Meteoritics & Planetary Science 39, Supplement, A177–A197 (2004)
Abstract available online at http://meteoritics.org

Report

Oral histories in meteoritics and planetary science: XII. Gerald J. Wasserburg

Ursula B. MARVIN

Harvard-Smithsonian Center for Astrophysics, Cambridge, Massachusetts 02138, USA
E-mail: umarvin@cfa.harvard.edu

(Received 17 May 2004)

Abstract–In this interview, Gerald J. (Jerry) Wasserburg recounts how he entered the Geology Department at the University of Chicago in 1948 but switched to a major in physics, while maintaining links with geology, particularly geochemistry. He earned his Ph.D. in 1954 with a thesis on the new technique of potassium-argon dating under Harold C. Urey and Mark Inghram. After spending a year at Chicago as a post-doctoral research fellow with Urey, he joined the faculty at the California Institute of Technology where he ultimately advanced to the title of John D. MacArthur Professor of Geology and Geophysics. In the early 1960s, Wasserburg sought to achieve unprecedented sensitivity and precision in isotopic measurements by designing and directing the construction of the first digital output with magnet switching and on-line processing computer-controlled mass spectrometer. He promptly named his unique instrument, "Lunatic I," and his laboratory, the "Lunatic Asylum." Using that instrument and later ones, Wasserburg and his research group identified specific nucleosynthetic processes that produced isotopic anomalies in inclusions found in meteorites; investigated the origin and evolution of planetary bodies from the solar nebula; dated the oldest components in meteorites and in terrestrial and lunar rocks; and studied the oxygen in presolar grains and the astrophysical models of AGB stars. In addition to his labors in science, he served on policy-making committees and worked with other members to seek the highest standards for receiving and processing lunar samples and other planetary materials, and to forestall the elimination of the final three Apollo missions. Wasserburg has received many honors, including several honorary doctorates from universities at home and abroad, and the prestigious Crafoord Prize bestowed on him in 1986 by the Royal Swedish Academy of Sciences. In 1975, the Meteoritical Society awarded him its Leonard Medal and in 1987–1988, he served as President of the Society.

UBM: Jerry, I will begin by asking you what first aroused your interest in meteorites.

GJW: My interest in meteorites started when Harold Urey suggested that I should do my thesis on determining the ages of meteorites. I had had some interest in them before that, but not enough to actually want to take them apart and work on them. Minerals and crystals had been my first love.

UBM: How did that come about?

GJW: When I was in grade school, Alfred Hawkins, a professor of mineralogy at Rutgers, who lived a short distance from our house, gathered a bunch of kids, including me, to teach us about minerals and crystals. He also took us on field trips. I was hooked. I persuaded my parents to drive me to Franklin Furnace, now and then, where I could collect minerals (when I could get through the fence).

UBM: I have heard you speak of having specimens from Brazil.

GJW: Ah, yes, in 1939, my family made a trip to the World's Fair in New York where I found the Brazilian Exposition full of beautiful crystals and rocks. I was thrilled. When I got home, I wrote to the sponsor and, in no time, I was put in touch with a Brazilian woman who corresponded with me for years and sent me gifts of beautiful mineral specimens. I never saw her but she had to be a beautiful and gracious woman. I am still in love with her.

UBM: Do you still have any of the specimens she sent you?

Fig. 1. The Meteoritical Society's Leonard Medalist (GJW) and President (UBM) in 1975 at the banquet in the wine caverns at Vouvray during the meeting at Tours, France. The bottle yet to be opened contains a fine Beaulieu Vineyard Private Reserve 1968 from California.

GJW: There are a few topaz crystals in a cigar box in the garage.

UBM: Clearly, you were in graduate school at the University of Chicago when you asked Urey about a thesis topic. Had you come to Chicago straight from high school?

GJW: No. When I got out of the Army (which I had joined by forging an earlier date on my birth certificate with the help of my sister, Libby) I had to finish high school. Up until then, I just had a "wartime diploma." Then, supported by the GI Bill, I entered night school at Rutgers, which is in my hometown of New Brunswick, New Jersey. After two years, Dr. Henri Bader, my mentor at Rutgers, advised me to go to a better school and to take physics, math, and chemistry if I hoped to contribute to geology. That was the best advice I ever got from anybody.

UBM: So you followed it?

GJW: Yes, I did. I applied to the University of Chicago and to Princeton, and was fortunate to be rejected by Princeton but accepted by Chicago. I honestly think the traditional type of program at Princeton would have driven me crazy, but the University of Chicago turned out to be an extremely exciting place. I arrived in Chicago in 1948 and was allowed to sample around and to move ahead as fast as I could. Chicago had highly imaginative, world-renowned scientists on the faculty. Willard (Bill) Libby was beginning to measure ^{14}C in nature. Harold Urey was just beginning his work on oxygen isotopic fractionation and looking into the origin of meteorites, the Moon, and everything else. One semester, Urey and Harrison Brown gave a new course on the origin of the solar system with a good bit on meteorites. I found the course to be fascinating but also a bit confusing. Today, with lots more knowledge, I still find it to be interesting but confusing.

UBM: What did you choose as a major?

GJW: I started out in the Geology Department and then switched to Physics so that I could follow Henri Bader's advice. Since I was the first geology student to get an A in the beginning physics course, they let me in. I began to work on phase equilibria and crystal structure with Fritz Laves and Julian Goldsmith. I finished the undergraduate program in physics and passed the qualifying exam for admission to graduate studies in physics. Arrangements in the Geology Department and the Physics Department were made so that I was allowed to carry through a program in geology without taking stratigraphy and paleontology (if I took graduate courses in physics). When M. King Hubbert used to come visiting, Julian Goldsmith arranged for me to be interviewed by him—that is, to be "grilled" by the King. He told me to study continuum mechanics. At that time, the Geology Department was beginning a shift from strictly standard courses to those with a strong flavor of geochemistry. This shift began with the appointment of Tom F. W. Barth, a petrologist-geochemist from Norway, who brought along Hans Ramberg, a Norwegian specialist in the thermodynamics of rock-forming processes. The two of them were preaching the efficacy of chemical diffusion for forming granites. Also present was Kalervo Rankama from Finland. He was a geochemist of the V. M. Goldschmidt type, using optical spectrography and wet chemical analyses. The heavy tome, titled *Geochemistry,* that Rankama had co-authored with Thure G. Sahama was published in English in 1950. In Ramberg's course, we had to read some of Victor Goldschmidt's papers in the Norske Videnskapsakademi and Norsk Geologisk Tidsskrift (in German). Then, much later in 1954, Goldschmidt's papers were compiled in a book and published posthumously. We got an early draft of parts of an English translation and could crank out copies on the machine we called the purple poop sheet.

UBM: A mimeograph. A messy kind of machine.

GJW: Yes, it was. I used to have purple ink all over me, after trying to run off copies of parts of the book or whatever important subject somebody else had just written about. Then, of course, I met you back then. Chaisson was your name, as I remember it.

UBM: Yes, from an earlier marriage. I was there not as a student but as a research assistant to Julian Goldsmith, who was making synthetic feldspars in his electric furnaces to study their structures. The first X-ray diffraction equipment in the department was installed shortly after I arrived, so I made use of what I had learned in Cliff Frondel's X-ray course at Harvard. For a short while, before Fritz Laves and Mike Frueh arrived, I was the most experienced operator of the X-ray diffraction equipment. I loved the university and listened to lectures in just about everything.

GJW: Did you go to the Institute for Nuclear Studies? That place was full of nuclear chemists, high-energy physicists, nuclear physicists, metallurgists, all sorts of crazy people giving it an extremely high intellectual level. A colloquium was held regularly on Thursday afternoons. Enrico Fermi always sat in the front row, spinning his yellow Scripto pencil. Edward Teller was there, and Bill Libby, Harold Urey, Herb Anderson, Subrahmanyan Chandrasekhar, Leo Szillard (sometimes), Marcel Shein, Murph Goldberger, Gregor Wentzel, Tony Turkevitch, Nate Sugraman, Maria Mayer, Clyde Hutchinson, and on, and on. Plus distinguished visitors from abroad.

UBM: The graduate students also were a pretty remarkable bunch.

GJW: Yes, there was an exceptional group of students. The Urey group included Harmon Craig, Sol Silverman, Stanley Miller, and Cesare Emiliani. Early on there was the brilliant physical chemistry student, Joel McCrea working on paleotemperatures. George Tilton, Clair Patterson, and Ed Goldberg were graduate students in chemistry with Harrison Brown doing the new geochemistry. Sherry Rowland and Roman Schmitt were there, too. Jim Arnold was a post-doc working with Bill Libby. We, students, would mostly hide in the back of the seminar room for fear that someone would ask us a question or call us up and ask us to tell this group of people what we were doing.

UBM: That was a special time shortly after the war when there was the beginning of federal money to support research and people had developed new techniques and were searching for new ideas and new problems. In your opinion, has there been anything quite like it since?

GJW: There were then no federally funded fellowships, though. The Atomic Energy Commission fellowships did not start those until about 1954. The Office of Naval Research was the first to bring in direct federal support. It was the source of funding for Julian's research. There was, however, very little support for geology. Urey had to go to the GSA for a special small grant to develop his investigations of paleotemperatures. As for whether there has been anything like that flowering since then, the question is whether we would recognize it. Today the big problem is the sharp increase in corporate support tied with confidentiality and with many professors simultaneously starting or running companies. This matter needs attention. Furthermore, the thinly spread out National Science Foundation support is not the governing or sustaining agent for research in universities today. It is a monomolecular layer of money that barely keeps things alive and is functioning with sub-minimal rations. The big revolution today has happened in biology.

UBM: How about in cosmochemistry?

GJW: Well, yes, in cosmochemistry the revolution is actually here now. The space program was an enormously great energizer. People leading the field are making cosmochemical observations that are directly related to the fundamental chemistry and evolution of the universe. This is an extraordinary occurrence and, once again, it is forcing people at very diverse levels and fields to speak to each other. I spend about half of my time talking to people in astrophysics and cosmology. Cosmochemistry is as vital to them as their fields are to us. So a great intellectual flowering like the one we remember in Chicago is going on now. Perhaps the main difference is that, instead of being heavily concentrated in one place where everybody in the world you wanted to talk to seemed to be within a stone's throw, the activity today is disseminated among many centers of excellence which require more exceptional talent and more financial support. A big problem is the shortage of truly outstanding young people with a sound quantitative understanding of basic sciences and a deep interest in natural science.

UBM: You didn't do your thesis in geology, did you?

GJW: No, I didn't, but I maintained my link with the department. At first, I began some experiments in the physics lab on the diffusion of oxygen in glasses, with much help from Sam Epstein, a new postdoctoral fellow at that time. He taught me how to do thermo calculations. Then, I found myself hired by Urey as a research assistant mainly running a mass spectrometer. That supplemented my GI Bill income. One of my early duties was to read the proof and check all the equations in Urey's manuscript for his book, *The planets.* This gave me a broad education about the formation of the solar system and thermochemistry. In 1951, I took my Bachelor's Degree in physics and then I asked Urey about doing a thesis under his supervision. Urey asked: "Why don't you try to work on dating meteorites by using the decay of potassium to argon-40?"

UBM: The potassium-argon method was very new at that time, wasn't it?

GJW: It was new enough for me to ask Urey if he thought it ever could be of any significance.

UBM: He must have said, "Yes."

GJW: He did. Urey thought meteorite formation ages were of great importance, but the results being obtained on irons by the uranium-helium method struck him as crazy. He was upset with the numbers that Fritz Paneth in England was reporting. Paneth, a refugee from Germany/Austria was then in the University of Durham. He had done some wonderful work, but his values of U and Th in the metals were giving wild ages—10 billion years, 2 billion years, 20 billion years. Paneth could only get volumetric measurements of He; he could not analyze for ^{3}He, and ^{4}He. That was a very serious problem. He also made great efforts to measure U and Th in iron meteorites. Then in 1950, E. K. Gerling, in Leningrad, issued a preliminary report on potassium-argon ages with what looked like some very funny numbers. He had no isotopic analyses, either. So Urey said, "Well, that's a problem you should look at." I said I would. And, in fact, after having listened to his course with Harrison Brown, I thought the determination of meteorite ages would be an interesting topic to work on.

UBM: Where did you begin?

GJW: Urey arranged for me to work jointly with Mark Inghram in physics and to have access to his lab at the Argonne National Laboratory under the guidance of R. J. Hayden. Thus I had two doctorate "fathers." The rest of the time I was at a lab at the newly established Institute for Nuclear Studies at the university. I started looking into the potassium/argon method and found that the most basic problems had not been solved. For example, the decay constants weren't known, and it wasn't at all clear how the system behaved in nature. What I insisted on doing, not to Harold's joy, was to establish the methodology by working on terrestrial samples first, and to compare ^{206}Pb, ^{207}Pb, ^{238}U, and ^{232}Th ages with ^{40}K-^{40}Ar ages. He was just livid about it when I postponed working strictly on meteorites, but I wanted the study to be well-grounded. Besides, meteorites were not that easy to come by.

Working with Urey and Mark Inghram was quite an experience. Urey would always carry out order of magnitude calculations on everything to see what might and what might not work. He was always full of ideas (his!) and wanted to talk about them. He wanted important results. He almost never came in to my lab. Mark would be in his lab days, nights, and weekends. He would make me argue through an experiment—that is how I learned how to design a good experiment. Mark was an exceptional and creative experimentalist. My wife, Naomi, felt that my life was patterned on Mark's, although Mark hated theory.

UBM: You had a good source of meteorite samples in the Field Museum, didn't you?

GJW: No, I didn't. Urey had no samples. He instructed me to get my samples from Dr. Sharat Roy, the curator at the Field Museum. When I appeared at Dr. Roy's office as a graduate student, I saw these great, big, boulder-sized chrondrites sitting around the museum, but Roy treated me as if I were some kind of madman. The notion was unthinkable to him that he ever should sacrifice a gram of a meteorite to give to a young jerk like me.

UBM: Actually, I'm not too surprised. In 1949, when I was working with Julian and Fritz Laves on the K-feldspar adularia, we went to the Field Museum and asked Roy for samples of adularia. First, Roy took adularia to be some kind of microfossil; then he said he would see what he had and let us know. He soon sent a message that, no, he would not give us any samples of adularia.

GJW: Urey would not try to run interference for me by going to Roy and saying, "See here, give us some pieces of meteorites so we can do our work."

UBM: It might not have helped. Roy announced at an early meeting of the Meteoritical Society that the Field Museum could not give out any more samples because it had issued a new catalog of meteorites and giving pieces away would make the printed list of the weights wrong.

GJW: Whatever his reasoning, he raised an absolutely impenetrable barrier to me, so all the specimens I worked on I bought from Harvey Nininger's catalog.

UBM: Did you really?

GJW: Yes, in fact, I still have the original labels from the samples that I purchased from Nininger in a drawer some place. I am much in his debt for making meteorites available.

UBM: So you set up a laboratory to establish the basis for meteorite chronology by first dating terrestrial minerals.

GJW: Yes, I did. I first wanted to understand the relationship between different age methods. Then, the sources of the rare gases in the Earth: helium, neon, argon, krypton, and xenon fascinated me (also nitrogen—all following on Lord Rayleigh's work in England). I did not want to focus on the ages per se; I wanted to get to fundamentals.

UBM: What terrestrial minerals did you start working on?

GJW: Being familiar with crystal structures, I decided to work on a mineral with a closed lattice that would trap argon, so I picked feldspar. This was a serious mistake, as I would learn later on. I didn't bother to do micas because they are sheet silicates with extraordinary cleavages so I figured they would leak like sieves.

In setting up my lab under Inghram's tutelage, and working closely with R. J. Hayden, I learned how to blow glass, to build equipment, run instruments, measure properly, think logically, and to identify important problems. I learned a lot from R. J. Hayden. I often had George Wetherill as a discussion companion on trips out to Argonne. George had come to the university to study physics a couple of years before I did. He was very smart and had a lot of very interesting ideas. We traveled to Argonne and worked under tight security with guards carrying tommy guns present in the entrance and in the stock room on weekends.

UBM: To protect nuclear secrets?

GJW: I think that was evident. This was after World War II when we all had lots of memories and scars from the war. The bomb tests and nuclear reactors were parts of the very threatening Cold War. This was the height of the "communists in every corner" craze: there were the hearings held by Senator Joseph McCarthy to seek out reds in the government, in the army, and in Hollywood, and the trial of Julius and Ethel Rosenberg for stealing bomb secrets at Los Alamos. Joe Stalin was in charge of the USSR, and Mao Tse Tung took over China in 1949. Darkness at Noon lay over most of Eurasia. The Korean War started in 1950. We were in control of nuclear weapons (or thought we were), and then, the race with the Soviets for the H bomb started. In 1954, Gordon Gray chaired the Atomic Energy Commission's investigation that withdrew his security clearance from the nuclear physicist, J. Robert Oppenheimer, who had previously chaired the Commission but opposed development of the H bomb. Enrico Fermi pleaded with Edward Teller, father of the H bomb, to make his peace with Oppenheimer to no avail. Everything was in turmoil. The time was troubled and threatening, but also exciting. The intellectual and technical horizons were broad and open. There was so much to learn. Edward Teller, at the University of Chicago, was always traveling and would talk to his students during taxi rides to and from the Midway Airport. Otherwise, except for line-ups at his office door, he was unavailable. The great hero to us, students, then was Walt Kelly, the cartoonist who derided the hysteria in his strip, *Pogo.* "This book is good for the burning," announced a half-page advertisement in *The New York Times* for his new comic book, *Lucy, the jellyfish,* that depicted the invisible spy testifying for the book before the House Unamerican Activities Committee. This Committee and McCarthy were often the target of Walt Kelly's wonderful, playful wit. I still have some of the old original comics. Nothing changes!

I remember one day being called into Urey's office. When I got there, he said that he could not see me because he was rather agitated. When I inquired what was wrong, he said, "Some newspaper people are coming to interview me. They want to know what we should do if the Soviet Union were to send an atomic bomb into the New York Harbor on a Polish ship. I do not know what to tell them." I asked what he thought should be done. He said, "I don't know." I suggested that he tell them precisely that he did not know what to do. He responded, "I can't do that, they expect me to tell them something useful."

UBM: Do you know what he did tell them when they arrived?

GJW: No, I don't.

UBM: Did you approach your thesis by doing experimental work right away?

GJW: No. Before I began the experiments, I wrote a paper on the theory of diffusion of argon in crystals and showed the solutions. The results had far broader implications than I had ever imagined. So, knowing about diffusion, I figured that with feldspar, I had picked the right horse, which was wrong.

UBM: But you didn't learn it was wrong right away, did you?

GJW: No, I learned that after I finished my thesis and got my Ph.D. However, despite the error (not known at the time), the thesis itself was very successful. I set up the potassium/argon method in the modern sense, I suppose, with absolutely calibrated isotope dilution determinations of Ar and with precise measurements of potassium and very low blanks. I learned that Gerling's values of potassium were wrong by a factor of two because, at that time, nobody had clean enough reagents. Gerling was a real pioneer and working under extremely difficult circumstances. I took great pains to make clean reagents. Looking ahead I could see that once we got the techniques right it would be a great advantage to be able to apply the $^{40}K/^{40}Ar$ method to the dating of terrestrial rocks, as well as to meteorites. ^{40}K has a long half-life and, unlike uranium which is limited to a few ores and sparse accessory minerals like uraninite, zircon and sphene, potassium is ubiquitous in rocks. At that time, only George Tilton and Clair Patterson were working on zircon, and sphene. Potassium could be used to date igneous and metamorphic rocks of all ages, and authigenic minerals—those that form in sedimentary rocks.

UBM: I have heard a story that you once came to work and found your lab had disappeared altogether.

GJW: Yes, that happened. Not long after I got to Chicago, I met a graduate student in physiology, Naomi Zelda Orlick, and quickly developed a passionate interest in physiology and an ever-increasing interest in her. In 1951, we went off to get married and were gone for two weeks. When we got back, I rushed to my lab at a late hour on Sunday night and found all my equipment was gone! Admittedly, I hadn't told Urey exactly when I was leaving, or why, or for how long. He thought I had simply quit so he gave my lab to somebody else. Eventually, I was assigned space in the basement of the Institute of Metals, which I then blew up in a dumb accident.

UBM: Even after that, they didn't throw you out! You published sections of your thesis as you went along, didn't you?

GJW: Yes. My first paper, with Hayden, redetermined (or tried to determine) the branching ratio of ^{40}K. We measured the $^{40}A/^{40}K$ ratio in K-feldspars from a deposit in Canada that had some uraninites associated with it, dated by A. O. C. Nier. We could find no evidence of loss of argon by diffusion if we used appropriate decay constants, so I was convinced we were on the right track. We could go on to dating ancient igneous and metamorphic rocks and younger sedimentary rocks by their feldspars. Then, we turned to chondrites and obtained $^{40}K/^{40}Ar$ ages for Beardsley and Forest City of 4.57 and 4.8×10^9 years, respectively, and published the results in 1954 in *Physical Review.*

UBM: This must have been at about the same time that Clair Patterson was using the lead-lead method and determining the age of the Earth as 4.55×10^9 years.

GJW: Yes, it was. Clair had recently become a research associate at Caltech, but Caltech was just getting started and had no instruments, so Patterson came back to Chicago to carry out his Pb and U measurements on the mass spectrometer at Argonne. His lab was across the hall from mine and we worked in association some of the time. In 1953, Pat measured the Pb isotopic composition on U-free iron sulfide (troilite) in the Canyon Diablo iron meteorite. This yielded the magical "primordial" lead composition. He refused to calculate an age from this data because he wanted the numbers to stand by themselves. I could not argue him into doing "the" calculation. Fiesel Houtermans, in Bern, immediately recognized what had to be done as soon as the article by Patterson, Brown, Tilton, and Inghram appeared in *Physical Review.*

UBM: Right. And in December 1953, Fiesel, using Patterson's primordial lead value, published the age of the Earth as 4500 ± 300 million years. But by then, Patterson had reported his own calculation at two meetings and been quoted in *Chemical and Engineering News* as saying the Earth is 4.6 billion years old. In as much as he did the crucial lead analyses, he gets the credit. Perhaps your carping at him had more effect than you think.

Wasn't that back when the Hubble "constant" seemed to imply that the universe was only two billion years old, but isotopic analyses were showing that meteorites and the Earth were more than four billion years old?

GJW: Yes. The astronomy professor, Chandrasekhar, whose campus office was in the Institute for Nuclear Studies, would drop in on me now and then to see how the meteorite dating was going. Eventually, the astronomers had to so some tinkering with the Hubble constant and lengthened the age of the universe by a factor of two, and later, by factors of three or more.

UBM: When did you finish your thesis?

GJW: In 1954. My doctoral exam was intense. Hans Ramberg chaired the committee, which included Harold Urey, Mark Inghram, Julian Goldsmith, and Bill Libby. Libby, in particular, asked me about short-lived isotopes instead of the long-lived ones that were my specialty. Afterward, they left me sitting outside for an hour, getting more and more nervous. Finally, the door opened and they congratulated me. They said they had been very interested in my dissertation and began to discuss it but then they got into a heated argument and almost forgot I was there! Ramberg remarked that I had written the shortest Ph.D. thesis in the history of the university—about 30 pages of articles scattered through *Physical Review, Nature, Geochimica et Cosmochimica Acta,* and a preprint of a chapter in Henry Faul's book, *Nuclear geology.* Next, I needed to find a job.

UBM: I would think that, with your broad training, you would be unusually well-qualified and much in demand.

GJW: It didn't work out that way. Nobody knew what to do with a part-geologist, part-physicist, part-mass spectrometrist. It was a strange mix. Fortunately, Urey offered me a position for a year as a postdoctoral research fellow at the Institute for Nuclear Studies (later renamed the Fermi Institute). So I stayed on working with Urey and continued my research while job-hunting. Let me tell you a story about Urey.

Shortly after finishing my doctorate exam, I received a message from Lucille McCormick: Professor Urey wanted to see me. She was the "Vorzimmer Drache" for both H. C. Urey and W. F. Libby. When I entered his office, he was sitting at his desk behind a pile of papers, peering closely at a gigantic K&E slide rule with a magnifying glass attached for "high precision." There were no real computers back then. I stood at attention at the foot of his desk, while he continued with his calculation. After a bit, Urey looked up and said, "Young man, what do you want?" I replied, "Well, sir, Miss McCormick said that you asked to see me." He looked at me firmly and said, "Wasserburg, don't call me sir!" I responded, "Alright, Professor Urey." He then said, "Don't call me Professor, call me..." He stared up at the ceiling and then returned to his calculations. After about five minutes, I just left. Well, I had my PhD, but still did not know what to call him. I remembered that some years earlier, Urey had said about General Leslie Groves, the director of the Manhattan atom bomb project, "He never called me Harold." Well, about ten years after my visit to his office, Urey asked me to call him Harold and, with some hesitation, I did. I greatly respected him: he drove me nuts, but he taught me an enormous amount. Naomi and I became fast friends with "Harold" and his wonderful and gracious wife, Frieda.

UBM: Great story! But did you ever figure out why he told Miss McCormick he wanted to see you?

GJW: Sure. He wanted me to address him informally since I was now a Ph.D, but he couldn't tell me just how.

UBM: When did you learn that your choosing feldspar for K-Ar dating was a mistake?

GJW: It was about then. George Wetherill read my publications and checked my results. Then he called and informed me that the branching ratio Hayden and I had used for ^{40}Ar decay was wrong because of diffusion from the feldspars, which were leaking like sieves. He said that, if I had run micas, I would have had higher ^{40}Ar/^{40}K ratios by a long shot.

I was devastated. This was the whole basis of my thesis. I went to talk with Urey and he said: "Young man, if you find you are on the wrong track, you should get off it and onto the right one as soon as possible."

It turned out, though, that I had been effectively correcting for argon diffusion from the feldspar, so my age numbers were basically correct, but I really was not using the right decay constant.

UBM: So, you had the right answer for the wrong reasons?

GJW: More or less. I got back in touch with George, who was then at the Department of Terrestrial Magnetism (DTM) in Washington, and we wrote a joint paper, along with Tom Aldrich, also at the DTM, that discussed diffusion of gases and made my error clear.

UBM: What was your next project?

GJW: I began dating authigenic minerals to establish an absolute chronology of stratigraphic time. Naturally, I began to use potassic micas. I focused on glauconite, a member of the biotite family that forms green pellets in limestones and detrital sediments. I collected many of my own samples and assembled a suite ranging in age from Cambrian to recent. I also dated authigenic feldspars from some limestones. Meanwhile, I was traveling to and fro, interviewing for jobs.

UBM: Where did you go?

GJW: First, I went to see Willard Libby to ask his advice. I knew there was no hope of getting a faculty position at Chicago. They sent their progeny elsewhere—a wise practice. Libby told me I should go to a national lab and set up a big rock dating system. If I decided on that, I should let him know and he would help. He added that, if I was interested in learning about low-level counting techniques, I should see Friedrich Begemann, a post-doc from Göttingen and Bern, who was counting tritium in his lab. That way, I got to meet Fred Begemann and also to meet Margareta, a student staying at the International House, whom Fred later married. Naomi and I have been friends with Fred and Margareta ever since.

UBM: You evidently did not take Libby's suggestion of going to a big national lab. Where else did you have interviews?

GJW: At the urging of George Wetherill and George Tilton, I was invited to meet with Phil Abelson and Merle Tuve at the DTM. Nothing came of it. They must have decided the DTM already had enough Chicago people. Then, on the recommendation of Julian Goldsmith, I visited Frank Tuttle at Penn State College. Penn State offered me an assistant professorship to teach mineralogy, to set up a mass spectrometer lab, and to open a geochronology program—if I could raise the money for it. It sounded good, except that my mineralogy course was supposed to feature well-logging. My dating of authigenic minerals would come in handy for this, but well-logging didn't appeal to me. Mark Inghram recommended me to Al Nier, who chaired the physics department at the University of Minnesota. Nier invited me to come and look around and give a seminar. He thought I should have a joint appointment in the physics and geology departments. I was cordially received by him and stayed at his home. He showed me the labs and the excellent machine shop. All was going well until I gave my seminar on the measurement of absolute geologic time. Afterward, a questioner asked why anybody would want to measure absolute geologic time. I assumed he must be a physicist and explained that for most of geologic time, we have only incomplete stratigraphic sequences, with no sure way to correlate them or to judge the intervals of time between them. I compared the difference between stratigraphic and absolute dating with that between thermodynamics and kinetics. The questioner was George A. Thiel, chair of the Geology Department, and that answer ended my job opportunity at Minnesota.

UBM: Whatever could have prompted him to ask a question like that?

GJW: Who knows? I do not think he liked what came to be called "black box" scientists.

UBM: What came next?

GJW: N. Allen Riley, who had been on the faculty at Chicago, was running a company called CalResearch at La Habra, California. He already had hired Sol Silverman, a brilliant former student of Urey's. The company was interested in sedimentary processes. Riley showed me all around and then made me a handsome offer. I was tempted. Then, partly through the influence of Sam Epstein, a former colleague in Chicago, and partly because of a talk I had given at the Geological Society of America meeting in Los Angeles in 1954, I was asked to give a seminar at Caltech.

UBM: What was your GSA talk about?

GJW: I presented my work on potassium-argon dating of meteorites, sediments, and the oldest terrestrial rocks. To find the oldest rocks on Earth I had looked for granite cobbles in sediments cut by very old pegmatites that had been dated. With the help of A. M. MacGregor of the Rhodesia Geological Survey, I got some cobbles in which the feldspars gave ages of up to 3.3×10^9 years.

UBM: Those must have been just about the oldest dated terrestrial rocks at that time.

GJW: They were "the oldest" terrestrial rocks at that time and for many years to come.

UBM: I presume that nobody at Caltech asked you why you wanted to do absolute dating of rocks.

GJW: They certainly didn't. Later on, back at home in Chicago, I got a telephone call from Midway Airport one very cold, snowy evening. It was from Bob Sharp, the chair of the Division of Geological Sciences at Caltech. Bob was stopping over between flights from Washington to Los Angeles and wanted to see me then and there. I drove out to Midway and we walked around in the cold wind. He said they wanted me to come to Caltech. I would have to teach mineralogy and part of the field geology course, but they could give me small support to set up a lab. I said I would consider it and went home to discuss it with Naomi. The money wasn't as good as that offered at CalResearch but the outlook for the kind of research I wanted to do was very, very good. We decided to move to California. I had a job!

UBM: I feel I should say, "Congratulations!" So, in 1955 you moved to the salubrious climate of southern California with carte blanche to set up your own research laboratory?

GJW: Well, carte blanche is hardly the right description. I intended to establish an independent research program on rare gases in nature and to apply the $^{40}K/^{40}Ar$ method to the dating of geologic processes. For this, I would have to design and build a high-sensitivity mass spectrometer for noble gases and to set up a chemistry laboratory. For measurements of K, U, and Th, I also needed access to the thermal ionization mass spectrometer (TIMS) modified from Inghram's design that already was there and operating at an acceptable level. The support by Caltech was good, but everything had to be designed, constructed, and made to work. There was no money for technical support.

UBM: Geochemistry was fairly new to Caltech at that time, wasn't it?

GJW: Isotopic geochemistry at Caltech was only three years old. It was introduced to Caltech by Bob Sharp, who had perceived the importance of this new and exciting field that had been pioneered at Chicago. His first recruit was Harrison Brown, who came from Chicago in 1952. Three more arrived from Chicago soon afterward: Heinz Lowenstam, the distinguished paleontologist who had worked with Urey on paleotemperatures, Sam Epstein, a postdoc of Urey's who had pioneered precise measurements of oxygen and hydrogen isotopes and their relationship to temperatures, and Clair Patterson. Bob Sharp also brought planetary sciences into the department, but it was not acceptable to change the department's name to Earth and Planetary Sciences as that did not keep geology in the fore.

UBM: Why didn't Sharp try to snare Urey himself?

GJW: Perhaps he did; I don't know. However, once he picked Harrison Brown, there was no way he could get Urey. Urey developed an intense dislike for Brown. Urey stayed at Chicago for three more years and then he moved to southern California to accept an appointment as a distinguished professor-at-large at La Jolla. He was very unhappy about the mandatory retirement at the University of Chicago.

UBM: Caltech had long been know for its leadership in paleontology, hadn't it?

GJW: Yes, particularly vertebrate paleontology, owing to the great treasure trove of specimens recovered from the nearby La Brea tar pits. The stairway up to my first office had a magnificent ichthyosaurus on the wall, and the hallway had reconstructions of a saber-toothed tiger and a camel. Caltech also had strong programs in structural and field geology and it was very well-known for its outstanding geophysics led by Professors Beno Gutenberg, Hugo Benioff, and Charles Richter.

UBM: I am most impressed with Bob Sharp's introduction, mainly from Chicago, of isotopic geochemistry into the Division at Catech. He had not attended Chicago himself. In fact, he earned his Ph.D, at Harvard in 1935 and to this day, he lists himself as a geomorphologist!

GJW: He is a geomorphologist who had a clear view of the future of geoscience. However, his emphasis on geochemistry brought forth angry reactions from some alumni, who claimed it just was not geology and had no business being there. This outside dissatisfaction continued for many years and was vigorously expressed. Bob was accused of "selling out" to the chemists. Of course, Linus Pauling and Lee Dubridge were big supporters of Bob's efforts.

UBM: How did your research on rare gases progress?

GJW: I decided to build an all-metal mass spectrometer to analyze rare gases. That was before John Reynolds, at Berkeley, had come up with the crazy but brilliant, idea of building an all-glass static spectrometer. I had support from several sources but things went slowly. Meanwhile, I set up a system for separating and purifying the gases. The rare gas work went very well with the studies by Robert G. Zartman on He and Ar in natural gases. I built the Henearkrxe for doing all the rare gases. I also started theoretical studies on the effect of water on silicate melts and wrote a paper that was unpopular for a long time because of my use of theory. Linus Pauling was gracious enough to discuss this work with me. My conclusions proved to be reliable, in the long run. As a result of my theoretical work George C. (Christian) Kennedy, at UCLA, invited me to work with him on the SiO_2-H_2O system. This led to an important study of the upper three-phase region of the system and to the determination of the critical endpoint. I would sometimes live with the Kennedys amidst magnificent pre-Columbian and modern art (including some embarrassing fakes) and an orchid collection. George was a true intuitive genius. He could sniff out important things at a glance and was fearless and mean. George loved a fight. He could not calculate anything but knew where to look and what to do. In spite of his wild shenanigans, we were very close until his death. During that time, I got to know David Griggs at UCLA very well and would visit him in his lab and at home. There was always a mixture of feelings on my part because of Dave's role in the Oppenheimer affair (he testified against Oppenheimer at the Gray hearings), but we got along well and I respected his deep theoretical insights into geoscience problems. Griggs was a real pioneer in many areas scientific, military, technological, and political. I also taught some courses.

UBM: Standard courses, or your own brand?

GJW: Standard field geology and pretty standard mineralogy, at first. The field geology mapping course at Tick Canyon in Los Angeles County was not my cup of tea. The mineralogy was my brand, with as much solid state physics as I could put in. Then, I observed that many of the graduate students knew no math or physics—unless they had been undergrads at Caltech. So I designed a course I called Geomath. In it, I presented the basics of vector analysis, linear transformations, simple linear differential equations, elements of fluid flow, and so on, with specific applications to geologic phenomena. I urged the students to work together and with the graduate teaching assistants, who were from geophysics and could do the math. The results were positive. Student skills in basic math steadily improved and so did their understanding of geologic processes. I taught that course for many years and enjoyed it. I don't know if any of the students did.

UBM: What links did you form with the physics department?

GJW: I had hopes of being able to establish interdivisional contacts, I asked Robert Bacher, chair of the Division of Physics, Math and Astronomy, if it would be possible for me to have a joint appointment. I soon learned that a joint appointment would be out of the question, but I would be welcome to establish contacts with the Physics faculty. This led to my regular attendance at the Kellogg Lab's seminars and the Thursday Physics Colloquia, where President Lee DuBridge always sat in the front row with Bob Bacher, showing that new science (particularly physics) was the center of intellectual activity at Caltech. I soon got involved in problems with the Lauritsens, the Burbidges, Margaret and Geoff, Willy Fowler, Fred Hoyle, Bob Christy, Charley Barnes, and other luminaries. The science was exciting and the Kellogg Lab parties, which went on until all hours on Friday night, were phenomenal.

UBM: We must be progressing in time toward October 4, 1957, so I will ask where you were when you first heard of the launching of Sputnik I?

GJW: I was sitting in the kitchen of my home chatting with Naomi and Fiesel Houtermans, who was visiting Caltech and was our guest for dinner that evening. Fiesel was a Professor of Physics at the University of Bern having left

Germany following World War II. He had built up a world-class research program in nuclear geophysics. We switched on the radio and heard the announcement of Sputnik. Then we went out to stare into the night sky. We saw nothing unusual, but Fiesel said: "Now there will be a new word in our language."

UBM: I assume he meant, "Sputnik," which would be the same in all languages—Russian, English, German, and so on, as well as in the discourse of science. Did you sense at that moment that your career was likely to change significantly?

GJW: I think I did, although I had no idea of just what to expect. NASA was founded in 1958 during President Eisenhower's administration as a civilian, not a military, agency. I am glad to say that, after doing a critical reassessment of our national technological priorities, Senator Lyndon B. Johnson played a strong leadership role in laying out the legislation for the U. S. civil space program. Later on, while he was Vice-President and then President, he continued to lead the country in a peacetime effort of great technical demands and scientific potential, along with the lust for enhancing the international prestige of the United States. The preparations and results would be openly shared with the international community. This was something completely new that would impact industry, universities, and society in general. However, its influence wouldn't be fully apparent for several more years. We were in a race with the Soviets but in a peaceful enterprise with clear manifestation of high technology and, of course, the implied military capabilities.

UBM: What did you work on in the immediate future?

GJW: I took on several problems. One of them was the time lapse between the "last" rapid neutron capture (r-process) nucleosynthetic event and the formation of the solar system. One clue to this would be finding excesses of ^{129}Xe, for which I had searched unsuccessfully in meteorites back when I was working with Hayden. Then in 1960, John Reynolds, using his all-glass mass spectrometer, detected ^{129}Xe in the Richardton chondrite and took it as evidence that the extinct nuclide, ^{129}I, had been present in the early solar system. This was the very first evidence that a now-extinct radioactivity was present in the early solar system and it revolutionized cosmochemistry. Reynolds calculated that 400 million years elapsed between nucleogenesis and the formation of the solar system. However, it was clear that ongoing formation of elements in stars was the real process being dated, so it had to do with the duration of nucleosynthesis and when it stopped. We calculated that only ~100 million years elapsed before the solar system formed but that the production was going on for 10^{10} years. This timescale would come into question 16 years later with the discovery of ^{26}Al.

From our measurements of K/U in rocks and ^{40}Ar/^{4}He in gases, I calculated that the Earth is not a huge chondrite, even though Earth's heat flow values could (strictly by coincidence) be taken to match chondritic values. I collected samples of batholiths and dated them to test J. Tuzo Wilson's onion structure of continental growth (long before plate tectonics showed it to be wrong). I looked into the problem of discordant ages of minerals in the same rock and the migration of elements and isotopes during metamorphism with Marvin Lamphere and Arden Albee. We concluded that no rocks actually behave as closed systems. I also served as a visiting professor of mineralogy at Kiel, Germany, during the summer of 1960.

UBM: I've heard that you gave your lectures in German.

GJW: The lectures were in "Wasserburg Deutsch"—all genders were random, but I think I got my ideas across.

UBM: I know you were offered a professorship at Harvard. When was that?

GJW: It was in 1963 and I very nearly accepted it. I began to get interested in solid state physics, which was absent at Caltech due to an earlier decision by Bob Bacher. I felt it was time for me to make a change and do something new. I particularly liked the fact that Francis Birch was there. He was a world leader in both theoretical and experimental geophysics and one of my heroes. During our visit, when Naomi had lunch with Barbara Birch and remarked that I was "hired" at Caltech in 1955, Barbara responded: "One hires a gardener, the University appoints a professor." Naomi remembers that very well. Generous support was offered for me to develop my own research interests—solid state physics and its implications for Earth science. I think they wanted to "appoint" me.

UBM: What kept you at Caltech?

GJW: Urgent pleas from Willy Fowler and Tommy Lauritsen, among others, and an invitation to become a formal participating member of the Kellogg Lab with substantial resources. Willy played a major positive role. He was always very good to me, a friend, colleague, and great supporter. Having made the decision, my biggest question was: Now, what shall I do that is new?

UBM: I expect to hear about the advent of Lunatic I.

GJW: That's right, you are. I decided it was high time to give up data processing with an old Marchand calculator and reading spectrometer charts with a K&E steel ruler, a magnifying glass, and a 9H pencil. I decided to build a fully programmable, digital output, computer-controlled mass spectrometer with on-line data processing. To achieve improved precision and sensitivity, I chose a single detector system (although there actually were two: one for high currents and one for single-ion counting). To maintain stability and eliminate source fractionation, I had to use the magnet for rapid switching from one field value to another. It all had to be controlled by a computer. This was a very tall order for the early 1960s. There was some money, but not much. A lot of the parts came from C&H Surplus, dug up by Vic Nenow. A proposal to the NSF for support was rejected when the referees said that a mass spectrometer should never cost that much—one could buy a commercial machine for much less money (but they all gave grossly inadequate performances).

Fortunately, colleagues Willy Fowler and Tommy Lauritsen knew of a very bright young physics major, Dimitri A. Papanastassiou (DAP), who might come and work with me. DAP had been working on the tandem accelerator at Kellogg and grasped my problems immediately. We traveled all over the country with Curt Bauman and Vic Nenow to inspect equipment and designs, and got help from some very capable, technically knowledgeable people. I finally ordered a magnificent, fast, stable magnet. When it was installed in the petrology lab, DAP and I mapped the field in and out of the magnet as a function of position. We calculated the ion trajectories for optimal focusing and high transmission. Everything we designed was built to exquisite precision at the Central Engineering Shop, while I monitored and checked every detail. Finally, DAP and I laid out over one kilometer of shielded twisted pair cable through the tunnels to the computer center to have it interfaced to an IBM 1800. When the spectrometer was turned on in 1968, it worked! This work could not have been done without the brilliance, innovativeness, and dedication of Vic Nenow and Curt Bauman. At first, we had problems with the thin-lens Nier source we were using, so we switched to a thick-lens source and had Lunatic I, a real dream of an instrument.

UBM: Which is still running, is it not?

GJW: Indeed, it is; along with Lunatic III.

UBM: So you called your lab the Lunatic Asylum.

GJW: We did, much to the dismay of the NSF and *Science,* and others.

UBM: Now you were all set for the Apollo samples, which were due to be returned from the Moon the following year, 1969.

GJW: Yes, but first DAP used Lunatic I for his thesis. He did Rb-Sr dating of eucrites and established the new reference value, BABI (Basaltic Achondrite Best Initial), that provides a baseline value for the evolution of $^{87}Sr/^{86}Sr$ in the solar system. He received the first Clark medal from the Geochemical Society—a very well-deserved award.

UBM: Later on, in 1969, if I remember correctly, the two of you showed that all eucrites formed within about two million years of one another. That was fantastically close timing in comparison to the limits of error published up to then.

GJW: Thank you, I must agree. The isochron we got was all within the plus or minus error limits of the precision available on all the other instruments in the world. That advantage continued for some years.

UBM: But let's go back for a moment to your research circa 1965.

GJW: By then, the ages of stony meteorites were clustered at ~4.5 eons, so we turned to irons. Fritz Paneth's early efforts to date irons by the uranium-helium method had come to grief in 1955 when measurements by Hamaguchi, Reed, and Turkevich showed there is essentially no U or Th in the metal (as had been predicted by Urey on thermodynamic grounds). Paneth's helium values were due to most of the helium coming from cosmic-ray bombardment. Dating of metals was essentially impossible by any available method, so I worked with my colleague, Don Burnett at Caltech, and Clifford Frondel at Harvard, to date silicate minerals that we separated out of certain irons. You probably had something to do with that. In 1965, we started on Weekaroo Station from which the silicates gave an average Rb/Sr age of 4.5 eons. Several other irons gave the same result.

UBM: So that solved the problem of dating iron meteorites, and we all could conclude that stony meteorites, iron meteorites, and the Earth formed about 4.5 eons ago.

GJW: Right. We also found clear evidence that some iron meteorites never were cores of differentiated planets. They were metal-rich plums embedded in silicates in different stages of differentiation about 4.5 eons ago. One of our best examples was the Colomera iron from Granada in Spain. It had an 11 cm crystal of potassium feldspar embedded in the metallic Ni-Fe.

UBM: And K-feldspar belongs in crustal granites instead of in core-mantle boundary assemblages!

GJW: Right. We got an even bigger surprise with Kodaikanal from India. It is a shocked iron full of silicates that yielded a low age of about 3.8 eons by both the Rb/Sr and K/Ar methods.

UBM: How did you interpret that?

GJW: It was then clear that some meteorites formed or remelted very late. At the time we filed this information away for later consideration. One thing we learned from our experience with silicates in irons was that to extract and manipulate individual grains and silicate droplets from meteorites we would have to miniaturize our equipment and techniques. I experimented with various dental tools and designed an x-y microscope stage that permitted easy handling of grains down to 10 μm across.

UBM: And for your work on that scale you would need clean conditions.

GJW: We absolutely would. So I found ways to miniaturize our chemical procedures using only superclean reagents in a superclean environment. We all adopted clean room clothes and used whiteboards instead of blackboards. We became adept at handling microsamples without contamination and obtaining far superior data than was possible before, even after full mineralogical characterization. All this put us into an exceptional position to analyze and carry out experiments on lunar samples.

UBM: You had heavy responsibilities with planning for the lunar samples, didn't you?

GJW: I did. In 1967, Wilmot Hess, Chief of the Science and Applications Program of the Manned Space Craft Center (MSC) in Houston, invited me to serve on a group to advise NASA on the handling of lunar samples. This was the Lunar

Sample Analysis Planning Team (LSAPT; called "less-apt"). We were to assess the situation at the newly constructed Lunar Receiving Laboratory (LRL) with respect to sample documentation, processing, and the allocation of samples to approved investigators. But the prevention of contamination of the Earth by lunar biohazards was the first order of the day—as was required by the International Outer Space Treaty. So on their arrival at the LRL, the samples would be maintained in quarantine. Much of the building was given over to quarantine facilities in which the astronauts would be sequestered in special living quarters while lunar rocks and soils were tested for pathogens by being used for culturing plates, growing plants, and being fed to white mice and Japanese quail.

UBM: Which were watched from behind barriers to see if they would visibly sicken and expire from lunar biotoxins?

GJW: Yes. It was a version of the "Andromeda strain" movie, which I thought was hilarious. It was a form of titillation for the public—otherwise, they were just rocks. To us they were Moon rocks! The first rocks studied from another known planet.

UBM: Where were the lunar samples to be processed?

GJW: The non-biological rock-science at LRL was run by Peter R. Bell, a gamma-ray spectrometrist from Oak Ridge National Laboratory. He specialized in counting radioactivity and would be looking for both primordial and cosmic ray-induced effects on lunar samples. Neither Bell nor anyone else in a managerial or supervisory position at MSC had any knowledge of meteorites or of modern studies of terrestrial rocks.

As the only sample-handling apparatus, Bell had built a gigantic vacuum chamber, the F-201, with access by the sort of neoprene gloves used on space suits. He planned to have the sample return container (SRC) washed in sodium hypochlorite solution to kill any clinging moon bugs before being introduced to the F-201. An obvious problem with this was that the SRC might not be hermetically sealed. Any Moon dust on the gasket could cause a leak and soak the samples in sodium hypochlorite—a thought LSAPT members found sickening. After its bath, the container was to be opened in the vacuum chamber. The lunar rocks would be manipulated with wrenches, hammers, chisels, and forceps, and selected samples would be shot through pneumatic tubes into the area for study and analysis. Worst of all, there were no proper tools, containers, or supplies for sample preparation, no sound procedures for sample processing and documentation, and no trained personnel to carry out these things.

UBM: And the lunar samples were due to arrive in less than two years?

GJW: By then they were due in about 10 months. No one in a position of responsibility had taken lunar sample science seriously. Forming LSAPT seemed to have been an afterthought. One of the first issues we faced was deciding whether to have most of the sample analyses performed in-house at the LRL or to distribute samples to members of the scientific community. We concluded that the major scientific investigations should be carried out elsewhere by scientists approved for their expertise in various disciplines. We proposed that when the SRC was opened, each sample in turn should be numbered and photographed and then described by a Preliminary Examination Team (PET) equipped to provide a basic characterization. The descriptions would be circulated to those scientists whom NASA had approved as Principal Investigators. The scientists, in turn, would apply to LSAPT for portions of specific samples to analyze. LSAPT would recommend the distribution of samples with consideration of sample size and type and the skills and quality expected of the PI and the collaborators. The committee would encourage optimal results by assigning portions of the same sample to different investigators with complementary capabilities. LSAPT's allocation schedule would be submitted to NASA Headquarters for approval.

UBM: So the whole system for processing and allocation, used throughout the missions, was set up by that initial LSAPT. I think it worked very well.

GJW: It went through various developments over the years, but finally it did work well.

Another big question we faced was a request to management from Bell to construct a second F-201 as back-up for the first. LSAPT voted unanimously that one F-201 (a huge Rube Goldberg apparatus) was quite enough. We urged that lunar rocks and soils should be processed in glove-chambers filled with filtered dry nitrogen, so that the whole procedure could be carried out in one place with nothing sent flying off through pneumatic tubes. In any case, clean room procedures were essential, so we had to get rid of all organic matter, including lard in polishing compounds, lead in paint and solder, wood in the preparation chamber for the outbound mission, and talcum powder, which was available in large cases for dusting the operator's arms before they put on the neoprene gloves.

UBM: With all those things present in the receiving area, they must have had utter confidence that the vacuum chambers never would leak.

GJW: They had a big lesson to learn.

UBM: Tell me about the "Four Horsemen."

GJW: LSAPT was a diverse group of good scientists covering many fields. After a while, several members left the committee, perhaps thinking that working with NASA was not worth their while. Some individuals said these things were NASA's problem, they had important work to do in their laboratories and could not waste their time. They were replaced by others, but four of us, Jim Arnold (the only one who was a member of the National Academy of Science), Paul Gast (who had landed the job I had interviewed for in Minnesota after I blew it), Robert Walker (who later founded the McDonnell Center for Space Science at Washington

University in St. Louis), and I, worked closely together with great intensity to make lunar sample science succeed. We were "The Four Horsemen."

UBM: Four Horsemen generally imply an apocalypse, but I guess you were trying to avoid one. Did the four of you see to it that things were all set for the lunar samples?

GJW: Not immediately. As the time for Apollo 11 approached, I visited the MSC and found that no real progress had been made. I went to P. R. Bell and Wilmot Hess and demanded action. They consulted with the director and then authorized me to go back to California and acquire the necessary laboratory equipment. I flew back to L. A. the same evening, much to Naomi's surprise, and went out the next day searching for stainless steel and aluminum items in supply houses for restaurant kitchens, hospitals, dental offices, and certain industrial plants. Then, I took sheaves of sketches to the shop at Caltech where I had all sorts of things made: vials, hammers, chisels, sieves, trays, mortars and pestles, rock saws, clamps, sample splitters, and so on. These things were shipped on an emergency basis to the LRL where new rooms were being built outside the quarantine area. (No purchase orders had been issued, so Caltech backed my commitments, but several years passed before NASA reimbursed Caltech.) Meanwhile, Bob Walker had found a plant near St. Louis that made small plastic vials with conical interior bottoms (ideal for lunar soils), so he sat at the plant getting them produced and shipped to the LRL.

UBM: So, it was due to these not-quite-official efforts that the LRL was reasonably well prepared to receive the Apollo 11 samples!

GJW: Minimally prepared would be a better description.

UBM: Wasn't it about that time when the Allende meteorite exploded over northern Mexico and showered down tons of carbonaceous chondrite fragments?

GJW: Yes, it was! That was a strange and wonderful event. The meteorite fell on Saturday, February 8, 1969, at 1:05 in the morning. And on Monday, Gene Shoemaker stopped me in the hall to make sure I knew about it. He said Elbert King, the newly appointed Lunar Sample Curator, already had been to the area and brought pieces back to Houston that were being counted for cosmogenic isotopes. Gene asked if I wouldn't like to collect some, too.

UBM: No doubt, you said YES.

GJW: I certainly did. We arranged for a joint program with the University of Mexico. Then, I got in touch with Don Elston, a friend at the USGS in Flagstaff, who said he would fly me down there if I would appear at dawn at a small airport in New Mexico. It was a terrible hassle for me: I had no cash on hand, so I literally broke my kids' piggy bank on the patio.

UBM: Your kids must have been great savers.

GJW: Well, but they hadn't saved enough pennies, so I borrowed from my mother who had arrived that evening. She demanded an IOU. Naomi was furious, with good reason, for my sudden departure leaving her with my mother.

UBM: But somehow you managed to meet Don and he piloted you to Allende?

GJW: He landed at Parral, the nearest airport, and we arranged for transportation to Allende. We did some searching through the brush but I was not very good at finding meteorites. I got stuck up with thorns and needles, while the sharp-eyed local people kept spotting meteorites. Roy Clarke and Brian Mason, from the Smithsonian in Washington, arrived soon afterward and mapped the strewn field.

UBM: Brian described their activities in some detail in Oral History No. 5 of this series.

GJW: I read his account with interest. We collected and acquired samples, shared half of them with our colleagues from the University of Mexico, and brought a good collection back to Caltech. Allende provided tons of material, just when labs around the world were gearing up to analyze lunar samples. It was wonderfully interesting stuff on which to test our new techniques. Allende contained the famous "white" inclusions of Ca-Al-rich minerals, which still remain the object of world-wide study. These were the ones that you and John Wood first reported on. Where is that sample with the peraluminous glass that you found?

UBM: It is no more, sad to say. The sample was very small to begin with, and I used part of it for an optical immersion which showed minute octahedra of spinel embedded in an isotropic medium that looked like glass. Then, I took a film of the same sample on our new microfocus X-ray machine and found nothing on it except spinel lines. The isotropic medium was structureless, so we concluded it was glass. Next, we made a probe mount to get the bulk composition and then mixed up a batch of chemicals to duplicate the composition. When we melted and cooled the batch, we got an artificial sample with minute spinel octahedra embedded in isotropic glass. In our paper, we published pictures and analyses of both the natural and synthetic materials. My X-ray and probe mounts were lost track of decades ago, and neither we nor any one else ever has found a similar sample in Allende. But, in meteoritics, I think we have to get used to singular examples of things.

GJW: Your group published the first mineralogical description in 1970, and Clarke et al. described the event as a whole the same year. But we postponed most of our research on Allende until we were well along in our studies of the lunar samples.

UBM: It was only five months after the fall of Allende that Apollo 11 landed on the Moon. Was the Lunatic Asylum holding a big party on July 20th to watch the show?

GJW: We certainly were. Our entire group (students, faculty, postdocs, and their families) came to our house to witness the first Moon walk in the history of the world! I found the sight of those ghostly figures of space-suited astronauts bouncing along and collecting samples to be exciting and awesome.

UBM: Our group partied at John Wood's home and felt the same way. How did things go when the Apollo 11 samples reached the LRL?

GJW: I had gotten back to MSC sometime before the samples arrived and found that the system had changed only a little. Elbert King had been appointed as Curator and given working space in a trailer outside the LRL. But, of course, quarantine still held first place. Nevertheless, when the return module carrying the astronauts and the sample container splashed down into the Pacific Ocean, the decision was made to pull them out immediately rather than leave them bobbing in the water until the entire module could be lifted onto the aircraft carrier. Of course, bringing the astronauts onto the flight deck actually broke quarantine once and for all. Any Moon dust clinging to them contaminated the tropical Pacific air and waters. Lunar pathogens, if any, were with us to stay. Nevertheless, the astronauts dutifully performed the charade of walking into the sterile module in which they would be transported to the LRL. The door was sealed behind them and they smiled and waved through the window, giving the impression the protocols had been obeyed.

UBM: That could not have fooled the medical doctor directing the LRL or any of his staff. Maybe some thought it would fool the press and the public.

GJW: Well, it was part of the titillating charade. Whatever they thought, they went through with the planned procedures and started processing samples in the F-201. Through its windows, LSAPT members could see nothing except dark rocks covered with dust. We got our first briefing from a PET member who described, among other things, beautiful goblet-like things decorated with droplets. It was hard to interpret what we had never seen before, but when we finally got a good look at them Cliff Frondel spoke up: "Like in Buck Rogers, these rocks were 'zapped'." Cliff hit it on the head. Small impacting particles of sufficiently high velocity to cause melting, as well as cratering, cracking, and spalling, had speckled the exposed surfaces of the lunar rocks with glass-lined "zap pits," many of them rimmed with droplets looking just like the splash of milk that Harold Edgerton at MIT had photographed decades before at ultra-high-speed. This showed us that bombardment of the airless Moon by debris from space has excavated everything from the immense craters we knew about down to these micron-sized pits. This was space erosion! The impacting particles were not relativistic as someone guessed, just fast!

UBM: To illustrate the range of sizes, I often have shown pictures, side-by-side, of a zap pit a few micrometers across and of the Oriental Basin with rings up to 1900 km across.

How did the lunar sample processing go in the F-201?

GJW: It was a disaster. Late one night LSAPT was informed of a quarantine violation. When we gathered we were shown a videotape of the opening of the SRC inside the F-201. Suddenly, a white blob sailed across the chamber and rocks, soils, and tools flew in every direction. One of the neoprene gloves had been punctured, no doubt by a prick from needle-nosed forceps. The operator's white glove liner had been sucked through the hole, and the vacuum pumps were trying to pull in his hand and arm. His associates lined up behind him and tried to pull him out, with no luck until the pumps went off. Then almost everybody (other than those who hid!) retired to living quarters in the quarantine facility. This was another charade. The quarantine was not environmentally tight. Cockroaches could go in and out under the doors.

UBM: I can vouch for that. After a much later mission, I was in the processing area and saw a minute cockroach crawling along the rounded join between the white-tiled floor and white-tiled wall.

GJW: I believe it. Robin Brett, who was Chief of the Geochemistry Division at MSC, was trying to do science and also to represent management, knew all about it. He would tell delightful stories about all these goings on. After the F-201 catastrophe, the only place available for processing was in the Biological Preparation Area where approximately one square meter of space was made available for the Apollo 11 samples. The rest of the building belonged to the Bio-Med people. There was no proper storage space for the sub-samples, so they were placed in all sorts of containers and then stored in a safe in an office outside of quarantine.

One night, thoroughly disheartened, I got some motel stationery and drafted a letter to someone—possibly to Thomas Paine, the NASA administrator in Washington. I described the basic problems, emphasized the importance of the samples as the real treasures returned from the Moon, and urged the need for immediate attention to the matter. The next day, I brought my draft to LSAPT where everyone agreed there was a problem but only the other three Horsemen favored sending out the document. Arnold, Gast, Walker, and I revised and improved the letter and had it typed. Bob Walker knew Tom Paine so we signed it and mailed it to him.

UBM: For the cardinal sin of bypassing the chain of command, you all might have been thrown out of the program.

GJW: Or court-martialed. But it actually worked out rather well. Soon afterward at a meeting with Dr. Robert Gilruth, Director of MSC, he asked why we sent a letter communicating the problems at the LRL to the NASA Administrator. After a short silence, Jim Arnold spoke up and told him we wrote to the person we knew. That started a new era of communication between MSC and NASA with LSAPT as de facto representative of the lunar science community. Gilruth called me to his office and asked for details of our problems. I proposed that the two of us should go over to the LRL unannounced and inspect the laboratory. When I showed him the Bioprep Area used for lunar samples, he saw the problems immediately. He simply had not been informed of them earlier. Things began to improve, but more than a year would pass before a new lunar sample facility would be constructed and equipped.

UBM: And today that facility still regularly fulfills requests for lunar samples and it also curates Antarctic meteorites and cosmic dust samples, and is preparing to handle samples from comets.

GJW: Right—thanks to Noel Hinners at NASA Headquarters, who strongly supported the creation of this facility. Today, the present lab is a world-class facility for handling extraterrestrial materials.

UBM: What sort of Apollo 11 samples did you work on in the Lunatic Asylum?

GJW: We got a few grams of soil and a few grams of basaltic rocks. They were glorious: the freshest rock samples we ever saw. Since there was no water on the Moon, the minerals were clear and showed no trace of aqueous weathering. And, in fact, the Moon was found to be depleted in all volatile elements.

UBM: Having no water on the Moon (not even a trace of OH in any lunar mineral) was one of the biggest surprises awaiting all of us. Many people had thought the lunar rilles formed as water courses; some talked about permafrost in the lunar soils and aqueous veins and pegmatites in the crust; and Urey had argued for a while that the dark maria were carbonaceous lake beds.

GJW: All those ideas went out the window almost on the first day. Another surprise was that the mare basalts were old.

UBM: According to the astrogeologists, Mare Tranquillitatis, where Apollo 11 landed, should have been one of the younger maria.

GJW: It actually was one of the oldest. We dated minute samples of several basalts as 3.65 ± 0.05 eons, but their feldspars had different values of $^{87}Sr/^{86}Sr$ which meant that although they were the same age, they came from different magma chambers. Then, we found soil breccias containing feldspars with $^{87}Sr/^{86}Sr$ just above BABI, indicating that they crystallized almost 4.5×10^9 years ago. These feldspars were older by far than any rock on the Earth. This, and other evidence, told us that the original lunar crust formed very rapidly. Before that, we had supposed all planetary crusts formed slowly. Of course, we monitored neutron exposures by measuring Gd and Sm isotopic shifts, a technique that later reappeared to do Sm-Nd dating.

UBM: Our group, led by John Wood, found a small proportion of feldspar-rich clasts in the soil samples that we interpreted as impact debris from the ancient highlands.

GJW: That was an important observation that proved to be correct in later missions. The highlands are the important areas still requiring more study. The recognition of an anorthositic crust changed everyone's view of early planet differentiation. Yet another vexing problem with Apollo 11 was that the bulk lunar soils yielded older ages than the basalts they lay on. If the soils were erosional blankets, like those on Earth, how could they be older than the bedrock? We finally concluded that the soils looked old because they contain a strong component (the "magic component") of the earliest lunar crust, highly enriched in K, Rb, U, Th, and so on.

UBM: Your results showed that the Moon is even farther from being a huge chondritic body than the Earth is.

GJW: That's right, and the Moon was not the source of basaltic achondrites, as some had predicted. The samples also closed out the perennial hot Moon versus cold Moon controversy. The basaltic lavas made it clear that the Moon once was hot but now it is cold. The dispute about tektites from the Moon was largely resolved in the negative when no major lunar rock type was found of tektite composition—although, John O'Keefe at Goddard Space Flight Center, continued to argue for it.

I want to add that I thought the first Lunar Science Conference, held in downtown Houston in January, 1970, was a marvelous coming together of scientists in a great diversity of fields from many parts of the world, all focusing on trying to understand the Moon. It formed the basis of the planetary science community that still is going strong.

UBM: I fully agree. Strictly on the side: I remember that some lunar scientists were crestfallen to discover that even though we were the first gathering of lunar scientists in world history, we were not the biggest conference in town. Our hotel was packed with cotton-growers wearing tags that read: "We do not grow sick cotton." Rumor had it that Harmon Craig, one of your former colleagues at Chicago, inserted himself into one of their sessions and gave an ad hoc talk on the role of rare earths in growing cotton. Was that true?

GJW: [Roaring with laughter.] I certainly wouldn't put it past him!

UBM: When did the dry nitrogen cabinets replace the F-201?

GJW: The nitrogen line was up and running for Apollo 14. Paul Gast and I had met with the engineering staff during the Apollo 12 conference to design the nitrogen processing line. It was to be built in front of the old F-102 beast, which had been on display to members of congress and the President. But there was almost no space available for work in the LRL, and we still were governed by the quarantine rules. So even for Apollo 12, conditions were very much make-do, with numerous threats and some injuries to sample integrity.

Once again, I decided more action was needed and asked for an appointment with the President's Science Advisor, Edward David. Never before had I been in contact with officials at this level, and I wondered if he would see me. But, by some special grace, the appointment was granted. I took George Wetherill with me hoping his presence would add gravity to the interview. When we entered the outer office, we found cabinets displaying beautiful mineral specimens. These brought a big smile to my face: they indicated that there was a real chance Edward David would understand our concern for the lunar rocks. He listened with considerable understanding and concern and said he would be in contact with the NASA Administrator to have them look into the matter. I have been very fond of him ever since.

UBM: So once again, you by-passed the chain of command. This time in a BIG way!

GJW: Yes, we did. George and I soon were summoned to NASA Headquarters to meet with Homer Newell, the Chief Science Officer. He asked what our problems were and why we had taken them to the White House. We explained in detail what the problems were and why we went to the White House. At the next LSAPT meeting, a special session was called with an oversight group consisting of Gilruth, several senior NASA officials, Frank Press, and others. Gilruth asked us just what it was that we scientists wanted? We even were asking for changes in the EVAs.

UBM: I suppose that was a wildly heretical thing to do. After all, the EVAs (extra-vehicular activities) on the Moon were, literally, somebody else's turf. They were strictly the responsibility of the Astrogeology Branch of the USGS under the expert direction of Eugene Shoemaker.

GJW: That's right. The astrogeologists mapped the lunar surface and interpreted lunar stratigraphy on the basis of standard field observations: overlapping formations, cross-cutting relationships, and states of preservation. They proposed landing sites, trained the astronauts, and scheduled each moment of their EVAs, including the collecting of rocks, soils, and core samples, and making measurements of heat flow, seismicity, solar wind, and so on. This was important work.

UBM: What changes were you asking for?

GJW: As an example, Jim Arnold explained to Gilruth that we would like to know which side was *up* on a lunar rock, because that would be the surface on which we could measure the intensity of recent solar wind effects. We also wanted rakes coarse enough to collect lunar pebbles, not just fine soils. We wanted more working space at the LRL, along with proper sample handling procedures and much better documentation methods. Above all, we did not want critical portions from the drill cores to be used for growing plants and the feeding of Japanese quail and white mice. The outcome of this session was beneficial. We no longer were perceived as crazy, irresponsible scientists but as rational investigators with legitimate concerns and urgent desires to optimize the scientific return from the samples. Jack Sevier, at MSC, was at the meeting and he took up our case. He worked long, hard, and skillfully to get sample science integrated into the system.

UBM: How long did the quarantine continue? Inasmuch as they found no carbon in the Apollo 11 and 12 rocks, the quarantine seemed useless.

GJW: It was worse than useless. None of the rocks contained identifiable organic material, but the National Academy originally supported the quarantine without providing for any review of how well it was working or whether it still was needed. Quarantine continued through Apollo 14 but, meanwhile, Jim Arnold managed to communicate LSAPT's concerns to the National Academy.

UBM: I understand there was big excitement when they saw that plants grew better in lunar soil than they did in quartz sand.

GJW: Until somebody pointed out plants always thrive in volcanic soils because of all the nutrients in it. For my part, I snitched from the LRL a case of 8 × 10 glossy photographs of a white mouse being fed lunar rocks. I attached a note saying: "This is a white mouse in the LRL eating one gram of your lunar sample. WHAT DO YOU THINK OF THAT?" I sent copies at my personal expense to members of the House and Senate and the scientific community at large. Quarantine began to look so foolish it was ended after Apollo 14.

UBM: You would approve of quarantine for samples from Mars, wouldn't you?

GJW: No, I wouldn't. What we should do is to prevent martian samples from being contaminated by Earth stuff. An organic compound from Mars or a bug from Mars is interesting. Organic stuff from Earth on Mars rocks is uselessly confusing. We have to learn something! We already have martian rocks on Earth that fell here as meteorites and they certainly were not sterilized on the way in. Fireballs don't even drive the extraterrestrial water out of carbonaceous chondrites. We also have lunar meteorites, although we didn't recognize them as such until the first one was collected in Antarctica in 1982 and described by Brian Mason ten years after the Apollo missions ended. Meteorites, cometary debris, interstellar dust, all sorts of exotic materials, fall on Earth every day. Why should we quarantine just those rocks we collect? In my mind, the perception that we need a quarantine springs from a desire to titillate the public mind with possible dangers to make sample returns seem more exciting and risky.

I once signed a petition with Urey to President Lyndon Johnson emphasizing that it would be far more efficient and cost effective to return lunar samples using robotic systems. We were not opposed to the Apollo program, but if we were going to do science, it was the simple truth. Robotic systems are more effective and less expensive. I feel the same way about martian samples.

UBM: But, as you learned on LSAPT, the scientific study of lunar samples was at the bottom of NASA's list. In fact, scientific study of the Moon was not on President Kennedy's list and would not have been on NASA's had not Harold Urey, Gene Shoemaker, and others lobbied hard for it.

GJW: I realize that. And, as I said earlier, I was not opposed to the Apollo program. I understood well enough that President Kennedy's promise of sending a man to the Moon and returning him safely within the decade was strictly for the purpose of enhancing our national prestige, after it had been badly wounded by the Sputniks. The question is how do you do science, and can you do science while riding on the coattail of someone with a very different purpose. The answer is: yes you can, but carefully, like porcupines making love.

UBM: So we are lucky to have got any science done when it actually was that picture of the stars and stripes planted on the Moon that fulfilled NASA's basic purpose for the Apollo program, which had cost 24 billion dollars over the six years since planning for it began. If the astronauts had come home immediately after unfurling the flag, without collecting or measuring anything, most people would have been happy. Nevertheless, soon after their return headlines were blaring out the news that 48 pounds of Moon rocks had cost 24 billion dollars. I insist that the $24 billion paid for the flag picture—the rocks were free of charge.

GJW: But the headlines implying the rocks were too expensive may have helped with generating disapproval for feeding them to white mice.

UBM: I'm glad if they had some use. Early in 1970, we were in suspense during the flight of Apollo 13, which had a major explosion en route and still returned the astronauts to Earth against all odds.

GJW: That great crew was in incredible jeopardy. I, personally, did not see any way out for them. After it was all over, I happened to meet Jim Lovell, the Commander, in the LRL parking lot near his red Corvette. I told him: "I didn't think you guys ever would come back." Jim said: "That thought never occurred to us. We knew the system would work." He was right. The system would and did work. They were in constant communication with mission control, and the highly knowledgeable people there guided them through the critical steps that brought them safely home. In its way, Apollo 13 was the most remarkable mission, with an umbilical cord of radio, and people, and help between the capsule and Earth. Later on, the crew gave me a photo I keep on my wall. It says: "Sorry we couldn't bring back any rocks."

UBM: 1970 was a year of big changes: Tony Calio replaced Bill Hess as Director of Science and applications at MSC, and Paul Gast, one of your Horsemen, moved to Houston from Columbia and took over as chief of the Division of Lunar and Earth Sciences.

GJW: Calio was a great help to us. He represented management but he immersed himself in the issues at hand. He actively chaired LSAPT, took part in all its discussions (no matter how heated), and developed a great respect for the samples. He wanted to get the best science out of them. We all developed a high level of respect for Tony's judgment. And needless to say, it was great having Paul Gast in-house. Some years later Bob Walker arranged for Calio to receive an honorary doctorate from Washington University. It was a great affair—we much respected and loved Tony for all he had done.

UBM: Then came Apollo 14.

GJW: And a huge crisis in NASA. Three months or so before the launch, on January 31, 1971, I took a vacation with my family on a secluded beach at a place with no telephone. But it was not completely secluded: during a walk, we came upon an acquaintance in aeronautics at Caltech who told me he had heard a radio announcement that all future Apollo flights had been cancelled!

That ended our vacation. I rushed home in time to go to the GSA meeting in Minneapolis where I met with the other three Horsemen. We were devastated by the thought of losing all three scheduled missions—they were the ones with the greatest possibilities for sampling and exploration. We decided to go to war. (My colleague, Arden Albee, used to call me "the General" back then.) We would get members of the scientific community to write to their congressmen and senators, and anyone else they could think of in the government. They also would write to scientific societies, including the National Academy. I hired a secretary, out of my personal money, to aid in sending letters and telegrams. The well-known cartoonist, Conrad, published a cartoon showing the disappearance of the Moon and of Apollo. I got permission to reproduce it and sent out hundreds of copies with the comment that for 25¢ per person per year in the USA, the Apollo program could continue. My arithmetic was not quite right, but congressmen began to ask: "Who is this big lobby, and where did they come from?"

UBM: They might well ask, in as much as they couldn't recognize it as a lobby contributing to their reelection campaigns.

GJW: It got their attention, though. Soon afterward, Paul Gast called to tell me the White House was about to host a dinner for some NASA officials and two representatives of the science community. Jim Arnold and Bob Walker were the chosen ones and President Nixon had invited them. At the end of the evening, Jim and Bob thanked the President for their invitation and were about to leave when President Nixon shook their hands and said: "Let me see, gentlemen, if I remember: fifteen, sixteen, and seventeen." We had won!

UBM: I heard this story from Bob Walker. He was elated. He had heard straight from the ultimate authority that those three Apollo missions would fly.

GJW: And the missions gathered a great treasure of samples and data.

UBM: But even that didn't end all your programmatic troubles.

GJW: No, it didn't. Our next major issue was the need for a proper facility outside the LRL (which was really a bio-prep medical area) for processing, documentation, and retrievable storage of lunar samples. The samples should be kept in an inert atmosphere and fully protected from terrestrial contamination. The facility would require a trained staff with excellent technical skills and it also should include research scientists. There would be no hope of obtaining enough space at the LRL or of persuading the biomed people of the need for long-term processing after the quarantines were over.

UBM: I would expect that convincing NASA of the need for such a facility would not be easy.

GJW: It was not at all easy. Some officials didn't want to bother with samples any longer than they had to. But we were aided by John Naugle, NASA Associate Administrator for the Office of Space Science (OSS), and by Noel Hinners, whom Naugle had appointed as Deputy Director of the Lunar Science Program. Homer Newell paid serious attention and supported the founding of a lunar science program that would function outside the Office of Manned Science Flight and in the Office of Space Science. A design for the facility was prepared and submitted as a line item in NASA's budget. Proponents then were faced with two problems: first, to convince Congress that the facility was needed, and second to make certain the building was designed to take into account long-term hazards.

UBM: I suppose Congress thought the LRL already was a lunar sample facility.

GJW: They did. And the LRL had been built not long before at the considerable cost of eight million dollars. We had interviews with numerous congressional staffers and with newspaper reporters. Early one morning, I was awakened in my hotel room in Washington by a friend who informed me I had just received Senator William Proxmire's Golden Fleece Award, "For spending millions of dollars for a wheelbarrow load of rocks."

UBM: Planned, no doubt, to display Proxmire's guardianship of the public purse. But, of course, it displayed his ignorance of lunar samples and of what the money actually was spent for.

GJW: He certainly was not ready to address this as an issue of substance.

UBM: Natural hazards in the area of the Manned Spacecraft Center must have raised serious problems. That part of the coastal plain is plagued by land subsidence, faulting, flooding, and hurricanes.

GJW: Some of the land was subsiding six inches a year. One motel on Clear Lake had boats in front of the doors where originally there had been cars. And railings along the freeway into Houston, and roof-beams of buildings in the nearby Ellington Air Force Base showed offsets due to geological faulting that resulted from subsidence.

UBM: What did you do about that?

GJW: We got together a committee of experts in risk assessment including the Director of the Hurricane Center in Florida. High level officials from NASA and the local government attended our meetings. Hurricanes, high waters, and violent winds were frequent on the Texas Gulf Coast, which the Hurricane Center director called the "Gust Coast." But discussing hazards with local officials was very difficult; they could not tolerate reports of risks that might discourage development. Nevertheless, our recommendation to NASA Headquarters stipulated that the building should be designed to withstand an 80-year flood, and the Lunar Sample Facility, itself, should be located on an elevated second floor.

To defend our assessment to NASA and to the Congress, I copied notes from Sam Houston's report, written in the 1830s, of seeing a large ship stranded on the Gulf Coast 50 miles from the sea. Sam remarked that immense winds must, on occasion, blow ships far inland. More persuasive was a detailed, up-to-date analysis of the faulting and subsidence prepared by Huel Clanton, a geologist at MSC.

UBM: Clanton was an expert on these things. He once gave me a fascinating tour of the area with all its faults and explained how the withdrawal of ground water beneath built-up communities had left them liable to subsidence. So, you and the Horsemen were instrumental in getting Apollos 15, 16, and 17 to fly, and also got the Lunar Sample Facility built and outfitted on the 2nd floor of Building 37 in time to properly process their samples without going through quarantine?

GJW: Yes, and those samples were of great interest. One of the major conclusions of our study of breccias at the Lunatic Asylum was that the Moon was subject to a massive bombardment ~3.95 to 3.8 × 10^9 years ago that destroyed most of the first 0.5 eons of lunar history. Indeed, there are clasts in the breccias that are ~4.5 × 10^9 years old. We called it "the terminal lunar cataclysm" and assumed that it must have occurred on the Earth and other planets of the inner solar system—although, we were puzzled about a source of the projectiles.

UBM: Have you found evidence of a terminal bombardment on the Earth?

GJW: That would be very difficult to do, but recent studies have shown some evidence of such ancient impacts. The bombardment would have destroyed or changed the existing atmosphere and delayed the formation of permanent life forms on Earth until after 3.9–4.0 eons ago. With respect to other planetary bodies, do you remember the Kodaikanal iron meteorite with silicates dating to ~3.8 eons? It is the right age.

UBM: In your papers and diagrams, you describe major impacts occurring between ~4.5 and 4.0 eons and then a tremendous spike in bigger impacts that lasted until ~3.8 eons. Others, including Ralph Baldwin, in his interview for this series argue for major impacts throughout that time span.

GJW: Indeed, most people thought the terminal lunar cataclysm was nonsense. However, there must have been a rather sharp turn off at the end of the major bombardment and accumulation of debris that formed the Moon. Then, there would have been a relatively quiescent period with some impacts, of course, followed by the great basin-forming impacts we recognize. Graham Ryder, at the LPI, has provided supporting evidence of this, and more recently Stephen Moorbath, at Oxford, has described terrestrial evidence for such late impacts.

UBM: To me, the ancient age of the lunar landscape was the most impressive insight to come from the sample dating you and others reported. Virtually all lunar geology is early Precambrian. All the great basins were formed during the Hadean (pregeologic) Eon on Earth, which ended ~3.8 eons ago. Mare basalts flooded across the lunar lowlands between about 3.9 and 3.0 eons ago and then the major eruptions ceased, while the Earth's was still in the late Archean. Plenty of cratering has pock-marked the Moon since then, but the only prominent younger feature is the bright-rayed crater, Tycho, which is Mesozoic. It reportedly formed about 110,000 million years ago during the age of the dinosaurs.

GJW: The Earth is too big and internally active for our history to have stopped in the Archaen. It has sustained many orogenies and many big impacts since then, including the one 65 million years ago that ended the age of the dinosaurs and, fortunately, gave us mammals a chance to evolve. As for the age of Tycho, I am not so sure.

UBM: After the Apollo 17 mission returned in December, 1972, we expected to see no more freshly collected lunar samples.

GJW: That's right. But the Soviet Union's Luna 16 robotic mission had returned a 100-gram core sample of soil in 1970, and Luna 20 had returned 30 grams early in 1972. Still ahead was Luna 24, which brought back 170 grams in 1974. The Luna samples added new types of rock to the lunar inventory. Paul Gast and I thought it was high time for a sample exchange and the Soviet Academy agreed, so we arranged for the U. S. to send Apollo samples to the USSR and they sent Luna samples to us. It was a delicate affair. We started it through Mike Duke, who was then the Lunar Sample Curator. Before that we had sent many Apollo samples to groups in other countries, but this exchange with the USSR made lunar science truly international.

UBM: I was serving on LSAPT when we allocated our portion of the Luna 24 samples. It was very exciting to learn what was in them. While you were working on lunar samples, you began serious research on Allende, didn't you?

GJW: Yes, we did. We focused on the refractory calcium-aluminum-rich inclusions as possible early condensates from a cooling gas of solar composition. Dimitri, Chris Gray, and I found that the initial $^{87}Sr/^{86}Sr$ in some of them was distinctly lower than in BABI, making a new reference value we called ALL (for Allende). This proved that some of these objects were the most ancient material in meteorites. We concluded it was now time to re-do a search in this older material for evidence of ^{26}Al in the early solar system. In Minnesota, Dave Black and Bob Pepin, had discovered almost pure ^{22}Ne in a meteorite in 1973 and argued that it must occur in dust grains that formed around other stars. We always had thought, after many fruitless searches and lots of errors, that the primeval solar nebula had to be homogeneous.

UBM: That's understandable—after all your searches, any other conclusion would have seemed to be too ad hoc.

GJW: But then, in 1973, Bob Clayton and his group at the University of Chicago, reported finding excesses of ^{16}O in CAIs, which we all thought must have come from a supernova. That opened up possibilities of anomalies in many elements. We searched for effects in Ca and Mg and found both excesses and depletions in ^{26}Mg. From more intensive work, we could establish that for most samples the increase was precisely correlated with ^{27}Al, which would be correlated with the isotope of the same element from ^{26}Al. Finally, we constructed internal isochrons proving the early existence of ^{26}Al, which has a short mean life of only $\sim 1.05 \times 10^6$ years. This had to be produced by nucleosynthesis in a nearby star (some people think it was the Sun) just before the solar system formed. By this means, we established meteorites as carriers of products of astrophysical processes and reduced the timescale between the injection of freshly synthesized nuclei into the nebula to less than three million years. In addition, the ^{26}Al provided an early source of the heat that was required to melt bodies early in the history of the solar system, as Urey had proposed.

UBM: These findings aroused great excitement. Ed Anders told me in the first interview for this series, that he and his group (following Harold Urey) had assumed the existence of ^{26}Al in the early solar system to provide heat for melting achondrites and irons. But their paper had been rejected and a physicist had written him that their reliance on extinct nuclides was ridiculous. Now you had provided clear evidence for one extinct nuclide and, suddenly, everybody seemed to be looking for other anomalies.

GJW: They were. At the Lunatic Asylum with Bob Kelly, we went on to discover ^{107}Ag in irons due to decay of ^{107}Pd, which had to be made with neutrons in a star. Our results showed that core formation in planets took place less than 10 million years after the solar system formed.

UBM: You likened the Allende meteorite to Pandora's box.

GJW: We did, because when we opened it, it led to many surprises and some shocks. Isotopic anomalies flew out of the box in such numbers that they invalidated the prevailing law of constant atomic weights, and they revealed flaws in the standard stellar models. Anomalies were then found in many additional meteorites as technical skills increased and we were able to analyze submicron interstellar grains. From David Black's work on neon-E came the search for its carrier conducted by Ed Anders and colleagues at the University of Chicago. This led to the important work at Washington University in St. Louis. Hope is more likely to take the form of additional anomalies and few explanations. I should add that the techniques we used on Allende and the more general cosmochemical problems now being pursued, derived directly from those we developed in the Apollo program.

UBM: Tell me about Project Oldstone, your trip to Greenland looking for Earth's oldest rocks.

GJW: In 1973, with Arden Albee and his son Jamie, and my son Chuck, we made up a party of seven, to check out anorthositic rocks in West Greenland that were reportedly as old as the lunar highlands rocks. The anorthositic lunar crust proposed by John Wood and your group made the comparative study of Earth's ancient anorthosites and very early crust (the "oldest" Earth rocks) very exciting. Much effort was required to get permission from the Danish government to do field work in Greenland, but we succeeded through the aid of Victor MacGregor, who was living in the Inuit settlement of Atangamik and mapping the area. We boarded a Danish coast guard cutter and MacGregor guided us into the fjords and onto the outcrops where we drilled and blasted to obtain an extensive suite of samples. Our analyses showed that the Earth is younger than we all supposed: it formed ~4.47 eons rather than 4.55 eons ago. This makes the Earth the same age as the Moon, and indicates that the two bodies formed 50–80 million years after the birth of the solar system. By the way, the areas we worked in had been mapped by Hans Ramberg and his wife several decades earlier.

UBM: So this figure brings the age of the Earth slightly below the value Patterson determined 50 years ago! Haven't you also applied new dating techniques to some of the Earth"s youngest rocks?

GJW: By counting atoms instead of decays, we increased the sensitivities and precision of ^{234}U-^{230}Th dating of carbonates by a very large factor. This allows us to trace Pleistocene and Holocene tectonic events and climate changes. And by reversing the high voltage and magnetic fields of our mass spectrometers, we establish precise and highly sensitive measurements of negative osmium ion complexes in seawater and hydrothermal vents and, of course, in meteorites. Today, there are lots of new techniques and novel approaches by many groups.

UBM: After the Apollo missions ended, I believe you continued to advise NASA with respect to other planetary missions.

GJW: I did. John Naugle invited me to serve NASA as an advisor. I sat in on discussions of possible new missions like the Jupiter Orbiter Probe (later called Galileo) and evaluations of the status of those already approved, including the Vikings. I quickly learned that, once again, the emphasis was on purely biological analyses. Nothing about Mars was seen as being important except the search for life. The National Academy of Science even said so! The two Viking spacecraft, scheduled to land in 1976, would not be carrying any instruments for measuring the composition of the martian soils or rocks, only the atmosphere. This deficiency was corrected, but only in part, and at the very last minute.

UBM: I think I can hear you pounding the table. But NASA must have been glad to have something interesting to report when the Vikings detected no positive signs of life. In fact, the Vikings made some analyses of the martian soils that, later on, were shown to be a fair match to the bulk composition of the Shergotty basaltic achondrite.

GJW: And the Viking analyses of the martian atmosphere by Al Nier matched those of gases trapped in the Shergottite, EETA79001, collected in Antarctica in 1979.

UBM: Reports of that atmospheric match thrilled meteoriticists everywhere. Shortly thereafter, similar results from other shergottites and from nakhlites and chassingites led most people to accept the whole SNC clan as martian meteorites.

GJW: These meteorites always had differed substantially from common asteroidal types. They were igneous lavas or cumulate rocks with extraordinarily youthful crystallization ages, ranging from ~1.3 billion years down to only 2 or 3 hundred million years. They also were more oxidized and contained a different range of accessory minerals, including some with traces of water as we found. Such meteorites always seemed to demand a parent body larger than the Moon; one that could maintain igneous activity throughout most of the age of the solar system. Nakhla had to come from some (then) unknown terrestrial planet as Dimitri Papanastassiou and I pointed out.

UBM: Then, once we had concluded that martian achondrites were youthful, a martian pyroxenite (ALH 84001), an igneous cumulate rock, proved to be 4.0 to 4.5 billion years old, making it our only sample, so far, of the ancient crust of Mars! In 1994, that unlikely host rock was reported to contain possible evidence of early life on Mars. What do you think about the "fossils" in that rock?

GJW: I don't think about them. They have become a sort of science fiction bubble, used more for marketing than for understanding or doing science. The exciting thing is the recent discoveries of microbes that thrive on Earth under extremely high temperatures and of microbes much smaller than we thought. That is interesting.

UBM: Before we conclude, tell me a bit about COMPLEX.

GJW: That was the Space Science Board's Committee on Planetary and Lunar Exploration, which I was invited to chair. I concluded that COMPLEX should provide NASA with advice on specific goals of planetary exploration based on a long-term strategy. It should compile a prioritized list of scientific experiments and observations to be carried out in sequence; layout the basic technical requirements, including launch capabilities for achieving substantial scientific goals; and provide for evaluation of ongoing programs and identification of areas requiring immediate attention. The members were outstanding scientists, representing many disciplines, who showed a deep dedication to these goals. We met a lot, and suffered a lot, and learned a lot. I spent long hours on the phone with Al Cameron, at your Center for Astrophysics, and with Gene Levy, at the University of Arizona, and exceedingly long hours with Mike McElroy, at Harvard, but we finally produced a report that was adopted by the Space Science Board as a policy document for guiding planetary exploration. But, I must add that the space shuttle and other proposed piloted missions worked against the best interests of doing space science or space exploration. I note, with regret, that the People's Republic of China has committed itself to piloted explorations of the Moon. These missions may gain them prestige, but in my view, they will foreclose the best possibilities for advances in science and technology. COMPLEX is, of course, continuing to address urgent issues in planetary exploration, particularly Mars.

UBM: Thank you very much, Jerry, for describing to me how you and your students and colleagues at the Asylum have altered astrophysical models with your isotopic analyses of presolar grains, how you have developed new information on the earliest history of the Earth-Moon system, and on both the earliest and the most recent history of the Earth itself. I also appreciate hearing how you have helped to guide programs of lunar and planetary exploration toward high scientific goals.

Acknowledgments–I wish to thank the Council of the Meteoritical Society for their support of this effort. This interview was edited in consultation with Professor Wasserburg.

SELECTED REFERENCES

Albee A. L., Burnett D. S., Chodos A. A., Eugster O., Huneke J. C., Papanastassiou D. A., Podosek F. A., Russ G. P., Sanz H. G., Tera F., and Wasserburg G. J. 1970. Ages, irradiation history, and chemical composition of lunar rocks from the Sea of Tranquility. *Science* 167:463–466.

Bogard D. D., Burnett D. S., and Wasserburg G. J. 1969. Cosmogenic rare gases and the ^{40}K-^{40}Ar age of the Kodaikanal iron meteorite. *Earth and Planetary Science Letters* 5:273–281.

Burnett D. S. and Wasserburg G. J. 1967. Evidence for the formation of an iron meteorite at 3.8×10^9 years. *Earth and Planetary Science Letters* 2:137–147.

Busso M., Gallino R., and Wasserburg G. J. 1999. Nucleosynthesis in AGB stars: Relevance for galactic enrichment and solar system formation. *Annual Reviews of Astronomy and Astrophysics* 37: 239–309.

Chen J. H. and Wasserburg G. J. 1989. The isotope composition of Ag in iron meteorites and the presence of ^{107}Pd in protoplanets. *Geochimica et Cosmochimica Acta* 54:1729–1743.

Chen J. H., Curran H. A., White B., and Wasserburg G. J. 1991. Precise chronology of the last interglacial period: ^{234}U-^{230}Th data from coral reefs in the Bahamas. *Geological Society of America Bulletin* 103:82–97.

Craig H., Miller S. L., and Wasserburg G. J., editors. 1964. *Isotopic and cosmic chemistry* (Dedicated to Harold C. Urey). Amsterdam: North Holland Publishing Co.

Creaser R. A., Papanastassiou D. A., and Wasserburg G. J. 1991. Negative thermal ion mass spectrometry of osmium, rhenium, and iridium. *Geochimica et Cosmochimica Acta* 55:397–401.

DePaolo D. J. and Wasserburg G. J. 1976. Nd isotopic variations and petrogenetic models. *Geophysical Research Letters* 3:249–252.

Edwards R. L., Chen J. H., and Wasserburg G. J. 1987. ^{238}U-^{234}U-^{230}Th-^{232}Th systematics and the precise measurement of time over the past 500,000 years. *Earth and Planetary Science Letters* 81:175–192.

Eugster O., Tera F., Burnett D. S., and Wasserburg G. J. 1970. Neutron capture effects in Gd from the Norton Country meteorite. *Earth and Planetary Science Letters* 7:436–440.

Gancarz A. J. and Wasserburg G. J. 1977. Initial Pb of the Amitsoq Gneiss, West Greenland, and implications for the age of the Earth. *Geochimica et Cosmochimica Acta* 41:1283–1301.

Gray C. M., Papanastassiou D. A., and Wasserburg G. J. 1973. The identification of early condensates from the solar nebula. *Icarus* 20:213–239.

Jacobsen S. B. and Wasserburg G. J. 1979. The mean age of mantle and crustal reservoirs. *Journal of Geophysical Research* 84: 7411–7427.

Jessberger E. K., Huneke J. C., and Wasserburg G. J. 1974. Evidence for a ~4.5 aeon age of plagioclase clasts in a lunar highland breccia. *Nature* 248:199–202.

Kelly W. R. and Wasserburg G. J. 1978. Evidence of the existence of 107Pd in the early solar system. *Geophysical Research Letters* 5: 1079–1082.

Kennedy G. C., Wasserburg J. G., Heard H. C., and Newton R. 1962. The upper three-phase region in the SiO_2-H_2O system. *American Journal of Science* 260:501–521.

Lee C.-T., Wasserburg G. J., and Kyte F. R. 2003. Platinum group elements and rhenium in marine sediments across the K/T boundary: Constraints on Re-PGE transport in the marine environment. *Geochimica et Cosmochimica Acta* 67:655–670.

Lee T., Papanastassiou D. A., and Wasserburg G. J. 1977. Aluminum-26 in the early solar system: fossil or fuel? *The Astrophysical Journal* 211:L107–L110.

Lemarchand D. and Wasserburg G. J. Forthcoming. Rate-controlled Calcium isotope fractionation in synthetic calcite. *Geochimica Cosmochimica Acta.*

Marvin U. B., Wood J. A., and Dickey J. S., Jr. 1970. Ca-Al-rich phases in the Allende meteorite. *Earth and Planetary Science Letters* 7:346–350.

Papanastassiou D. A. and Wasserburg G. J. 1969. Initial strontium isotopic adundances and the resolution of small time differences in the formation of planetary objects. *Earth and Planetary Science Letters* 5:361–376.

Papanastassiou D. A., Wasserburg G. J., and Burnett D. S. 1970. Rb-Sr ages of lunar rocks from the Sea of Tranquility. *Earth and Planetary Science Letters* 8:1–19.

Qian Y.-Z. and Wasserburg G. J. Forthcoming. Chemical evolution of galaxies and enrichment of the intergalactic medium. *The Astrophysical Journal.*

Russell S. S., Srinivasan G., Huss G. R., Wasserburg G. J., and MacPherson G. J. 1996. Evidence for widespread ^{26}Al in the solar nebula and constraints for nebula timescales. *Science* 273:757–762.

Schramm D. N. and Wasserburg G. J. 1970. Nucleochronologies and the mean age of the elements. *The Astrophysical Journal* 162:57–69.

Schramm D. N., Tera F., and Wasserburg G. J. 1970. The isotopic abundance of ^{26}Mg and limits of ^{26}Al in the early solar system. *Earth and Planetary Science Letters* 10:44–59.

Sharma M., Papanastassiou D. A., and Wasserburg G. J. 1997. The concentration and isotopic composition of osmium in the oceans. *Geochimica et Cosmochimica Acta* 61:3787–3799.

Tera F., Papanastassiou D. A., and Wasserburg G. J. 1974. Isotopic evidence for a terminal lunar cataclysm. *Earth and Planetary Science Letters* 22:1–21.

Urey H. C. 1952. *The planets: Their origin and development.* New Haven: Yale University Press. 245 p.

Wasserburg G. J. 1954. ^{40}Ar-^{40}K dating. In *Nuclear geology: A symposium on nuclear phenomena in the earth sciences,* edited by Faul H. New York: John Wiley & Sons. pp. 341–349.

Wasserburg, G. J. 1957. The effects of H_2O in silicate systems. *Journal of Geology* 65:15–23.

Wasserburg G. J. 1966. Geochronology and isotopic data bearing on development of the continental crust. In *Advances in Earth science,* Hurley P. M, editor. Cambridge Massachusetts: MIT Press. pp. 431–459.

Wasserburg G. J. 1987. *Isotopic abundances: Inferences on solar system and planetary evolution.* Royal Swedish Academy of Sciences. 62 p. Also: *Earth and Planetary Science Letters* 86: 129–173.

Wasserburg G. J., Burnett D. S. and Frondel C. 1965. Strontium-rubidium age of an iron meteorite. *Science* 150:1814–1818.

Wasserburg G. J. and Hayden R. J. 1955. Age of meteorites by the ^{40}Ar-^{40}K method. *Physical Review* 97:86–87.

Wasserburg G. J., Huneke J. C., and Burnett D. S. 1969. Correlation between fission tracks and fission-type xenon from an extinct radioactivity. *Physical Review Letters* 22:1198–1201.

Wasserburg G. J., Papanastassiou D. A., Nenow E. V., and Bauman C. A. 1969. A programmable magnetic field mass spectrometer with on-line data processing. *Reviews of Scientific Instruments* 40:288–295.

Wetherill G. W., Wasserburg G. J., Aldrich L. T., Tilton G. R. and Hayden R. J. 1956. Decay constants of ^{40}K as determined by the radiogenic argon content of potassium minerals. *Physical Review* 103:987–989.

Zartman R. E., Wasserburg G. J., and Reynolds J. H. 1961. Helium, argon, and carbon in some natural gases. *Journal of Geophysical Research* 66:277–306.