Harold Urey's lab was in the back, same size lab and all the equipment was fine but the windows were actually looked out on nothing in particular and I thought I knew the pecking order, you know? [*Laughter*]

ARNOLD: And would certainly have been totally prepared to respect it so I asked Harold on arrival why this was so and he said, “Oh, I chose it that way” because he said, “I didn’t wanna be distracted by—from the beauties of nature that I was thinking about to the beauties of nature.” [*Laughter*]

ARNOLD: There on the outside so that—. I said and that held. That was his policy until one day the whales migrate in this area and the whales were migrating south in January as they always do and a whale appeared not far from the end of the Scripps Pier, not very much further out, and a grey whale is a quite astonishing sight and so for an hour, an hour and a half, you and I and everybody in the lab were standing out on our balconies watching this whale. Maybe not so long but we were watching it and talking about it and at lunch I mentioned this to Dr. Urey and I said, “You know, I never wasted any time looking at the view very much up to now but this morning we really did” and when I described it he stopped for a minute and he said, “Well, Jim, next time you can corrupt me a little.” [*Laughter*]

ARNOLD: So okay now we move on and the many incidents happened over the course of the next year and a half or whatever it was but the moment came toward the end of Lal’s stay when another man, a contemporary of Harold Urey’s and a man of much the same temperament, Carl Hubbs, was a marine biologist at Scripps, discovered that a dead whale had washed up on the beach in front of Torrey Pines Park, about two or three miles north of Scripps so he happily got an appropriate boat and he and his graduate students jumped in the boat and they went up there and they started cutting up the whale and making all sorts of observations but this was very inconvenient so he decided that wouldn’t work and he and his graduate students got _____ [inaudible] whatever it was they dragged that dead whale back into the water and beached it right in front of my lab, down below, on the beach. [*Laughter*]

ARNOLD: Now that is something extraordinary. I had lived in Chicago when the stock yards were still there and the wind would blow from the wrong direction. It was pretty impressive but nothing like this and we were finding all sorts of excuses to go to the library and this sort of thing and literally the third day Harold Urey noticed the smell and as I learned the first part of the story, he went into his lab and he said to these people, “What is that terrible smell?” And they said, “It’s a dead whale” and he said, “Where is it?” And he said, “It’s in front of Jim’s lab.” So, he went into the lab and he opened the door of my lab in the usual impetuous way. We’re talking of a 70-year-old man and I’m that age now myself so another calibration point.

LAL: [*Laughs*]

ARNOLD: Lal and I we're sitting my desk I can't—. I don't used to wave my hands of the tape but I was behind my desk, desk was out in the room and I was essentially against the wall. Lal was also behind my desk and we were calculating something and we were discussing and in storms Harold Urey and he asked me where the dead whale was and I said, "It's down on the beach" and he started toward the window. When he got halfway to the window, it suddenly hit me what was going to happen and I stood up. I don't know whether I could have but I always thought afterwards that I would've tackled him if I had been on the other side of the desk but it seemed hopeless so I yelled at him at the top of my voice. He didn't hear me. That was the way he was when he was thinking about something. I had many examples in the social occasion, sitting around talking with him, he suddenly stops talking.

You speak to him again, he does not reply. He's thinking about science and some idea has come to him or something like this so I had much experience with that kind of thing. He walked. He smashed that plate-glass window about as big as that wall over there into small pieces. There was glass on the beach and we were scared to death.

You can well imagine. Well the something that protects people in that case. He had a cut on his forehead. He broke one of his glasses fell off and broke and I think he had a cut on his wrist.

LAL: That's all? Nothing, nothing touched him, nothing else?

ARNOLD: It didn't stop him. He noticed that.

LAL: That.

KOHL: [*Laughs*]

LAL: That's right.

ARNOLD: And you know were hovering over him. We were so concerned. Two hours later he was sitting in a seminar asking tough questions, absolutely. This is a little window on the remarkable personal qualities of that man. He was also extraordinarily kind and generous. He was a wonderful colleague and fortunately and at my age I think about this he was extremely durable.

I think he wrote his last scientific paper when he was 85 or something of that sort so it was altogether wonderful experience knowing him but that particular moment stands out very much in both of our minds. [*Laughs*]

LAL: Jim, you should tell about the Buildings and Grounds people what they did next day.

ARNOLD: Yes.

LAL: That's the most—. Because it's—.

ARNOLD: You're right.

LAL: Yeah, yeah.

ARNOLD: You're right. Usually the Buildings and Grounds people were a little slow. Well, you know the place was expanding very fast and they had lots of problems. I don't really want to criticize them but you know if you asked them to come and fix something on Tuesday maybe they would be done on Friday or maybe they'd be done on the following Monday or sometimes rarely even later but that very same—you know the very next day, first of all there was plywood over the thing and the next day they were in there measuring for the new glass. I don't remember how long it was but it was a very short time 'til they had a new, huge piece of glass in there with a broad stripe painted right across it. [*Laughter*]

LAL: They were very scared if I remember.

ARNOLD: Yeah.

LAL: That this had already involved in this, they can really get into trouble so they had just wanted—. They were praying to God that nothing happened to them.

KOHL: Right.

ARNOLD: Yeah, well they were certainly I mean Roger Revelle of course he probably heard the crash because his office was just down at the other end and certainly you can imagine that the news spread like wildfire. The place was small you know then and we all knew about it in no time. Yes, I would imagine that a story in the papers that the—this famous man who's the best-known person there had been damaged very much by this would've been very, very uncomfortable. Yeah, the green flash perhaps I should mention that.

LAL: Yes, please.

KOHL: That's one thought. I had heard from ____ [*inaudible*] people I think from Roger Revelle about the green flash and like most people who hear about it first I thought it was mythical. I thought there really wasn't such a thing and we didn't pay any attention to it until one day, Carl Eckhart came into my lab. Carl Eckhart was another remarkable man in those days, the most famous scientist at SIO before the new wave arrived.

LAL: Right.

ARNOLD: A wonderful theoretical physicist and a wonderful man.

LAL: In the class of _____ [*inaudible*] or something.

ARNOLD: Yes, right.

LAL: Above that.

ARNOLD: And oh, he said, “It looks like people have a green flash” so, okay, we all went out with him and we saw it. The green flash, the last bit of the sun that sets is green. I once saw it in the Tata Institute, even bluish-green.

LAL: Oh, right, oh.

ARNOLD: Beautiful two days-in-a-row so after that of course we developed a kind of green flash ceremony in my lab down below.

LAL: Right, right.

ARNOLD: When the sunset time came around if anybody noticed you would say, “Green flash” and that was like as if you were having tea time or something, we’d all go out and stand on the balcony and talk to each other as human beings while the sun continued to set and finally we would comment on how good or not good a green flash it was. There were people who came into my lab skeptical and some of them left skeptical but in my experience, I haven’t encountered many people who have seen a green flash unless somebody showed them how to see it or how look at it.

LAL: How to see it because I couldn’t see it for a long time because I was thinking there’ll be a flash, you know?

ARNOLD: Yes, the name is [*crosstalk*]—

LAL: My English problem. I was never thinking the disc, then when Jim told me [crosstalk]—-

KOHL: Yeah.

LAL: The disc becomes green then I started seeing it every day practically.

ARNOLD: Yes, yes.

LAL: It was so easy.

ARNOLD: It was very good down there. It's much harder if you're here [crosstalk]—-

LAL: Now you can't see it anymore because the sky—the horizon is yellow and red.

ARNOLD: That, too but also, we are much—. The horizon is much farther away up here on the upper campus.

LAL: Uh-huh, uh-huh.

KOHL: We used to see it before the trees grew too tall.

ARNOLD: Yes.

LAL: Right.

ARNOLD: Now and then.

KOHL: Yeah.

ARNOLD: Now and then but it—not so—I noticed a big difference when we moved up.

LAL: If you are not continuing it very much further I want to tell you one story of Harold Urey [*crosstalk*]—-

ARNOLD: Please

LAL: In which I was involved, it's nothing to do with the Scripps as such directly so once when Aruna and I were here, Harold ____ [*inaudible*] actually told us that we can use their guest house because nobody is there so we were living in the guest house and then Harold got a medal from the Greece Academy, the Greek Academy so then he and Frieda went away and they told us to look after the house and we said we are very glad to do that and during their absence, which was about 8 days or so or 9 days somebody broke into their house and we didn't know about this and there were several of the drawers of the closet open and things were removed there so we were very upset that this all happens during our stay there and we were feeling terrible so the day Harold Urey, I received him, brought him home about 8:30 in the evening and we were wondering whether we should tell him tomorrow and let him sleep [laughs] tonight but I said that knowing him I mean he would probably get very me that "Why didn't you tell me yesterday?"

ARNOLD: Yeah.

LAL: "Why are you telling me today?" So, I told him. He immediately goes there. He opens this, he says, "Yes, my medal, this thing is gone but don't worry that's imitation medal, the real medal is in the safe" and all this but there was a watch, which was very sentimental for me and this and that and he looked and then he said, "Okay, Frieda, let's go to bed." So, he was more or less separate but I was feeling terrible about this. Two days the fellow who had stolen all these things, he came to know by reading the medal copy and that he had stolen—did a burglary in a Nobel Prize winner's house and he felt extremely guilty so he wrote in very bad handwriting so he's not recognized by police and all that saying that he didn't know that you were such a great, distinguished person, I am returning everything which I took from your house and here is $5.00 for you to fix the window."

KOHL: [*Laughs*]

LAL: "But one request I have is please do not report and do not show this to the police, they might trace it to me."

ARNOLD: [*Laughs*]

LAL: So, I felt so relieved that he—that this matter—. But can you imagine a thief having conscience? [*Laughter*]

ARNOLD: Yeah, oh my God.

LAL: [*Laughs*] And I just cannot ever imagine that so this was something, which is a great story in our lifetime.

ARNOLD: Yes, I should say so.

LAL: Okay, I—. You are the boss; you decide whether you want to continue or you are tired. It's getting to be 5:00.

KOHL: Do we have time on the tape or—-?

ARNOLD: I don't have—. I've run down a little but I—.

LAL: Why don't you tell us something?

KOHL: No, I—.

LAL: Okay.

KOHL: This is not my era. The only other thing that I kind of was curious about—. I mean there are a number of bits and pieces in here but when did Norman Fong and Florence start with you?

ARNOLD: Okay Norman was the first of the two. I had a technician when we first came. Of course, we advertised for a technician, the one in Princeton did not come with us and I wouldn't have wanted him much anyway but Maurice Anderson [?] was the first one who was a wonderful character, a Canadian, I don't wanna spin out the stories about him but he was a very interesting character and then he left. He got some job that was more suitable to him and he needed more money and so on and so I advertised for another technician and we had four or five people and Norman turned up and we took to each other. I remember that interview.

LAL: Right.

ARNOLD: I liked him and I don't know whether you were involved or not.

LAL: No, I was not directly involved.

ARNOLD: But at any rate [*crosstalk*]—

LAL: Yes.

ARNOLD: He certainly came before you left.

LAL: Right.

ARNOLD: And he was an immediate success of course.

KOHL: Right.

LAL: Very helpful.

ARNOLD: Yes.

KOHL: Yeah.

ARNOLD: Uh—. Ingenious and you knew him and we all knew him and that was the beginning of a very long association. At that time my secretary was another wonder woman who is Margie Cassidy [?].

LAL: Mm.

ARNOLD: Who was Ernest Lawrence's daughter, by the way.

LAL: Right. [*Laughs*]

ARNOLD: And an extraordinary person.

KOHL: That's right.

ARNOLD: Entirely different from Florence, at that time a very young, very attractive young woman, very tall, and wonderful sense of humor, very smart. Then she left. Her father-in-law, Cy Cassidy, her then father-in-law, hired her away from me, which I was a little annoyed with him about and I had a second secretary named Clara Fleming [?] who is still around in Solana Beach someplace and I run into her now and then and then Florence, I think, was 67, something like that and again you know Clara went away and Florence came around again, was looking for someone and Florence was available and, again, it was a situation where I think we immediately found each other congenial and you both know what a remarkable secretary she has been and of course that did turn out. I mean that was another team of a different kind that that was Norman and Florence and myself, everybody else came-and-went but for a very, very long period it was the three of us.

LAL: How many years you said total, Norman Fong?

ARNOLD: Well, I think it was about 25 years.

LAL: Twenty-five years, that's for a technician to work with somebody for, right.

ARNOLD: And of course, he was a—. Yeah, his background training was in physics and electronics and that sort of thing as it was electronics-with-vacuum tubes as it was back in the primitive days but—and we had other people. We had quite a few other people, an electronics technician, electronic engineer, on our staff from time-to-time and then Dalia [?] in the end as a chemist and chemistry technician but Norman had a place that none of the rest although some like Larry Pennon [?] I feel close to but—.

LAL: And he was a unique help postdoc because they didn't know where to go so everybody will go to Norman Fong for help, you see?

ARNOLD: Yes.

LAL: So that was really—.

KOHL: Not only was he good at fixing things and building things and making things work but he knew everybody [*crosstalk*]—

ARNOLD: Yes.

KOHL: And he could always find a way through his systems.

LAL: Yes.

ARNOLD: The great—. We used to you know go and drink coffee with other technicians and I noticed at once that we got value out of this.

LAL: Right.

KOHL: [*Laughs*] Right.

ARNOLD: It was a little on the top of Mayer Hall a little bit more extended still there was this kind of—what shall I say—circle of technicians and they you know if he couldn't solve the problem or if he didn't have the thing that was needed, he knew who to go to.

LAL: Right.

ARNOLD: And the work got done. He was—. He always loved to have something different when we—we were—we had these bombardments that you were preparing, you mentioned earlier, Lal [*crosstalk*]—-

LAL: Yeah, yeah.

ARNOLD: On the roof, that was quite a logistic challenge to get all those first of all to get even to buy full-sized gas cylinders then to coat them properly, then to reassemble them so they were safe with a lot gas in them to get them shipped and get them up on the mountain, all of that had to be arranged. Later we haven't talked about the gamma ray experiment stuff, which started at about that time. I'll have session on that, too, and he was deeply involved in that and all sorts of things that h had to do and it was great and he's still with us. I mean he's still around San Diego and I still see him once in a while and he's a real friend, too.

LAL: But I must say, Maurice Anderson, for example, he was not very much of a help but Jim was very tolerant. He didn't take any strong action on him. Actually, I remember Maurice Anderson once because I heard everything what you said, if Maurice Anderson wanted his salary to be raised so he went and told Jim a long story and after it's one to cut it short. I don't want to take too much time on that. So essentially at the end Maurice Anderson says, "Jim, isn't this a very sad story?"

And Jim of course knew that he is trying to get a raise from him [*crosstalk*]—

ARNOLD: Of course.

LAL: And all this so you know what Jim said? You won't believe it. Jim said, "Yes, it's a very sad story." [*Laughs*] That's all he said. [*Laughter*]

I'll never forget this, you know, because then Maurice Anderson knew that [*laughs*] nothing could be done. [*Laughter*] But this is the most pleasant way of disagreeing. [*Laughter*] Because the acting is appearing now [?].

KOHL: Totally.

ARNOLD: No, no, no well maybe it is time to wind this up.

KOHL: Right.

ARNOLD: I think we're getting close.

[END OF INTERVIEW]