

Report

Oral histories in meteoritics and planetary science—XV: Donald D. Bogard

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(Received 06 December 2011; revision accepted 10 January 2012)

Abstract—Donald D. Bogard (Don, Fig. 1) became interested in meteorites after seeing the Fayetteville meteorite in an undergraduate astronomy class at the University of Arkansas. During his graduate studies with Paul Kuroda at Arkansas, Don helped discover the Xe decay products of ^{244}Pu . After a postdoctoral period at Caltech, where he learned much from Jerry Wasserburg, Peter Eberhardt, Don Burnett, and Sam Epstein, Don became one of a number of young Ph.D. scientists hired by NASA's Manned Spacecraft Center to set up the Lunar Receiving Laboratory (LRL) and to perform a preliminary examination of Apollo samples. In collaboration with Oliver Schaeffer (SUNY), Joseph Zähringer (Max Planck, Heidelberg), and Raymond Davis (Brookhaven National Laboratory), he built a gas analysis laboratory at JSC, and the noble gas portion of this laboratory remained operational until he retired in 2010. At NASA, Don worked on the lunar regolith, performed pioneering work on cosmic ray produced noble gas isotopes and Ar-Ar dating, the latter for important insights into the thermal and shock history of meteorites and lunar samples. During this work, he discovered that the trapped gases in SNC meteorites were very similar to those of the Martian atmosphere and thus established their Martian origin. Among Don's many administrative accomplishments are helping to establish the Antarctic meteorite and cosmic dust processing programs at JSC and serving as a NASA-HQ discipline scientist, where he advanced peer review and helped create new programs. Don is a recipient of NASA's Scientific Achievement and Exceptional Service Medals and the Meteoritical Society's Leonard Medal.

ARKANSAS, PAUL KURODA, AND CALTECH

DS: How did you become interested in meteorites?

DB: Coming from the oil fields of west Texas, I was an undergraduate major in geology at the University of Arkansas in Fayetteville, the city of my birth. I had always had a broad interest in natural science, and I took a course in astronomy taught by Davis Richardson, a mathematics professor. During that course, he passed around the main mass of the Fayetteville meteorite, which at that time had not really been studied. I had not thought about meteorites before, but I was intrigued over the story he told about them. This was about the time I switched my major from geology to chemistry, and became aware of the research projects being pursued by Professor Paul K. Kuroda and his several

graduate students. A short time after that, still as an undergraduate, I had a summer research project for which Professor Kuroda was my mentor. I didn't work with Professor Kuroda, but with one of his senior graduate students, Hirofumi Arino. The project was not on meteorites, but involved chemical separation of fission products produced from uranium. In the late 1950s and 1960s, Kuroda had three types of studies. One used the Cockcroft–Walton accelerator in the chemistry department to produce 14 MeV neutrons to make all kinds of new and unstable isotopes and isomeric states, whose properties were then characterized. This period was the golden age of nuclear chemistry. A number of laboratories were doing this type of work, and that was a very vibrant laboratory at the University of Arkansas. A second theme of Kuroda's research was using radioactive



Fig. 1. Donald Bogard.

fallout from nuclear-weapons testing around the world to understand the process of nuclear explosions and the materials used, but also to understand the global circulation of the atmosphere. The nuclear explosions produced radioactive tracers in the atmosphere, and these would fractionate from one another according to their half-lives and chemical properties. Kuroda had giant rain-water collectors on the roof of the chemistry building, which gathered these particles washed out of the atmosphere. Kuroda also flew dust collectors on high altitude aircraft; this was long before the NASA cosmic dust program. I remember one incident where several Kuroda graduate students were on their knees on the roof of the chemistry department with Geiger counters looking among the leaves for radioactive particles, part of the bomb itself, and we found one. Radiochemistry performed on this single particle yielded a scientific paper. I was not involved in those studies, but in those years if you were a new graduate student you got pulled into a number of things like that.

My undergraduate research, however, was more related to Kuroda's third research theme, which was to determine fission-decay yield curves for various heavy isotopes. At the University of Tokyo in the late 1930s and early 1940s, first as a graduate student and then as the youngest professor in the nuclear science department, Kuroda had participated in pioneering research to understand the basic science of the nuclear fission process. He had some peripheral involvement in the Japanese effort to produce an atomic bomb. At the end of the war, the occupying allied forces forbade the

university from doing any nuclear research, probably with the mistaken premise that any nuclear science might relate to nuclear weapons. So Kuroda turned to studies of natural radioactivities in spring waters. Kuroda won the chemistry prize of Japan award in 1948, and this shortly led to him emigrating to the United States.

DS: Then you graduated with a BS in 1962 when he was still a young member of the Arkansas faculty.

DB: Yes, and I stayed at Arkansas for graduate studies largely because of Kuroda and because his research interests gave me a way to combine chemistry and geology. For my master's work, building on some of Kuroda's earlier research in Japan, I studied the radium decay series in spring waters from Hot Springs, Arkansas. In fact, I have collected samples for analysis under the main street along bath-house row at Hot Springs. Most visitors do not realize that the main river still runs under the street, with many springs directly feeding into it. I have been in the subbasements of some of those bath houses, where the bath houses were built over springs and where the hot water is still coming out.

DS: They are still active. Those bath houses are still in business.

DB: Yes. I would scrape up carbonate deposits and take them back to the laboratory to do the radiochemistry. So I was a radiochemist in those days. The main field of nuclear chemistry withered away because most of the work on new isotopes and new isomeric states had been done. Natural radioactivities became more popular, and remain popular today because they are part of environmental sciences.

For my Ph.D., I switched my research to meteorites. To put it in perspective, I think it was Harrison Brown in about the late 1940s who predicted that the nucleosynthesis of the elements in stars should have produced isotopes that have decayed away but left stable decay products. In particular, he predicted the existence of ^{129}I that should have decayed to ^{129}Xe . In the mid-1950s, as most of us know, John Reynolds at Berkeley went looking for this decay product and found it. I might mention an interesting aside to that story told to me by Gerry Wasserburg when I was a postdoc at Caltech. In the early 1950s, and before Reynolds' research, Gerry looked for this decay product in three different chondrites but did not find it. Unfortunately he picked three chondrites that do not contain the ^{129}Xe excess. Kuroda, in reading Brown's paper, realized—because of his nuclear background—that there should be another Xe isotope anomaly produced by fission decay of ^{244}Pu . Kuroda had never done mass spectrometry but he thought that he would purchase a mass spectrometer and look for this fission Xe.

DS: So to that point he had just done beta counting and gamma counting?

DB: Oh, at that point we had all kinds of varied counting facilities. Alpha, beta, and gamma counting were all used for the decay scheme studies, the transmutation studies with the Cockcroft–Walton accelerator, and the radioactivities in rainwater. I did alpha counting for my master's, because most of the heavy isotopes decay by alpha. I might make an aside for the younger people that in those days counting depended on setting the energy discriminator to a desired range, collecting the data, then setting the energy discriminator to a slighter different energy range and repeating the process. All we had in those days were single channel counters, and you counted one energy at a time. Today, one acquires the entire energy spectrum in one counting episode. We had Tracer Labs' beta counters surrounded by enormous piles of lead and two hemispheres of mercury to keep down the background. I did a lot of that for my master's. You would put your prepared sample on a small paper disk and insert it inside the counter. You would typically have a mixture of isotopes and would count over a period of time so you could deconvolute isotopes with different half-lives. There were a variety of techniques and we had excellent facilities.

And it was dirty chemistry. Contamination with stable isotopes did not matter; one had only to be careful about contaminating with radioactivities.

DS: There was a rumor in the chemistry department that whenever the chairman threatened to take a laboratory away from him he would spill some isotope.

DB: I was in one of those laboratories! We had to brick over part of the floor. Kuroda's students also determined details of the fission yield curves for several nuclides, both spontaneous fission and fission induced by energetic particles. They even studied fission induced by energetic photons, which were produced using the Cockcroft–Walton accelerator. He had in the department many kilograms of pitchblende ore from many parts of the world. You had to have special permission from the AEC to work on uranium ores that still had ^{235}U in them. Kuroda's students would dissolve kilogram quantities of that material in fuming nitric acid and do full scale radiochemistry, to the point that green uranium salts would grow in the hood and acid would eat the hoods apart. Needless-to-say, safety concerns were a little different then. We had a room behind the smaller lecture hall, as a matter of fact, where your environmental chamber was once set up, where we had all kinds of ore material stored.

DS: They were still there when I was hired in 1981.

DB: That was where I had my first office and I could look up and see the stuff. On one occasion I thought that I had better check the area, and a Geiger counter went absolutely crazy. So I pointed this out to Paul. "Oh you

are afraid of the radioactivity," he said, and he found me another spot.

Kuroda had a subtle and effective way of persuading people to his point of view. I have said that if a person had the combination of Paul Kuroda's finesse with the direct aggressiveness of Jerry Wasserburg (with whom I later interacted), it would be an irresistible force.

So let's get back to the ^{244}Pu story and how it decays to produce xenon. Kuroda said that since he had never done mass spectrometry he would buy a mass spectrometer. John Reynolds had developed an all-glass mass spectrometer because it would lower the blank levels of xenon. Xenon being a heavy element tends to be sticky in metal mass spectrometers. Reynolds had a glass blower at Berkeley named Corbet, who was a real wizard at working with glass. So Kuroda bought an all-glass mass spectrometer from Berkeley. Oliver Manuel, a relatively new graduate student with Kuroda at that time; spent a year in Reynolds's laboratory at Berkeley learning mass spectrometry. It was about the same time Bob Pepin was there, and Craig Merrihue was there too, I think.

DS: And Grenville Turner?

DB: Turner was there as a visiting scientist. Manuel came back with the new mass spectrometer. Now Manuel worked on other problems, including measuring noble gases in Fayetteville, the meteorite that first got me interested in meteorites. But he did not work on the plutonium problem. During this time, Marvin Rowe came to the group as a Ph.D. student and I became a Ph.D. student, switching over from the hot springs studies. So we picked up the vision Kuroda had to find the decay products of extinct ^{244}Pu . At the time, Reynolds was interested in those meteorites that had lots of volatiles in them, namely chondrites, especially carbonaceous chondrites. He even said at one time why should anyone ever want to look at achondrites; there's no gas in them? This was where we wanted to look because they contained those elements that tended to scavenge uranium.

DS: The high Ca rocks.

DB: High Ca, high Ba; these elements correlate with U and Pu. So we were looking at achondrites.

There is an interesting story here on how Rowe and I missed the Mars meteorite discovery. In those days achondrites were classified as either Ca-rich or Ca-poor. The Nakhilites were just a subclass of Ca-poor achondrites. Both Ca-rich and Ca-poor achondrites contain Xe due to spallation by cosmic rays. No one had published data for spallation Xe. We noticed that there was a lot of fission xenon in the Ca-rich meteorites, but we had to correct for the spallation xenon in order to get the fission yield spectrum. Strangely, there were two meteorites with no fission xenon, so we measured and published the pattern for spallation xenon for these two

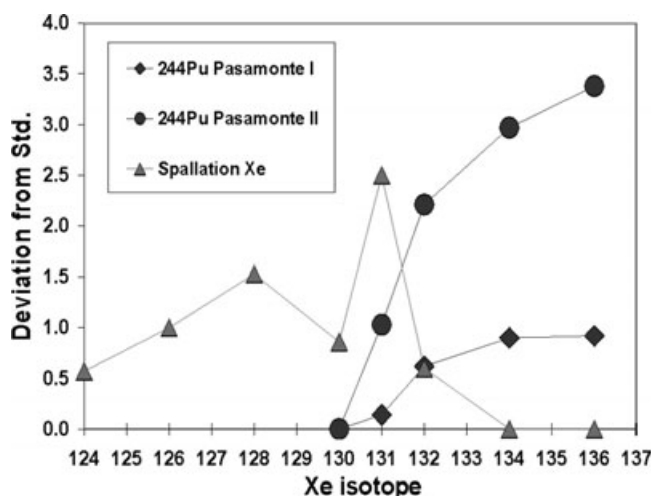


Fig. 2. Excesses of individual Xe isotopes in the Pasamonte eucrite relative to primitive Xe trapped in carbonaceous meteorites. This fission yield curve differed in detail from spontaneous fission and neutron-induced fission of U and was attributed to fission of ^{244}Pu . To correct for Xe isotopes produced by cosmic ray interactions, the yield curve for spallation Xe was also measured in meteorites Nakhla and Lafayette. (Data from Rowe and Bogard [1966] and Rowe et al. [1966].)

meteorites. Then, with the spallogenic component defined, we could subtract that from meteorites that did have fission xenon. The corrected Xe patterns in the Pasamonte achondrite did not look like uranium fission xenon, and we attributed it to fission of extinct plutonium-244 (Fig. 2). Later, Calvin Alexander and others measured the fission Xe produced from ^{244}Pu synthesized in a reactor from uranium. It matched the xenon in the meteorites.

So we had found evidence for the second short-lived isotope in meteorites, the first being ^{129}I . The products of both of those [^{129}I and ^{244}Pu] are of course isotopes of xenon. Now you may ask, what were the two achondrites used to define the cosmogenic xenon? Nakhla and Lafayette! Two Martian meteorites! We wondered at the time, “Why did these things not contain fission xenon?” It never crossed our mind that these were from a totally different parent body!

DS: Okay! These things happen!

DB: Sometimes you miss serendipity in science. Wasserburg missed iodine-129!

DS: So this was your Ph.D. work. You got your Ph.D. in 1967?

DB: 1966. Marvin and I got our degrees in the same year. With Kuroda, we published a number of papers on noble gases, mostly in *JGR* and *GCA*. I went to Caltech to do postdoc work with Jerry Wasserburg. In his earlier years, Wasserburg had worked with terrestrial environments and gases, and that’s what attracted me to

him. I was interested in going back to those interests. When I got there, although Wasserburg’s noble gas laboratory was available, it was rather rundown. He was about to leave for Switzerland on sabbatical for a year. Peter Eberhardt was changing places with him. Wasserburg had wanted to start a project of measuring K-Ar ages of iron meteorites, using the silicates contained in some in irons. Those were the years before the Ar-Ar technique was extensively used. So Peter and I rebuilt the noble gas extraction system. I must say that working with Peter was a good thing, and I learned much from him. He had excellent techniques. He had worked in so many laboratories in his career and knew how to build things.

DS: So Wasserburg had allowed his laboratories to deteriorate?

DB: The noble gas laboratory wasn’t in much use any more. It was a laboratory he had used in the 1950s. As an aside, Wasserburg was already thinking about building his new Lunatic mass spectrometers and was already involved in other work. By that time Wasserburg had worked with Don Burnett using thermal emission mass spectrometers over in the other building, Claire Patterson’s laboratory. Potassium-argon was a natural extension to what they were doing. I worked some with Don Burnett. We measured potassium by isotope dilution on the thermal ionization mass spectrometers. Jerry came back, and for the second year I was there I interacted more with him. We published a variety of papers. Jack Huneke came toward the end of the time I was there. I had already begun some work on xenon in silicate inclusions in iron meteorites, and together we published a paper on that. The last few months I was at CalTech, I worked with Sam Epstein, a very nice person. With Sam’s advice, I built a gas handling system to extract hydrogen from gas-rich meteorites. The question we were addressing was, “What is the hydrogen-deuterium ratio in the Sun?” This was before the Moon landing and lunar samples, and meteorites were the only samples for which we had solar wind. So I built the gas handling system, and we measured the D/H on Epstein’s mass spectrometer. We never published that work because we came to the conclusion that these meteorites had greatly exchanged hydrogen with the atmosphere and that the terrestrial contamination issue was a real problem. Several prior papers had been published on H in meteorites, and we felt those results also reflected contamination.

DS: You were looking at ordinary chondrites?

DB: We were looking at gas-rich meteorites, like Fayetteville, where we expected the solar gases to be, but of course you get other components like weathering products. Then in the spring of 1968, I was looking around for a place to go. Let me tell you a story. I shared an office with Dimitri Papanastassiou at Caltech. Dimitri

introduced me to Greek coffee! He had this brass container with a wooden handle in which he would make coffee. He would fill it to about one-third with the most finely ground coffee, ground like flour, add one-third sugar, and then he would fill it with water and heat it over a Bunsen burner. You were wired after one of those! And if you didn't wash out your cup right away it would dry and you would have to use a chisel or ice pick to get it out.

DS: How many times did you try that?

DB: Only a few times!

DS: Time to move you on to Houston.

MANNED SPACECRAFT CENTER

DB: After 2 years at CalTech, I was looking for a place to go in spring of 1968. I actually had an offer from the University of Maryland as an assistant professor of chemistry. NASA-Goddard had a noble gas mass spectrometer that they weren't using. The concept was that I would build around that mass spectrometer at Goddard, where they were anxious to have someone use it, and I would be an assistant professor at Maryland. But Wasserburg directed me to Houston and the LRL, which at that time was being constructed. The building had been completed but the science laboratories inside were just beginning. The plan was that when the samples were returned, I would be part of a science team assembled to perform measurements on them. The protocol was that beginning at the time the astronauts departed the moon there would be a 3-week biological quarantine period for both them and anything that came back with them, lunar samples, spacecraft, everything.

DS: You started at the Manned Spacecraft Center during 1968?

DB: 1968. One part of the LRL was set up to quarantine both the astronauts and the spacecraft, but another part of the laboratory was to perform scientific analysis under partial quarantine; the samples would be under quarantine but the people wouldn't. I'll explain that a little later. Anyway, Jerry was becoming very interested in the LRL and he told the people there that they ought to talk to me. At about that time I got a call from Oliver Schaeffer at SUNY. Earlier Oliver had been at Brookhaven National Lab. He was asked by the SUNY administration to advise them on setting up a geology department. He did and he became the first chair. He had the NASA grant to establish inside the LRL a laboratory to analyze for inorganic gases, including noble gases, and it occupied part of the third floor of building 37. The other half of the third floor was for organic analysis, and the PI [principal investigator] for that was Klaus Biemann of MIT. Later, he was the PI on Viking who did the organic analysis, and one of his co-Is, Toby Owen, was involved in the noble gas

analysis made by Viking of the Martian atmosphere. There were other PIs for sample analyses in the LRL under lunar quarantine. Some actually built a laboratory; some only participated in the analysis. Ross Taylor of Australia, for example, gave a lot of advice on spark source mass spectrometry used for elemental abundance determinations and participated in those analyses. There were people there like Cliff Frondel of Harvard; he studied the mineralogy and petrology but didn't actually contribute to building laboratories. There were different levels of contribution by various people at the universities. Schaeffer came to me at CalTech and said that he realized that within the bureaucratic environment of NASA, what he really needed to make his laboratory viable was someone on the inside. This concept had already permeated the NASA management at the Manned Spacecraft Center (MSC, now the Johnson Space Center), because in 1968 they hired several young scientists like myself. The way the early LRL science was set up was that NASA would award grants to a few selected scientists at universities to build specific laboratories, and each scientist would assemble a team of both other university scientists and of recently hired NASA scientists. Many scientists and even more contractor technical persons were involved in those early lunar analyses.

I am going to digress just a little bit to give some background to that. Earlier MSC had hired scientists like Don Flory, Elbert King, and others, and they had some of the early ideas as to how you would handle lunar samples when you got them back. In fact, I remember having a conversation with Don Flory as to how they had to convince NASA engineers of the desirability of bringing back samples, that those dirty rocks could have some value to them. MSC was an engineering center, and the basic concept was: we are going to send humans to the Moon, and we are going to bring them back, and that defines success. The idea of bringing samples back and sample science was not on the early agenda. These early scientists, both inside NASA as well as those on the outside, sold the potential scientific value of lunar samples to the agency.

DS: That was the view among the engineers as late as 1968?

DB: No, I am talking about '64, '65. Many of the early geologists at MSC were involved in astronaut training in the field, and there was a sizeable group of young scientists there by the mid-1960s. But their specific job was astronaut training and advising on science, not particularly to build laboratories. In 1968, MSC brought in about eight young scientists specifically to help build analytical laboratories in the LRL and to work with the quarantined lunar samples. Some of these people were Dan Anderson, Jeff Warner, Bill Greenwood, Grant

Heiken, Don Morrison, Earnest Schonfeld, and of course myself. The radiation counting laboratory, built about 40 feet below the LRL to lower cosmic ray background, was considered one of the premier radiation counting laboratories in the world for those kinds of measurement. Peter R. Bell, originally from Oak Ridge, became the director of the LRL and was also the inspiration behind the F-201 vacuum chamber. The F-201 chamber was designed to process the early lunar samples under a vacuum in a lunar-like environment. However, it was a technology before its time; there were a lot of difficulties, and it was later abandoned. There's a lesson there if you ever want to revisit it for asteroid or comet sample return. A huge amount of money and effort was put into processing samples under vacuum, but it proved very difficult to do, and a switch was made to nitrogen flow cabinets.

Anyway, in 1968 the LRL had a whole suite of young scientists specifically brought in for two purposes: to help build and operate the lunar sample science laboratories, while working with a group of visiting scientists from universities, and to perform biological testing of lunar material exposed to living organisms. So Schaeffer visited me at CalTech in February 1968, and encouraged me to apply for a NASA position, which I did. I visited Houston, I think it was on my way back from the American Geophysical Union meeting in Washington, D.C. in April. As soon as I stepped off the plane it was like a sauna, and I said, "What kind of place is this?" But I was really impressed with the potential of what NASA had here, and Schaeffer encouraged me. Joseph Zähringer was involved in the noble gas analyses. Ray Davis from Brookhaven National Lab was also a PI in the gas laboratory for the purpose of measuring radioactive argon. Davis, who later became a Nobel Prize winner in physics for his work on using argon isotopes to detect solar neutrinos, was one of the nicest scientists I have ever worked with. So, I saw a lot of interesting things going on in those early years at MSC.

DS: And a lot of interesting people.

DB: Yes. It was exciting. So that first visit made up my mind. I came to MSC and helped build the inorganic gas analysis laboratory. Schaeffer, Zähringer, Funkhouser (who was later at Michigan) were involved and we had six contractor people. Finally, the samples came and all I can say is that it was a strange way to do science, behind a quarantine barrier. This facility was visited by the Commandant of Fort Dietrich Maryland in early 1969. Fort Dietrich was where the United States worked on biological weapons. He congratulated us on what a good facility we had. It was a state of the art biological quarantine facility for its time, which really worried us on the inside who knew about all the ways it leaked. But the facility advisors were really serious about

this as a quarantine facility. Most of the scientists there for the purpose of studying lunar samples thought that this was a huge amount of overkill.

DS: Yes. Yes.

DB: The organization of the LRL at that time was that there was a science part of it, that is to say a physical, chemical, analytical side of it, and there was a management structure for that. Then there was a separate management structure and suite of science personnel for the biological part of it, the testing of living organisms and maintaining the quarantine part of the facility. Biological testing involved exposing lunar material to various organisms, everything from minnows to corn plants, all sorts of living things inside nitrogen cabinets—the same type of cabinets we now use to curate lunar samples and Antarctic meteorites. We had agronomists, biologists, veterinarians, and medical doctors, all kinds of people, and that part of the LRL had a different management structure within MSC.

DS: I suppose it is easy to smile now, but at the time it was a new planet!

DB: Oh yes, you cannot imagine. Looking back, now we say, "Well sure that's the way they did it," but at the time we had to invent things as we went along. No one had ever done any of this before. No one really knew how to do it. And remember, at that time we were very ignorant about the Moon.

DS: The likelihood of danger was slight, but serious if it was there.

DB: The people who maintained the biological testing and the quarantine facilities were very serious about this. It was the people on the physical and geological side that were not so serious about the quarantine. Consequently, we went through all kinds of testing of the quarantine, testing of the experimental protocol, and so on. We always carried face masks on our hips; we would have spill alarms during which we were supposed to wear these masks and stay in our place. I remember at least once having spent 16 or 18 hours in the laboratory because a spill alarm went off. Zähringer ended up trying to teach us some dumb German card game but we were all too tired. During spill alarms, you weren't supposed to do anything, you couldn't do any sort of operations, but you couldn't leave the laboratory. So management would call you up on the phone, and if you said "hello" you would get bawled out because you didn't have your mask on. So we learned to answer, "blah, blah, blah blah," and if some other scientist answered, we would take our mask off and talk. There were all sorts of crazy things like that. But during a couple of the missions, Apollo 11 and 12, we knew they were really serious about the quarantine when they determined that during the scientific testing operations there was the potential of

what they called “a spill,” when samples came into contact, or potential contact, with the people. People could go out at night, so it was important for the concept of quarantine for them not to come into contact with the samples. For that reason the astronauts and the people in there to support them, the cooks and so on, stayed there continuously, they never left, but scientists working in the other part of the laboratory could go out. They had to shower their way out, they had to run naked through an ultra-violet-lit room for example, so that they could go outside. Well, on a couple of these potential spills there was a possibility of contamination and on those missions, the scientists involved were moved into the separate part of the LRL where the astronauts were. Cliff Frondel thought that was a good thing because he actually got to sit down one-on-one with the astronauts and examine in great detail what they saw and experienced on the Moon. Joe Zähringer was one of those who joined the astronauts.

In our part of the gas analysis laboratory, we had two responsibilities. One was to measure the noble gases in selected lunar samples, and one was to measure the chemically active gases. We measured inactive gases by mass spectrometry and the active gases by a gas reaction experiment, in which a sample was sent in a rabbit from the F201 chamber through a tube to what was called the Physical Chemistry Test Lab. Remember, most sample operations occurred either under vacuum or inside nitrogen cabinets, so the sample, originally under vacuum, was now in a cell where it could be exposed to a variety of potentially reactive gases, e.g., nitrogen, oxygen, CO₂, and any gas reaction products formed could be examined with a gas chromatograph. So we analyzed the inorganic gases and noble gases while Ray Davis looked at the radioactive gases. The LRL performed a wide variety of experiments. Money seemed to be no limitation in those years, but we were trying to do things that had not been done before in very difficult ways, so we had, as I said, to invent the process as we went along.

As an aside, my experience of how serious some viewed the quarantine came in October of 1969, when I was one of four LRL scientists that went to the Center for Disease Control in Atlanta for a meeting of the ICBC [Interagency Committee on Back Contamination]. This was the biological and quarantine oversight committee made up of people from organizations like public health, the departments of health and agriculture, and the universities. I was there to present the results of the physical sciences; the other three were there to present results of the biological testing. For the first time I realized that there was a large science community out there that considered these things very seriously. You got a lot of joking about the quarantine among the scientists

at the LRL, but we went along with it because we had to, but we weren't true believers.

DS: Just geologists.

DB: For example, if you had any kind of equipment service personnel come into the LRL quarantined area, you did not tell them until after they had come in that they couldn't take their equipment out until the quarantine period was over. This did not make them very happy.

DS: It's a different culture.

DB: Yes, it is hard to imagine what a different culture it was and what a hard environment it was to work in. Let me give another perspective. During quarantine operations of Apollo 11, there were two thousand members of the press registered to come into MSC, and at any time there would be several hundred of them outside the main door of the LRL. There were two groups of lunar scientists. There was the group that worked on the samples inside the barrier, and by the way we had shifts working around the clock, but the press did not know who we were. We would come and go without being asked questions, although we knew the details of what was going on. Then there was the outside group of advisory scientists who were presumably coordinating the activity. But, because they were not behind the barrier they did not know many details of what was going on. Some of the former group would come out from inside the barrier once a day and give briefings to the science advisory group and the LRL managers about what we were finding. The press knew the outside advisory group and they were the ones bombarded with questions. For a while, Oliver Schaeffer and Ross Taylor played cat and mouse in these briefings. There was this big controversy about the Moon at that time. Is it, as Harold Urey argued, an old primitive body—the Rosetta stone of the early solar system—or is it as Gene Shoemaker and other members of the US Geological Survey argued, young and volcanic, like Hawaii? The age of the samples would be critical. When the first lunar box from Apollo 11 was opened—in the F201 vacuum chamber—it was pretty obvious the rocks were volcanic. They sure did not look weathered, they looked young and fresh. Harold Urey and the proponents of the old primitive Moon were very disappointed. This whole question was very well known to the members of the press. They were much attuned to these two views and what these samples might mean for one side or the other. Shoemaker was busy talking up the attributes of these volcanic samples. But Taylor and Schaeffer knew the age was critical. Taylor's group had measured potassium, and Schaeffer's group had measured argon-40, so we had had a potassium-argon age for days. Schaeffer and Taylor had only reported these values to one significant figure. The advisory

group, wanting to know the rock ages, kept saying that you must know the numbers better than that, and they kept saying “no, no, we don’t.” They played cat and mouse and wouldn’t tell the advisory committee and the outside world what the age was. You had to make life exciting.

DS: As if it wasn’t exciting enough.

DB: Being discouraged about the initial science finding, Harold Urey had already gone back to La Jolla. One day I decided this had gone on too long, and I was scheduled to give the daily report. I gave the K and ⁴⁰Ar results to two significant figures. Those scientists working with the samples had been instructed that we were not supposed to do science, only make measurements. How does one take new data on the first lunar samples without doing science? Well, the day I gave the scientific briefing, I said we are not supposed to do this, but if you combine these two abundances here is an age you get for the rocks. Immediately, Jim Arnold left the briefing room, went to the telephone, and called Urey. He said, “Harold come back to Houston, the lunar rocks are old! You were right after all!”

Well the story got out to the press before Gene Shoemaker had heard of the age, and when he came to the building later that day he was bombarded by the press. “What’s this Dr. Shoemaker, we’ve learned that the lunar rocks are old. What does this mean for your theory?” Gene was denying it, “No that can’t be, you’ve heard wrong.” Of course, Shoemaker and Urey were both right; the Moon is old, and volcanic, and primitive in a way. In 1969, we had controversial theories about the Moon, but we were so ignorant about its composition; everyone was so attuned to those samples.

DS: Very exciting.

DB: I can’t imagine that kind of excitement ever being duplicated. You can only have one first sample return. Even if we bring Mars samples back it will not be the first sample from another planetary object. Not only will it not be the first, we now know far more about Mars from the Martian meteorites and orbital missions than we knew then about the Moon.

DS: For sure. For sure.

DB: It’s hard again to overemphasize the excitement on the part of all participants. We knew we were making history.

DS: Well, that’s what we are trying to capture here.

BACK TO METEORITES

DB: After the lunar sample returns, I was still interested in meteorites and wanted to get back to them. One of the things that attracted me to come to MSC in 1968 was that when I talked to Wilmot Hess, who was the director of the physical sciences part of MSC at the

time, I emphasized that I was not just interested in working on the returned lunar samples but also the long-term possibility of doing research on other extraterrestrial materials at MSC, and I explained my interest in meteorites. He totally agreed. He said that this was his vision also. At that time MSC was also hiring Dan Anderson, whose work I knew, and Dan was planning to install a thermal emission mass spectrometer. I saw the potential there of having both thermal emission and noble gas spectrometry at JSC. Some of the young scientists who had been hired to build the science laboratories in the LRL left JSC after the first few Apollo missions, but a few stayed on, and Dan was one of them.

DS: These were like postdoc appointments, 1 or 2 years?

DB: No, they were like me, civil service positions, hired to build the science laboratories and analyze the samples returned from the Moon. But that was over and the question was what we do now. The LRL scientists were the beginning of the laboratory-based planetary science group still at JSC.

DS: Oh, I see.

DB: A few of the LRL scientists shared my vision of creating programs of long-term research. Also, around 1970, other scientists like Robin Brett, Paul Gast, and 10 new hires brought in by Gast, joined MSC. Two new hires into the LRL, Bob Clark and Mike Reynolds, were recent Ph.D.s of Kuroda. Of all the LRL scientists who participated in Apollo 11 quarantine analyses, I was the last to retire. Of the 10 new civil servants brought in later by Gast, only Chuck Meyer, Everett Gibson, and Larry Nyquist remain today. I was fortunate in that I had an operating laboratory for noble gas analysis on the third floor of building 37. About 1971, I became a PI for lunar sample research, and I remained a lunar PI throughout my stay at NASA. I had two lunar research interests. I was interested in the regolith, particularly the core tubes and the drive tubes. To flesh that out just a little bit, I worked for quite a bit, almost a decade, with Dave McKay and Dick Morris on trying to understand regolith processes. Those were the years when Dave McKay was doing a lot of the mineralogical particle characterization.

DS: A lot of SEM work.

DB: Yes. Dick Morris really honed the I_s/FeO parameter, the magnetic technique to quantify the maturity of the soil. I looked at solar wind and cosmic ray products, depositional history of different layers, and we were interested in the composition of solar wind. We were trying to understand the whole regolith depositional process using different cores and different core tubes. My group alone and in collaboration with others spent a lot of effort on this over the years, and published a number

of papers. For me at least, and I think also for Dick Morris, it became disappointing in that you could study a given core tube thoroughly, think you understand it very well in terms of its history, different depositional eras, but still not be able to predict what the nearby regolith was doing. The regolith operates by a stochastic process. It's variable. So it wasn't clear to us even with all this characterization of the regolith where we could take it. Workers are still struggling with some of these ideas, you know. So by the early 1980s, I quit working on the regolith.

But I was interested in meteorites, and in the early 1970s I went back to meteorite research and with cosmic ray interactions in particular. I worked with people who had acquired recently fallen meteorites, people like Lou Rancitelli and John Evans at Battelle Labs, who measured radionuclides from cosmic ray interactions, while I did the stable cosmogenic noble gases. We published a paper demonstrating the role of the 11-year solar cycle in the production of these isotopes. My group also did work trying to understand details of the cosmic ray exposure of meteorites. Now quite a bit of work had been done in the 1950s and 1960s, especially on irons, but there was just a beginning of detailed work on stones. To calculate an exposure age, you've got to have a production rate for the nuclear products. You can easily measure a cosmogenic product, but in order to know the time period of cosmic ray exposure, you've got to divide the product abundance by its production rate, and that can vary. The rate at which an isotope is produced depends on a lot of things. It depends on the abundance of target elements, it depends on shielding, what kind of chemical composition, which also modulates the cosmic ray flux in the meteorites, so a lot of things that were not really known for stony meteorites. I had Phil Cressy as a collaborator, he was at Goddard at that time; he later went to headquarters as a manager. Phil took apart the Bruderheim chondrite, separating it into different minerals. This work really emphasized how difficult it is to do mineral separations in chondrites; small grains have really intergrown. But as well as it could be done, we got the different minerals, the chemistry was done in the Goddard laboratories, I measured the cosmogenic noble gases, and we determined and published elemental production rates for stone meteorites. Knowing the composition of each mineral separate and the cosmic ray products you can work out factors for the different elemental products.

Later my group at MSC did a different type of study on the Keyes, Oklahoma meteorite, a big chondrite find, about 100 kg, something like that. We brought it to JSC and we had it cored.

DS: Three mutually perpendicular cores.

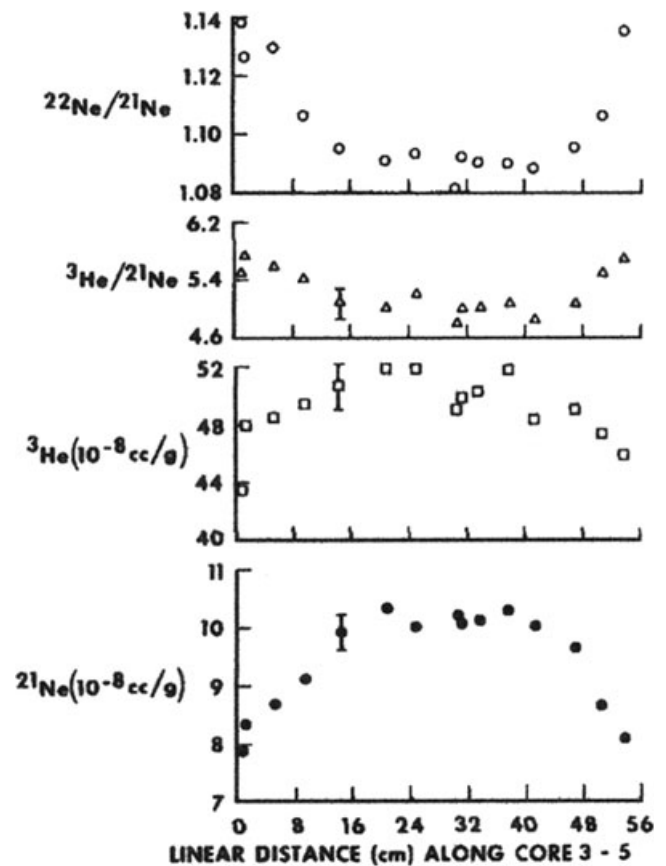


Fig. 3. Profiles of cosmogenic nuclides along three mutually perpendicular cores in the Keyes meteorite (Wright et al. 1973).

DB: Yes, in the big machine shop over in building 10, three orthogonal cores, the x, y, z directions. From all these samples at different subsurface depths, we measured the cosmogenic noble gases, which then gave depth profiles of their relative production rates (Fig. 3). This had been done previously for irons, but had not really been done to that degree for stones. Since we now had the elemental production factors and the depth or shielding factors, Cressy and I then published a paper on putting those together. In this paper we gave cosmogenic noble gas production rates for various types of meteorite of "average" shielding. Those production rates were used by others for a good decade. Other laboratories have done more sophisticated work since then. But, you know, in our field not much work is done anymore on cosmogenic nuclides. It was big in the 1960s and 1970s.

DS: Trying to understand the fundamental processes.

DB: Not just understanding the process but measuring the cosmic ray products in things. Meteoritics has lost quite a lot of capability in that area. Some day we are going to bring back samples from asteroids and Mars, or more samples from the Moon, and we are

going to need to reinvent these capabilities and dig up some of these old papers.

DB: In 1974 I got interested in the Ar-Ar technique using neutron activation.

DS: Tell me about that.

DB: You recall I had done classical potassium–argon dating of iron meteorites at Caltech in the mid-1960s so it was a natural direction for me. Argon is very good for recording moderate thermal histories. It is less valuable for recording when a rock actually forms, especially if that rock has been metamorphosed or undergone impact heating. Grenville Turner had previously worked on some shocked chondrites and demonstrated that argon ages are reset. We knew from classical He-U ages and classical K-Ar ages that L chondrites in particular were shock-heated, but some H chondrites also give very young ages.

DS: They have lost the gas.

DB: Yes, people supposed that impact heating had caused this, and I thought this would be a good project to work on. I taught myself the Ar-Ar method, which was not difficult because I had used ion accelerators and had a nuclear chemistry background. Plus, in those years it was easier to get approval to work with radioactive materials than it is now.

DS: Yes, in those years we could drive around the street with activities. You don't have a reactor here? For Ar-Ar, you would have to send the samples elsewhere.

DB: You know activation analysis for the sake of determining elemental abundances used to be very big. It's largely been supplanted now by other techniques, particularly ICPMS. But in the 1960s, 1970s, and 1980s it was very widespread. Regular NAA requires thermal neutrons, and a lot of reactors can give you thermal neutrons. But, for argon–argon dating you need 2.9 MeV neutrons, and to get these you have to go into the core of the reactor. Some reactors do not permit you to get into the core, or they don't give a high enough flux. As you know, uranium fission produces neutrons of a few MeV, but most reactors thermalize the neutrons as quickly as possible. I tested a number of nuclear reactors over the years, several of which are closed now. Accessing fast neutrons for Ar-Ar dating is becoming increasingly difficult. More and more reactors are closing because of the general public misunderstanding of these things.

DS: . . . And they are aging.

DB: They are aging and no new ones are coming on line. Do you know that most of the income of research reactors these days comes from making radioisotopes for pharmaceuticals. I started using the Brookhaven reactor, which is now closed, I used the Los Alamos reactor, which is closed. Reactors others have used for Ar-Ar dating, in California, in Denver, and in Michigan are

closed. I briefly used the NIST reactor, and I considered using the Oak Ridge reactor. It was really not suitable, and its function has been replaced by the spallation source. They use an accelerator to shoot heavy ions at an uranium target to yield high fluxes of energetic neutrons, and do that on a large scale compared to what cosmic rays do in space.

DS: There is a big reactor in Columbia, Missouri.

METEORITES FROM MARS

DB: I most recently used the reactor in Missouri. But I am saying all this to make the point that there really are few reactors suitable for this kind of work. People doing terrestrial Ar dating use the reactor in Oregon. It's a much smaller reactor, but terrestrial samples have much higher potassium than most meteorites and don't require as high a flux. In any event, I got into Ar-Ar dating of meteorites. And now we have my second miss of recognizing meteorites coming from Mars. In the late 1970s, we knew Shergotty had a very young potassium–argon age, about 200 Ma. I went to Larry Nyquist, because we both were interested in how shock could reset ages; Larry was interested in shock effects on rubidium–strontium ages. I said, look Larry, here is this achondrite with a very young Ar-Ar age, reset by shock heating. It only has two minerals, pyroxene and plagioclase, and they are easily separated. If you want an ideal experiment to look at the effect of shock heating on Rb-Sr, here's the sample. So we both analyzed Shergotty in 1978–1979, and we concluded that impact on the Shergotty asteroid parent body reset the age. For thermal modeling, we used some shock temperatures estimated by Mike Duke.

DS: That was from the maskelynitization of the feldspar?

DB: Yes, which is an indicator of shock in these meteorites. This was well before most people considered that shergottites might be from Mars. We did not entertain at all in that paper the idea that these meteorites are from Mars. We were viewing these as asteroidal materials, just as Rowe and I did for the nakhlites in 1965. After the Shergotty study, I was still pursuing the issue of how the shock impact process affects age, when Elephant Moraine (EET) 79001 comes along. It has these big shock-melt inclusions in it. Well it was a natural sample for me to look at. This thing was so shocked that parts of it had melted.

DS: . . . And the blebs are large, 1 or 2 cm!

DB: It's not the only shergottite with these melt inclusions, but it is the only one with big inclusions. I proceeded to do Ar-Ar dating on the melt inclusions of EET 79001. Well, when you get an age of 6 Ga and you get a strange argon spectrum; I thought something

is different here. Now I had my third opportunity to identify Martian meteorites. This is the summer of 1982, and by this time some people are starting to speculate that these meteorites might be from Mars. My perspective on the speculation at that time is that no one was willing to stick their neck out and definitively say these meteorites are from Mars. People would suggest that they were from Mars, give some argument, and then step back. I was not a believer in the idea; I thought the arguments were weak. The main argument we had known for a long time, which was that they had young chronologies. I had been working with young K-Ar ages for years and I knew that there were many ways of obtaining young ages. Larry had observed that you could shock shergottites and get young Rb-Sr ages. Another argument for Mars was that there were some chemical similarities with chemical data measured by Viking, but these were weak arguments. There were oxygen isotope data, but oxygen isotopes show differences among many meteorites types, and oxygen certainly didn't tell you where they were from. So, I was not particularly impressed with any of these arguments, but I did know that I had some very strange argon data.

Pratt Johnson at that time was working with me in the laboratory, and I should mention one more name. During the LRL period, when we had the lunar samples, there was a staff of subcontractors and several of them worked in the gas laboratory. At the end of the lunar sample return one of those people stayed on with me, Walter Hirsch, and he worked with me on many of the projects involving lunar regolith and cosmogenic nuclides. Walter left and Pratt Johnson replaced him. Pratt had formally worked in the physical chemistry test laboratory of the LRL during the quarantine and later in the lunar sample curator's office.

Back to the story. Pratt and I set up a whole new gas extraction system, starting from scratch. We did this to make sure we didn't have any kind of unusual noble gas memory. Now I had already submitted an abstract to the upcoming Meteoritical Society meeting in St. Louis in October 1982, largely based on data I hoped to get for Ar-Ar dating of the shock event. (Nobody else ever does that, right?) That's the abstract that was published in *Meteoritics* and made available at the meeting. That summer, we analyzed He through Xe in a sample of EET 79001 melt glass. I immediately saw that the gases were unusual, so I sat down and thought of all the possible explanations I could. I came up ultimately with a truth table. Gas characteristics along one axis, explanations along the other. I was very much aware of the Viking results for the Martian atmosphere. Al Nier used a mass spectrometer to measure the atmosphere during the Viking descent, and Klaus Biemann had a mass

spectrometer on the surface that measured noble gases in the atmosphere. Much of that effort was led by Tobias Owen, who was on the science team.

DS: This is all on the spacecraft.

DB: Yes, on the Viking lander on the surface. If you go back and look at some of those measurements the data are fairly crude. If you look at the xenon spectrum published from the Viking results, masses 132 and 129 are clearly present, and some minor blips for the other isotopes. They only measured krypton masses 84 and 86. They also measured the Ar isotopes, but really no measurements for Ne and He. The early value for the $^{40}\text{Ar}/^{36}\text{Ar}$ ratio was about 3000, which later was lowered to 2500 ± 500 . So the data were not real precise. The nitrogen results were interesting because we already knew from the Moon that the $^{15}\text{N}/^{14}\text{N}$ ratio showed a lot of variation, and Viking indicated it was strongly fractionated in Mars' atmosphere. People had speculated that nitrogen might reflect atmospheric loss from Mars over time. So by late September, I did not have an Ar-Ar age for EET 79001 glass, but something much more interesting. In spite of my earlier skepticism, I came to the conclusion that these data really did look like the Mars Viking results. Here was my third chance to argue for a meteorite from Mars, and I was still hesitating. I remember that Jay Melosh said you can't get meteorites off Mars. Theorists had said that a force sufficient for ejection would destroy the rocks. You have to remember this is before the first lunar meteorite, this is 1982, so I was a little nervous but only three or four people in the world knew that I had changed my talk. At the beginning of my talk, I said that I had changed my talk, but I did not give any hints. Ed Anders later said that as soon as I started to consider possible explanations of the noble gas data we had measured in EET 79001, he knew where I was going. But I was ill during the meeting, mild flu or something. I spent much of the meeting in my room sleeping, while people were looking for me. They wanted to talk about these results. When Clark Chapman asked me how did it feel to give the most discussed talk at the meeting I said, "I feel terrible, Clark."

I was surprised at how readily people accepted the conclusion. Bob Pepin and Richard Becker then measured the $^{15}\text{N}/^{14}\text{N}$ ratio in a sample of the same glass I gave them and found it significantly elevated. There was a lot of widespread publicity about the data and story, and even about how Robbie Score had found the meteorite years earlier. The *National Enquirer* even had an article about these fragments being clunkers from alien spacecraft that dumped their load over Antarctica.

DS: Yes.

DB: But I gave the following logical argument. It's like comparing fingerprints. You have to have so many

points of agreement between prints to feel confident of your conclusions. Nobody had measured Martian ages, or Martian oxygen isotopes, or Martian chemistry very well. But with the noble gases and later the nitrogen results, it was a different level of comparison we could now make. That's why so many people accepted the conclusion.

DS: You had the argon ratio, you had the nitrogen ratio.

DB: We also had the $^{129}\text{Xe}/^{132}\text{Xe}$ ratio, and the high ^{40}Ar content clearly was not in situ decay. As I said, the apparent Ar-Ar age was 6 Ga and these shergottites are only 170–180 Ma old.

DS: So were you nervous? Were you excited?

DB: Yes, well, more concerned. Will people accept this argument and, as I said, I wasn't feeling well. But as I said too, I was surprised at how readily people accepted this.

DB: I gave my talk in October 1982. I wrote a paper for *Science*. *Science* couldn't decide what to do with it. Today they'll publish about arsenic-based life in California ponds or cyanobacteria in pieces of Europa, they seem unafraid now. But at that time *Science* was more conservative. The paper got mostly positive reviews, but *Science* really didn't know what to do. They wouldn't accept it, they wouldn't reject it. I gave a very similar talk at the March 1983 meeting of the LPSC. Immediately afterwards a *Science* reporter came up and was enthusiastic about the work and wanted me to submit a paper to *Science*! I said, "You've had it for 4 months now and you won't tell me what you are going to do with it"! They accepted it immediately after that. It was published.

DS: This is the Bogard and Johnson paper.

DB: Yes. It appeared in *Science* in 1983. Also in 1983 the first lunar meteorite was recognized. In 1998, Dan Garrison and I published more extensive data on noble gases in the Martian atmosphere (Fig. 4).

ANTARCTIC METEORITE CURATION

DB: I was involved in the early discussions that started the program to curate Antarctic meteorites, and I was on the initial Antarctic Meteorite Working Group. I also directed setting up the laboratory at JSC to process the Antarctic meteorites.

DS: This is in building 31?

DB: We used the original facility prepared for the early lunar samples when they came out of quarantine, before the lunar annex, 31-N, was built in the late 1970s. Larry Haskin had asked me to think about how those old lunar processing facilities might be used to curate some valuable meteorites for the benefit of meteorite researchers. I wrote a report on this topic.

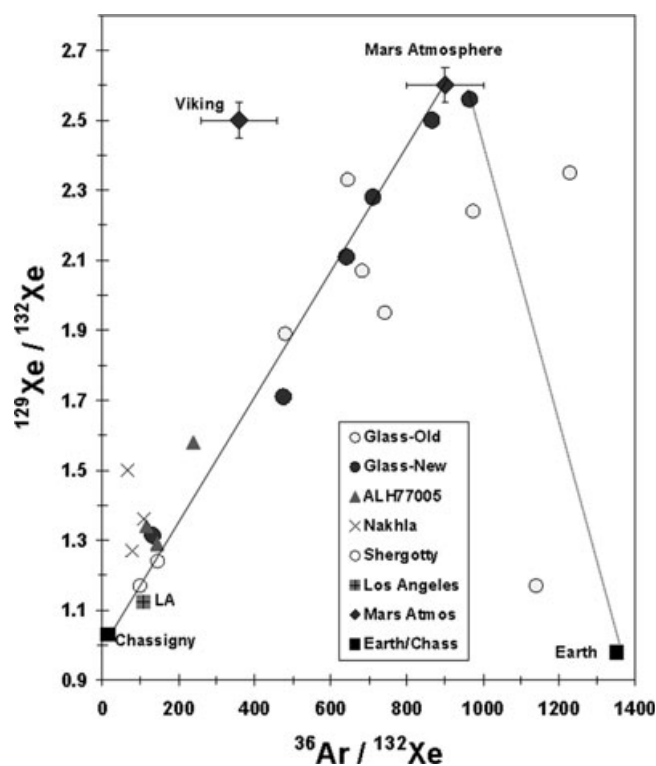


Fig. 4. Following the recognition of Martian atmospheric gases in impact glass of one Martian meteorite (Bogard and Johnson 1983), additional analyses of shocked Martian meteorites further defined the Martian atmospheric composition. Plots of $^{129}\text{Xe}/^{132}\text{Xe}$ isotopic ratio against $^{36}\text{Ar}/^{132}\text{Xe}$ elemental ratio (shown) and of $^{129}\text{Xe}/^{132}\text{Xe}$ against $^{84}\text{Kr}/^{132}\text{Xe}$ derived from analyses of impact glass and other samples from several Martian meteorites define $^{129}\text{Xe}/^{132}\text{Xe}$ and the Ar, Kr, and Xe mixing ratio for the Martian atmosphere more precisely than that measured by Viking. (After Bogard and Garrison 1998a.)

DS: In what context was Larry Haskin involved?

DB: He was director of our planetary science division at JSC after Paul Gast. He thought there were meteorites out there that would benefit from the same careful handling as the lunar samples, and we had a vacant facility. So I wrote a report, which I discussed with Haskin, Bill Phinney and Mike Duke, who was the curator then. Afterwards, John Annexstad, an old Antarctic explorer at JSC, read the report. He was aware that Bill Cassidy had only recently come back from Antarctica with a dozen meteorites found on the ice. This is before most people were aware that the Japanese were finding meteorites there. John commented that these Antarctic meteorite finds might yield some interesting science, and our facility at JSC could curate them. He suggested that we contact Cassidy, which we did. Cassidy liked the idea, but the NSF, which supported Cassidy's Antarctic search, was more

cautious. We contacted several meteoriticists; some liked the idea, some didn't. With only preliminary NASA and NSF support, a committee of scientists was formed to advise on the concept and how it might be initiated. Because meteorites recovered using federal funding would be considered government property, representatives from the Smithsonian Institution were included. The basic concept worked out was that NSF would fund recovery of these meteorites, NASA would handle the curation and allocation for scientific study of the meteorites, and the Smithsonian would be the ultimate depository of the meteorites. So everybody got a little piece of the pie. It took 2 or 3 years but we ultimately got a signed three-agency agreement. Do you know how difficult it is to get a signed three-agency agreement in the federal government?

DS: One is good, right?

DB: Yeah. But everybody saw some potential benefit for their side. And, to be candid, in the late 1970s, I think funding for the JSC curatorial funding was in some jeopardy because lunar research was winding down. "What have you done for us lately?" Suddenly, we had a whole new justification for curation. New samples to work with. So I think it helped preserve the curation funding for a few years. While I am on that theme let me just mention this, and I'll come back to meteorites later. In 1984, I became the NASA discipline scientist for some of the headquarters' grant programs, and early on I had a discussion with Bill Quaide about Don Brownlee's work with cosmic dust particles. Bill suggested that I think about NASA getting directly involved in the collection and curation of cosmic dust. I thought about it and agreed with him. I formed a management advisory group of sample scientists, and for our first meeting we had Brownlee and Bob Walker come and advocate for the science that could be obtained from cosmic dust studies. After lots of discussion, the working group agreed that this was worth NASA involvement. However, they were concerned where to get the funds to get started. I pulled \$100,000 out of research funding that year to pay for initial design and construction of dust collectors that would fly on high altitude aircraft out of Ames. A portion of the old lunar curation facility next to the Antarctic meteorite facility was later modified to process the dust particles. That was the beginning of the cosmic dust program at JSC. That experience curating cosmic dust was very valuable later in building a laboratory and curating the Stardust samples at JSC.

DS: This would have been mid-1980s.

DB: Yes. And as you know cosmic dust grew into a whole subprogram within planetary materials. In my opinion, starting cosmic dust curation at JSC several years after the beginning of Antarctic meteorite curation was begun,

further justified keeping the whole JSC curation effort healthy at a time when research on lunar samples was still in decline. Now back to the Antarctic meteorites . . .

DS: Bill Cassidy had to write that NSF proposal to collect Antarctic meteorite several times.

DB: He had already got the grant to do it for 1 year. He found about a dozen meteorites in the 1977–1978 season. It was in 1977–1978 that the new lunar sample building was built. It was in 1978 that an official multi-agency Antarctic meteorite program began to come together. In 1978 we set up the Antarctic meteorite working Group, and at JSC we tested a variety of materials that could be used to collect meteorites for Cassidy's 1978–1979 trip to Antarctica. A few of us took various materials into a -40° cold room at JSC and tested how various plastics, tapes, etc. would behave at that temperature. Then we loaded a variety of collection materials into metal isopods and shipped them to Antarctica for Cassidy and his team to use. Later, we established a simple method of reporting the meteorites found, the *Antarctic Meteorite Newsletter*. I defined the weathering and fracturing categories still reported today.

DS: A, B, C.

DB: Yes. The lunar curator at that time, Pat Butler, told me that I had ruined some processors by making them think for themselves while curating meteorites, so they were no use in curating lunar samples anymore! The concept in lunar curation is that everything goes by the book. My concept was, "Think for yourself! Reason it out! What's the best way to do this? Let's be inventive."

DS: Take ownership of the work.

DB: Yes, I had some good people working with me. Robbie Score and Trudy King were two of them. We would discuss things. "What is the best way to proceed here." Several people on the outside had a concern that we would become so bureaucratic about handling and curating these samples, that they would not be able to get at them. They were worried that we would treat them like lunar samples. Headquarters got a letter from a good friend of mine, a PI who will remain unnamed, saying that all we were going to do was measure a bunch of weathered meteorites and fill the literature with bad data. I had to answer this letter. That's the way it works in the government; they send letters down to the operational level. The concern proved not to be true. I think that natural weathering is very different in Antarctic meteorites. The level of contamination is generally low. The program discovered different types of valuable meteorites. The first recognized Martian and lunar meteorites were Antarctic.

DS: Primitive materials?

DB: Yes, and others.

DS: And how many meteorites are there in the Antarctic collection?

DB: Now, thousands, and many in Japan. I think it is a good program, evident by the fact that it is still ongoing and still justified within NSF. And by the way, using meteorite curation to help maintain support for overall curation funding can be extrapolated over to NSF. Every time the NSF thought that collecting meteorites in Antarctica was growing stale, the program would come up with something unusual, like a Martian meteorite, that would get NSF all excited again, and they would extend the collection a few more years. You've got to keep the administration excited to keep these things funded.

DS: You have got to keep the scientists excited too.

DB: Oh yes. But to get back, we did very simple preliminary examinations of the meteorites. We didn't want to do research the science community would do. However, we had to report in the newsletter some meteorite characteristics, because different people are interested in different things. We made a lot of thin sections of these samples, sometimes from very tiny chips, and Brian Mason examined them at the Smithsonian. He recognized the first lunar meteorite, but he did not give it away, not even to the Meteorite Working Group. He told us later that he knew it was lunar. Go back and read the original description. He doesn't say it was lunar but he knew! He did say it has lots of lunar characteristics, it resembled the lunar samples. Brian was playing the game fairly, okay.

So now there is this potentially lunar meteorite, Allan Hills 81005. A lot of people requested it, and we had this special issue of *JGR-Letters*, for which I acted as guest editor. It was pretty clear from all the studies reported in that issue that it was lunar. Why was that so clear? Back to my idea on comparing fingerprints. We knew by that time a lot about the Moon. We now knew a lot about this meteorite. Like fingerprints, there were many characteristics to match, many more than I was able to do with EET 79001.

DS: But the same dynamics experts that were telling us we couldn't get meteorites from Mars were telling us we couldn't get . . .

DB: . . . Meteorites from the Moon. There's an important point here. Good data trumps theory.

DS: Eventually.

DB: So the theorists went back, changed some of the input parameters, discovered new ways of kicking objects from the surface of planets, and found, "Oh yes, you can do it after all."

DS: It happened, so you can do it!

DB: Those were fun times. In the mid-1980s, there was a lot of excitement over Antarctic meteorites. As you know Larry Nyquist and I have done a lot of work on Martian meteorites, and we still do. If you look here at my door, I put up my recent publications and most of them concern Martian meteorites. In the last couple of years I have

published about eight papers on Ar-Ar ages of Martian meteorites. A lot of people still work on these meteorites, and we have learned a lot about Mars from them.

DS: We are now up to about 60 meteorites?

DS: There's a similar number for lunar meteorites.

MORE RECENT RESEARCH

DB: I also continued to pursue the theme of how to use Ar-Ar to study thermal events. Let me read you the first couple of sentences out of my recent review paper on K-Ar ages of meteorites, published in 2011 in *Chemie der Erde*. "Whereas most radiometric chronometers give formation ages of individual meteorites >4.55 Ga, the potassium-argon radio-chronometer rarely gives times of meteorite formation. Instead, K-Ar ages obtained by the Ar-Ar technique span the entire range of the solar system and typically measure the diverse thermal histories of meteorites or their parent objects, as produced by internal parent body metamorphism or impact heating." So I am pursuing how to use this chronometer for the evolving thermal history of these objects.

DS: But by stepwise heating you can . . .

DB: Stepwise heating, in a sense, is analogous to doing mineral separations used to get Rb-Sr or Sm-Nd ages. You are separating varying amounts of the parent and the daughter to get an isochron. Stepwise heating in Ar-Ar separates by mineral, but it can also separate grain surfaces from grain interiors, different grain diameters, or phases. I talk about this in terms of Ar diffusion domains.

DS: From the plateau you can get an age?

DB: I take the point of view that a plateau is only part of the way of determining a reliable age. Lot of laboratories still do that. Ar-Ar has become ubiquitous in terrestrial applications. But it is the case that meteorites are a lot more complicated because they have many more Ar components in them. Nothing illustrates that better than Martian shergottites.

DS: They have longer timescales and more events.

DB: They have cosmogenic gas, they've got Martian atmosphere, it turns out they've got Martian interior components in addition to the normal decay products. And then you've got the normal complications of the Ar-Ar technique, such as diffusion loss and redistribution of ^{39}Ar by recoil during irradiation. There can often be many components and parameters involved in interpreting the spectrum. So I take the view that a plateau simply means we have several temperature extractions that show the same age. It is only when I think I have a reasonable explanation for all parts of the age spectrum that I feel comfortable using a plateau to claim a reliable age. If there is a lot going on that I don't understand, how do I know that is a true plateau and

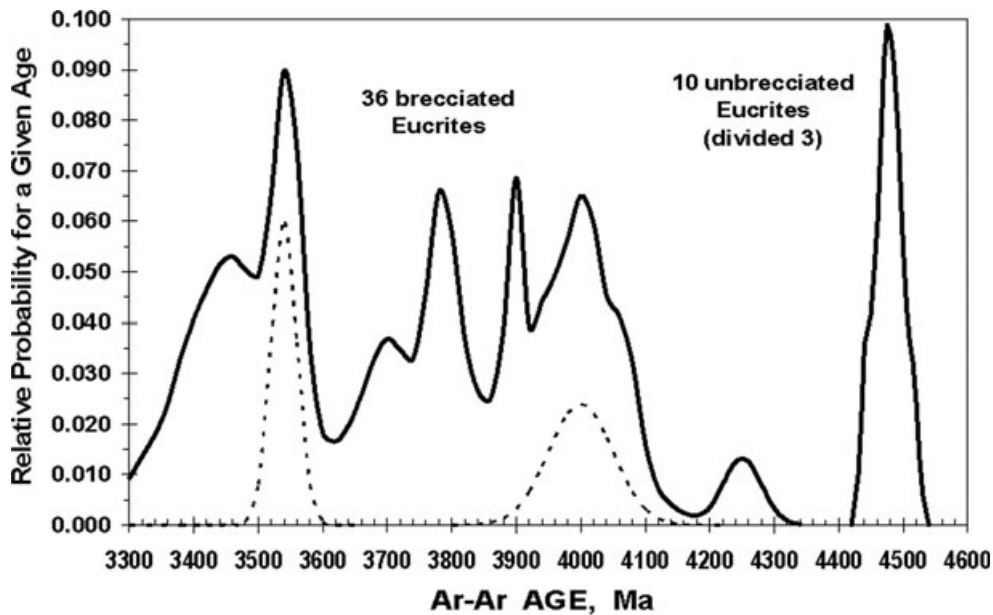


Fig. 5. Decades ago, radiometric ages of lunar samples led to the suggestion that the Moon was strongly bombarded and heated by large objects approximately 3.9 Ga. This age-probability graph demonstrates that this heavy bombardment also occurred on the eucrite parent asteroid, likely 4 Vesta, and in about the same time period. Plotted is a histogram of Ar-Ar ages for 36 brecciated eucrites, whose ages were reset by the bombardment, and 10 unbrecciated eucrites with much older ages. (From Bogard 1995, 2011.)

not a false one? Jisun Park, who has been a postdoc with me most recently, and I have demonstrated that one Martian shergottite gives a nicely linear isochron and a nicely defined age, but a wrong age. Other things are going on, other components are present. This is the kind of complication rarely found in terrestrial applications.

In addition to Martian meteorites, I became interested in the Ar-Ar ages of eucrites. I published a discussion of their ages in my 1995 review paper (Fig. 5).

DS: In *MAPS*?

DB: Yes. Tim Swindle tells me he still regards the 1995 paper as the best review paper in the field. There I decided not just to present some ages but discuss the whole idea of Ar diffusion and the complications in interpreting data with the eucrites. I reviewed a lot of ages, both as a literature survey as well as my own data. I pointed out that the distribution of K-Ar ages of eucrites very closely resembles the distribution of ages for lunar highland rocks. I concluded that both samples reflect the early the cataclysm, the late heavy bombardment period, which no one really understood. I suggested that the same bombardment happened on Vesta and the inner solar system as on the Moon. It's the same population of objects. So this is another suite of samples that we can use to determine the source and timing of these impacting objects. That has been a theme I have followed now for 20 yr.

Dan Garrison, who replaced Pratt Johnson in my laboratory in the late 1980s, and I published another paper

in *MAPS* in 2003 on eucrites that pursued that theme. Incidentally, Dan was a master's student of Charles Hohenberg, but now is a contractor manager at JSC.

DS: And it still all hung together.

DB: Yes, I think so.

DS: This brings us pretty much up to date as far as your research activity. You have talked to us about your management experience with the curation laboratories at JSC. Are there any other recollections about managing funding programs?

MANAGEMENT, PEER REVIEW, AND PROGRAMS

DB: That episode of my career was interesting. Let me tell you how I came to manage NASA research grants. I did not seek the job. In 1984, I was heavily involved in the Antarctic meteorite program and was the Antarctic meteorite curator. At NASA Headquarters, Bevin French went on a sabbatical, and Larry Taylor moved from Tennessee to NASA Headquarters for a year to manage some of these programs. Headquarters was having trouble finding people to manage grant programs, and Taylor convinced Jeff Briggs, who was head of the planetary sciences division at that time, that this was a job he could farm out to the NASA centers. Now at about the same time, or a little before, there was this growing idea in planetary of the peer review process and how it is handled. We may think now that strong peer review has always been there, but that was not

always the case. For many years the program manager had a lot more say in what got funded. That change actually began in planetary science. So HQ decided as a test case to farm grants in a research program out to JSC and get Bogard to manage it. I was vetted by a few people who thought that I would be fair. Mike Duke came into my office one day, this suggestion had come to him, and presented the idea to me, totally out of the blue. No one had done this before. I said okay, but I can't do this and keep up the Antarctic Meteorite curator's job. "We'll get someone else to do that," Mike said. So I was thrown into the lion's den. I had a couple of months to prepare a proposal for the planetary materials program and defend that to the science management at Headquarters. Bill Quaide, head of planetary's grant research, was a big help. It apparently went off very well. I perceived that strong peer review was important and being fair was important. As I said there was also this rising tendency toward peer review playing a more important role in the decisions. From the beginning I wanted to write letters to the investigators, giving them the strengths and weaknesses of their proposal and telling them what the likely chances of funding were. This met a lot of opposition at Headquarters. They did not want to put it in writing and document a lot of what went on. But I thought this was part of the fairness.

DS: This was after the review and after the panel meeting.

DB: Yes. My first panel meeting was in August or September after I took this over in the spring of 1984. I very much used peer review, and I wanted to pass on to the PIs the summary statement from the panel review. This I did, along with an anticipated funding level. That doesn't mean panel summaries were always followed exactly, but I wanted them to be an important part, and Briggs also liked that. He wanted this peer review to be very open. The whole science directorate was interested in peer review, in making it a major part of the process, and was trying to spread it to planetary astronomy, atmospheres, and so on. The director of science also told astronomy and solar physics and some of the others that they should look at planetary sciences for an example of using peer review. To jump ahead, lawyers got hold of this whole process years later and set down the legalities of how to do it. In my opinion, this thwarted some of the early intent of an open program and getting some of this information back to the PIs. I think there is less of this now than there used to be. Of course in those days the planetary review panels met at the LPI. I have often said that you can't avoid bias in the review process. What you need to do is move the bias around so it doesn't stay in the same place. I've also made the observation that people who serve on a panel have an

advantage, if only for the reason that they have a better perspective of what's going on. But I don't think that advantage lasts for long after they leave the panel. Different people, different ideas, and things change. So I have always been satisfied with the way the peer review operated. Was it perfect? No. Do I know a better system? No.

DS: How long did you do that?

DB: 1984–1992. Let me tell you how it ended. Science at NASA headquarters liked the way this was going and wanted to farm programs out to more NASA centers. For a while Origins went with Joe Nuth at Goddard. But before we discuss that, let me get back to something. Quaide and I had a discussion early on with Joe Boyce and some others. NSF was not supporting much in early earth history. Quaide wanted to encourage that work. He had two purposes here, and the second was that he was always looking for new research money. Let me say, the planetary community never had a better advocate than Bill Quaide. Not only was his heart in the right place, he knew how to do things. He was always looking for ways to bring in more money, and it was clear that there was no way to add more money into existing programs. What you needed was to invent new programs. Well he was able to get approved this new program in early crustal genesis, but he was not able to get any new money for it. Quaide had to carve money out of existing programs. I think early crustal genesis did serve the purpose of getting NSF more involved in this research. For a while we had a separate review panel for early crustal genesis proposals, but then we merged this program with the others. Some of the PIs stayed on a while; others picked up NSF money. Another idea was to start up a new program, later called Origins. We had a round-table discussion in Quaide's office. Quaide said we needed to have a program that appealed to all divisions of NASA science, because to get a new program you had to have as broad a support as possible. Joe Boyce advocated a more Earth-oriented program. However, I had been writing reports for Briggs about the interesting new developments in cosmology and samples coming out of planetary, you know, CAIs, isotopic anomalies, diamonds, all this stuff. So I said here is a field we are involved in that's got astrophysics and astronomy. Let's build a program around that. Bill Quaide liked that. So Bill had a workshop to which lots of people were invited, including myself. We fleshed out the concept of an Origins program. Other NASA divisions liked it. We never got much additional money for it, and it evolved away from our original idea, more toward an astrophysics program, but it became a major, separate program. So maybe there is a message there for research scientists. It is hard to justify getting more for doing what you are already doing; it's easier if you come up

with new ideas and new exciting possibilities. Planetary seems to have had more luck in that regard lately.

DS: Did you enjoy your 8 years doing that?

DB: Yes, and I tried to be fair, and most people agreed I was fair. I tried as much as possible to use peer review as a major element, and I think that succeeded. I had to learn as I went along how to work with grants and the whole funding side of NASA. I had one advantage. I was at JSC and the grants were issued through JSC. The JSC grants office did not live in the goldfish bowl that is DC, and the funding people were willing to do things they would never get away with at Headquarters. Let me give you an example.

Often peer review would be over, but I didn't know until well into the fiscal year what my budget would be. I had a number of PIs whose grants came due early in the fiscal year. If they were still unfunded or not told what their budget would be, their universities would get nervous. So I had to develop little techniques about funding. Sometimes I would inform PIs of the larger funded grants an anticipated 12-month funding level. Then, if I got a program budget cut, I would tell them that we would only fund 11 months of that level. First time I did they this, they were just sure I was trying to cut their budgets. The grants people at JSC were willing to go along with that. Then, in my next year's budget I would plan from the beginning to give these PIs a 13-month budget to restore their anniversary date. You had to keep flexibility. I learned how to plan for new investigators, and how to fund a small amount of instrumentation. I would ask the review panels, in addition to prioritizing the new investigators, to rank small requests for equipment. You may remember, we used to tell people in the call for proposals that we would entertain these requests. These became contingency funds. I would then tell the strongly ranked, repeat investigators right away that they would be funded. New people or people on the edge would remain there for a while until I had a final budget. If I got budget cuts, I would take it out of equipment, borderline PIs, and new projects. You had to learn ways to tell the PIs as soon as possible where they stood, and as I said, I did it in writing and that made NASA HQ nervous. I saw my job as making the flow of money fair, quick, and with the least trauma possible.

DS: You said you would say how you came to quit.

DB: Okay. NASA headquarters said they wanted to farm out some grant management to other centers. They liked this way of doing business. Joe Nuth was a postdoc at Goddard, went to headquarters working with Boyce and others, so he was aware of how the system worked. The Origins program was farmed out to him at Goddard. Then Wes Huntress came in as science director. He had a different idea. He wanted to hire as HQ civil servants several new Ph.D.s. He could not hire

them right away, so he brought a few in on short-time appointments, Tammy Dickinson for example. Huntress pulled the grants back from the centers. Then Dan Goldin became NASA Administrator and reduced HQ civil servant staffing. That produced a problem of how to administer research grants. Later much of grants administration was shifted to Goddard.

METEORITICAL SOCIETY

DS: Okay you have told us about your research career and a little bit about NASA administration, why don't you tell us about your involvement with the Meteoritical Society.

DB: I was secretary for 6 years. It started in 1979 at the Heidelberg meeting of the Meteoritical Society during a tour to Rothenburg. We were getting back on the bus, and Paul Pellas and Robin Brett, then members of the election committee, came over and sat with me, and I thought, "Something is up." This is the way they explained it. They had a candidate for the election of secretary of the Meteoritical Society and would I mind if they used my name as an alternate candidate to run against this person, who they expected to be elected. Well it was a set-up! They had decided that I should be the candidate. So I was elected. It was very interesting serving with different people on the committee. There are people who dominate and people who sit back. It makes you think about politics and politicians. A member of Congress to be effective probably has to be aggressive, otherwise he would just be buried by his more aggressive colleagues. Of course, you can be so aggressive that you turn off your colleagues too, so I think there is a balance there that you have to have, and I saw that at work on the Council.

DS: This was which years?

DB: It would have been late 1970s to mid-1980s. I was also chair of the Leonard Medal Committee for 2 years and on that committee for 5 years. It was during those years that Al Nier's widow proposed the Nier Prize.

DS: You received the Leonard Medal too.

DB: That was much later.

DS: 2002.

DB: The meeting at UCLA. I was not even at the Rome meeting in 2001 where that award was announced. Dan Garrison was there with his wife, but was sight-seeing during the announcement. When he returned, people were asking what he thought about Bogard getting the award and he was caught by surprise too.

DS: I think this is a great point to finish on. You got the Leonard Medal. Thank you very much Don.

DB: Yes, that's a medal of peers. I've got various NASA awards, including exceptional scientific achievement

and exceptional service medals, but they are administrative. That's why I particularly appreciate the Leonard Medal. It is an award of peers.

Acknowledgments and notes—This interview was recorded on March 7, 2011, and edited by the author and D. B. I am grateful to NASA for financial support. I am also grateful to Tim Jull, Grenville Turner, and Hazel Sears for reviews and Hazel Sears for proofing.

Editorial Handling—Dr. A. J. Timothy Jull

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