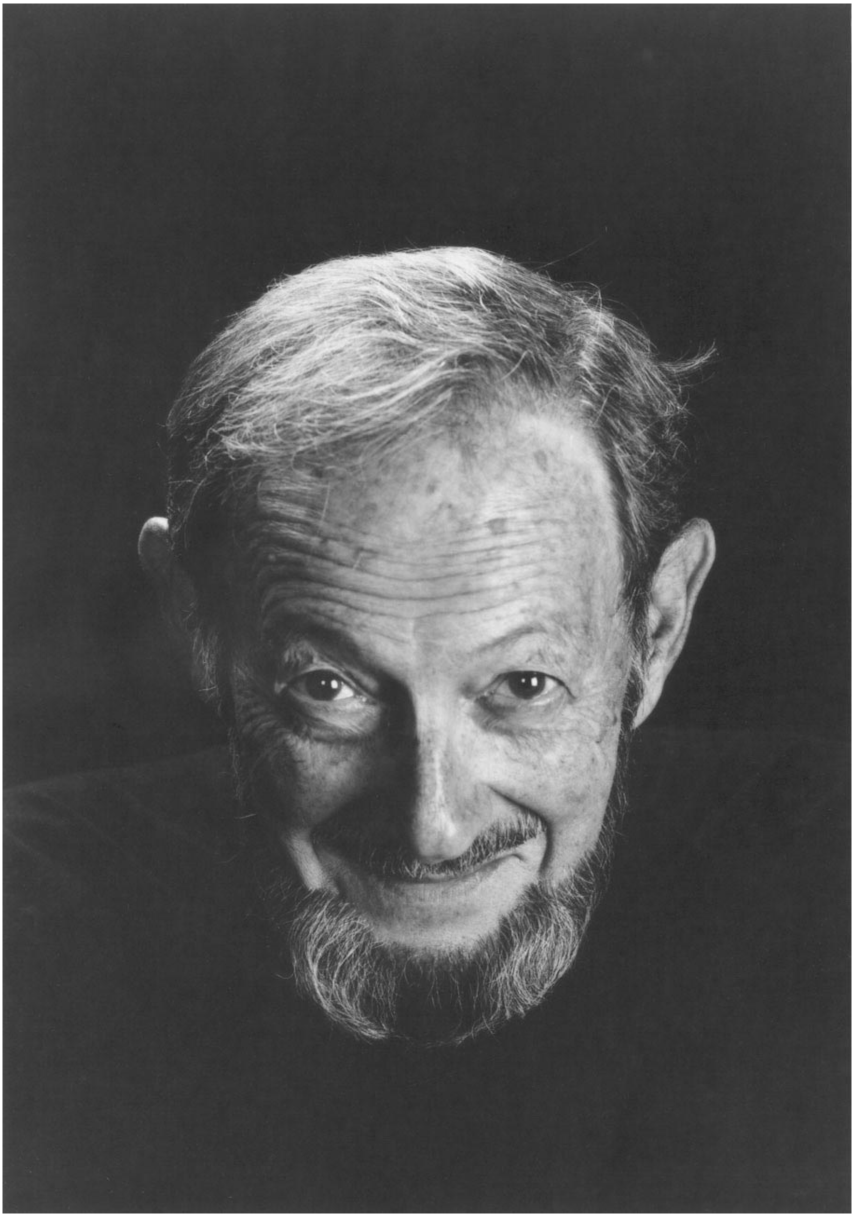

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ISOTOPIC ADVENTURES—Geological, Planetological, and Cosmic

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If you are smart and work hard, you will get by, if you are lucky.
Advice from Prof. Andy Lawson to a young student.

INTRODUCTION

When asked to write a summary of my professional life, I had no notion of how difficult it would be. There is the form and the substance, the remembrances and the reality, the discipline and the hope, the frustration of not finding a way, and the satisfaction of finding a trail. Of all of the experiences, the greatest one, the biggest turn-on, is the rare feeling that one has some understanding of nature. I have had the honor and privilege of receiving the Crafoord Prize of the Royal Swedish Academy from the King of Sweden, but my greatest occasions of excitement came when I thought I had possibly understood something. The greatest satisfaction comes in feeling that the work I have done or contributed to has some beauty. At the moment, I am finishing up a paper on groundwater transport in the Ojo Alamo aquifer, having just finished a paper with C.T. Lee and Frank Kyte on the chemistry and transport of PGE in the oceans. Just weeks earlier, I returned from an astrophysics conference on “Low Z at low z and high z,” (low chemical elemental abundances at low and high cosmological red shifts) led by Y.-Z. Qian at the University of Minnesota. It has been said that the virtue of being “old” is that you can live in the past as well as the present. Thinking about the past is not my way, except to tell old stories over good wines at dinner with friends. However, the truth is that the particular problem that I am working on at any instant is, to me, the most important thing in the world. Chasing some particular idea and some observations of nature make up the real present and govern my immediate future. In the chase, I met Devendra and Aruna Lal. He was fascinated with Indian puzzles and natural puzzles, particularly those cosmic-ray induced. Our mutual interest in natural puzzles has been a continuous source of entertainment and mutual support. Friendly interaction and critical comments on science and life and what might be true comes regularly from Karl Turekian (a Columbia product displaced to Yale). It has been my privilege to have had an exciting, productive career that continues to the present day. It is my intent to continue the exploration and the chase. The reader is warned that this report is not chronological, as the actual science in different areas often occurred concurrently.

I was born in New Brunswick, NJ, on March 25, 1927. My sister Libby Helen was born 11 months later. We were very close in both age and mutual affection throughout her life. Our mother read to us at the very earliest age. As children we were much encouraged. Our parents, Charles and Sarah Levine Wasserburg, were born in New York City. My father was orphaned at an early age and was raised to a large extent by his sister Rose. My grandparents, Morris and Minnie Levine, moved to New Brunswick around 1900. My mother grew up and went to school there, but her education stopped in junior high, as high school was not considered necessary for young ladies. After secretarial school, she worked for her father in a paint and wallpaper store. My grandmother was a loving person. She could not read or write. I remember trying to teach her to write her name. Morris Levine was quite successful in the paint business and real estate. When the depression came, the bottom sort of fell out. Our family did all right, but things went to quite a low level. My father dropped the hardware business and became an insurance salesman on small town and rural routes. What I remember is that things were not terrible, but I heard they used to be good. At least my father was working. The family, which included my mother's sister (Aunt Bessie) and her family, were very close and would visit regularly between New Brunswick and Shamokin, PA (a hard-coal-mining town). These visits always included the Fourth of July, Thanksgiving, and Passover.

I immensely enjoyed kindergarten (I fell in love with my teacher) and did very well in primary school and skipped grades. This made me the shrimp in the class, which led to problems. Our home was a small house built by my grandfather for speculation on the outskirts of town between a dirt road and the main avenue. When my grandfather Morris died, our economic difficulties increased and my grandmother moved in with us.

A professor of mineralogy, A.O. Hawkins, lived approximately 1 mile from our house and took in a bunch of kids to teach them about crystals and mineralogy. He would take us on field trips. I was hooked. I would go camping with a homemade sleeping bag, and my parents indulged me by taking me up to Franklin Furnace to collect minerals (when you could get through the fence!). I became a Boy Scout and earned many merit badges. I made it to Life Scout but could not swim well enough to make Eagle.

Around this time, life began to get difficult. The war clouds appeared over Europe and seemed very threatening. The rise of anti-Semitism in the United States with the activities of the German-America Bund, and Father Coughlin went along with the rise of Nazism. The neighborhood we lived in became hostile and in many instances threatening and quite violent. It was not something that I understood. Our families were either Democrats or Republicans—we were Americans—what was all this about?

By this time, I was in junior high school and had begun to turn into a very difficult kid. However, a biology teacher, Miss Keim, took me under her wing and let me do experiments and microscopy in her laboratory, even during non-class hours. This was a great stimulus.

In 1939, the family went to the World's Fair, the Trilon and the Hemisphere, the World of the Future of Science and Technology, and best of all the Brazilian Exposition—full of beautiful crystals and rocks—I was in heaven. As soon as we got home, I wrote a letter to the exhibitors and lo and behold I was put in touch with a beautiful Brazilian woman (I never saw her, but she had to be beautiful!). She maintained a correspondence with me and sent me gifts of beautiful mineral specimens. A couple by the name of MacGregor from Seattle, WA corresponded with me and sent me a polished thunder egg. Great!! My cousin Al also sent me a box of mineral goodies. The whole world of crystals, fluorescence, and geology became my focus. The family beamed. However, the social and neighborhood environment had a very bad effect. In high school I turned into a more bona fide juvenile delinquent, flunking courses left and right—some courses three times—and getting into real fights. My mother had always said, "Turn the other cheek." I did not find that effective in the world in which I was living. My father and mother would sometimes take me for a car ride around the Rahway Reformatory and would tell me I would be in there if I did not straighten up. My parents seemed to spend more time at school than I, as I was regularly expelled.

In the fall of 1939, the Germans invaded Poland. I joined the Air Observing Corps and would go out before dawn to the observation post and look for German planes. They were a bit far away but this was good for patriotism. I even gave lectures on the subject. The true imminence of war became even more evident, and then came December 7, 1941. I got a job at the Franklin Arsenal cleaning creosote from machine guns and rifles and crating and moving armaments (with a rather tough crew). One day my sister, Libby, and I got my birth certificate, some ink eradicator, and improved my age. I then enlisted in the Army. It still required my parents' permission even with the forged age. There was, I found out later, a family meeting where it was decided that the Army was better than the Rahway Reformatory. I was assigned to the Signal Corps, given the serial number 12206488, and did basic training. After training I suffered an extreme staphlococcus infection and was put into a ward of soldiers with infections caught in the Pacific jungles. In spite of this, I stayed with my unit when it was assigned overseas. Instead of the Pacific, where we thought we were going, we were assigned to the ETO (European Theater of Operations). We landed in early autumn in Normandy from a LSI (Landing Ship Infantry) and were charged with running the operations center overlooking the beach. My first view of a French city was St. Lo—almost completely destroyed. What buildings stood were sliced open with plumbing and toilets hanging out. It was a disaster. When the Battle of the Bulge broke out, there was a call for volunteers and I volunteered for the infantry. After some time in a REPO DEPO (replacement depot) we were sent off to the front. My buddy, K.R. Jewell III, was assigned to the 2nd Infantry Division. I was not, but decided that I would go to his outfit anyway.

I saw service as a rifleman with the 2nd Division and ended up in Pilzen, Czechoslovakia, meeting the Soviet Army. (I again met the Soviet Army under less auspicious circumstances in 1968 when I was a U.S. delegate to the International Geological Congress in Prague after hosting the Soviet geochemistry delegation at dinner.) Then we were promptly shipped back to the States for the planned invasion of Japan. The devastating attacks by nuclear bombs on Hiroshima and Nagasaki ended the war to the good fortune of many of us. We were then sent to Camp Swift, TX, where I spent some time in the stockade. My old buddy K.R. Jewell III became Sergeant Major after recovering from a severe wound. My mustering out orders came and I returned to my parents' home in New Brunswick. There, I went back to high school and graduated. My father took me to a tutor to learn elementary mathematics. It turned out that sin x did not mean what I thought. Supported by the GI Bill, I was then permitted to enter Rutgers just as it was becoming a State University. Because of my grossly inadequate background, it was only possible for me to get into night school. There, the lights turned on and I was allowed to enter the regular University. As my mother said, "They found out you could read in the daytime."

At Rutgers, I was privileged to meet Prof. Henri Bader. Bader was a student of Niggli, and an expert in crystallography, symmetry, avalanches, and ice physics. Bader and his wife Adele took me under their wings, tutored me, and mentored me. His advice and his love of science fiction, including some real science written by authors like Willy Ley, were stimulating. After two years, Bader told me that it was time for me to go to a better school and admonished me to study physics, mathematics, and chemistry if I intended to make any contributions to geology. The rest of my development simply followed this magnificent guidance. It was my good fortune to be accepted at the University of Chicago and to be rejected by Princeton. Just before leaving Rutgers, I went to a physics seminar at Princeton given by George Gamow on the production of the chemical elements during what was to be called Big Bang.

At Chicago, I was free to roam and, in the presence of truly imaginative and great scientists, could move as fast as I was capable in the company of motivated students of high intellect. I was surrounded by excellence, standards of high achievement, and lots of excitement. I was in the Geology Department but became a Physics major. I was excused from the Baraboo field course because of my experience in glacier flow studies in Alaska as Bader's field assistant. Harold C. Urey and Harrison Brown gave a course on the formation of the Solar System. I found the lectures stimulating and confusing. Some things don't change even if you understand a lot more. The future was up for grabs.

Hans Ramberg was espousing on entropy (a green gas according to him) and diffusion in rocks (as the main drivers of granitization). Urey was starting to work on oxygen isotopic fractionation in nature and on the origin of meteorites, the moon, and everything else. Bill Libby was just beginning to measure ^{14}C in nature. The Institutes of Nuclear Studies for Metals and for Radiobiology were just founded. The professors were creative, brilliant, and very demanding. The students were either highly committed veterans who really wanted to do something and get done, or very young, brilliant, fresh kids with awesome intellects.

After pursuing phase equilibria and crystal structure, I then tried to study diffusion of oxygen with lots of help from a kind and very helpful new postdoctoral fellow—Sam Epstein. As Urey began vigorously pursuing the oxygen problem, I was hired as a research assistant running a mass spectrometer. I needed to earn money to supplement the GI Bill income and to help cover tuition. I was excused from taking paleontology and stratigraphy as I had taken the physics qualifying exam. Murph Goldberger, a new hotshot theoretician, tipped me off that I had passed the examination. We were at a physics cocktail party at the time, one to which Bob Ginsburg (later to become king of coral reefs) had obtained an invitation for me. Later, when Goldberger was employed as President of Caltech, he presented me with the John D. MacArthur Professorship. This was undoubtedly payback for having endured the torture of Murph's first effort at teaching quantum mechanics. In 1951, I decided to discuss a possible thesis under Urey's supervision. He said, "Why don't you try to work on dating meteorites by using the decay of K to ^{40}A." "Do you think this could be of any significance?" I asked. He said, "Well, some Russians (e.g., E.K. Gerling) have gotten some funny results and I do not believe them. You can work together with Mark Inghram in Physics." So the deal was struck and I went to work jointly between Inghram in Physics and Urey in Chemistry and the Institute for Nuclear Studies (INS). The research was carried out between the Argonne National Laboratory (ANL) with R.J. Hayden and the University of Chicago. George Wetherill was my frequent discussion companion on trips to ANL (under tight security conditions—stockroom guards with tommy guns). John Reynolds had recently left for Berkeley after studying Xe to put bounds on the rate of double β decay using natural samples.

In the maelstrom of studying in Rosenwald Museum, I was fortunate enough to hear a mellifluous voice announcing that she was out of matches. I then helped liberate some bricks from the law school construction site for a bookcase in her room in the girls dorms (which I never got in to see). Her name was Naomi Zelda Orlick, a graduate student in physiology. I then developed a passionate interest in the field of physiology, even to the point of kissing a young lady in a lab coat just out of a dissection lab. One thing led to another (even putting up with me through the physics qualifying exam), and we decided to get married. Tosh Mayeda told Urey that Naomi and Jerry were going to get married. He harrumphed "Well, I suppose that is a good thing." We married in December 1951. On return from our honeymoon, I rushed to "my" lab late Sunday night. All my equipment had disappeared. When I went to see Urey on Monday, full of fear and trepidation, he said, "Well, young man. You were gone for two weeks so I thought you had quit, so I gave the lab to someone else." Finally, I was assigned a space in the basement of the Institute for Metals. Fifty years later, Naomi is still my first wife and my first friend. We have two handsome, talented sons, Charles and Daniel, and four beautiful grandchildren. They have all tolerated, endured, and suffered me in my scientific passions.

On Thursday afternoons, the physics research colloquium gathered at the Institute. A wide variety of intense and exciting seminars regularly took place. A host of intellectual greats, Fermi, Urey, Chandrasekhar, Teller, Libby, Turkevich, Maria G. Mayer, and on and on, were all in attendance and interacting with the speakers. We listened to lectures on all possible subject matters. Students and postdocs tried to stay in the back of the room to avoid being called up. I remember one seminar in 1954 as I was finishing my degree when Keith Runcorn came into town and talked about the magnetic field, polar wandering, and continental drift. During the seminar, Fermi proposed a mechanism of polar wandering, causing Urey some displeasure. New ideas, new discoveries, and new methodologies were the focus—all subjected to critical analysis. These were the days when the Hubble "constant" gave ages for the universe that were less than those of some rocks. Chandrasekhar would occasionally drop by to find out about meteorite ages.

During my years at Chicago, Harmon Craig (Ham) and I were highly interactive. As the work on paleotemperatures matured, Cesare Emiliani came over and began his famous work on δ^{18}O in foraminifera. Stanley Miller was a classmate working with Urey on the synthesis of amino acids and "the origin of life." Chicago was the seed bed and first flower garden for the modern era of isotopic geochemistry. There were starts in other places (Columbia, MIT) and a few were successful, but the combination of intellectual ferment and technical skills gave the clear lead to Chicago. The times were of overwhelming commitment to science, coupled with the worries of a troubled world that has stayed, as always, troubled and very dangerous. I learned how to measure, how to blow glass, how to build equipment and to run instruments, how to think, and how to identify exciting and important problems. The work load was enormous and the standards very high. Mark Inghram was my real teacher of experimental physics. He believed you had to be able to do anything and that an experiment had to be designed. I have tried to follow his example—including long hours in the lab, definitely including weekends (Figure 1).

FINDING A JOB

My doctoral exam was intense. The committee consisted of Julian Goldsmith, Mark G. Inghram III, W.F. Libby, Hans Ramberg, and Harold C. Urey. The questioning was quite severe. Libby in particular grilled me on the whole U-Th decay series, for which I was not fully prepared. The long-lived parents and stable daughters were my focus. The short-lived, intermediate radioactivities were not of great interest to me except for Rn (although I returned to the U-Th decay series 40 years later). After the exam, I was asked to leave and sat outside for an hour. I was scared. The committee then came out and congratulated me. "Was my exam so bad?" I asked. "Oh, no, not at all, we were all quite interested and got into a heated argument and forgot you were out there," the Chairman replied. Hans Ramberg was very happy and said, "You have written the shortest thesis in the history of the University." My thesis was approximately 30 pages long, composed only of two letters in *The Physical Review,* one in *Nature*, and an article in press in *Geochimica et Cosmochimica Acta*, and an article in the book, *Nuclear Geology* (edited by Henry Faul). The book was later translated into Russian.

With the the exam over, I had to find a job. It was not clear what anyone would want to do with a part-geologist, part-physicist, part–mass spectrometrist. The order of my qualifications did not matter; it was clear that I was a strange mix. It was my wish to find a job and do research. Urey offered me a position to stay on with him as a research fellow at the INS. The office assigned to me was around the corner from the Urey-Libby suite, next to A. (Tony) Turkevich's office and near

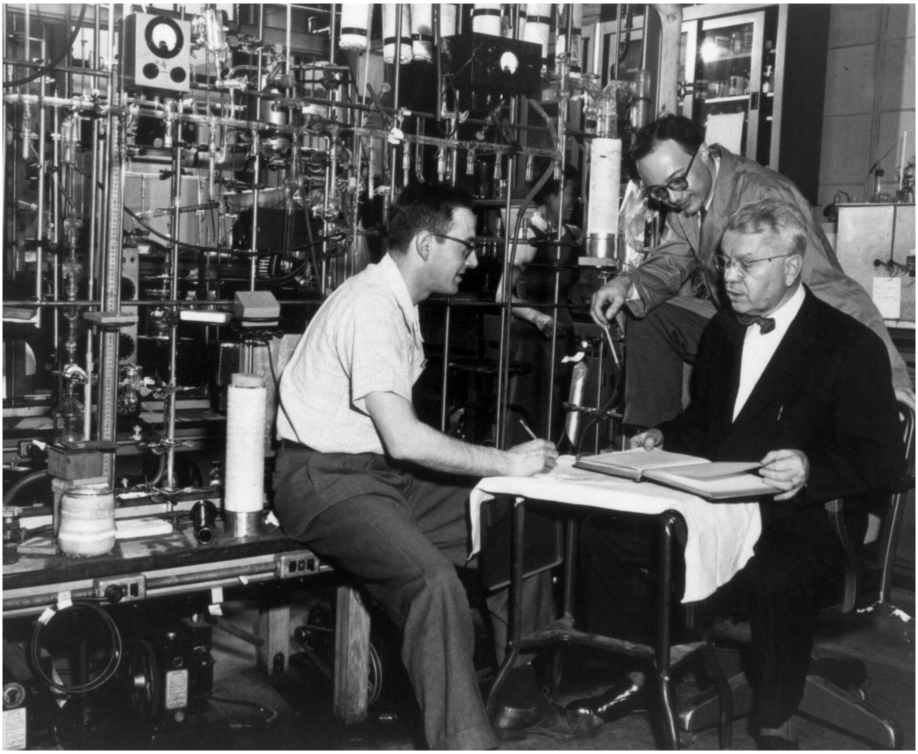

Figure 1 Harold C. Urey, Cesare Emiliani, and G.J. Wasserburg in Urey's lab, in the Institute for Nuclear Studies (now the Fermi Institute). Photo taken in 1953, University of Chicago archives.

N. Sugarman's. My office was across from where George Tilton, Claire Patterson, and Ed Goldberg were or had been working. Harmon Craig, who had graduated the year before, was already in the office and I joined him there. Craig was an exciting and stimulating office mate. He would sit with his legs crossed and discourse brilliantly on all possible subjects in an animated and disrespectful way, with his foot oscillating at a kilohertz. My second lab (which I had demolished with an explosion) was still in the basement, but I was also given another lab on the fourth floor (post Ph.D.). Later, when Claire (Pud) Patterson (then a research associate at Caltech) came back to Chicago to do Pb and U measurements or write up a paper, his lab was across the hall from mine. Claire did some preparation at Caltech, but the final work, particularly all of the mass spectrometry, was done at Chicago and ANL. There was always a feeling that the work he did was started at Chicago and, to a large extent, executed at Chicago using Mark Inghram's instruments (i.e., not really a "Caltech" product). Caltech was just getting started and had no instruments.

During this period, a new research fellow arrived, a quiet fellow from Denmark by the name of Willi Dansgaard who was assigned the project of following up on the $^{18}O/^{16}O$ work on waters that Sam Epstein had started. Dansgaard would upset me because he kept the window open in the middle of a Chicago winter in order to collect water samples from a pan he placed on the sooty, concrete sill of the Institute window. I thought nothing good can come of this madness. It is too cold. I would shout excitedly, “Willi, please close the damned window!” Willi also got to meet the King of Sweden.

There was certainly no possibility for a faculty position at Chicago. Their view was sound—send your progeny elsewhere. A visit with Urey about job possibilities yielded only general advice. Craig had discussed jobs with W.F. Libby, who had a record of helping his students find jobs. When Ham returned from his visit, he told me that Libby said “I will get you a job, but you had better take it or I will cut your X’s off!” It was thus, with some hesitation, that I went to see Libby. Of course, I was nervous and fearful. I entered into the entry office of Lucille McCormick, smoking a cigarette, and informed her that I had an appointment with Professor Libby. She called him out. He appeared at the door carrying a 100-ml beaker of coffee. “Would you care for some of my coffee,” he said. “No sir, no thank you,” I replied. “Come in and make yourself at home.” In I went carrying my cigarette and sat down in the visitor’s chair, which was on a Persian carpet. He began to talk in his typical fashion and sip his coffee. The ash on the cigarette grew longer and longer and I finally cupped one hand under it so the ashes would not fall on the carpet. The butt was getting quite hot and I finally could only hold it at the base. His face was just a few inches from mine. It was getting painful and the long ash fell in my palm. “Would you like an ash tray?” he finally inquired. “Oh, yes sir,” I said. He slid his chair back to the window and picked up a glass ashtray, reached into his back pocket and pulled out a clean handkerchief. Then he thoroughly wiped out the ashtray and closely inspected it. Finally he handed it to me—I was really hurting by then. I put out the cigarette as best I could. He immediately picked up the ashtray, emptied it into the wastebasket, took out his handkerchief again and wiped the ashtray out. Then he said, “Well, young man, what can I do for you?” By this time I was in a cold sweat. “I wanted to ask you opinion about getting a job.” “You should go to a national lab and set up a big rock dating system,” he said. “Think it over. If you want to do that, let me know. By the way, as you want to learn something about low-level counting techniques, I will get people in my laboratory to instruct you. There is a fellow here by the name of Begemann who will help you out—he does tritium.” I thanked him profusely and got out as rapidly as possible.

After escaping and calming down for a few days, I went over to Libby’s lab and introduced myself to Fred Begemann, a recent postdoc who came from Göttingen, Germany by way of Prof. Fritz Houtermans, then in Bern, Switzerland. Fred was doing ^{90}Sr and tritium in waters (and vintage wines). When I met Fred, I told him how surprised I was to be let into Libby’s lab considering how poorly he thought of me. “Oh no,” Fred said, “he thinks very highly of you and instructed me to give you all the help you wanted.” This favorable evaluation was certainly a surprise to me. With Fred’s help I tried, for a short time, to learn something about counting techniques and the interpretation of the data. Fred and his girlfriend, Margaretta, a student from International House and now his wife, have been friends of Naomi and me ever since then.

I returned to my lab and continued work on dating sediments. The idea was to use authigenic minerals in well-defined stratigraphic positions to establish an absolute chronology of stratigraphic time. I carried out field work and sample collection on the coastal plain of New Jersey. Extensive collections were obtained of glauconitic marls in Eocene and Cretaceous sections. Glauconitic limestones from the Cambrian (Franconian) were obtained. Through my friendship with the feldspar kings (Fritz Laves and Julian Goldsmith), I also sought to use authigenic feldspars to date carbonate sediments. Word of this research leaked out and some oil companies began to take interest. From a purely scholarly point of view, a big lift came when Harry Thode, the great chemist-mass spectrometrist from McMasters University, arrived for a visit to see Urey. He had sent Sam Epstein to work with Urey. He then came to chat with the young folks. He took Craig and me to dinner—a most pleasant evening, talking science and enjoying his interest and his kind and gracious personality.

Meteorites were not to be forgotten, and stimulated by a short conversation with Hans Suess in the elevator at INS, I decided to look for excess ^{129}Xe from ^{129}I decay. This was the first time that a direct effort was made to establish the time interval between “the formation of the elements and the last crystallization” of a meteorite. It started a whole new type of research and led to John Reynolds’ discovery in 1960 of ^{129}I in the early Solar System. The results of both the sediment dating and the ^{129}I search were reported at the Washington AGU meeting in 1954. That meeting was quite exciting. Our session met in a small, smoke-filled room, overflowing with people. When I later inquired why we could not have a larger room, some said, “We are just VG & P and a minor section of the AGU. We are lucky that they even let us in!” An interview was arranged for me with Merle Tuve and Phil Abelson by George Wetherill and George Tilton who suggested that they hire me. The interview went well, but I guess they decided that they had enough Chicago people. Before coming to the meeting, I had received an invitation to a private evening session hosted by Earl Ingerson for the purpose of organizing a Geochemical Society. Naturally, I assumed that Urey was invited. When I mentioned the affair to him, he told me that he had not been invited. This new society was to be for geologists who did some sort of geochemistry, not for pioneering chemists. This was absolutely offensive to me and I refused to go. The notion that it was being led by people who were not doing, and never did, real creative work, but who were trying to grasp control of this intellectual and technical revolution, was simply unacceptable to me. It was not my desire to be co-opted by their like. Approximately four decades later, I was invited to give the first Ingerson lecture and refused.

A job interview then came from Penn State, presumably from a suggestion made by Julian Goldsmith to Frank Tuttle. I stayed at Tuttle’s house and was much impressed with his machine shop and his incredible skill at building apparatus. At the end, I was offered an Assistant Professorship teaching mineralogy related to well logging, and was to set up a geochronology lab if I could raise the money. Somehow, I was not excited by this opportunity. Penn State later hired Leonard Herzog from MIT who wanted to start a mass spectrometer manufacturing company and had tried to get Craig and me to join him in the effort. We both declined as we were more interested in science than in commerce.

Mark Inghram contacted Al Nier (Chairman of Physics at Minnesota) about me and I was invited to give a seminar. I stayed at the Nier home. Nier's idea was that I should have a joint position in Physics and Geology. He offered the use of his shops and labs. Sam Goldich supported this idea from the geology side. The Geology Department had long carried forward chemical analyses of rocks as a service in support of their research program. (This was the real role of chemistry in geology!) I was to teach something in both departments. Al took me on a tour of the shops and introduced me to Buddy Thorness who was a master designer and builder of instruments. The title of my seminar was "The Measurement of Absolute Geologic Time." The seminar was in the steep-tiered amphitheater of the old Physics building. I was scared and probably gave a poor presentation. Naturally, there were questions at the end. Mostly, they were on nuclear physics and experimental methods. Toward the end of the questioning, a tall, thin gentleman stood up in one of the uppermost tiers and asked, "Why would anyone want to measure absolute geologic time?" I craned my head back to look at him. Well, this must be a physicist asking such a question, I thought and replied, "For most of geologic time, we have only sequences if we are lucky. The correlation of time between events at different places is usually not possible for most of the geologic record. When we do have a time sequence, $t_1 > t_2 = t_2 > t_3$, we do not have a measure of rates. In this case, the difference is like that between thermodynamics and kinetics." This answer was the end of my job opportunity at Minnesota. The questioner was Thiel, the Chairman of the Geology Department. I went back to Chicago without a job. Some years later, Paul Gast, a brilliant and outstanding scientist, was hired.

At a GSA meeting in Los Angeles in 1954, I presented the work on dating meteorites, sediments, and the oldest rocks. My approach to the "oldest rock" problem was to find granite cobbles in sediments cut by very old pegmatites that had been dated. With the help of F.J. Pettijohn (Chicago) and the extremely kind efforts of A.M. MacGregor (Rhodesia Geological Survey), I got some cobbles that gave feldspar-argon ages of up to 3.3 Æ. After the talk, there was a crowded lunch. A graduate student (J. Lipson) of John Reynolds' had come down to hear the talk. He sat on the floor next to my table and interrogated me during lunch about the experimental methods as they were going to set up the K-A method at Berkeley (there was still no "r" in argon).

An invitation came from N. Allen Riley of CalResearch Corp. to join them. They had previously hired Sol Silverman, a profound, deep-thinking student of Urey's. Naomi and I were invited to an elegant lunch and a tour of the facilities. They actually offered me a job! Even the pay was good. Then I was asked to give a seminar at Caltech. Sam Epstein had directed their attention to me and was an enthusiastic supporter. We went back to Chicago. One very cold winter evening in 1955 I got a phone call from R.P. Sharp from the Midway airport. Could I meet him there? He was on his way back from Washington and wished to talk with me. I drove out to the airport and met Bob. He had just purchased a box lunch to sustain himself for the rest of the trip to California. "Let's go for a walk," he said. So we walked in the dark around the airport in the cold wind with ice on the street. "We want you come to Caltech. The weather is a lot more pleasant and there is a great opportunity for you. We will get you some small support to set up a lab," he said. The salary was a lot less than CalResearch had offered and the weather was the same as in La Habra, but some of my old friends were there and it looked as if this were the chance to do the science I wanted to do. I would have to teach mineralogy and part of field geology. I was happy about half of this offer. Sometime after stepping across an icy strip with the cold wind blowing, I told him I would consider it if I could be fully independent of Harrison Brown. He said yes.

I talked it over with Naomi and we agreed to move to California. Sam and Dianne Epstein were key in supporting this decision. Now I had a job. Once I recovered from a violent reaction to a penicillin shot, we left for Pasadena sometime in the fall of 1955. In Chicago, I had a fully set up experiment to redo the double β decay investigation started by John Reynolds during his thesis—a problem that interested Fermi. Unfortunately it was not possible for me to complete it. I was off to California. I had a job!!!

While still a postdoctoral fellow at INS, Wetherill called and informed me that the branching ratio for ^{40}K decay that Hayden and I had determined using feldspars was wrong because of diffusion loss from feldspars, whereas micas had much less loss. This was a great shock to me as it was the focus of my thesis. I had selected feldspars over micas because I felt diffusion in a framework structure would be less than in a structure made of weakly bound sheets. I was deeply disturbed and went to talk with Urey. He said, "Young man, if you find you are on the wrong train, then you should get off as soon as possible and get on the right one." A joint paper with Wetherill and Tom Aldrich (a Nier student) at the Department of Terrestrial Magnetism (DTM) was then published and the issues of diffusion and my error made clear.

CALTECH—FROM HERE TO ETERNITY

Among the many faculty appointments at Caltech in 1955 were C.R. Allen, M. Gell-Mann, F. Press, L.T. Silver, and G.J. Wasserburg. The appointments in Geology were to strengthen structural and field geology, to fill out geochemistry, and to maintain and develop leadership in geophysics. The focus on geochemistry originated with R.P. Sharp in order to move into the new and exciting field of isotopic geochemistry that had started at the University of Chicago. This focus had the strong endorsement and support of Linus Pauling and Lee A. DuBridge. However, the venture into isotopic geochemistry brought forth strong-to-violent attacks against Sharp from alumni and the broader geologic community for "selling out to the chemists." Geochemistry was certainly not real geology!

Geochemical research at Caltech had been initiated in 1952 under the direction of Harrison Brown who had been attracted from the University of Chicago. The distinguished paleontologist, Heinz Lowenstam, was appointed to the Division. He had close connections with Urey in the early work on paleotemperatures and had been a pioneer in the study of fossil coral reefs. Sam Epstein, who was a postdoctoral fellow with Urey and who had led the research work on precise and reliable oxygen isotope measurements and their relationship to temperature, was soon appointed and promoted to Associate Professor. Claire Patterson was made a Research Fellow for an indefinite period without a professorial appointment. He was not interested in teaching, but in research.

My first office was on the third floor of Mudd, adjacent to the laboratory of W. Ott who was curator of the vertebrate collection. Sharp's office was two doors down the hall. The stairway had a magnificent icthyosaur mounted on the wall and the hallway was full of reconstructions of a saber-tooth tiger and a camel. This represented the past excellence of Caltech in vertebrate paleontology and the great resources of the La Brea Tar Pits. The conversion to isotopic geochemistry was a major change and an intrusion. At my first seminar as an Assistant Professor I presented a model of ^{14}C transport between the atmosphere, the mixed layer, and the deep sea. I regularly attended the Geology Division Seminars. It was always exciting and irritating when, later, someone like Keith Runcorn came through.

Caltech in 1955 was a very different place than it is today. It was really very small, both in physical size and in the size of the faculty, particularly in the postdoctoral fellow and graduate student population. The campus was bounded by San Pasqual and California streets. The East-West rows of buildings, built in what someone called Modern Mayan style, had a core of eucalyptus trees leading up to Throop Hall (later destroyed by an earthquake), followed by the olive walk flanked by the student houses and ending at the Athenaeum. The Athenaeum did not have particularly good food. No wine or liquor could be served. Faculty could have a cabinet to keep their alcoholic beverages. The trustees were adamantly opposed to serving wines. The coat and tie rule was strictly enforced, both at lunch and dinner. There was a collection of old ties (very broad ones) and jackets that you could borrow if you forgot yours or if some unfortunate guest arrived improperly attired. The cafeteria (called The Greasy) was in a cluster of barracks that were also part of the student and waiter housing. There was a barber shop. The barber would go to DuBridge's office to cut his hair. The Institute officers were the President and the Division Chairmen who held their position for life or until retirement. The full authority of running the academic affairs lay solely with President DuBridge and the Division Chairmen. There was Culbertson Hall for lectures and performances and a parking lot where South Mudd now stands. Two of my parking lot neighbors were Dick Feynman and Fritz Zwicky. Walking with them into work led to vigorous and valuable discussions and friendships. I learned a lot in the parking lot.

A sense of family and intimacy was evident within the Caltech community. The young faculty were often invited to dinner parties by the senior faculty. Soon after we arrived, we were invited as new faculty by Hertha and Beno Gutenberg to a dinner party at their home. We felt greatly honored to be guests of the Gutenbergs. Beno was a legendary figure in geophysics. The other young guests were Billy and Frank Press and Margaret and Murray Gell-Mann. Naomi and Jesse Greenstein were there to balance out the age distribution. (We recently had lunch with Jesse, now in his 92nd year and still feisty!) Upon being introduced to the Greensteins, Jesse said, "I hate all of you brilliant young people!" This was a kind of enormous compliment and a spur both to the subsequent conversation and our careers. Rudd and Harrison Brown often entertained all of the geochemistry group at their home. Those evenings were exciting and quite relaxed. Jean and Bob Sharp would hold annual parties for the Division that were always a chance to meet, chat, and gossip.

RESEARCH AT CALTECH

Through a large Atomic Energy Commission (AEC) contract to Brown, a geochemical research program was started in the basement of the Mudd building. There were technical staff supported by the contract, including C. McKinney (from Chicago), an electronics engineer with strong instrumental experience, Curtis A. Bauman, a master machinist, and E. Victor Nenow, an extremely ingenious and inventive electronics technician. The resources from the grant also supported the research programs of Sam Epstein, Claire Patterson, and L.T. Silver (a protégé of Brown's and an excellent petrologist–field geologist) and to some extent supported me.

Brown was the senior Professor in this research group. Intellectually very gifted and a visionary, he was adept at fund-raising and had a high profile both nationally and internationally. He spoke widely about geochemical research and represented the activities of the group. However, he was never directly involved in experiments nor in conducting the research. He taught a course (with other people's help) and often ran seminars and meetings. The whole activity was referred to on campus as "Brownian motion." The first main efforts of the group were to construct a mass spectrometer for oxygen and a thermal ionization mass spectrometer (TIMS) following the designs used at Chicago in Urey's lab and in Mark Inghram's labs at the University of Chicago and ANL. C. McKinney was in charge of the instrumentation. Epstein worked closely with him and guided both the design and the performance of the oxygen instrument. Patterson had a clear need for a TIMS instrument, but he was not at all a hardware person, focusing instead on the problems of low-level chemistry. Patterson always disliked and distrusted engineers, had little affection for hardware scientists, and was only interested in "the science." Brown would usually be in his office considering larger problems mostly related to societal issues. His secretary, Evelyn Brown, a very gifted and enthusiastic individual, was in charge of the office, which had a growing staff of people who often were cutting out newspaper and journal articles for or about Brown. Frequently, a swarm of people would accompany him to the Athenaeum for lunch—all in a sort of Brownian motion.

It was my intent to carry out an independent research program on rare gases in nature and to apply the ^{40}K-^{40}A method to dating geologic processes. I needed to design and build a high-sensitivity mass spectrometer for noble gases and to set up a chemistry laboratory. I also needed access to the TIMS instrument for measurement of K, U, and Th. Bob Sharp authorized some financial support to aid in building an instrument and arranged with Brown for me to have some access to the support staff associated with the AEC contract. There was already a major effort toward building an instrument for D/H measurements for Sam Epstein, and the TIMS machine was working at an acceptable level. With McKinney and beginning from scratch, I worked through the ion optics and decided on an all-metal spectrometer using the newly designed Cu metal gaskets and Alpert-type valves and valve seats. This was well before Reynolds came up with the crazy but brilliant idea of a glass machine. Essentially, nothing was available commercially from the electronics to the hardware. Progress was very slow, so for a while I worked on finishing up the work from Chicago. My first instrument was for analyzing rare gases—He, Ne, A, Kr, Xe (the HENEAKRXE)—complete with big glass to metal couplings, big mercury pumps, and a magnet on tracks. An oven was to be lowered to cover the spectrometer for bake out. NSF funds were obtained to support the research program on rare gases in nature. While trying to build the instrument, I set up a system for separating and purifying the different gases.

The first problem attacked was the origin and evolution of He and Ar in natural gases. Undertaken by R.E. Zartman, this study yielded data and a theory that gave deep insight into natural gas systems, the relationships with crustal and mantle sources, and atmospheric interactions. Studies in many other laboratories followed up on his work. One sample from a CO_2 well in New Mexico was found to have no detectable atmospheric component and would lead to a major discovery. Because the rare-gas spectrometer was not yet completed, I made the isotopic measurements of the gases in John Reynolds' laboratory at Berkeley. While running Reynolds' spectrometer, I remember its wild behavior when the big accelerator pulsed. Later, when HENEAKRXE was completed and worked, Bob Sharp gave me a bottle of Korbel Champagne. That bottle is somewhere in the garage.

I also started some theoretical studies that proved quite fruitful. A paper on the effects of H_2O in silicate melts showed the quantitative relations between H_2O in an albite melt and the freezing point depression as a function of pressure (there is a factor of 2 error in it!). This treatment considered the configurational entropy of the system and predicted the relative abundance of OH and H_2O species in the melt. Linus Pauling would generously talk with me about the problem and was very supportive. This model was unpopular for many years because of my use of theory. It proved to be a quite reliable approach. I also tried to do some IR work in the Chemistry Division, but the IR techniques then available were quite primitive. The approach was tested much later and expanded and improved by E. Stolper using good IR techniques. Later Zhang, Stolper, and I also used my model to study oxygen diffusion, all of which clearly showed the role of OH and H_2O in the system.

The success of my first efforts on the effects of H_2O attracted some interest and led to a collaboration with George C. Kennedy at UCLA. We experimentally determined the critical endpoint of the SiO_2-water system. This led to some recognition for my theoretical thermodynamic studies and experiments on problems of geologic importance. It was during my work at UCLA that Kennedy discovered an error in pressure calibration by the G.E. team that led to Kennedy's KATS (Kilo-atmospheres) into KITTENS (little KATS) announcement at the high pressure physics meeting where we supposedly were to present our work on SiO_2-water. The discovery of this error raised the question of whether the phase diagram or the procedure was patented and led to a big lawsuit between G.E. and DeBeers, who also then tried to enter the synthetic diamond business with Kennedy as a principal consultant.

My laboratory space assignment was on the second floor of Arms, as this was where the mineralogy and petrology labs were, along with a sort of wet chemistry lab with tables covered with zinc sheets. The matter at hand was to convert the old benches to a reasonably modern lab and to get started on the spectrometer. One day, a fellow assistant professor walked in and gave me a mineralogy quiz (with specimens) to determine whether I was qualified, according to his standards, to teach the course. He left satisfied, I guess. In the early years, I often carpooled with him. One day on the way home he told me that he was the one to do U-Th-Pb and Rb-Sr studies and my area was to be argon and the noble gases. That was quite an announcement! I had published U-Th-Pb work before coming to Caltech. Proprietary rights to the chart of the nuclides was not understandable to me, nor were field rights to geological regions. These proprietary claims led to future stresses and disruptions. Over the years at Caltech, I have had three Division Chairmen come to my office and inform me that I should not work on some element or isotope because they "belonged" to a colleague. Similar things happened later in another field, when some stars or stellar processes were considered to be personal property. Well, *ad astra per aspera*.

The matter of building the spectrometer and lab proved difficult. The only person with engineering knowledge and any experience in building instruments within the Division was C. McKinney. Essentially, all priorities and actions were left to his discretion. After a while, I submitted drawings to the Central Engineering Shop and instructed them to start building. This caused a major battle as it usurped McKinny's authority. I met with Bob Sharp and told him that I was competent to get the work done but must have the right to get stuff built in the shops, and that I would need help from C. Bauman and V. Nenow. Without that, it was impossible to proceed. He concurred and I then went to work. McKinney was consulted and was of real help but I designed, built, and moved ahead on my own. My life was living in the laboratory, doing the design, helping with the construction and installation, and trying to get the instrument to work. This was what I was used to doing.

TEACHING COURSES

My teaching involvement in Field Geology in Tick Canyon with Clarence Allen was something I went along with, but it was not my cup of tea. The mineralogy course that I decided to teach reflected my interests in symmetry, crystal structure, lattice energy, and vibrational modes in crystals. Although a standard text was used, the works of Bragg & Bragg and of Linus Pauling were included with as much of Evans' crystal chemistry as could be worked in. High-pressure phase transformations were covered and included the new polymorphs of SiO_2 as they were discovered. I learned a lot and it is possible that the students learned something. The students, all Caltech undergraduates, were quite bright. The materials they had learned in math, physics, and chemistry were integrated and brought into the mineralogy lectures, homework problems, and labs. Sometimes my lectures were scheduled for 8:00 a.m. The students (all male, of course, at that time) often showed up in grungy bathrobes. One lecture they came in early and set up a recording machine. No one came to the lecture, the machine just switched on. So I gave the hour lecture and announced an exam on the material. They came to the next class.

On considering the graduate students in geology, I was saddened to find that they had no basic physics or math (unless they had been Caltech undergraduates). The geophysics graduate students were much better prepared in mathematics, but their conception of physics was seismic wave propagation. They did not then, and do not now, have any training in the physics of the past 80+ years (e.g., old-fashioned "modern physics"). It seems that in geology and geophysics, things have not changed all that much with regard to undergraduate training in our universities. Students take some basic science and it is then not used in their major. It seemed necessary to raise the level up and I volunteered to teach a course (required) that was called Geomath.

The courses in math and physics outside of the Division were too difficult, too advanced, and appeared unconnected to geology. They were useful only if you knew enough to recognize their value. I think that teaching in math and physics would be improved if examples from earth sciences and astronomy were regularly included in problems. The geomath course I put together covered the basics of vector analysis, linear transformations, stress-strain tensors, simple linear differential equations, elements of fluid flow (with Gauss's theorem), and included viscosity. The basic one-dimensional heat flow and diffusion equations were given and the nature of the solutions discussed. Examples were given in areas relating to geologic problems so that the connections could be recognized. For instance, in vector analysis, the reciprocal lattice and the elements of X-ray diffraction were given. Linear transformations were related to strain. Linear differential equations were studied and related to transport problems in the earth. The course was intense and the classes were full. Homework was assigned and done regularly and marked by geophysics students. The students were urged to work with each other and the TA. The results were positive in that student skills in basic math were raised and the utility of these formal treatments was connected to geologic problems. Taught for many years, the benefits of this course began to show up in the oral exams. I do not know if anyone enjoyed the course.

After a time, it appeared desirable to change the basic course structure in geology-petrology. At this time, W.B. Kamb had joined the faculty. A three-quarter series was initiated, consisting of igneous petrology (R.H. Jahns and G.J. Wasserburg), metamorphic petrology (A.E.J. Engel and S. Epstein), and structural geology (C.R. Allen and W.B. Kamb). This wedded the senior and more classical members of the faculty with the young group. We all planned the courses jointly and attended each other's lectures. There was continuous interaction between the co-lecturers and the students. The courses were very difficult but an enormous success. In the case of igneous petrology, there were lectures, labs, field trips, and an oral presentation by the students. Thermodynamics and phase equilibria were tied together. Phase diagrams were calculated from first principles. Transport theory (element diffusion and heat flow) were intertwined. The mechanics of magma transport (magmachanics) were covered, including the problem of "the adiabatic elevator," a treatment that moved matter from very high to low pressure, from solid to some liquid. Topics such as magma genesis, heat generation, and the heat budget of Earth with and without convection were included. Isotopes were used as needed, but not as the core of the course. Stokes Law for dust settling in the oceans and the rising of diapiric magmas was presented. We looked at rocks in the lab and in the field and discoursed on their origin both in the classical manner with regard to inferences based on field observations and also in terms of broader conceptual and dynamical principles. The class notes were widely circulated, and when Hatten Yoder and C.E. Tilley came as visiting professors to lecture on igneous processes, they were much impressed with what had been accomplished in the courses at Caltech. They thought a book should have been written on the course. To me, the research was more exciting. A student by the name of H.P. Taylor was in the first course offering. I think he got an A^+. Overall, the team teaching had a strong energizing effect on both the faculty and the students. The students recognized that what we were doing was both interesting and important. Later on, with the disappearance of the senior faculty, the courses continued with single instructors.

I then taught a course in geochemistry of radiogenic isotopes—a silly title. The coverage was from the broader cosmochemical-nuclear astrophysics viewpoint to global planetary problems and to geological problems. Again, the students found it very difficult, particularly if they did not, as usual, have an adequate background. I have often felt very disappointed that this material was not included in the new curriculum of the planetary science discipline that was to become a key part of the Division. I suppose it was considered as too geological. The implications for planetary sciences were and are enormous. This has been an inadequacy of our graduate education system here.

In an effort to establish interdivisional contacts on campus, I approached Robert Bacher (Chairman of the Division of Physics, Math & Astronomy) and asked if it would be possible to have a joint appointment. He indicated that although there was interest in physics in part of my research field, a joint appointment was simply out of the question. I was, however, free to establish contacts in Physics. I began to attend the Thursday Physics Colloquia where DuBridge was always present in the front row with Robert Bacher. They set an important example, as they showed that new science was the theme of the Institute (especially for Physics). Science was the center of focus of intellectual activity. I also got involved in the activities of the Kellogg Radiation Laboratory. The science was exciting and the lab parties, well, they took care of Friday night, with time for recuperation on Saturday. Willy, Charlie, Tommy Lauritsen, and the whole gang always welcomed me.

The work with Margaret Burbidge, Geoff Burbidge, Willy Fowler, and Fred Hoyle was vigorous, exciting, and expansive. They relied heavily on the abundance of nuclear species by H. Suess and Urey and the relationship to nuclear systematics. The original search for ^{129}I by R.J. Hayden and me was of great interest to them and was cited in the B^{2}FH (Burbidge, Burbidge, Fowler & Hoyle) paper (Burbidge et al. 1957). As a result of their leadership and of mutual interests, I was warmly welcomed in Kellogg, which became my second home at the Institute. The isotopic studies that I began to undertake were tests of some of their models and ideas (often wild). I remember once sitting in Charlie Lauritsen's office having a chat and looking at the array of high honors on the walls from all over the world. I thought, "Boy, it must really be something to be so famous." Later I discovered that it was nothing, except for the private satisfaction of thinking you may have done something to understand nature a bit better.

I got involved in exploring the stochastics of age distribution patterns. This led to a very difficult mathematical problem that I ultimately, stubbornly solved. I had asked Guido Muench if he knew anything about it. He looked over the equation and told me that it was completely unknown to him. When I submitted it for publication in a mathematical journal, it came back rejected with an insinuation that it was plagiarized from a classic paper by Chandrasekar & Muench (1951). I went home, had a very, very stiff drink of whiskey, and went to bed. Later, when I showed the devastating review to Guido, he said, "Oh, yes, now that I think of it, Chandrasekar and I did write a paper on that." A summary with geologic (and later lunar) applications was finally published. Some years later I was overjoyed to find Guido very depressed at the rejection of a paper he had written, as its topic was "well-known" in some other arcane journal.

In 1957, we were fortunate to have F. (Fiesel) Houtermans, then a Professor of Physics at the University of Bern, as a visitor. F. Begemann, J. Geiss (who was my successor as postdoc for Urey at Chicago), and P. Eberhardt were his associates, and Houtermans had built up a world-class research program in nuclear geophysics in Bern after the war. This laboratory went on to great distinction in several areas. A most distinguished scientist with an extraordinary background (Germany to the Soviet Union and back to Germany after the Russo-German Pact), Houtermans was a great story teller, with lots of stories and anecdotes, all witty and profound. If I had looked more carefully in Göttingen in 1945 during the war, I would have found him there (as well as finding Hans Suess and Fritz Laves). He and Atkinson had first proposed a possible nuclear reaction to run the Sun. One evening (October 4, 1957), he was having dinner with Naomi and me at our home in Altadena. We sat in the kitchen chatting and the radio news was turned on. Sputnik was announced. We went out to stare into the night sky. Houtermans said, “Now there will be a new word in our language.” Fiesel understood.

This singular achievement by the Soviet Union ignited the United States into a critical reassessment of its technological priorities and then into action. Under the leadership of Lyndon B. Johnson, the Space Act was enacted. A new agency was formed and a new endeavor begun in response to the Cold War and the focal symbolic Soviet achievement. In addition to the ICBMs (intercontinental ballistic missiles), the United States and the Soviets were now to face each other in a pacific arena, one with enormous technical demands and great scientific potential under the leadership in the United States of a civilian not a military agency. The planning and results were to be open and shared with the international community. The space program was to be a major national enterprise with impact on all aspects of society, including the universities and industry.

SEARCH FOR ^{129}Xe

The search for ^{129}Xe excesses that Hayden and I started was further pursued by Reynolds, who again found negative results. However, in 1960 he reported the important discovery of isotopically anomalous xenon and large ^{129}Xe excesses (*). In a series of brilliant experiments, he showed that ^{129}Xe* was correlated with ^{127}I. It was then clear that ^{129}I was present in the early Solar System with an abundance of $^{129}\mathrm{I}/^{127}\mathrm{I} \approx 1.0 \times 10^{-4}$ (Reynolds 1960). The first report appeared with the calculated time of “nucleogenesis” being only 4×10^8 years before the formation of the Solar System. Willy Fowler called me and said, “Does this mean that galactic production is out?” I went away and came back the next day with the calculation and some notes and gave them to Willy and Fred Hoyle to use. Willy said “We will publish this and you are to be the first author.” The issue was nucleogenesis (à la Alpher et al. 1948) or nucleosynthesis over a galactic timescale with stellar evolution (à la Burbidge et al. 1957 and Cameron 1957). It was clear that uniform production over 10^{10} years was fully compatible with the observations, but that a timescale of $\sim 10^8$ years between the “last” *r*-process event (rapid neutron capture) and Solar System formation was required. This sort of made everyone happy, as $\sim 10^8$ years was the duration of a galactic year. People proposed that passing through the galactic arms of density waves enhanced star formation. This timescale governed the thinking for 16 years until the discovery of ^{26}Al.

A CO_2 well (Harding Co., NM) studied with Zartman had almost no atmospheric contamination. When Reynolds decided to look for excess ^{129}Xe in the earth, I proposed this well gas as a sample worthy of study. It was the right one! A small, but real, excess of ^{129}Xe was found and could not be attributed to terrestrial nuclear reactions. Following the paper’s publication (Butler et al. 1963), numerous investigations have confirmed this result. Reynolds' work showed, to our surprise, that ^{129}I had been present in Earth and hence that Earth was not now completely outgassed; there were some primordial gases remaining in the mantle. These and other primordial and radiogenic rare gases have now been found in mantle rocks by Clarke, Beg & Craig (1969), Mamyrin et al. 1969, Craig & Lupton (1976), and Allègre & Luck (1980), and exhibit a clear correlation of fission Xe (mostly from ^{244}Pu) with ^{129}Xe. They connect the chronology of Earth with that of the meteorites and with the requirement that Earth had to accumulate from mostly outgassed materials $\sim 10^8$ years later than the formation of the Solar System. (This time is now subject to question!)

One day, Willy and Fred came back quite upset from a meeting where G.J.F. MacDonald had spoken. It appeared that the K/U ratio they had guesstimated from nucleosynthetic considerations was in conflict with the standing model for the heat budget of the earth. The heat flow from the earth, if you assumed the chondritic K/U value, was found to be a perfect match for that produced in chondrites. I called this the chondritic coincidence. "What do you make of this?" Willy asked. I went away and the next day laid out the arguments against what had been brought forward and gave them a series of notes. From our studies of the K/U ratio in rocks and of the ^{40}Ar/^{4}He in gases, it was evident that Earth was not chondritic in composition. Paul Gast had earlier criticized the chondritic model based on observed relative abundances—in particular, Rb/Sr and ^{87}Sr/^{86}Sr. After a long and heated discussion, we all agreed to write a paper on the matter. Willy declared that the authorship would be in reverse alphabetical order. In our paper, we demonstrated that the K/U ratio was $\sim 10^4$, not 8×10^4, as found for chondrites, ending the chondritic Earth model. The paper established rules for global abundance patterns for Earth that led to the idea that the abundance of refractory elements relative to Si are like chondrites, but that the volatiles are greatly depleted. This was later found to be even more so in the case of the Moon. When told that Francis Birch had discussed this paper in his Presidential Address to the GSA, I knew that the argument had registered.

My main focus was on geochronology and the matter of the growth of continents. The onion structure growth model (well before plate tectonics) of J. Tuzo Wilson was my target. Bob Zartman did a classic study of the chronology of a batholith in the Llano Estacado. Marvin Lanphere and I worked on the Precambrian of Arizona, California, Nevada, and Utah. This work utilized the ^{87}Rb-^{87}Sr system as well as ^{40}K-^{40}Ar (no longer A) and some U-Th-Pb work. There again arose proprietary claims to the ownership of vast areas of geology that led to painful conflicts. I persevered in spite of the conflicts. With the generous cooperation of George Wetherill and help from Tom Aldrich, I learned the existing art of Rb-Sr analysis while their guest at DTM, and we published some papers together. I set up the ^{87}Rb-^{87}Sr method at Caltech and generated ultrapure gravimetric standards for absolute tracer calibrations. At that time, the best purity for Sr and Rb available had at least 1% contaminants. Producing new standards required going back to the old atomic weight procedures. They were demanding and difficult but the resulting absolute determinations were worth the suffering.

Diffusion processes had always been a concern (and trouble) of mine. After Nier (1939), the problem of interpreting U-Th-Pb ages was in limbo. The first insightful approach was by Wetherill (1956) in his famous Concordia paper. Tilton (1960) then showed that using a continuous volume diffusion model (which I had published during my thesis) generated a linear data array that had erroneous time implications (the false second event). This was a most important characteristic and implied that two-stage evolution models were, or could be, in error. However, L.T. Silver and S. Deutsch then found that some data arrays were not in conformance with Tilton's model. I then proposed a generalization of Tilton's model with consideration of radiation damage. There was vigorous opposition and jealousy to my doing theoretical work on data that appeared both in a journal and on the screen at meetings. I developed a rigorous diffusion model that addressed this problem of diffusion with radiation damage and the apparent second event. The calculations were done using IBM punch cards and it was not fun for my research assistant—a very promising fellow named Tony Dahlen. He delivered! The results were useful to me in understanding the diffusion problem, but turned out to be extremely important in the future and in very different areas where the same theory applied. Most people did not/do not understand what was done. For me, this mathematical analysis was challenging and time consuming, but it gave me a thorough physical understanding and had wide utility to other problems that arose. It really wasn't fancy math, just tedious.

The problem of "discordant ages" (i.e., different minerals and rocks from the same body giving widely different ages) was, and remains, a major issue in geochronology. With the presence at Caltech of Arden L. Albee, an expert in field geology and metamorphism, we carried out an intensive investigation of the redistribution of elements and isotopes during metamorphism. The focus of our attention was the "World Beater Complex" of the Panamint Range in Death Valley. With Marvin Lanphere, an intense and thorough study was conducted covering the field geology, structure, and geochronology. This major study showed how the metamorphism of a mantled Precambrian gneiss dome responded to metamorphism in the late Mesozoic. Even though original textures and structures were well preserved, extensive element redistribution and almost complete local isotopic homogenization occurred throughout the complex. The ^{87}Rb-^{87}Sr, ^{40}K-^{40}A results on many different minerals, together with U-Th-Pb data by G.R. Tilton, exposed the subtle-to-extreme variations that occur in nature and greatly aided in interpretation of geologic data during metamorphism. These results were published in the book "Isotopic and Cosmic Chemistry," dedicated to Urey on his 70th birthday. At that time, I thought that he was quite old (being five years younger than I am now). The "total rock" systems were shown to be only very roughly closed (not closed)—even for 85-kg samples. This experience of combining field, petrologic, mineralogic, and isotopic data led to the approach that we were later to use on lunar samples and on a meteorite that came from another terrestrial planet. The close working relationship with Albee would continue to be both personally enjoyable and scientifically satisfying.

E. Hellner, Professor of Mineralogy at Kiel, Germany, invited me to be a visiting professor during the summer of 1960. The other visitors included Jerome and Isabella Karle. I was to talk on isotopes and Karle to lecture on solving the "phase angle problem" in X-ray diffraction. The invitation was a challenge, as the time was not very distant from the very dark past. I had been in Germany as a soldier during the war and found the visit both interesting and threatening. Hellner's idea was to do something for the rebuilding of German universities using the United States as a model, based on his Chicago experience. Karle's lectures were most impressive, but I was convinced that no solution could come from this most complex approach—I was certainly wrong! He, too, got to meet the King of Sweden. The experience was quite worthwhile, as I got a clear view into the structure and organization of German universities, with their focus on "the Professor" in all of the University and Research Institute functions and the absence of any "departmental" characteristics—one library for mineralogy, one for geology, one for geophysics, etc. To get into another professor's library took special permission. "If you had one machine shop, who could decide priorities?" I was asked. I also got to meet a wide variety of people, some of whom were part of that very, very dark past, and some of whom were quite enlightened. My interest in the research and education structures in Germany continued for a long time. I also worried about the counter-examples in the United States where there were only rapidly diminishing technical infrastructures to support research other than what the PI (principal investigator) could get. We remain in this position today throughout the United States. The research support infrastructure has, in fact, become worse.

The 70th birthday party for Urey was held at the home of Harmon and Valerie Craig, that is, in the house, on the porch, and in the garden. Ham and family had made a gigantic weather balloon with the craters of the Moon painted on it and put a spotlight on it so that Harold could have his own moon. Naomi and I brought a nude, life-sized mannequin perched across the top of the seat in our car from Pasadena. This attracted a lot of attention on the freeway! For the occasion, we dressed the mannequin in a diaphanous, half-draped gown that the Craig girls helped put on. The mannequin wore a laurel wreath and was posed bowing and extending another laurel wreath. We presented it to Harold who was sitting on a mock throne. He kept it for a few weeks and then secretly gave it away to his machinist, feeling the mannequin was too risqué a thing to be seen with.

In 1963, I was offered a professorship at Harvard. I felt that this was the time for a change. My interest in solid state physics was aroused and the implications for earth science loomed. We had many friends at Harvard, and one of my heroes, the great Francis Birch was there, leading in both theory and experimental work with the resources of the Fund for Geophysics at his disposal. When word got out at Caltech, Willy Fowler and Tommy Lauritsen turned up the heat and entreated me to stay. I told them that it was time for me to do something new and innovative, possibly solid state physics. They said stay and do what you want here. I was invited to be a formal and participating member of the Kellogg Lab with substantial resources. After much arm-twisting and hugs by friends, Naomi and I decided to stay.

The question then became, what would I do that was new? There was essentially no solid state physics being done at Caltech. The techniques of measurement that had developed out of World War II had been utilized widely and were available to the field at large. Many of these capabilities had severe limitations. I had spent 15 years under a lamp on the kitchen table with a very sharp 9H pencil, a stainless steel straight edge and infinite rolls of recorder chart paper. Reading the charts, tabulating the numbers, calculating on a thumping Marchand calculator and then checking the calculations had become my whole function when not in the lab or the lecture room. When my Marchand thumped too hard one last time and went up in smoke, I said this is the end. The limits of precision had been reached with this approach and exhaustion came with the means of data acquisition and processing.

A PROGRAMMABLE MASS SPECTROMETER

I decided the right thing to do was to build a fully programmable digital output mass spectrometer with clean ion optics, rapid data collection to eliminate ion beam instabilities, great linear detectors, and on-line data processing using a computer. I set as my goal to improve precision and sensitivity and to cut out the manual labor. The problem of detectors was clear: Multiple detectors would eliminate beam switching, but without a large gyration radius, it would be extremely difficult to collect multiple ion beams and even more difficult if the mass differences changed with each experiment. So a single detector system was the choice, although there were actually two: one for high currents and one for single-ion counting. Then the problem was how to switch the beam. Voltage switching is trivial but causes large changes in mass fractionation, which would hurt the accuracy. That left the magnet for switching. It had to be rapidly switched from one field value to another while remaining very stable. Then, of course, it all had to be under computer control—a very tall order for the early 1960s.

Somewhere along the line, after I talked to the Kellogg group about my approach, a young fellow was guided toward me by Tommy Lauritsen. His name was Dimitri A. Papanastassiou (better known as DAP). A very bright physics student, he had been working on the accelerator at Kellogg. The suggestion was that he might work with me. With DAP, Curt Bauman, and E. Victor Nenow, we moved ahead. We traveled all over the country to learn and inspect equipment and designs. A magnificent, fast, stable magnet was ordered from Magnion. In the petrology lab, DAP and I mapped the field in and out of the magnet as a function of position. The ion trajectories were then calculated to obtain z focusing and high transmission. High voltages (15–20 KeV) were chosen to obtain a clean stiff beam.

The vacuum envelope was designed and then everything was built at the Central Engineering Shop (CES) to exquisite precision. After a while, it was not clear as to whether I was an employee of CES or of the Division. Every detail was checked and monitored by me. The whole instrument was laid out on a rigid ion optical bench to maintain tolerances. We designed, built, and installed a high-transmission beam valve. Then came the control problem. Many components were obtained by Vic Nenow from C&H (a "war surplus" parts store). Manual digital switches were initially installed. Then the problem of a computer came up. All we could do then was to lay out over one kilometer of shielded twisted pair cable through the tunnels to the computer center with an IBM 1800 and plead with the center to aid with the interfacing. DAP and I worked very closely and his physics skills and experimental insights were (and are) most impressive. When the spectrometer was turned on (1968) it worked, except that the thin-lens, Nier source broke down at high voltage and secondary electrons sprayed all over the detector. The high voltage made a beautiful beam but raised hell everywhere else. We switched to a thick-lens source and shielded the collector (almost hermetically). The result was the Lunatic I spectrometer—a real dream of an instrument (Figure 2). We could measure $\sim 10^7$ ^{210}Pb atoms that were placed on the filament in 1968. We ruined the multiplier because the lead was radioactive. Sample sizes went down, precision went up, and we were free to tackle many exciting problems. The spectrometer has been upgraded with new electronics and computers, but it flies today just as well as it did then. The precision established was close to that obtainable by ion counting statistics for a hefty ion beam (30 ppm in precision). This instrument became the standard of performance and was widely emulated both inside and

Figure 2 Filling liquid nitrogen in the ion source trap of the Lunatic I (circa 1970). Caltech archives.

outside of the United States. The only profit we had was in the pride of our design and the incredible science that has been accomplished with it.

The research and development for this new generation instrument was done with support from NSF Physics to Kellogg, Sloan funds, NASA funds, and some Institute funds. The NASA funds were injected into laboratory work through the intervention of Jack Craig, then J.E. Webb's assistant, a fellow University of Chicago graduate with a broad sense of vision, eclectic values, and a hell of a sense of humor. Having the funding come from these sources had long-term consequences. My proposal to NSF Earth Sciences to develop the Lunatic spectrometer had been flatly rejected with the reviewers' comments that it was most unreasonable to expend such a large amount of money ($150,000, I think) for a mass spectrometer. Anything over $50,000 was a waste of money. NSF reviewers claimed that there were adequate instruments available commercially to do the job. Several years later I was sent a proposal to review for the purchase (for $250,000) of a poor quality, commercial version of the Lunatic I. It was not possible for me to comment on it. I went home and got a very stiff drink and went to bed. NSF supported that new proposal and only marginal quality results ever came from that "new" advanced system. This was, and has been, my experience with NSF Earth Sciences—anything that was a really new development, either technically or intellectually, was to be rejected. Innovation and creativity were not their hallmark. Support at low-to-minimal levels to "keep things going" appeared then to be their management scheme. Support of infrastructure was, of course, anathema.

After the Lunatic I was built and in full and successful operation, I was informed by the Chairman that a second Lunatic machine would be built for a colleague with Division support. I presume this was done so he could play on a more level field. I could not object so I said to go ahead with the Lunatic II and provided the plans and advice. People who visit the Lunatic Asylum always ask "where is the Lunatic II?". Following its construction, one day I received an invitation to the dedication of the instrument. The invitation came in the form of a fancy box, within which was the LUNATIC II label that had been put on the machine in the shop. The invitation was to the dedication of the "Dulcinea" spectrometer. It would be an understatement to say that I was both offended and angry. I demanded that the original label be put back on the instrument that I had invented, designed, built, and patented. I wonder what science ever got done and published using this instrument?

BIRTH OF THE LUNATIC ASYLUM

Once the Lunatic I was working, DAP went to work on his distinguished thesis—Rb-Sr dating of eucritic meteorites (basaltic achondrites). These have very low Rb and lots of Sr. The results of this work exhibited the power and potential of the Lunatic I. All of the data defined an isochron that fit inside the error bars of previous measurements. This study established the reference value, BABI (basaltic achondrite best initial), that also serves as a baseline value for the evolution of

$^{87}Sr/^{86}Sr$ in the Solar System. The choice of eucrites was, in part, based on the information obtained from the Tony Turkevich α-backscattering analysis of lunar soils from the Surveyor 5, 6, and 7 missions. It appeared that the lunar surface contained substantial amounts of basaltic material. Future study of lunar problems involving returned samples would obviously require that we be able to handle basic rocks. Coupled with this, we had extensive experience with mineral separation, both macro and micro, and had refined the sample preparation procedures to give very low levels of contamination. We could run very small samples and obtain precise data. These techniques and skills were honed on large terrestrial samples and very small meteorite samples. The whole process of obtaining a date was, of course, complex and required the participation of highly skilled and dedicated individuals who respected clean versus not-so-clean or dirty sample handling procedures. The Lunatic Asylum was born from this.

In the geophysical theory fringe of research in the budding Lunatic Asylum there was Rick O'Connell. He paid attention to everything and developed the theory of a theoretical phase transition MOHO (the Mohorovičić seismic discontinuity in the mantle) with me (in two volumes) and then developed the theory of real mantle flow and sea level by himself. We also had a NATO Summer Fellow from France (how could France have a NATO Fellow?) His name was Claude Allègre. He had at that time a disdain for fine wines and French cheese. This opinion has, I believe, changed with the years. Claude was accompanied by a beautiful young woman with the name Claude Allègre—somehow I could distinguish between them. Allègre had read the World Beater report and came to learn Texas English from Jim Aronson, and to learn some of the arts that DAP and the Lunatic Asylum were practicing. Claude went back and brought about a rebirth of isotopic geochemistry in France and stimulated its spread throughout Europe. His distinguished career as a creative scientist (and politician) have kept us in intimate competition and friendship.

METEORITE STUDIES

The ages of stony meteorites had been more or less settled. There remained the mystery of determining the ages of iron meteorites. Patterson had established the initial isotopic composition of Pb for the Solar System from analysis of troilite in iron meteorites. This was the basis for calculation of the ages of stony meteorites and the so-called Age of Earth. Early efforts to date iron meteorites by F. Paneth were not valid because essentially no U or Th existed in the metal phase, as pointed out by Urey on thermodynamic grounds. Measurements by Hamaguchi, Reed, and Turkevich proved this to be the case. There was, however, abundant He, later found to be the result of cosmic ray production (^{4}He and ^{3}He). Efforts to determine ^{40}K-^{40}A ages gave a range between 5 and 13 Æ. This value conflicted strongly with models of nucleosynthesis. With D.S. Burnett and C. Frondel, we used silicate inclusions in iron meteorites as a basis of dating. Our first effort on Weekeroo Station proved successful. The inclusions showed a wide range in Rb/Sr and gave an age of approximately 4.5 Æ. We then applied this technique to several other iron meteorites and the results were about the same, which solved the problem.

One of the samples that I got was Colomera—the whole mass of 129 kg was shipped by the Spanish government, along with an agreement that H.G. and Maria Santillana Sanz of the Junta de Energia Nucleare would be trained in the lab. This meteorite arrived in a beautifully made wooden crate. We all stood in the hall to open it. After unscrewing the lid, there was a tightly fitting sheet of finished wood inside. When this was gently pried out, there was a drawing of Sanz with his legs in chains attached to a piano where he was playing Colomera's Bolero. The sky overhead had flying cacti, space ships, and autographs of the Staff of the Junta. I was shown standing over Sanz beating him with a cat-o-nine tails containing micrometeorites. The picture still hangs in the lab. When we got the meteorite out, it was a mass of iron full of little silicate globules. Dick Feynman came to look it over and to talk about it. We had to lie on the floor to study the cut face. The surface was rusty and I decided to clean it. We placed it on a pick-up truck and took it to JPL (Jet Propulsion Laboratory) where they had a TiO_2 sandblasting machine that I could use during the lunch hour. I put on heavy rubber gear, gloves and helmet, and began to sandblast the surface. It started to appear more metallic but one area was very rusty and I could not clean it up. The day was hot and I stopped to rest, lifted the visor on my helmet and lit up a cigarette. Then I noticed a strong light reflecting in my eyes. It was coming off the meteorite. On inspection it was the cleavage surface of a giant crystal, 11-cm long. Wow, it must be a big pyroxene, I thought. It turned out to be a potassium feldspar crystal ($^{87}Sr/^{86}Sr = 8.45$!). Of course we then determined the age of Colomera. Colomera and many other iron meteorites were not planetary cores. They were plums of iron in a pudding of silicates and represented different stages in the segregation of metal in protoplanets—all at around 4.5 Æ ago. The biggest surprise was Kodaikanal, a shocked iron full of silicates that was 3.8 Æ old as determined by both ^{87}Rb-^{87}Sr and ^{40}K-^{40}A methods. What a strange age. We would return to this kind of number on the Moon.

In all of the above work, both on stony meteorites and silicate inclusions on irons, we had to develop techniques for extracting and handling small-to-minute inclusions from large, tough masses of materials. Experience with extensive mineral separations of very large terrestrial samples was good preparation for this work. But to move ahead in this area required miniaturizing the techniques—most of the inclusions were only 1 mm in diameter. The mineral separation procedures for 10–20 kg of a terrestrial rock could not be applied to 10–100 g of a meteorite, particularly when one is extracting trace or rare mineral phases (much less than 0.01%).

As my teeth have always been a terrible problem, I was condemned to spend lots of time with different dentists. To turn my mind away from the dentist's activity in my mouth, I would think about possible uses for the tools that were attacking me. We acquired and set up a sort of upscale dental laboratory to drill out and pry out small silicates from irons. With the need for micro-manipulation, I designed a combination of an x-y stage with a microscope that permitted handling grains down to approximately 10 μm with great facility. If you do all of your work on a surface that is in focus and then move things in and out of your field of view in a controlled way, it is possible to accomplish selection, picking, and depositing with ease. The samples had to be put in some vials, but plastic was too electrostatic and glass was no good. Small polished stainless steel vials were made, big enough to hold with your fingers and easy to get microsamples into and out of without loss. Weighing paper in great sheets on polished aluminum or stainless steel was the working surface of choice.

The mineral separation procedures had to be miniaturized and kept clean. This meant a new generation of tools, procedures, and reagents used in a controlled, clean environment. The Franz magnetic separator could not dribble old debris into the new sample under study so it was redesigned with a covered track and special feed. Cross-contamination had to be eliminated. The problem of the amount of contamination from the subsequent chemical procedures (the blanks) had been faced by others before. The standard approach was to increase the sample size for a given amount of reagent. This led to a dead end. After some thought, I decided that as we had only very small samples, the only approach was to miniaturize the chemical procedures using superclean reagents (which we made) under superclean conditions in a miniaturized clean environment. This meant working with very small samples and required higher efficiency ionization in the source and high transmission in the mass spectrometry. These techniques later had considerable impact on DOE work with Pu when Alex Gancarz went to Los Alamos and transferred the technology to the lab over the protests of the radiochemists.

It turned out that this was the correct approach and proved highly successful. This approach also required the scientists and technicians to be a sort of high priesthood in cleanliness and control. Everyone finally got together and it worked. But vigilance was always necessary (Sr in toothpaste and in chalk dust, leaded gasoline was a horror!, Ag bolo ties!, isotopic tracers!). Clean lab clothes became the uniform; blackboards were out and whiteboards were in. All of the work followed on careful petrography and mineralogy, which was basic to everything. The importance of the technical developments outlined above for all future work of the lab was enormous. It was a direct consequence of attacking problems on meteorites. We went from working on large terrestrial samples using essentially standard techniques to working on microsamples, handling and processing them without contamination, and obtaining far superior data and full mineralogical characterization. The art and activity of the Lunatic Asylum and all of its inmates made this possible. When the lunar samples arrived, we were thus in an exceptional position both to define and to carry out experiments of merit. A 100-μm anorthite grain was not a formidable opponent!

THE PLUTONIUM HUNT AND GALACTIC TIMESCALES

The presence of the *r*-process nuclide, ^{129}I in meteorites, had been established by J. Reynolds. From considerations of nucleosynthesis it was clear that ^{244}Pu would be produced along with U and Th in the so-called *r*-process (rapid neutron capture).

Clear evidence had been found in a basaltic meteorite of Xe with excesses of the unshielded, neutron-rich isotopes (Rowe & Kuroda, 1965), presumably due to ^{244}Pu spontaneous fission. R.L. Fleischer, P.B. Price, and R. Walker then discovered high densities of charged particle tracks in meteoritic minerals that appeared to be fission tracks far in excess of the U present and not produced by cosmic rays. Again, this finding was attributed to ^{244}Pu. Now there were two questions: Could the excess fission-like tracks be correlated with fission products?, and What was the fission decay rate and mass yield of ^{244}Pu? Cantelaube, Maurette, and Pellas had found excess tracks in phosphates (a U-rich mineral) in the St. Severin chondrite. I obtained a sample of St. Severin from Paris and carried out a mineral separation with guidance from Paul Pellas. We had been fortunate to attract Jack Huneke to the lab from Peter Signer's lab at ETH. Huneke was an expert in mass spectrometric analysis of rare gases. He led all of the studies in the Lunatic Asylum in this field. He later led in the work on PANURGE, the first high-resolution ion probe, starting from its inception. J. Huneke, D. Burnett, and I extracted and measured the Xe on the HENEARKRXE. Rich in U, Th, and REE, the phosphate grains with the excess tracks were found to be full of almost pure fissiogenic Xe, whereas the minerals with low-track density, and low U and Th, had none. The Xe isotopic composition was the same as the best estimates for a fission-type Xe component in an achondrite. It was now unambiguously clear that fission Xe and fission tracks were associated and that they were due to spontaneous fission of a transuranic nuclide in the early Solar System (most likely ^{244}Pu). The ratio of ^{244}Pu/^{238}U at the time of meteorite formation/metamorphism was obtained, thus opening up a cottage industry of understanding Pu/U/Th chemical fractionation in meteorites, and the abundance of transuranic elements in the early Solar System (Wasserburg et al. 1969).

We had two rather short-lived *r*-process nuclei, ^{129}I and ^{244}Pu (which we called R_x), as well as ^{235}U, ^{238}U, and ^{232}Th. The final identification of R_x as ^{244}Pu came later with the direct measurement of ^{244}Pu fission yields at Berkeley. It seemed reasonable to establish an *r*-process nucleochronology for the Galaxy. After hearing my lecture on this, Willy Fowler felt that it was time to send another conscript from Physics to suffer under my guidance. This was David N. Schramm—a brilliant, restless intellect who was an Olympic class wrestler and great arguer. He came to work with me on this problem. Schramm did a prodigious amount of calculations. I opened the door one night, somewhat surprised when he appeared at my home with a big box full of computer output. "Now we have to write a paper," he said. "OK, let's get to work." I had to give a seminar in Astronomy at UCLA the next week and wondered what I could say that was really new. Going through the output had shown some simple regularities. Being simple-minded, I felt that there had to be some simple rules. After puzzling about the equations, it became clear that the output reflected a simple asymptotic form of a series expansion of the integrals. I called Dave and Willy the next morning (Saturday) and laid out my analysis. This avoided all the heavy modeling and could explain the regular, unchanging results that Willy kept getting. Dave Schramm moved ahead at lightning speed and we produced a paper on nucleochronologies and the mean age of the elements. Schramm recognized all the deeper astrophysical connections. The relationships between timescales inferred from short-, intermediate-, and long-lived nuclei were laid out and appear to have served as the basic model since that time. We showed that the $^{129}I/^{127}I$ ratio required a time interval of $\sim 10^8$ years between the last *r* event and Solar System formation. This result would come back to haunt everyone.

Fowler felt that Schramm should really learn (and be disciplined by) an experimental art and urged him to continue with me. So Schramm went problem hunting. A paper appeared claiming to have found large excesses of ^{26}Mg due to ^{26}Al decay. He immediately recognized the enormous implications of this report and said, "I am going to do ^{26}Al and prove them wrong." My comment was, "Dave, this is a great problem but please consider that as a thesis project you will be in a funny position. You will either confirm their results, or, if there is nothing there, you will be looked on as a spoiler." Competition was what Schramm knew and loved. Fear of losing was not a concept he recognized. All that had to be done was to learn how to measure Mg isotopes. With Fouad Tera's participation, the chemical separations and blanks were put into good order. The mass spectrometry was another matter, as very small samples had to be run with very high precision.

One day I came to the office and found a spectrometer chart from Schramm on the door, showing an intense Mg^+ beam. When I saw David and congratulated him, he said, "It is no good. The beam was very unstable!" "Well," I replied, "the problem of licking the stability when you have no beam is trivial. You have a beam, now the rest will follow." After intense effort and with great skill and much beating on him about experimental precision, accuracy, and calibration (not the main interests of a theoretician), the problem was cracked. My beating on Schramm, an Olympic class wrestler, is not the right description of our sponsor-student relationship. Definitive measurements were made with very high precision using the Lunatic I spectrometer that DAP and I had built. The original claim of finding ^{26}Mg was proven false and real bounds were set on the ^{26}Al problem. Along with this result came a penetrating analysis of the implications regarding planetary heat sources and nucleosynthesis. The last conclusion was, "however, because of its short, mean lifetime, the possibility that ^{26}Al was melting objects a few million years prior—can not be ruled out." Then Schramm went off to Texas and did theory with the same verve and fearlessness (including nucleosynthesis and cosmology), but always hammering on the precision of people's data. The ^{26}Al problem did not reappear for several years and the focus would be on Allende and a young student of Schramm's by the name of Typhoon Lee from the University of Texas.

APOLLO AND ALL THAT

An invitation came to me in 1967 from Wilmot Hess, Chief of the Science and Applications Program of the Manned Space Flight Center (MSC) in Houston. The invitation was to serve as an advisor to NASA regarding the handling of lunar samples. The group was called the Lunar Sample Analysis Planning Team (LSAPT), phonetically pronounced “less apt.” Our job was to guide the Directorate in what and how to deal with the lunar samples to be returned by Apollo. I had earlier signed a joint petition with Urey to President Lyndon B. Johnson emphasizing that it would be far more efficient and cost effective to return lunar samples using robotic systems. This was not in opposition to the Apollo program, but if you were going to do science, it was the simple truth. My views on this point have not changed, but apply now to future matters of doing robotic science on Mars versus a piloted Mars exploration and manned colonization program. The “manned” exploration and colonization approach is fine for science fiction but not for science or scientific exploration

LSAPT originally met at the Manned Spacecraft Flight Center in Building #2—the major administrative office in Clear Lake City/Houston. Note that Building #1 was the Public Affairs Building, which explains many things, although the numbers have been switched since Apollo. Our charge was to assess the situation in the Lunar Receiving Laboratory (the LRL) with regard to the handling of lunar samples, their processing, and the allocation of materials to approved investigators. The LRL had as its main function the containment in quarantine of lunar samples and of astronauts to prevent the possible contamination of our planet. This charge was a direct consequence of the International Outer Space Treaty signed by the United States that was concerned with forward (i.e., to other planets) and back contamination (to the earth). The possible contaminants were, of course, not inorganic toxins, but some feared and unidentified biological agent. This approach was much supported by the National Academy of Science position and by esteemed individuals like Joshua Lederberg and Melvin Calvin and voiced by the public science figure Carl Sagan.

An interagency commission on back contamination (ICBC) was created to oversee the quarantine. I worked with part of the ICBC group. The director of the LRL was an M.D., Dr. Chuck Berry. Biohazards were the main issue and the laboratory was a quarantine facility (Q). The science of the Moon was in a very low second place. The rock-science part of the laboratory was run by P.R. Bell, an outstanding γ-ray spectrometrist from the Oak Ridge National Laboratory. His focus was on counting radioactivity—primordial and cosmic ray-induced effects on lunar samples. For this purpose, a great underground bunker was built. There was effectively no one in a managerial or supervisory position who had any knowledge of meteoritics or of the modern study of terrestrial rocks. The geology issues were controlled by the USGS Astrogeology group under Eugene Shoemaker. They had as principal concerns (*a*) lunar mapping and stratigraphy and the interpretations thereof, (*b*) the difficult problems and risks in choice of landing sites, and (*c*) training of the astronauts and laying out of Extra Vehicular Activities (EVAs) for the astronauts to perform. Their scientific concerns were predominantly those of a field geologist’s/stratigrapher’s view of the Moon.

Most of the LRL was devoted to biologic cultures and observing the white mice, Japanese quail, and culture plates—all after exposure to lunar rocks and soils. Upon appropriate clean-up and clothes changes, ingress through the Q barrier was through a long corridor of high-intensity UV irradiation. Everything was done in sealed and sterilizable glove boxes. Rows of sterilizing machines occupied the halls, and rooms with white mice and culturing systems were everywhere. The whole system was a sort of effort to look like the Fort Dietrick operation–the federal center for biochemical weapons.

The first meeting of LSAPT was devoted to a general overview and to discussion of sampling; in particular, the contingency sample that was to be rapidly snatched up and stuffed into a urine bag if things went awry. Then we saw the major and only sample handling system—the F-201—a gigantic vacuum chamber with access by neoprene gloves. The sample return container (SRC) was to be washed in sodium hypochlorite to kill any dangerous pathogens. There was a problem as one could not be sure the SRC was (or could be) hermitically sealed. Moon dust on the gasket would make sealing extremely difficult. The thought of soaking the lunar samples in this mix was sickening.

There were no tools, containers, or supplies for preparation of the samples. Nor had sound procedures for documentation and processing been prepared. The issue of setting up a preliminary examination team (PET) to obtain a quick assessment of the returned materials led to the question as to whether the broader scientific community would have a serious opportunity to carry out research or if an effective inside group (PET) would be in a position to do a large fraction of the work. We considered that the analytical techniques available in the lab should be rather restricted, just sufficient to provide a basic characterization. Don Bogard had set up a mass spectrometer to do argon (at LRL), which was important. The major scientific investigations were to be carried out by the general scientific community. The assignment of samples to the outside scientific community was to be made by LSAPT with consideration of the sample sizes, the skills, and quality of results expected of a PI and collaborators. The committee sought to define optimal scientific experiments for a sample by assignment to different investigators with complementary capabilities. Approval of the submitted plan was made by NASA Headquarters (HQ) in Washington.

The first big question put on the table was the request by P.R. Bell to build a second F-201 as a back-up for the first. LSAPT felt that one F-201 was quite enough. The problem of processing rocks in a gigantic vacuum chamber and shuttling samples around in pneumatic tubes seemed very risky and no one (inside or outside of the organization) had any experience with such processing. The F-201 sat near the front of the viewing room and had been shown to Congressmen, Senators, and the President as the focal point where lunar samples were to be processed. All of these sample-processing arrangements had been previously carried out inside the Agency with little involvement from other scientists, and with little or no interest or participation on their part. Sample quarantine was the guiding principle. No technical or science review procedures had been established until LSAPT was formed. Input from the National Academy of Science occurred much earlier and was generic, with little understanding or anticipation of issues, with no mechanisms considered for evaluating the process or the outcome, and no way of finding out if the recommendations were sound. The NRC (National Research Council) Committee, chaired by Harry Hess, gave its blessing to Apollo, properly listed some important general areas of research, but did not provide any mechanism for oversight, ongoing evaluation, or advice. All specifics were omitted. The committee had strong biological inputs. Most of the scientific community was disinterested in the problems of science with lunar samples and lunar exploration. They were more concerned with what they were already doing. The attitude was pretty much, "Well, I don't know if this will really happen and I have other things to do. Let us know if you get something." LSAPT voted the second F-201 down and urged that a sample processing system be set up with a filtered dry N_2 atmosphere. This could provide great flexibility in sample handling with containment and low risk.

The meetings continued with some regularity but progress was not visible. To some extent, the outside advisors had been brought in late, possibly because of insufficient interest on the part of both parties. Certainly, the status of science within MSC was low, and the science obtained from lunar samples, other than the novelty, was not recognized by NASA or by most of the scientific community. Inside the agency, the issue was almost exclusively the successful completion of Apollo. A goal of enormous difficulty and complexity, the Apollo program was part of an international competition with the USSR and was a national commitment. What, if anything, was to be done in science was not identified, except as possibly related to the flight mission. Some of the flight experiments were very good. It was thus very difficult to attract talented young people to go to Houston to serve in a purely support role.

The members of LSAPT were a diverse group covering many fields. The only LSAPT member who was also a member of the National Academy of Sciences (NAS) was Jim Arnold. The rest were just good scientists. We certainly did not have high profile or clout. The geochemists recognized the need to prevent contamination of the samples by terrestrial materials (e.g., lead would be bad, meaning no solder or paint anywhere; organics must be avoided, no lard in the polishing compound, etc.; and keeping talcum powder out of the labs) required inspection and control. There were cases of talcum powder to cover the operator's arms when using the neoprene gloves. Discovering hydrous minerals on the Moon would be very important but the hydrates should not come not from talcum powder. The approach was to select containers and tools made of a limited number of "clean" materials to reduce possible contaminants—stainless steel, aluminum, and Teflon were preferred. Soldered brass sieves were out.

NASA was overcoming the terrible fire on the launch pad at the Kennedy Space Center that cost the lives of three brave astronauts. A great deal of credit goes to George Low who led the system out of catastrophe and into function. These matters naturally governed everyone's activities. I developed an enormous respect for the technical and management skills in the Apollo Program. The NASA staff gave great care and preparation for different situations that might develop. Everyone had the powerful desire to make it work and to succeed. Apollo was a monumental engineering system directed toward great achievements, requiring teamwork, but with a burdensome documentation (man flight qualified), bureaucracy, and a reluctance to change anything that was already approved. We grew to understand the problems and to respect them, but we did not always agree with the procedures and goals. As we were much lower down in the system, we were not able to affect policy or decisions at the level where we could aid in getting things done. After a while, certain LSAPT members resigned saying sample handling was NASA's problem and that they could not waste their time on the matter anymore. Others members felt that we had a sort of sacred obligation to see to it that these first materials returned from another known planet were well taken care of and well studied. It was this deep commitment and involvement that helped make things work out. I felt that my role would be particularly important, as I was the only one on the team with experience in designing, building, and operating clean labs and instruments for doing leading-edge isotopic geochemisty. To a large extent, this experience came from working on microsamples from meteorites

Four of the LSAPT members—Jim Arnold, Paul Gast, Bob Walker, and myself—worked closely together and with great intentness. I suppose we were real believers in the task. When writing this section, Naomi and I stepped out on the deck of our home and looked at the crescent Moon in a starry sky reflected in the rippled waters of Woahink Lake. You could not see any stars in the reflection, although they were sharp in the clear sky. All we could see was the shimmering crescent moon, as if the rest of the universe did not exist. That was the way we looked at the world back then.

As the time for Apollo 11 approached, it was evident that things were not going well. The F-201 would, I thought, be my mausoleum. It was a great Rube Goldberg apparatus. After washing in sodium hypochlorite, the sample container was to go into the F-201, then sample preparation (with hammers, chisels, wrenches) was to take place in the vacuum chamber. Selected samples were to be placed by the operator into pneumatic tubes and shot from the F-201 into the area for study and analysis. The need for tools and equipment and training of personnel was overwhelming. Some talented people were now on the staff, but nothing was in place and functioning and no one was trained. Only the quarantine procedures were subject to test. On one inspection tour, I remember hearing the announcement on the PA system—the Japanese quail are laying very well. This was to be one of the controls after feeding Moon rocks to the quail.

As the time grew nearer, I again went to MSC (Manned Space Center) and found that little had been done to prepare for sample handling and processing. I went to P.R. Bell and Wilmot Hess and demanded that we immediately get the labs equipped. Upon consulting with the boss while I waited angrily in the outside office, I was finally called in and given authorization to go back to California and acquire laboratory equipment for "immediate" shipment to the LRL. So it was back to Pasadena. I had come to Houston that morning and was on my way back in the evening. Naomi was surprised to see me back so soon. I went off to get clean equipment on a short timescale. The best sources were manufacturers of kitchen equipment. Because of health regulations, no lead or heavy metals are used in the construction of restaurant kitchen tables and equipment. Industrial stainless steel kitchen tables and chairs were immediately ordered. Then dry boxes of aluminum and stainless steel with entry ports were found in industrial supply houses. A visit to dental and medical supply houses yielded stainless steel tweezers and tools. Then to the CES shops at Caltech where I initiated the design and construction of hammers, chisels, vials, sieves, trays, mortars and pestles, wire rock saws, rock saws, clamps, sample splitters, etc. No contract had been issued for the procurement. Caltech backed the commitment, but it took several years before NASA paid for the tools. Items were shipped on an emergency basis and rooms were being built in the LRL outside of the Q area. Bob Walker found a plant outside of St. Louis that made excellent small plastic vials and sat at the plant getting the vials produced and delivered them to MSC.

At the time of the Apollo 11 EVA (extravehicular activity—out of the LEM and moving around on the Moon), we had a lab party at our home in Altadena. The entire lab was there—a remarkable and outstandingly talented group of students, faculty, and postdocs: D.S. Burnett, A.A. Chodos, O.J. Eugster, J.C. Huneke, Fouad Tera, D.A. Papanastassiou, F.A. Podosek, G. Price Russ π, H.G. Sanz, Maria Sanz, Rick O'Connell, my wife Naomi, and our sons, Charles and Daniel. Watching the ghostly figures of the space-suited astronauts Neil Armstrong and Buzz Aldrin bouncing along on the first Moon walk and collecting samples was exciting and awesome. The result of Newtonian mechanics, the then modern technology, and a national will to do something grand and competitive (but not warlike), was a success. Everyone inside the program and those of us on the periphery were excited, impressed, and proud to be a small part. Now we were just worried about the return trip and getting a crack at the samples—whatever they were. The only sound knowledge we had about the material being collected came from the chemical analyses done by A. Turkevich and coworkers on Surveyors 5, 6, and 7. This study had actually provided basic information, but the real meaning was to come from Apollo 11. The rock pictures sent from Surveyor just stimulated random nonsensical speculations.

Then it was back to MSC Houston to work through the samples that were coming shortly. The system had not really changed a lot, although some things were beginning to move. The first curator of Lunar samples (Elbert King) was chosen and sort of had a place to work in a trailer, but the samples and sample science were minor decorations on the Apollo cake. The scientists were assigned motel rooms in Texas City, far from MSC, having been displaced by the press from our previously reserved rooms at the nearby Nassau Bay (home of the Boom Boom Room). Our offices were in a leaky trailer surrounded by a ditch that was usually flooded by the rains and was a moat over which I built a much-used bridge that lasted until the great Peter Zill (Ames Research Center and ICBC) overloaded it. It broke and sank into oblivion.

The aluminum trailer carrying the astronauts arrived at the LRL with the lunar samples. The first look at lunar material came when we peered through a window into the Q area and were shown some black smears of lunar soil on a white glove.

The view in the F-201 was not much better. All that could be seen in the SRC and the stainless pans were dark, heavily dust-coated rocks. The first report by PET was made to us in the LSAPT trailer, which was fully packed with people. The speaker was R.L. Smith, better known to us as Deaf Smith as he was extremely hard of hearing. He spoke from a platform. An excellent petrographer and an accurate describer, Smith got the first real look at the treasures. He began to explain that in the soil (regolith) there were little fragments of rock with coatings of vitreous luster. Further along he described the vitreous luster of an array of globules in a sort of trail on the rock matrix and drew colored sketches. I yelled, “Liquid silicates.” He replied, “I did not say that.” Smith would describe what he saw—no interpretations and no processes. Everyone was afraid of interpretation as none of us knew what to expect. All of us were wrong in our preconceptions. Then P.R. Bell, in the front row staring through his binoculars, said, “I am sorry. I do not see the basis for your statement.” Of course, Smith did not respond as he could not hear Bell. Bell repeated and expanded his question. Smith continued, ignoring the boss. This got Bell quite upset and he started again, when Cliff Frondel shouted, “Yell! Smith is deaf.” At which time the discussion got livelier as Smith responded, pointing to his colored sketches. Bell peered through his binoculars and then said, “I still don’t see what you are talking about.” At this point Smith got upset. Then someone else shouted out, “P.R. is color blind.” Smith then went on and described beautiful, minute, isolated goblet-like objects decorated with droplets in the lunar soil. No one knew what these were until much later. Cliff Frondel said, “You know, like in Buck Rogers, the rocks were ‘zapped.’” These were then the “zap craters,” formed by high velocity micrometeorite bombardment on the atmosphere-free moon. The impacting particles were of sufficiently high velocity (a fact not then known) to cause melting as well as cratering, cracking, and spalling. Bombardment was the long-term, on-going erosional mechanism, along with the giant craters we all knew about. The rock samples were speckled with zap craters all over their surfaces and were cracked open by the impacts. The absence of an atmosphere on the Moon made all the surface a target for all sizes of cosmic debris. That was space erosion!

As PET continued to work, we got photos with scribbled sample numbers and posted them on the trailer walls. Ross Taylor did the first spectrographic analyses producing Xerox copies of adding machine tape with scribbles on them. We tried to grasp what the dusty rocks were from the descriptions and the pictures and then had to decide what experiments should be done on them. Late one night there was a problem and we were called—a quarantine violation. We saw a video tape including a sound track of the operations inside the F-201 showing the opening of the SRC. We could see the operator’s gloved hands moving things—rocks, soil, tools. Then an ectoplasmic blob appeared and danced across the chamber. Rocks, soil, and tools flew about. Then a voice said, “Damn it, get me the hell out of here!” A glove had punctured and the white cotton glove liner was drawn into the vacuum chamber. The chamber, sucked by a gigantic array of vacuum pumps, was increasing in pressure but the operator still could not pull his arms out. Lunar soil and rocks and tools were going everywhere. A chain of associates formed behind him in a crazy sort of line dance trying to extricate him. When the pumps went off, it was over and everyone took up residence in quarantine, except for those individuals who hid to avoid being kept in Q. Essentially all the LRL space was in the hands of the bio-medical people and quarantine control. The actual living quarters for quarantined individuals (including the astronauts) were not environmentally tight. Cockroaches could go in and out under the doors. It was all unnecessary, a charade, and almost ruined any science.

After the F-201 catastrophe, the only place left to process the lunar samples in this great quarantine facility was in the Biological Preparation Area (Bioprep). It consisted of approximately one square meter of open bench space in the place where the biological equipment was prepared. The Apollo 11 samples were then processed here. This room also contained the sodium hypochlorite baths and sterilizers.

At night, exhausted, in a motel room in Texas City, I got some motel stationery and drafted a letter to someone, possibly the Administrator of NASA, Thomas Paine. I felt that things were quite bad and had become worse. In the draft, I tried to lay out the basic problems; the importance of the samples, which were the real scientific treasures returned from the moon; and the need to pay immediate attention to the matter. The next day, I brought my draft to the LSAPT meeting in the trailer. The Chairman of LSAPT was W. Hess, the Chief of Science and Applications. Everyone agreed that there was a problem. Only four individuals felt that some document must be prepared, signed, and sent to appropriate authorities. These were the four horsemen: J.R. Arnold, P.W. Gast, R. Walker, and myself. The draft was rewritten and improved, typed, and signed, but sent to whom? Bob Walker knew Tom Paine and said, “We will send it to him.” Off it went.

With the continuing work of PET, we had more complete descriptions of the samples and began to come up with tentative plans for sample distribution. Just to find storage space within the giant LRL, it turned out that samples were being carted off and placed in all sorts of random containers and put into a safe office outside of Q. All in all, we completed our job of assessing what had been returned and had laid out a plan that would distribute an adequate amount of the diverse materials to the 150 groups of approved principal investigators from many nations, while conserving the major mass of samples. These masses were for future study after there was some substantial scientific information. Then we waited and worried, not only about the problems at hand, but also about the fact that we had then been informed that the total national budget for about 100 PI groups in the United States was to be approximately one million dollars. These funds were to come from the Office of Manned Space Flight (OMSF) and were completely inadequate for the community to carry out the research they had been invited to perform and to which they were committed. We could not have imagined there was not to be enough monies to cover the salaries of the people in the research groups that we had organized to work on lunar samples, and yet we were supposed to go to work. The Space Science Office of NASA had no real functioning relationship with the science program of OMSF, and OMSF was interested in issues directly relating to manned flight and Apollo. This performance was more or less repeated later with the large space telescope (LST), which became the ST. Lack of planning and inadequate management involving the failure to include scientists who were involved in using the instruments to be within the management structure and in the procurement, control, and performance loop caused major problems that weren't corrected until after a catastrophe. Later, a fix was achieved with help from Jim Westphal and a team from JPL. The ST then became the Hubble Space Telescope, which, when it was repaired, was the source of ongoing major scientific achievements and a glory to NASA and the nation.

One day we were called to present an inventory and summary of the Apollo 11 sample collection. The presentation took place in the MSC Director's great hall. We sat down and the NASA representative gave a list of all the samples and their weights listed (in kilograms). At the end of this reading Dr. R. Gilruth, the Director of MSC, said in his wonderful bull frog voice, "Gentlemen, thank you very much for this clear presentation. I do have one question, what would the weights be in pounds?" The conversion factor was promptly given. The metric-English system has been a problem between scientists and U.S. engineers for a long time and as recent experience with a Mars mission testifies, the problem is alive and well today. Then Dr. Gilruth said, "I have another matter to discuss with you. Why did you send a letter communicating the problems here at the LRL to the Administrator?" There was silence and then Jim Arnold said, "Well Dr. Gilruth, we talked with the people that we knew." That was the end of the meeting and a start of communication between the MSC and NASA administration with LSAPT as the de facto representative of the "sample science" community.

Not too many days later, I got a phone call from Gilruth who said, "Come over here and let us look at the problem." I went to his office dressed in a rather grungy fashion. "Dr. Gilruth," I said, "let us go over to the LRL unannounced and inspect the laboratory." We did that. I showed him the Bioprep area and he quickly saw what the problems were and that they certainly needed attention. The concerns that we had expressed were not hallucinations or unreasonable concerns of mad scientists. He simply had not been informed. The overwhelming problem of the flight missions had been his real concern. Bob Gilruth and I became good friends after this and I developed a deep respect and affection for him. To some extent, the required quarantine and how it was carried forward had torqued the system excessively. The quarantine function had a life of its own and co-opted many people and laboratory functions. It misdirected the major efforts and resources.

Wilmot Hess resigned not much later from his position as Director of Science and Applications. His resignation led to an editorial in the New York Times about the loss of science. However, the problem was and is much deeper than the charge that NASA was losing science because of the Hess departure. The problem relates to mission-oriented agencies that must use science and technology to achieve their goals, but have as part (and only a small part) of their function, the carrying out of purely scientific research activities. The real question is how does such an agency of government carry out its charge and maintain the competence and integrity of the scientific component. How can it do this without reducing the science to a minor decoration without real function? How does it integrate the legitimate science into its major function and not simply use it as a justification for marketing the mission, but omitting or excluding science from both operations and management decisions? Last but not least, how can the scientists avoid being co-opted by the agency supporting the program?

A new Director of Science and Applications was chosen, Anthony J. Calio, a very practical and dedicated person who was fully willing to immerse himself in the issues at hand. While he represented MSC management, he was an active chairman of LSAPT and listened to and participated in the discussions and assessment of all the problems. He wanted to help science get done. All of us developed a high level of respect for his judgment, his intent to make things go, and his good will. He developed a great respect for the samples and their scientific importance. Calio, of course, had lots of restrictions as to what he could do and lots of other tasks besides running or participating in LSAPT. Having completed our immediate charge of the initial handling of the samples, the LSAPT members went back to their respective laboratories and awaited their allocations of the first lunar samples.

LUNAR SAMPLES

Our first allocation of lunar samples arrived by courier and was taken eagerly inside the Lunatic Asylum and opened. This was the first time that I had actually seen or handled a Moon rock in a really intimate way. All the pictures we had studied were pasted on the walls of the LSAPT trailer. They were our pinups. We could not fondle them. We could mostly look distantly through an LRL window at rocks, but these experiences were not the “real thing.” We received a few grams of soils and a few grams of basaltic rocks. The soil was very fine and very dark. Upon looking with a microscope, one could find small pieces—rarely 2 mm in size, but often only 100–200 μm. There were tiny glass beads of all sorts and some rare Fe-Ni grains. Almost every “big” piece had zap craters on the surface. A broken glass bead showed zap craters on the conchoidal fracture surface. There were even two glass goblets, just like the milk drop picture, making a sort of crown—not, however, milk droplets that were going to disappear, but fixed as glass. Smith’s descriptions were right, but the vitreous luster was obviously glass—glassy craters and splashed molten rock quenched into glass. We selected a few grains and, with John Delaney, studied them using the SEM at JPL. Almost everything had zap craters and glass splashes, down to a submicroscopic level. Little submillimeter grains of glass were broken and had zap craters on them. We all then discovered that on the airless Moon, the erosion is space erosion. We knew that impacts of big objects made big impact craters. Now we could see that this process happened continuously, with smaller infalling cosmic particles impacting at high velocities, melting themselves and the target, and squirting out streams and droplets of molten rock. The impacts also cracked the rocks open. These high-velocity particles were doing a sort of super sandblasting job on the lunar surface. The droplets of molten silicates (of all scales) formed like a water jet from a garden hose. No one had anticipated this. One shock expert concluded that the droplets were produced by almost relativistic dust grains. Much later, a researcher in Aerospace, upon looking in his files, found that he had earlier produced something like the microcraters by impacting particles at very high but reasonable speeds (2 km/s). The basalts were glorious—gray-black on the outside from space erosion, but a very brilliant black on the inside with the clear reflecting crystals. These crystals were the freshest, clearest minerals that I had ever seen. The penetrating, reactive aqueous weathering typical on Earth make terrestrial crystals appear clouded due to chemical alteration. On the Moon, there was essentially no water and the crystals remained clear since the time of their formation.

We all went to work with enormous excitement. Although we were very well prepared, we were rather fearful of doing a bad job. The amount of material was so small, it was not obvious one could succeed. Yet all the preparation paid off handsomely. We made pure separations of minor minerals by handpicking many thousands of grains. Chemical and mass spectrometric developments then permitted us to determine precise ages for these rocks. Ages of 3.65 ± 0.05 Æ were found for a basalt with an initial $^{87}Sr/^{86}Sr$ similar to that of a single plagioclase soil grain. This one plagioclase grain yielded the first major result. The $^{87}Sr/^{86}Sr$ was just slightly above BABI—and miles below anything from the earth. The Moon was greatly depleted in volatile elements. Although all five basalts analyzed gave the same age, the differences in $(^{87}Sr/^{86}Sr)_0$ required that they come from different magma sources in the Moon. The age was the time of basalts flooding the Sea of Tranquility from different magma sources within the Moon. The cause of melting was another question!

Soil as a bulk material and one micro-rock appeared to be $\sim$4.5 Æ old. A funny contrast existed—the soils (which are space-weathered rocks) were old and the rocks were younger. Otto Eugster & G. Price Russ π measured the cosmic ray reactions, including the secondary neutron exposure using remarkably precise Gd isotopic measurements. We also measured Xe implanted by the solar wind. It was found from measuring isotopic shifts in Gd as a neutron monitor, that the lunar regolith was mixed to a depth of approximately 6 m by bodies impacting over the past 3.5 Æ. How could the soils be old and the rocks young? We inferred that a part of the soils were enriched in the radioactive elements Rb, Sr, K, U, and Th and came from the lunar crust formed nearly 4.5 Æ ago. We were thus seeing back through the period of massive basaltic flooding to the early lunar crust. The "soils" represented a mixture of the early crust and the younger lithophile element poor basalts. The rocks were all old, older than any rock found on Earth up to that time. The Moon was not a chondrite, nor the source of the basaltic achondrites. Having formed a crust quite rapidly, the Moon was quite its own kind of planet. It was the first time that we saw that planetary crusts can form very rapidly and early.

On a very cold January day in 1970, the great rock festival began at Houston's Convention Center. Many dignitaries attended in addition to the research scientists. All groups were restricted from any prior public communication. Each science group had worked independently and all of the results were reserved for presentation at the First Lunar Science Conference. The great show began and what a show it was! The diversity of observations was remarkable. Some laboratories were well prepared and showed excellent quality results and insights, others were just not ready. Grenville Turner did an outstanding job on ^{40}Ar-^{39}Ar dating. The most outstanding thing was the richness and diversity of the scientific contributions from many different areas of science.

Studies ranged from the composition of the solar wind implanted on the surfaces of lunar rocks, to the composition and structure of the moon's interior. Great excitement mixed with great competition, of course. Coming from diverse scientific fields, some groups had difficulty understanding what other groups reported. At the end of the conference, some of the "experts" who had been theorizing extensively about the Moon prior to Apollo presented summary talks. Somehow, the real truth was not easily digestible and builders of fairy castles brought their theories out again and displayed them for one last show. They all more or less re-announced their old incorrect theories (including that of the thick dust layer through which the Lunar Module was supposed to sink out of sight). The hot Moon–cold Moon was gone (we went to a once hot, now cold moon), and the chronology of great crater formation have all changed. The expected young volcanoes that were chased through all the missions were nowhere to be found. Neither life nor water was to be found. The game had completely changed. Most of the old disputes were dead, most of the old surmises were wrong. The conference saw the inception of a new scientific community, one that defined not only lunar science, but the whole field of planetary science. This new, multi-disciplinary, scientific community would generate new concepts and diagnostic information that could be generalized to apply to more than one planetary body. There would be new theories—some of which would be wrong—new generations of ideas and problems. The technical skills required in order to make progress were clearly exhibited by leading groups at the conference. A new generation of highly refined instrumental and chemical techniques had established levels of precision and sensitivity far beyond what existed previously. These improvements permitted analysis of very small samples. The standards of excellence were evident and the drive to achieve these levels of excellence immediately spread throughout the scientific community. Of course, there would be the problem of finding the resources to upgrade the laboratories and to train the players. This scientific activity surrounding Apollo was the real fountainhead of what has become planetary science.

Sometime during the presentation of old, incorrect theories by the "old experts," Paul Gast and I left the hall in dismay and went into a room with the engineering staff to design the dry nitrogen processing line to replace the F-201. The new equipment was to be built in front of the old beast so as to obscure it. If and when the F-201 disappeared, it would no longer be the center of attraction from the viewing room.

There was an understanding that a special issue of *Science* was to contain all the first reports. We all prepared our papers, which were quite restricted in length. I had decided that the authorship of the lab's work should not be associated with one individual, but in respect to all the extremely creative and skilled people, there should be a laboratory name. As in the original abstract submission to the first Lunar Science Conference, I used the name "The Lunatic Asylum" for the lab with a footnote indicating the list of "inmates." The paper submitted to *Science* followed this format. After a time I received a call from the Editor's office informing me that our submission had received very favorable reviews, but there was one small problem. The Lunatic Asylum was not quite O.K. Thus the battle began. Phil Abelson, an old friend and the Managing Editor, called and said this was not acceptable. "Will this be referenced as Asylum, L in the literature? This will have a bad effect with members of the Congress if they think that scientists are not serious." It went on and on. I inquired whether the quality of the work and the presentation were inadequate. On the contrary, they assure me that it was absolutely first class work. I replied, "Well, then publish it!" "We'll see," was the response. Over the next week, I was visited by some of the most outstanding scientists who inquired what research we had been doing. Of course, I showed them all of our results and the preprint. This resulted in laudatory comments about the lab's achievements and raised comments from them about the Lunatic Asylum, suggesting that the name be dropped. After a bit, it was evident to me that this suggestion was orchestrated. I told Phil Abelson, if they wished, I would withdraw the paper and publish it elsewhere. They acquiesced and it appeared as submitted. The Lunatic Asylum became a regular word in the scientific literature in many journals. We have been cited as Asylum, L; sometimes as Albee, A.L., et al. Mail with little other information than Lunatic Asylum on the envelopes has been received from several countries. The various inmates have gone on to distinguished careers all over the world. The laboratory has continued to produce valuable scientific work that is published under that title and has continued to distinguish itself in a wide range of fields.[1]

NASA put together a team to present the results of Apollo 11 at a COSPAR meeting in June 1970, to be held in Leningrad. Getting in and out of the Soviet Union was not a cheering experience. I was invited to present a paper on the scientific returns from Apollo and to aid in preparing a general display. I chose the SEM photos and made a set of large (80 × 80 cm) mounted pictures of the zap craters and of lunar material. Put up for a general public exhibition in a large hall, they proved to be a focus of interest. As for my scientific presentation, when I got to the podium to deliver my address, there was a large draping of muslin fabric that was used as the screen for projecting the transparencies. The hall was filled. As I spoke, regaling the audience with the scientific discoveries,

[1]The full list of Lunatic Asylum papers can be obtained from isotopes@gps.caltech.edu.

even more people came in, sitting on the steps. They even became quite a bit noisy. At the end, the house was packed. Applause, and then the next speaker was introduced. It was Neil Armstrong. Later I told him he owed me a great debt for attracting so large an audience to his talk. This event put many things in perspective. The accomplishments of Apollo 11 were so large on the world scale that they can not be overestimated. Much vodka and toasting, and I escaped and left for home.

Apollo 12 launched in November 1969. We had to face the assessment of the new lunar sample collection, the allocation of materials, getting back to our own institutions to do our teaching, and doing our own analyses of the new samples that arrived immediately after the first rock festival. By this time, the operations in the LRL had somewhat improved but were still governed by the quarantine rules and Bio-Med activities. Almost no space was available. The new equipment had been delivered, installed, and was beginning to be put to use, but the conditions were still make-do, with substantial and ongoing risks to sample integrity. LSAPT at this time made a rubber stamp that said, “WE VIEW WITH ALARM.” We attached this stamp to our memos when serious problems arose, a not infrequent occurrence. Around this time I decided that more action was needed. I got an appointment to see the President’s Science Advisor, Edward David. It was my hope that sufficient attention would be given to enable “sample” science to proceed in a sound manner. After all, the usual view is that a rock is not an object of real interest or value. However, these rocks were the first samples ever obtained from another known planet. Previously, all that we had were samples of Earth, plus whatever debris fell in as meteorites from absolutely unknown sources. In addition, we had starlight. By some special grace, the appointment was granted. To balance the presentation, I asked George Wetherill to come with me in the hope that his more serious presence would convey some weight. I had never had contact with officials at this level and was very fearful that the Science Advisor would consider the visit as that of a special interest lobbying group. When we entered the outer office, I saw that there were display cabinets containing beautiful mineral specimens. This sight brought a big smile to my face—there was a real chance that Ed David might understand. He listened to the presentation with considerable understanding and sympathy and indicated that he would be in contact with the NASA Administrator to have them look into the matter. I have been fond of him ever since then. A short while later, Wetherill and I were asked to go to NASA HQ to meet with Dr. Homer Newell, the Chief Science Officer. He asked what the problems were and why had we gone to the White House. We described the issue carefully. Meanwhile, I was smoking a cigarette and there was no ash try—just a plate of hard candies. The candy plate solved the cigarette problem; however, Newell was a bit shocked

Sometime after this discussion, at one of the LSAPT meetings, a special session was called. An oversight group consisting of R. Gilruth, several senior NASA officials, Frank Press, and others was convened. Gilruth asked, “Just what is it that you scientists want? You are even asking for changes in EVA activities.” Jim

Arnold replied, “Well sir, for example, we would like to know which side of the rock is up when the astronauts collected it. You see, the recent solar wind sample should be on the upside.” We explained the basic problems about the sample handling, the need for a “man-qualified” rake to collect little pebbles, not just very fine regolith (e.g., lunar “soil”). We needed documentation methods in the LRL, space, and proper sample handling procedures. We did not want to lose the most critical samples to the feeding of animals and the growth of plants. We did not want large samples from the bottom of the drill core through the lunar surface to go to “bio-testing” protocols. The outcome of the session was beneficial. The LSAPT group were no longer perceived as a collection of crazy, irresponsible scientists, but represented legitimate concerns and requests that were operationally reasonable. Jack Sevier from MSC was present and developed into an extremely helpful person in trying to get sample science integrated into the system in some way. The meeting made evident that there was a community of rational, practical scientists who considered the lunar samples to be the major scientific return from Apollo, and who wanted to optimize this scientific return both in the mission and with the returned samples.

In 1970, I received the Arthur L. Day medal of the Geological Society of America. This was my first scientific award and it is greatly cherished. Many of my heroes were among the previous recipients. The award presentation ceremony and banquet at that time was quite formal—ladies in evening gowns and men in black tie. To me, the medal was major recognition for my efforts at attacking geologic problems with physics and chemistry. However, the outlook toward the “geochemists” by some sections of the community was expressed by Jim Giluly. He and his wife, in formal attire, came up to me as Naomi and I stepped down from the podium. Rather less than sober, he said, “I still say, you are no damned geologist”—another enlightened view of a “black box” scientist. I am glad that the viewpoint of the then Chief Geologist of the USGS has not persisted in the field. Currently, I am more concerned that young people are not worrying enough about the rocks. They seem to think that samples come out of vials!

We went to the Apollo 13 launch and came back to MSC to prepare a sample handling plan. Then it happened. “Houston, we have a problem!” Although regularly briefed, we found ourselves in a most distraught state. This great crew was in incredible jeopardy. I personally did not see that there would be a way out. When it was all over, I chatted with Jim Lovell in the parking lot next to his red Corvette. “Jim,” I said, “I did not think you guys would ever come back.” He replied, “That thought never occurred to us. We knew the system would work.” He was right; it would and did work. Apollo 13 was in its way the most remarkable mission. The incredible support and empathy, both within the system trying to solve the problem and throughout the whole world, was astonishing. The “marooned in space” crew became all of us or we became part of the crew. They were connected to Earth through the umbilicus of a communication link and were guided through this link to critical and intelligent action that solved the problem of their safe return. The skill, dedication, and bravery of Jim Lovell, John L. Sweigert, Jr., and Fred W. Haise were remarkable. They gave me a photo that is still on my wall that says—“Sorry we couldn’t bring back any rocks.”

The quarantine persisted. Individuals concerned that liverworts grew very well on lunar soils when watered, were informed that plants grew well on fresh volcanic ash, as it was full of nutrients. The international treaty signed by the United States certainly needed to be respected. However, instead of being a rational response, quarantine developed into an inflated and uncontrolled show. Somewhere along the line John Wood, who had done important work on the lunar highlands (the anorthositic crust), made a cartoon of Paul Gast and me floating above Tony Calio and Jim Lovell with a cornucopia maiden pouring out lunar samples to approved PIs. This cartoon was made into plastic-covered badges as a pass into the LRL (Figure 3).

Originally, scientists were unable to determine the content of organic matter in the Moon rocks reliably because of the extremely low levels. They found that the rocks contained no identifiable organic material. In the beginning, the National Academy had made a recommendation in support of the quarantine action. They then failed to make any effort to review (*a*) the efficacy of quarantine, (*b*) whether a continued need for quarantine existed, and (*c*) what impact quarantine was having. LSAPT had no means of access to the Academy. Jim Arnold, who was an Academy member, finally got the concerns of LSAPT expressed. From the Lunar Receiving Laboratory, I snitched a case of 8 × 10-inch 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 the photograph at my personal expense to members of the House and Senate and to the scientific community at large. After Apollo 14, the quarantine charade was finished. Now all that remained was to establish a real laboratory to handle and store lunar samples.

The quarantine approach described above persists to the present time, preventing a reasoned approach to the return of Martian samples. Even though we believe that Martian material, not to mention Moon rocks, cometary debris, meteorites, interstellar dust, and all sorts of exotic materials have already fallen onto Earth, quarantine is perceived as a necessity. It is my impression that this perception represents a desire to titillate the public mind with possible dangers, to make sample returns seem more exciting and risky. The real risk is that we fail to move ahead in a judicious but aggressive fashion toward understanding nature. I remember seeing the movie “The Andromeda Strain,” for which the LRL type quarantine system served as a sort of model. I laughed so hard through this wonderful type B thriller. People love wild fantasy more than the true adventures of nature. They often fear imagined dangers rather than real ones. I have even heard it later proposed at a meeting with J. Fletcher (with support from Carl Sagan) that a manned, earth-orbiting laboratory was necessary to study Martian samples in order to avoid the risk of infecting the earth. A special NAS study on the effects of an unsterilized entry probe on Jupiter was thus provoked by Sagan based on the hypothesized risk that we would contaminate that planet. The probe later flew on the Galileo

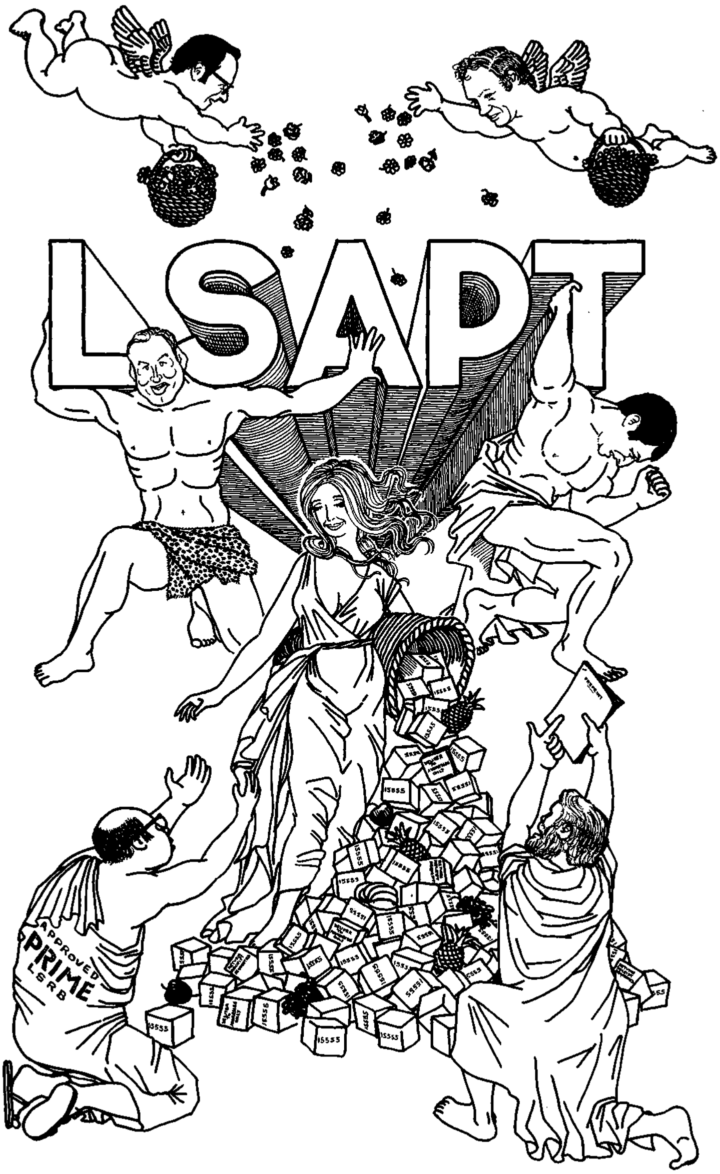

Figure 3 The Great Seal of LSAPT issued during Apollo 16 and used as a pass to the LRL. Persons shown are approved, overfed PI; poor, hungry, pleading unapproved scientist; and the LSAPT goddess doing late distribution of Apollo 15 samples of only one type (which investigators did not request). Jim Lovell and Tony Calio are holding up LSAPT, the cherubs are Paul W. Gast and G.J. Wasserburg. Created by John A. Wood during his term as deputy director (under commission to the LSAPT) using the government-supplied presentation pad, magic marker, and rubber cement in the leaky trailer.

mission. Some people will go to any length to titillate, particularly if it requires even larger expenditures and greater bureaucracies that they may lead. The upcoming Mars missions have again raised issues of quarantine, yet the official government proposal to put a human base on Mars does not refer to the international agreement on outbound contamination. Perhaps we would send sterilized astronauts without metabolic outputs. (Think of the human waste generated on the flight!).

During this time, MSC convinced P.W. Gast, a deep-thinking and imaginative geochemist, to join the center and to manage the science program. Fortunately for the program, Gast left his professorship at the University of Minnesota and took up the task of establishing a real science activity at MSC. Gast had the commitment, a sense of high values, and excellent scientific judgment. He brought integrity, function, and direction to the program and attracted more first-class scientists, including L. Haskin and L. Nyquist, to work at the center. He built a science team of merit with a charter to go to work. Paul and I were in continuous contact and worked together (one inside MSC, one outside at Caltech) to keep things on track. When I was not in Houston, we would talk on the phone for at least an hour a day, often late at night, in order to identify problems and the means of solving or avoiding them. During this period of time, my colleague and friend at Caltech, Arden Albee, would greet me as “General.”

Unfortunately, Paul Gast fell victim to an aggressive cancer, yet he worked up until the end. We continued to have lengthy talks on the phone, even when he was hospitalized. During the Apollo 14 mission, Tony Calio and I visited him in the St. Joseph’s Hospital, Houston. We were watching the EVA on television when some nursing attendants came by and said they thought the whole thing was being filmed in Hollywood and it was all a fake. We got a sad laugh out of this. Here was a fantastic human achievement yet so many people could not ascertain the difference between fantasy and fact—between apparitions and accomplishments. Paul died shortly thereafter, a terrible loss to his wife, Joyce, to their sons, Mark and David, and daughter Judy. The scientific community had lost a brilliant intellect and leader. I had lost a friend.

Sometime just after Apollo 14 (launched on January 31, 1971) I went off with Naomi, Charles, and Daniel for a vacation at the beach where there was no telephone. During a beach walk we met Anatol and Aydeth Roshko, who told me there was a radio announcement that the remaining Apollo missions would be cancelled. I went back to the cottage, got some change and a credit card and rushed up to the highway where there was a phone booth. The noise of the big rigs made it almost impossible to hear. The report was confirmed—but what to do? The J-series missions, those with the greatest range possibilities on the EVA, would be gone! The announcement ended my vacation. Now what? I decided it was time to go to war. At a GSA meeting in Milwaukee, Jim Arnold, Paul Gast, Bob Walker, and I got together. An effort would be made to get the scientific community to write to their Congressmen and Senators. I hired a secretary, out of personal money, to aid in preparing and sending letters, telegrams, etc., to all possible persons in the government. The great cartoonist, Conrad, published a cartoon showing the disappearance of the Moon and of the Apollo program. I got permission to reproduce the cartoon and had a large number of copies made with a comment added that for 25¢ per person per year in the USA, the Apollo program could continue. (The arithmetic was not quite correct.) Several hundred of these were sent out to government officials, Congress, and scientific societies, including the NAS. Interviews with the press got public attention. The combined effort, led by the four horsemen, had substantial impact in Washington circles. Some Congressmen asked, "Who was this big lobby, where did they come from?" One day not long after, I got a call from Paul Gast saying that there would be a dinner at the White House for some NASA officials and two representatives of the science community. Jim Arnold and Bob Walker were chosen and invited to dinner by President Nixon. At the end of the evening, Arnold and Walker thanked the President for the invitation to dinner and were about to leave. President Nixon shook their hands and said to them, "Let me see, gentlemen, if I remember—fifteen, sixteen, and seventeen." We knew then that we had won! If you want to know exactly what happened, ask Jim Arnold and Bob Walker. These final three missions flew and returned a treasure trove of scientific specimens and experimental results that are still a focus of research. I was personally overwhelmed with the results of our efforts. Apollos 15, 16, and 17 flew beautifully. The interval between launches continued to be less than six months. The pressures on the Agency were enormous and those of us working in support of the missions were heavily stressed. The final mission, Apollo 17, had the first scientist crew member, Harrison (Jack) Schmidt. They brought back the orange soil (a shocker with a volatile rich coating) and a host of wondrous things including an ancient dunite that we dated at 4.5 AE—but no young volcanics.

It was clear that the decision to redirect the remaining Saturn V systems was related to the high risks (as exemplified by Apollo 13) and the major policy—what would NASA do next. The goals subsequent to successfully completing the charge given by President J.F. Kennedy, that of "landing a man on the Moon and returning him safely to earth" were not defined. The success of the Apollo missions, well inside the promised time frame, meant that new objectives needed to be defined for this large and dynamic civil space enterprise to continue. von Braun's view was to fire the Saturn Vs off like firecrackers and then to go forward with a shuttle, tug, and lunar base program (you can see where we are now). A much smaller, yet significant, unmanned space exploration program was being carried out by NASA's Office of Space Science and was considered to flourish under the umbrella of the larger Apollo program. It was the effort to find a new, long-term objective with high demands that would govern NASA considerations after the feat of Apollo.

LUNAR SAMPLE FACILITY

The next issues of importance were the creation of a facility for processing, handling, documentation, and retrievable storage of lunar samples on a regular and ongoing basis, and the long-time storage of part of the sample collection at different, secure sites. It was evident that the LRL was not designed as a lunar sample facility, but rather as an inadequate quarantine site that had lunar samples in it. The large areas committed to quarantine control were not available for use. They could not readily be taken from control of the Bio-Med people nor could they be readily converted to ongoing processing of the lunar samples. This was impossible during the missions and after them.

On consulting with Michael Duke, the Curator of Lunar Samples, I suggested that a separate Lunar Sample Facility be created. This was proposed to NASA HQ. At that time, Noel Hinners, who had been in charge of the Bellcom operation (involving site selection and verification for NASA), was appointed to NASA as Deputy Director of the Lunar Science Program. He was appointed by John Naugle the Associate Administrator for the Office of Space Science (OSS). A lunar science program was then founded within the OSS with strong connections to the Manned Space Program, but with a franchise to manage and support the lunar sample science program inside of OSS. The concept of a Lunar Sample facility was not enthusiastically received. I began to acquire data and arguments to back up a presentation. It was necessary to have a building for the ongoing processing and research on lunar samples that could provide access to materials, under controlled conditions and with good documentation, to the general scientific community outside of the MSC, as well as to the research group already at work there. The facility also required an ongoing staff with the necessary training and technical skills. Just storing lunar samples in rock storage drawers with painted or stuck-on labels was not acceptable. Contamination control was necessary so that the samples were not exposed to terrestrial materials and were kept in an inert atmosphere. The object of lunar research was to understand another planet, not to discover terrestrial contamination.

There were opportunities for many interviews with the press, where the importance of the lunar samples was emphasized and the problem of keeping them from deterioration was explained. “Moon Rocks Rust,” and other statements appeared in the press in support of a sample facility. Many members of the Lunar Sample Science community wrote to their representatives, and some spoke with their senators to emphasize the importance of both the program and the issues regarding a sample facility. I remember one visit to NASA HQ where I ran into George Low in the hall. He said, “Wasserburg, if there is a Lunar Sample Facility, it is going to come out of your budget!” That was amusing as I was just an advisor and did not have a program budget. I felt that at least we had gotten some attention. A design was submitted and finally brought forward by the NASA Administrator as a line item in the budget. Proponents of the facility confronted two big problems. One was to convince the Congress that such a facility was needed. The second was to see that the building design would take into account long-term hazards. In the Houston area, hurricanes and subsidence were primary concerns.

Convincing Congress was a serious problem. The LRL had been built at considerable cost (12 million dollars) and was to have served as both a quarantine facility and a sample facility. For that time, the costs for the facility were substantial and required justification. Meetings with some senior Senate staff exposed concerns regarding further expenditures. A major opponent was Senator W. Proxmire. I visited with his senior counsel on several occasions in order to explain the current situation. Although I got an attentive and sympathetic ear, there was formidable opposition with a considerable exchange of letters and telegrams. Early one morning I was awakened in my hotel room in Washington by a call from Tony Morse. "Wasserburg, you just got the Golden Fleece award." An announcement had appeared awarding the Gold Fleece with the citation, "For spending millions of dollars for a wheelbarrow load of rocks." It was an unjustified attack. The purpose of the award was to exhibit to the public how a certain Senator appeared to be vigilantly protecting the public purse; it was not to identify and address an issue of substance, nor to protect the public from serious misappropriations.

The question of sample security against natural hazards arose. A committee of experts in risk assessment was formed, including the Director of the Hurricane Center in Florida. Many high level officials both at NASA and from local government attended. It was noted that hurricanes were frequent and very damaging, both from high waters and from violent winds. Strong objections to the assessment that this area of the Gulf Coast was subject to high risks from hurricanes and subsidence were voiced. The head of the National Hurricane Center privately referred to the Houston area as the "Gust Coast." Discussing hazards with local officials of an area is extremely difficult. As proponents of the local area and purveyors of good impressions of the region, they can not tolerate any report of local risks and local problems. When discussing subsidence (about 6" per year), I pointed out that the cute motel that had boats near the rooms was the result of the fact that the parking lot had been flooded over the past four years. Geological faults through Air Force bases were not acceptable. The committee recommended a structure sufficient to withstand an 80-year flood and associated winds. The sample facility was to be on an elevated second floor. Some samples were to be stored at dispersed sites with lower risks.

To defend the risk assessment, I got notes on the observations made by Sam Houston when he first came to the place where a city would bear his name. He described how, in his first traverse across the great costal plain of Texas, he saw a large ship stranded on the prairie, 50 miles from the sea. Houston remarked in his notes that there must, on occasion, be enormous storms that blew ships far inland. A visit to Galveston provided me with documents on the hurricane that destroyed that city in 1900. This hurricane was widely described as the greatest natural disaster in the history of the United States. Photographs of proposed long-term storage sites were obtained. One showed a proposed secure area and the building under 1 meter of water. All of this material was gathered up and sent to NASA HQ and to members of the House and Senate. To me, the hero of the effort was Huel Clanton who decisively showed, in his wonderful, low key, sort of Mark Twain way, all the graphic facts (geological, engineering, floods, and all) to the committee headed by Jim Lovell. Huel made a great deer chili.

When all the meetings were over, a facility was finally built with insightful action by Noel Hinners, then Director of the Lunar Science Program at NASA HQ. Hinners even committed the facility to include the handling and storage of meteorites and cosmic dust. There was a charge to the JSC operation that it both do research and serve as curator of these valuable extraterrestrial materials. These materials were to be provided to scientists throughout the world upon review of their requests by a committee of scientists and the curator. This program serves as an example of how to care for an increasingly diverse collection of rare extraterrestrial material and how to make them available to the general community for study and research. The quality of care and of sample preparation far exceeds what had ever been done before (or since) in museums. Proxmire's statement that this program was "a waste of money for a wheelbarrow of rocks" has proven to be wrong-headed and wrong. The lunar samples were the crown jewels of the Apollo missions—like meteorites, they are a resource for scientific discovery and the understanding of the history of Earth and of our Solar System.

THE MOON AND SIXPENCE OF SCIENCE

The activities inside of the Lunatic Asylum at Caltech grew from preparation and expectation to doing science with lunar samples. The technical and scientific skills that had been developed were now put to full use. The inmates of the Lunatic Asylum consisted of a band of extremely skilled and dedicated individuals whose expertise covered mineralogy, petrology, mineral separation, analytical chemistry, mass spectrometry, physics, electronics, and computers. This group of exceptional individuals worked together intimately from before Apollo 11 through Apollo 17 with fierce dedication and intensity directed toward the research goal of studying and understanding the moon. There were professors, research fellows, students, and engineers.[2] The personnel changed with time but the skills and dedication were

[2]Albee A.L., Allegre C.A., Anbar A.D., Andersson P.S., Armstrong J.T., Aronson J.L., Asplund N., Ball E., Banner J.L., Bar-Matthews M., Bauman C.A., Beets C.J., Begemann F., Bence A.E., Bernius M.T., Bielski-Zyskind M., Blake M.L., Blum J.D., Boctor N.Z., Bogard D.D., Bogdanovski O., Boothroyd A.I., Bradley J.G., Brigham C.A., Brown E., Brown J.A., Brownlee D.E., Burnett D.S., Busso M., Capo R.C., Carpenter A.B., Chen J.H., Choi B-G., Connolly H.C., Jr., Creaser R.A., Curtis D.B., Deloule E., DePaolo D.J., Derksen U., Derksen H., Dymek R.F., Eberhardt P., Edwards R.L., Eisenhauer A., El Goresy A., Esat T.M., Eugster O., Fahey A.J., Fourel F., Gallino R., Gancarz A.J., Gillespie A.R., Gnaser H., Grant J.A., Gray C.M., Haines E.L., Hedges L., Heinrich M., Holmden C., Hsu W., Huneke J.C., Huss G.R., Hutcheon I.D., Jacobsen S.B., Jacobson A.D., Jessberger E.K., Johnson M.E., Kaiser T., Karpenko S.F., Kastenmayer P., Kaufman A., Kawashima A., Kellogg L.H., Kelly W.R., Kennedy A.K., Krestina N., Land M., Lanphere M.A., LaTourrette T.Z., Lee C.-T., Lee T., Lemarchand D., Lippolt H.J., Martin C.E., Mason B., Massey A., Mazor E., McCulloch M.T., Meeker G.P., Miller J., Navon O., Naylor R.S., Nenow E.V., Ngo H.H., Nichols R.H., Jr., Niederer F.R., Nohda S., Nollett K.N., Oberli F., O'Connell R.J., Paillat O., Papanastassiou D.A., Peate D., Piano P., Pickett D.A., Piepgras D.J., Podosek F.A., Porcelli D., Prinzhofer A., Qian Y.-Z., Quick J.E., Radicati di Brozolo F., Ray L.A., Reid K., Reynolds B., Roy-Barman M., Russ G.P., Russell S.S., Russell W.A., Sackmann I.-J., Sanz H.G., Sanz Maria Schramm D.N., Settle D., Sharma M., Sharp C.M., Shaw H.F., Shen J.J-S Sheng Y.J., Smith S.P., Spivack A.J., Srinivasan G., Stecher O., Steiger R.H., Stein M., Stewart B.W., Stone J., Stordal M.C., Takeda H., Tera F., Teshima J., Tredoux M., Tricca A., Turner G., Venturelli K., Villa I.M., Völkening J., von Drach V., Wark D.A., Wasserburg G.J., Weis D.A., Wen T., Whitehead J.H., Wolfe S.H., Wright J., Wunderlich R.K., Young P., Zartman R.E., Zhang Y., Zipfel J., Zyskind J.

developed and maintained at the highest level. Our group developed ultraclean sample handling procedures; ultralow level chemistry with negligible contamination levels; and high-sensitivity, ultrahigh-precision mass spectrometric methods. The approach was to make better measurements on smaller samples with lower contamination rather than good measurements on large samples with not so low contamination. This led to precise measurements on millimeter-sized "rocks" with isotopic ratios determined to $\pm 0.4\ \varepsilon$u (4 parts in 10^5, two σ errors). The measurements went down to picomole quantities and then to femtomole quantities with good precision. An increasingly wide number of elements were studied that required the development of new microchemical and mass spectrometric procedures. The key to the chemistry came from the great skills and imagination of Fouad Tera. Almost all of the measurements were made on the Lunatic I spectrometer, an instrument that established the state of the art in 1969 and has performed at very high levels until the present day. The Lunatic III followed, also a gem of an instrument. The precision of these measurements is just now being exceeded. These efforts led to the determination of the precise ages of lunar rocks, their initial isotopic compositions, and a lunar chronology. Distinctive magma sources for lunar basalts of the same age and the evolution of magmas with time were laid out. The cessation of mare volcanism occurred $\sim$3 Æ ago. There were no young volcanics on the moon, disproving the inferences from lunar stratigraphy based on an assumed cratering rate. This inferred young volcanism had guided Gene Shoemaker and the Astrogeology Branch and governed the scheme of site selection and the scientific program of astronaut activities established during all EVAs. Shoemaker was a brilliant and charismatic scientist deeply involved in the manned lunar program—he was also a sort of scientific Peter Pan. Early formation of the lunar crust was established. After learning the art of ^{40}Ar-^{39}Ar from G. Turner, with E. Jessberger and J. Huneke, we applied the ^{40}Ar-^{39}Ar method to lunar samples. We found that the breccias, formed at $\sim$3.95 Æ, contained clasts preserving very primitive ^{86}Sr/^{86}Sr, as DAP and I had found earlier. These clasts had ^{40}Ar-^{39}Ar showing ages of 4.5 Æ. These clasts were from the ancient lunar crust, which was disrupted by the late bombardment. The Moon formed a crust very early—not by slow growth. Using the neutron fluence we established a method of "lunar neutron stratigraphy" for dating sedimentation of debris blankets (Cambrian age lunar sediments!) by G. Price Russ π, Don Burnett, and me.

The study of breccias showed the Moon to have undergone a last massive bombardment $\sim$3.95 Æ ago. It wiped out most of the intervening history and left the question of the source of this last big bombardment that occurred $\sim$0.5 Æ after formation of the Solar System. The surface of the Moon was covered with radiogenic lead produced between 4.45 Æ and 3.95 Æ ago. This massive late bombardment, called the terminal lunar cataclysm by F. Tera, DAP, and me, must also have occurred on Earth and most probably on other planets of the inner Solar System. It has been used as the basis of estimating ages of the inner Solar System planets from large craters on these bodies. Some recent studies now find evidence of these major impacting events on Earth. The late bombardment would have destroyed the existing atmosphere and constrain when life forms could develop and persist on Earth (after 3.9–4.0 Æ ago).

The Apollo period was not only a time of enormous public excitement and enthusiasm. It was a time of ongoing technical trials, the excitement of discovery, and an intellectual feast. Because the missions were launched in about six month centers, it was frenetic for all the participants inside and outside of NASA. As a scientist and teacher, it was even more exciting for me considering my responsibilities on LSAPT and at JSC (the Johnson Spacecraft Center changed from MSC to properly honor Lyndon B. Johnson). Johnson was the architect of the Space Act and the person most responsible for the initiation and fullfilment of J.F. Kennedy's great promise. Perhaps the most important thing we as scientists learned (or tried to learn) was how to think on a grand scale, to understand the evolution of a planet by studying a few rocks or grains. This approach led us to another terrestrial-like planet via the meteorite Nakhla. Papanastassiou and I, much to our surprise, found that Nakhla did not come from the (asteroidal) parent planets of regular meteorites, but had to come from another planet similar to Earth. Now it appears to be from Mars! The thinking got sharper and we generalized more. The concepts became broader and grander. Some ideas may be correct. It has been quite a trip, and everyone who passed through the Lunatic Asylum felt great satisfaction in his/her achievements.

The NASA program involving the Lunar samples strongly supported the participation of research groups from many nations and served to energize a broad scientific community throughout the world. The Soviet Bloc nations were not among them. A wider selection of lunar materials became available with the success of the robotic Luna 16, Luna 20, and Luna 24 missions by the Soviet Union, which returned samples obtained with a drilling device. Paul Gast and I thought it was a good time for a sample exchange. This was finally approved and the Soviet Academy provided some samples to NASA for the U.S. scientific community. Their preference, of course, was to give samples to other more "sympathetic" countries. The exchange also advanced some small steps toward cooperative activities between the United States and the USSR. Though the samples were very small, good work could be done on them thanks to the Apollo sample experience. The Lunatic Asylum was given the largest sample, weighing 0.06, shipped in a series of nested containers like a Russian matrioshka doll. The last container, a small gelatin capsule, contained "the rock." We did the petrography, petrochemistry, and mineral separations on this material and determined isotopic ages. The results fit into the framework that had been determined from the Apollo samples.

ALLENDE AND DOORS TO THE NEW WORLD

While we were working full tilt to prepare for lunar samples from Apollo, a strange and wonderful thing happened. Gene Shoemaker stopped me in the hall and said, "Did you hear, a carbonaceous chondrite has just fallen in Mexico? Elbert King has gotten a chunk and the Houston people are counting it. Would you like to get some?" Arrangements were made with the University of Mexico to carry out a joint program. Don Elston, a friend from the USGS, agreed to fly down there if I would appear at dawn at a small airport in New Mexico. There were lots of problems, including the fact that my wife's mother-in-law was arriving that evening. I had no money so I broke the kids' piggy bank on the patio, then asked my mother for a loan (she required an IOU). Naomi was madder than heck (with good reason!). I was off! We landed in Hidalgo del Parral (Chihuahua) and arranged transport to the little village of Pueblito de Allende. The big explosion from the infalling body was the focus of attention in the area—really full front-page news. We accumulated a considerable mass of the meteorite. I must admit that for all my crawling through the brush, I was not very successful in finding fragments. I did get well cut-up and stuck with thorns and needles. The local people were infinitely more observant and competent. The freshly fallen samples were collected and half were given to our colleagues from the University of Mexico. Roy Clarke and the Smithsonian group came and mapped the area of the fall. The wild stories surrounding this whole event are for another telling. We brought back a good collection to Caltech where it proved to be a major resource. The Allende meteorite was found to contain rather abundant "white" inclusions of Ca-Al-rich material (Clarke et al. 1970, Marvin et al. 1970). This meteorite became the object of world-wide study.

Concurrent with the lunar studies, we followed up on the study of the abundant refractory inclusions (CAIs) identified by the Smithsonian group in the Allende meteorite. Theoretical discussions by L. Grossman and earlier work by H.C. Lord III indicated that the bulk composition of CAIs matched with what would condense from a gas of solar composition at high temperature. Chris Gray, DAP, and I found initial $^{87}Sr/^{86}Sr$ in some CAIs that was distinctly below the BABI value (called ALL) and identified the inclusions as early condensates from the solar nebula. We concluded that this more ancient material opened the door for a new search for ^{26}Al. The bounds set earlier by Schramm, Tera, and myself were from later metamorphosed objects. Also stimulating the search was the discovery of NeE (almost pure ^{22}Ne) by D. Black and R. Pepin. D. Black's clear interpretation was that dust grains that formed around other stars were preserved in meteorites. However, evidence of intrinsic isotopic heterogeneity in the solar system had not been found for any abundant nuclei. The discovery in 1973 of ^{16}O excesses in CAIs by R.N. Clayton and his colleagues at the University of Chicago changed everything. The effects were large—a 4% increase in ^{16}O with $^{17}O/^{18}O$ the same as terrestrial. It would appear that large anomalies in many elements should be found if these effects were due to oxygen from a supernova. D.N. Schramm sent a brilliant student in astrophysics from the University of Texas up to Caltech to pursue these effects. Typhoon Lee arrived and we discussed problems and how to look for effects in Mg and Ca. We found small general isotopic anomalies in Mg (excesses and deficiencies). Then we showed that the excesses in ^{26}Mg were correlated with ^{27}Al and hence were due to an isotope of Al (e.g., ^{26}Al). Finally, "internal" isochrons were found for CAIs that showed that the typical value of

^{26}Al/^{27}Al $= 5 \times 10^{-5}$ for a wide number of samples, thus proving the existence of ^{26}Al ($\overline{\tau} = 1.05 \times 10^6$ years) in the early Solar System. The source of the ^{26}Al had to be nucleosynthesis in a star just before the Solar System formed. It might possibly have been produced by the early Sun due to extremely high solar flare activity. The discovery of ^{26}Al established a clear relation to larger-scale astrophysical processes and the formation of the Solar System and made for lots of excitement. The timescale of 70×10^6 years from ^{129}I for the last injection of freshly synthesized nuclei now had to be reduced to less than 3×10^6 years and represented nucleosynthetic activity of a different type. Some individuals thought that the ^{26}Al in the CAIs was incorporated as a ^{26}Mg fossil into the solar nebula (as they had persistently argued for ^{129}I—certainly an incorrect interpretation). Lee, DAP, and I wrote an article entitled, "Fossil or Fuel" (Lee et al. 1977). It was a bit difficult for astronomers and astrophysicists to really believe that "rocks come from stars" or that astrophysics can be done with rocks. They are slowly learning. Later, γ ray experiments by Mahoney et al. (1984) on the HEAO-3 spacecraft (High Energy Astronomical Observatory) detected γ-ray emission from the decay of ^{26}Al and found it to be the principal γ-ray line in the galaxy. This discovery has been expanded into the γ-ray mapping of the galaxy with the Comptel Telescope by von Balmoos, Diehl, and Schönfelder (von Balmoos et al. 1987). The ^{26}Al discovery got lots of attention. Even the airlines played a recording with a story about it accompanied by smooth music—enough to put you to sleep. We used the sound track to tell when the Lunatic Asylum door was not closed. Typhoon Lee (i.e., the Gentle Breeze), DAP, and I were ecstatic over the results (Figure 4).

What other elements would show the signature of isotopic variations reflecting incompletely mixed presolar sources? We were diligent and we were lucky. Lee found small anomalies in Ca and Malcolm McCulloch found small anomalies in Ba and Nd. Then lots of other isotopic anomalies were found throughout the periodic table (Ca, Sr, Sm, Ti, etc.). The law of constant atomic weights was wrong. Other workers then found more "anomalies" and all sorts of theories were devised to explain the data. Names of inclusions like C-1 and EK1-4-1 became well-known in the nuclear astrophysics community. What stellar processes could produce these "isotopic anomalies"? Late one afternoon as Dick Feynman visited me at home in my study, we were interrupted by frequent phone calls from the lab. He inquired why I was so upset. I replied, "It is disappointing. We have all these obvious nuclear effects and we do not understand them at all." He replied, "When you really find something truly anomalous and you do not understand it, then you have discovered something important." That is the way it was.

What other short-lived nuclei should we look for? There was ^{107}Pd that Urey had bugged me about in 1954 when R.J. Hayden and I did not find evidence of ^{129}I. (Bob) Kelly and John Larimer at Arizona State University (ASU) were studying condensation models and iron meteorites. They concluded that a certain class of Fe meteorites would show large fractionations between volatiles (like Ag) and platinum-group-elements (like Pd). Kelly came to the Lunatic Asylum specifically

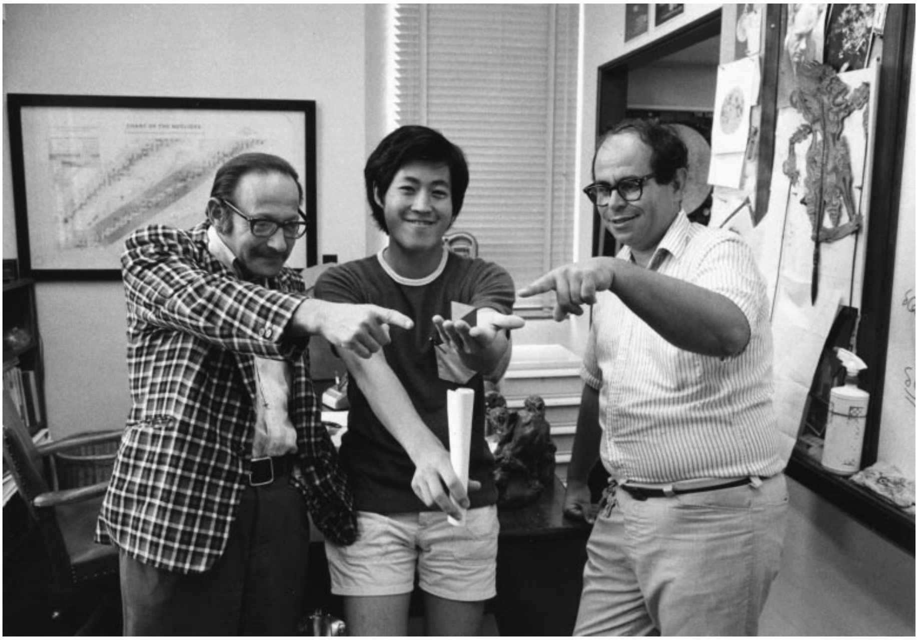

Figure 4 The happy trio of G.J. Wasserburg, Typhoon Lee, and Dimitri A. Papanastassiou (DAP). The Gentle Breeze (Lee) is holding a model of a spinel octahedron. An anorthite model full of $^{26}Mg^{*}$ was not available for the picture. Caltech Archives (1977).

to look for ^{107}Pd effects. All that stood in the way of progress was learning how to measure ^{107}Ag excesses at a level of $\sim 2 \times 10^{10}$ atoms per gram, without contamination in sample processing and eliminating isobaric interferences while obtaining high ionization efficiency. This major effort appeared to be just a technical tour de force with no other results. Then Kelly visited Clyde Tombaugh (the discoverer of Pluto) who had a chunk of the Santa Clara iron meteorite under his desk. This sample and the new techniques led to the discovery of ^{107}Ag excesses due to ^{107}Pd decay ($\bar{\tau} = 9 \times 10^{6}$ years). Jim Chen extensively explored these effects and showed that ^{107}Pd/^{108}Pd $\sim 2 \times 10^{-5}$ for a large number of iron meteorites, and set the time schedule for core formation. This proved that segregation of metallic FeNi and core formation in planets took place within less than 10^{7} years after the Solar System formed. One other thing—the ^{107}Pd had to be made with neutrons in a star (not protons). Then there was the question of how well can you measure time backward from now, not just relative to the floating timescale from extinct short-lived activities? Following up the curium search, Jim Chen and I showed that it was possible to measure the ^{207}Pb/^{206}Pb ages of CAIs and to resolve time differences of a few million years at the 4.559 Æ and to distinguish the ages from that of “more evolved” meteorites. (Chen & Wasserburg 1981).

(*a*)

(*b*)

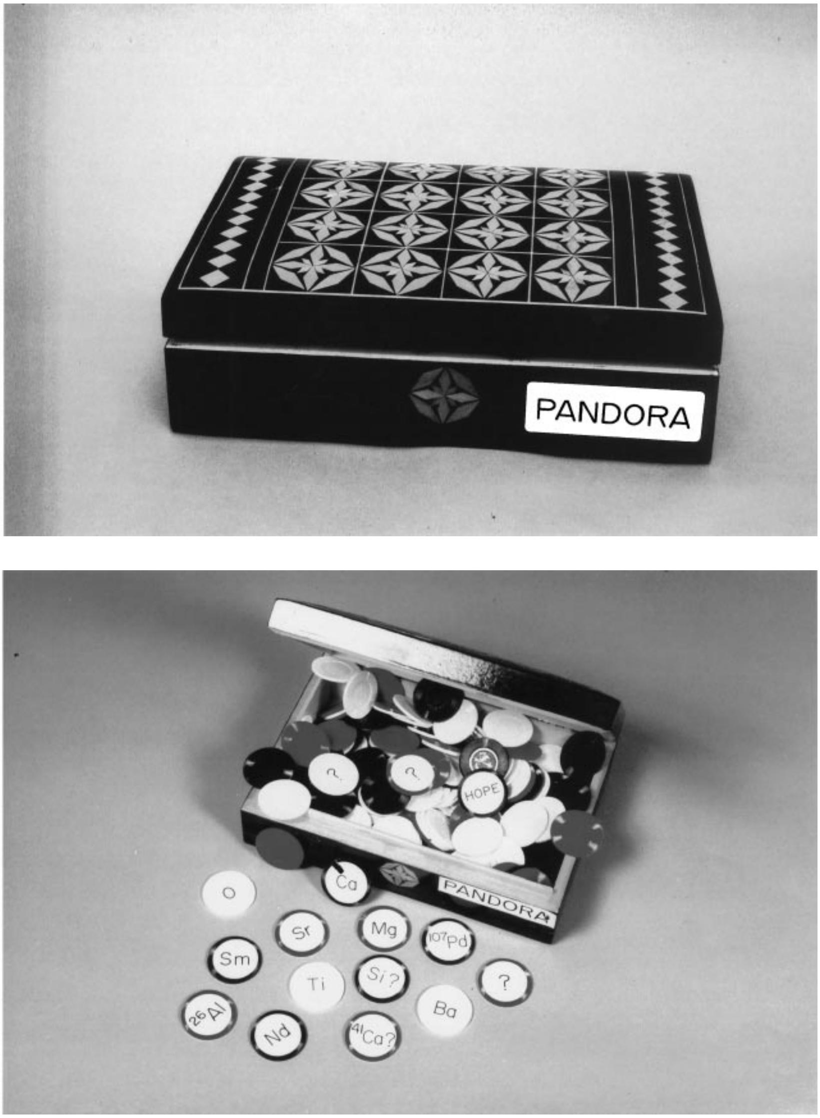

Figure 5 Allende was like Pandora's box. When opened, it led to many surprises and some shocks. Isotopic discoveries were later extended to all meteorites and some planets. (*a*) Pandora's Box closed: The "law" of constant atomic weights and other standard and erroneous assumptions are well covered. (*b*) The box soon after it was opened. Isotopic anomalies flew out of the box and infected everything. This became more widespread as technical skills increased, the samples became submicroscopic (down to submicron circumstellar dust grains), and the anomalies got bigger and maybe better. It is possible that hope (of rational explanations) is left in the box in the form of other "anomalies" (after Papanastassiou et al. 1978).

The Solar System, originally considered to be an isotopically homogenized mix of presolar material, was now known (*a*) to preserve incompletely mixed presolar components and (*b*) to have formed not very long after freshly synthesized nuclei from stars had been dumped into the mix. The extent of the heterogeneity was not fully clear—D. Black's work indicated possible preservation of presolar dust grains. The stellar sources of the late debris that made up the Solar System were not at all clear. The rules governing solar system formation were vague—one could only snoop, discover, and try to understand. When Jack Huneke left for Silicon Valley, we were fortunate to attract Ian Hutcheon from the University of Chicago. The Chicago group was not happy about this "theft." Hutcheon's sound knowledge of physics, meteorite petrology, and isotopes, as well as instrumentation, were a great addition. The search for ^{26}Al in chondrules by Hutcheon using PANURGE led to its discovery in a chondrite. He and John Armstrong followed the search for ^{41}Ca through the doublecross ($CaCa^{++}$ interferences) to a limit that was then later pursued to discovery of ^{41}Ca at the Tata Institute in Ahmedabad, India. The search by P. Eberhardt to find the "carrier" of NeE led to a separation of submicroscopic "goo" that contained all the NeE. Pursued by E. Anders and his colleagues, it turned out, after many wrong turns, that the carriers were in fact surviving circumstellar dust grains of different phases—carbon compounds (nanometer diamonds, graphite, SiC, and SiN). This was a very important discovery. Then, remarkable work with E. Zinner (Anders & Zinner 1993) exposed monstrous isotopic effects in C and N so that the data had to be displayed on a log-log scale. Some of the grains also had contained abundant ^{26}Al at the time of their formation. Almost indestructible, these grains formed around stars long before the Solar System formed and were swept up with other debris and saved in asteroids during the accretion phase of the Sun. Although I still have trouble believing that the bulk nanodiamonds with normal ^{13}C are presolar. Maybe they were formed from C-rich aggregates with some presolar stuff in it was shocked into diamonds in the early Solar System.

Because most of the meteoritic matter consists of oxides and not carbon compounds, a search for presolar, circumstellar oxide grains was necessary. Ian Hutcheon and Gary Huss succeeded using PANURGE, the first generation analytical ion probe with high-mass resolution (IMS-3F). This instrument was the consequence of an intense discussion between Slodzian and me in my office—how to best use his brilliant design of an ion imaging spectrometer as an analytical instrument. It was finally built by CAMECA with frequent interactions and became the workhorse SIMS for two decades. The name PANURGE came from the fraudulent promise to do all possible work (after a scandalous hero Panurge in Pantegruel by Rabelais, 1532). Hutcheon & Huss found an Al_2O_3 grain with ^{18}O substantially low, ^{17}O high, and lots of ^{26}Mg from ^{26}Al decay. This work and the wonderful extensive work by L. Nittler at Washington University, St. Louis, and further work by Gary Huss and later by B.G. Choi and Natasha Krestina in the Lunatic Asylum, led to the discovery of diverse presolar oxide grains. For the most part, they appear to be produced by asymptotic giant branch (AGB) stars just before they blow up. They frequently had contained abundant ^{26}Al. The oxygen isotopic results showed, however, that the standard stellar models had serious flaws.

We had started with the establishment of a high-resolution Sr chronometer for the very earliest Solar System using the methods referred to previously. With Allende, the limits on ^{26}Al in the early Solar System that we had established earlier, had to be looked at again in this more ancient material. We all had to look for a new set of rules, possibly living without any rules at all! The tools (both technical and intellectual) applied first to Allende (and then to many other meteorites) were a direct product of the Apollo experience and led to great scientific fruits.

PROJECT OLDSTONE

In 1971–1973, S. Moorbath and H. Baadsgaard reported finding ancient rocks in West Greenland of the same age as some lunar rocks. There were also abundant anorthosites in that general region as in the lunar highlands. This find provided an opportunity to compare ancient terrestrial and lunar rocks. We decided to mount a field party to West Greenland and established contact with V. MacGregor, who lived in the Inuit settlement of Atangmik and had extensively mapped the area. In 1972, we requested permission of the Ministry for Greenland and the Greenland Geological Survey (GGU). Getting this official permission from the Danish Government was one of the greatest difficulties facing our group. The government wished to insulate GGU-related research using approved groups from outside intruders. After a series of difficult encounters where the basis for United States recognition of Danish sovereignty over Greenland was discussed, we got permission in June 1973. With V. MacGregor’s aid, we contracted for the services of the Jens Jarl, a retired Danish coast guard cutter, and its giant Viking captain, Rudi Burghardt.

The Oldstone field party consisted of R. Dymek, A. Gancarz, R. Kiekhefer, Arden Albee, and myself, with our two young sons, Jamie Albee and Charles Wasserburg, as field hands. MacGregor, whom we called “the great white hunter,” skillfully guided us up and down the coast, into the fjords, onto the outcrops from the boat, teaching us the geology on a grand scale. D. Bridgewater showed us around the Godthåb region. The Lunatic Asylum party drilled and blasted to obtain an extensive suite of fresh samples. We also learned, at some risk, that Nobel detonating caps required higher voltages than U.S. caps. Study of the samples collected showed that the “model age” of Earth was younger than often announced in the literature and appeared to be the same as that determined for the moon, 4.47 Æ, not 4.55 Æ. This pair of planets formed 50–80 million years after the Solar System formed. The Oldstone collection is now resident and under study at Washington University with Prof. R. Dymek, who also took swimming lessons with A. Gancarz and G.J. Wasserburg in a Greenland fjord under the direction of

V. MacGregor. Much later I found that the student who assisted me in the high explosives work was color blind.

NASA, COMPLEX, AND THE SPACE SCIENCE BOARD (SSB)

The experience that I had gathered during the Apollo epoch led John Naugle, the Associate Administrator for Space Science, to invite me to serve as an advisor. This was a time of major changes developing in NASA. A decision by President Nixon sharply limited the fiscal resources directed toward NASA. One Saturn V launch vehicle was used for Apollo-Soyuz as an implement of international diplomacy. Another was used for Skylab as a step toward a space station. The space science program operated under the belief that the large manned program under the Office of Manned Space Flight (OMSF) provided the big umbrella under which a science program could be carried out. Naugle sought to carry forward various scientific investigations under this umbrella, and his successor, Noel Hinners, brought a keen sense of science and science leadership into the area. A very difficult task because science was a component but not the main objective of the agency. NASA was and still is dominated by the Manned (piloted) Space Program.

My initiation into the advisory function involved Viking, Pioneer Venus, Jupiter Orbiter Probe (later Project Galileo), and the study of the outer planets. I experienced the delight of seeing A.O.C. Nier appear at a committee meeting where he opened up a small briefcase containing a mini-mass spectrometer and actually carried out an analysis of air in the room. A lot of very talented individuals from the science community, from NASA centers, and from HQs regularly filled the meeting room. Besides considering issues relating to possible new missions, we were to evaluate approved missions and their status, including Viking. I then became aware that there were no instruments for measuring the composition of soils or rocks on Mars—a deficiency only partly corrected at the very last minute. The scientific experiment packages chosen for the Viking spacecraft had been guided by the SSB reports on planetary exploration, where purely biological studies and the search for life on Mars was greatly emphasized. This was not balanced within a plan to understand Mars as a planet in the framework of the formation and evolution of the Solar System. The search for “X?” was it! This overemphasis is seen in excess in some testimony to Congress, not to mention presentations to the press (even later in inflated claims of Mars Bugs pushed by Dan Goldin, then NASA Administrator, as another marketing device).

Werner von Braun made a speech regarding space exploration at the time when JPL was marketing the Grand Tour missions. I was concerned about the scientific instruments and the question of sequential learning. In response to a question regarding this, von Braun said, “You scientists are the high priests who are invited to the launching of our expeditions to give them your blessings.”

I was invited to join the SSB and to serve as Chair of the Committee on Planetary and Lunar Exploration (COMPLEX). This was undoubtedly arranged by Noel Hinners and John Naugle. The SSB had some outstanding scientists with very diverse interests. The purpose of the SSB was to counsel the Federal Government in the interest of obtaining the best U.S. space science program achievable within the complex political, institutional, and fiscal constraints. The principal goals seemed to be to support good space science in the wide number of fields represented by the committee membership, to provide support for new endeavors that NASA had identified and considered marketable, and to aid in the identification of new activities. The main focus was on the "planning wedge," a mythical slice of resources provided by the Associate Administrator of Space Science that might be available in subsequent years. This planning wedge was to serve as a guide for evaluating ongoing and future activities. The general scheme was to see what the SSB would support strongly (the recommendations of the SSB), particularly, with regard to new programs that were "in the works." Specific space missions almost completely defined these activities. An expert appreciation of what developments were taking place in the Federal Government, or that would take place, that would affect NASA and space science activities in particular, required an expertise and political insight not readily available to the SSB members. What was needed was a sort of policy ouija board. After a while, I felt that what the participants were doing was sort of a ritual war dance around the fire of the planning wedge (mostly governed by things in the works) with the division of this sacred imaginary pie going to those warriors who gave the most vigorous performance. The official SSB reccomendations were followed by disciplinary committee reports, which were to appear as appendices or as "working papers" to the formal report of the Board.

In the usual conduct of science, the planning and execution of a research program is typically a few years, after which you learn something and then figure out the next step. For space science missions, the timescale for planning and execution is typically a decade or more and the required resources are large. Sometimes, for deep space flight, it is a decade or more before any data are returned. The total time, from initiation to completion, is often a professional lifetime. Such missions cannot be done without the mutual, ongoing participation of a lead federal agency; the agency's engineering, science, and management teams; and the intimate involvement and commitment of scientists from universities and their laboratories—a process similar to some high-energy physics programs. Industrial competence and participation play a critical role, both in terms of technology and skills but also in heavy marketing. The difference between space science and physics is that once you have committed to a mission, the technology of the measurements to be made is fixed. When you are actually making the space flight measurements it is ten years later and you are using antiquated technology on the spacecraft. This time difference is a basic one. In order to do something really good, you need a library of new and innovative but sound experiments that are convertible to space flight instruments for use as part of a mission. It was obvious that some new types of measurements just had to be made in order to learn something important from other planets, not just taking the same ones that had been flown before on earlier missions. Once the commitment to a mission is made and the bird is about to fly, then there must also be an ongoing commitment to carry forward the program. This requires both support of the mission and the support and commitment of the personnel (science and engineering) who should carry through the mission. Of course, what is being pushed are the new missions. Surely no mission should live forever, just long enough to do great science. The agency, however, lives by new starts (with the fiscal and institutional commitments) and often leads to a philosophy that NASA lives by flight alone!

I was given the authority to put together a new committee. It consisted of outstanding scientists covering all fields of planetary science, including individuals with space flight experience, others with instrumental design and development experience, and some with deep scientific knowledge. By special arrangement, it even included one graduate student who was studying Jupiter. We came up with a report that was finally incorporated into the SSB report. The approach was to define a long-term strategy for scientific planetary exploration; it enunciated the scientific goals, the sequence of reaching these goals, and the strategy that should be followed. Primary and secondary objectives were defined so that the principal basis for defining a mission should serve as the basis for resource allocation. We worked intensively; held many meetings in order to review capabilities and resources; and listened to presentations from scientists, engineers and, managers describing the issues at hand. We thoroughly investigated the U.S. launch capabilities, anticipated national and NASA plans, and laid out the launch capabilities necessary to provide access to the planets. It was during this time that the marketing of the space shuttle began, and the accessibility of anything out of low Earth orbit was in severe doubt and was to remain that way for a long, painful time, with serious injury to U.S. capability in national security, commerce, and science.

After considering the matter, I concluded that COMPLEX should provide (*a*) advice based on a long-term strategy of specific scientific goals for planetary exploration; (*b*) prioritized scientific experiments or observations to be achieved in a sequential program; (*c*) a layout of the basic technical requirements, including launch capability necessary to carry forward the strategy and to achieve substantial scientific goals; and (*d*) an evaluation of the ongoing programs to ascertain if they were achieving their scientific objectives and to identify particular areas requiring attention. Two such areas were those of scientific instrument development and the requirements for post-flight analysis. It was my view that this full report should be reviewed and evaluated by the SSB, and if acceptable, after appropriate revisions, be adopted as Board policy. The recommendation of any particular mission was then to be based on the long-term strategy and not just on annual marketing schemes. I also felt that there had to be executive sessions of COMPLEX and of the SSB in order to separate the role of the NAS-NRC advisory function from the pressing needs of the Agency that sponsored the work. This is most important if the advice is to properly counsel the federal government.

This was quite an order. The dedication and deep interest of all the members of COMPLEX and the support of Dean P. Kastel, Executive Secretary, made things go. NASA also put up with very intensive and often intrusive studies of all pertinent matters. That Noel Hinners and John Naugle still talk to me is a compliment to their good character. COMPLEX could not have progressed without thorough and reliable technical and management knowledge, which certain unnamed individuals provided with honesty and penetrating insights. I just about had to move to Washington, D.C. The Cosmos Club sort of became a second and not comfortable home. The phone calls at the office and home (when I was home) became longer and longer. Discussions with Al Cameron were frequent; with Gene Levy, very frequent; and with Mike McElroy, lengthy and intense. They often tied up our home phone for hours. The measure of things is best seen when Naomi got a chance to have a very long phone call with someone. One of our sons retorted, "It must be from Mrs. McElroy."

We all suffered a lot and learned a lot. The resulting report, of which I am very proud, was adopted by the SSB as a policy document (Space Science Board 1975). It was not written by staff. It was written upon consultation with the members and with staff (principally concerning procedures, lucidity, formal NAS-NRC policy, and ethics). The general approach has, in part, served as a guide to other discipline areas. For some time, the COMPLEX reports were the principal strategic documents guiding planetary exploration. NASA very generously awarded me a second medal for distinguished public service for our work. I guess Noel Hinners convinced John Naugle and the Administrator that it was worth the suffering.

The problems of launch capability that were carefully laid out in the report came to seriously haunt the nation for over two decades. As I once said during a television interview, the United States has gotten into such shape that we can not get it up—referring, of course, to our launch capability. The SSB in this period was deeply involved in the space telescope and saw many of the problems coming. They helped lay out plans for the Space Telescope Institute. They saw and critically commented on the marketing, selling, and sale of the shuttle, which led to the problems alluded to above. This led to the move to a "Blue Shuttle" for the Air Force to operate out of Vandenberg AFB (now moth-balled). In all cases, the role of piloted space flight as the governing national program has led to problems interfacing with access to space, space science, and doing science inside a "manned space program." Now the Peoples Republic of China has committed itself to piloted space exploration of the moon. This replay may gain them prestige. It will play havoc with science and technology.

When I retired from the SSB, I gave a speech about the Cumaen Sybil and the value of prophetic advice. After a while, what is left of the advice is locked up on Capitoline Hill and only authorized priests appointed by the emperor are permitted to read it. Advice always needs a deranged prophetess to enunciate the lyrics.

SM-ND AND THE HIGH VALUE OF RARE EARTHS

In 1975, Gunther Lugmair of UC San Diego published a pioneering study on the use of ^{147}Sm decay to ^{143}Nd for dating basaltic achondrites (eucrites) and lunar basalts. What could one learn about Earth from another dating scheme?

A.L. Albee suggested to a very bright graduate student, D.J. DePaolo, that he might find something interesting to do in the Lunatic Asylum applying the Sm-Nd technique to the earth. The REE are of great importance because of their close chemical interrelationships. We followed up on the cosmochemistry and geochemistry of the REE pioneered by Roman Schmitt by adding isotopic shifts and time. Roman was a strong and effective lobbyist for lunar sample science. The techniques for precise measurement of REE had been established previously for Gd and Sm by Otto Eugster, Fouad Tera, and G. Price Russ π in order to measure the neutron fluence on the moon. Applying these techniques for Nd appeared tractable. DePaolo's thesis work produced extremely important results and opened up a whole new world of scientific research in the earth sciences. The highly refined techniques that we developed were then applied to a variety of terrestrial samples of known age. The first efforts showed that the initial $^{143}Nd/^{144}Nd$ of crustal rocks was very close to the evolution curve of a lithic reservoir, with Sm/Nd equal to that of chondrites. The present $^{143}Nd/^{144}Nd$ ratio for the evolution curve became CHUR—chondritic uniform reservoir (for REE and refractory elements) and was also the name of a beautiful Swiss city that my family had visited. This agreement of initial $^{143}Nd/^{144}Nd$ on samples covered ancient rocks from Greenland (see Project Oldstone, above) to some recent continental basalts. However, a sample of shale, representing average continental crust, had a much lower value ($\varepsilon_{Nd} = -14.4\,\varepsilon u$), whereas mid-ocean-ridge basalts (MORB) had much higher values ($\varepsilon_{Nd} \approx +10$). It was argued that partial melting in the mantle produced Sm/Nd fractionation, yielding a depleted upper mantle (with low REE and high Sm/Nd) and an enriched crust with high REE and low Sm/Nd. It was then found that the initial $^{87}Sr/^{86}Sr$ and $^{143}Nd/^{144}Nd$ in young basalts were anti-correlated—a reflection of the different chemical fractionation characteristics for the different parent-daughter elements resulting from partial melting coupled with the passage of time. This anti-correlation then related isotopic results to magma generation, chemical fractionation processes, Earth structure, and time. This provided a sound basis for estimating $^{87}Sr/^{86}Sr$ for the bulk Earth, and hence (Rb/Sr)$+$ for the bulk Earth that was far below the chondritic value. These works led to whole new fields of study directly related to large-scale Earth structure, Earth dynamics, and continental growth. These effects reflect the integrated history of mantle depletion, crust formation, and recycling. The group at the University of Paris, led by Claude Allègre, also played a leading role in this new field and in other areas mentioned here. Dr. Allègre and I shared the Crafoord Prize in 1986, partly in recognition of this work (Figure 6).

Use of Sm-Nd and Rb-Sr techniques could establish the time of both formation and metamorphism of continental provinces from the study of igneous, metamorphic, and sedimentary rocks as shown by M. McCulloch. Studies using these techniques also established the nature and extent of water-rock interactions in spreading centers and enabled the determination of the ages and initial isotopic composition of ophiolites. Ophiolites, thought to represent old oceanic spreading centers, were studied to establish the rules of formation of depleted mantle over geologic time. Thinking about the evolution of Earth's crust and the mantle was

Figure 6 G.J. Wasserburg and C.J. Allègre trying to remove an old model of Earth (Stockholm, 1986). It is not evident whether they would replace it with a superior model. Photo by Lasse Hedberg, Stockholm.

much like the problem of cosmochronology with the concept of the "mean age of the elements."

Following up on his work on Paleozoic ophiolites, Stein Jacobsen laid out a simple but rigorous model for the chemical and isotopic evolution of Earth. We showed that the MORB value of $\varepsilon_{Nd} \sim +10$ really represented the result of evolution of the depleted mantle over most of geologic time. These zero age rocks represented the continuous depletion of primitive material for a time of $\sim$3.8 Æ. The apparent mantle age of $\sim$1.7 Æ for a zero age MORB basalt was just the average time over which material in the mantle was depleted, mixed, and stored. Of course, it was even possible to measure CHUR directly in chondrites and to fix that number—which Jacobsen did. This work led to our field parties in the Southern Urals, supported by the Soviet Academy, and finally, after a very long delay, to the Polar Urals, aided by the Russian Academy, to study the depleted mantle. With Mukul Sharma we found some of the most depleted mantle materials, containing almost no REE but having extremely large Nd isotopic effects. The existence of isotopically and chemically distinctive mantle sources was clear. The synthesis of these facts and the more modern concepts of geodynamics remains an active area of research and conflict today. The location and "shape" of these major lithic reservoirs and the dynamics of their evolution is the subject of intense debate and study between the geodynamical and geochemical communities.

From an earth science point of view, these approaches had far-reaching effects. The ε representation (parts in 10^4) that I had made up for Sr during a plane trip to Houston [following Epstein's example of using δ (parts in 10^3)] for oxygen and carbon became part of the argot, joining model ages, BABI, ALL, mean ages, T-I diagrams, and lots of other fun things. It became mandatory to consider these basic isotopic-geochemical concepts when approaching general geologic petrogenetic geodynamic problems. The "young" bad boys, the black box scientists, the non-field geologist (i.e., not regular field-mapping geologists) were now an intrinsic part of the geological sciences in all of its parts—warts and all.

TAKING THE WATERS

The nuclei that are depositied on the sea floor are precipitated out of seawater. Many of these nuclides in solution had either not been measured directly or required a ton of seawater in order to carry out an analysis. The problem of Th, Nd, Sm, Re, Os, and Ir transport in rivers and seawater was a focus of attention. The techniques developed for REE in lunar samples led us into the field of oceanography. Once an ionization efficiency of $\sim$1% was obtained, the problem of $\varepsilon_{\mathrm{Nd}}$, Nd, and Sm in seawater opened up. Such study simply required the measurement of Nd isotopes in seawater where the concentrations are at the femtomole level. Don Piepgras carried through a series of heroic measurements and established the basic rules of Nd as a seawater tracer. He has never received proper professional recognition for his contributions. He and Mary Stordal found that Nd was a clear tracer of the water sources off of the continents, as $\varepsilon_{\mathrm{Nd}}$ reflected the age of the landmass being drained. This tracer is preserved for some time in the circulation of ocean water. Nd in water from Archean drainages was far different from Cenozoic drainages. A sample of seawater from the Pacific was different from the Atlantic sample. The eastward flow of dominantly North Atlantic deep water around Antarctica from the Pacific side through the Drake Passage was demonstrated. Of course, when we applied to the NSF to get support for this work, we were rejected as the physical oceanographers did not see any connection to oceanography. We were not "licensed" oceanographers. Well, onward, with hope, and possibly upward. The seagoing part of the Lunatic Asylum became very active and has continued to the present day with the work of Per Andersson of Stockholm and his studies of the Baltic Sea, rivers, and estuaries. The use of Nd as a tracer of ocean currents is now in broad use (in spite of the objections of some physical oceanographers) and is being vigorously applied to paleo-oceanography and sediment transport.

Sometimes science advances because of a new shovel and the urge to dig a hole somewhere. Sometimes science progresses because there is a need to excavate at a particular place and an appropriate or inappropriate shovel is used or invented. The search with Jim Chen for ^{247}Cm in the early Solar System led to a new generation of precise, sensitive uranium isotopic measurements but no detectable ^{247}Cm. ^{247}Cm occurred abundantly in a journal called *Nature*, but did not occur in nature. There we were with a great new shovel and no hole to dig. However, the precise measurement of ^{235}U/^{238}U on very small samples led to ^{234}U measurements, which in turn led to ^{230}Th measurements. The ^{234}U-^{230}Th chronometer had earlier been used by two generations of workers to date recent processes. This approach ran into the snags of complex natural systems and the limitations of the techniques then used (α-counting). The field remained stuck for several decades. Well, with a new shovel, you can dig a new hole or re-dig an old one. It was clear that counting the atoms was more direct and more precise than counting the number of decays. The crossover occurs where the time for counting decays exceeds the ionization efficiency times the mean lifetime of the isotopes under study.

While lecturing to my class on isotope geochemistry, I showed the new spectra on uranium to the class and discussed areas of potential application. These spectra attracted the attention of Larry Edwards, who took up this topic as a thesis problem. He showed that the sensitivity for dating carbonates could be improved over α-counting by a factor of $\sim 10^4$ and the precision improved by almost 10^2. That made a difference. The dating of formation of corals and other carbonates in the time range of zero years to $\sim$150 ka was now achieved with remarkable precision (15 $\pm$ 5 years to 130 $\pm$ 0.5 ka). This precision now permitted problems of Pleistocene and Holocene processes involving areas of climate change and tectonics to be attacked. A rebirth of ^{234}U-^{230}Th dating occurred. It was shown that the ^{14}C dating was subject to serious errors, as had been indicated from an earlier comparison of dendrochronology with ^{14}C ages. The ^{230}Th chronometer did not depend on the cosmic ray production rate or on the carbon cycle. The results of Edwards' thesis led to a blossoming in the field of studying Holocene and Pleistocene geology and climatology. This led in turn to a new generation of mass spectrometers with extremely high abundance sensitivity that I initiated with Karl Habfast and M.A.T. Finnigan. The new spectrometers allowed the U-Th series nuclei to be used to study disequilibrium in the U-Th series and migration rates—the latter was explored extensively by Allègre and his students. All of this water work was carried out with regular support from the DOE (Department of Energy).

WIZARDS OF Os

By the early 1960s, a search for additional useful radioactive decay schemes would include the decay of ^{187}Re $\rightarrow$ ^{187}Os. Herr and Hirth led efforts to measure Re and Os and tried their luck with molybdenites in order to estimate the decay constant of ^{187}Re and to pursue the dating of iron meteorites. The technical difficulties were so enormous that this work stopped. The basic problem was that measurement of the isotopes of PGE was limited by their high ionization potential. Allègre & Luck (1980) made a major step forward using surface ionization mass spectrometry to sputter off Os^+ ions after chemical separation. They obtained reliable results and determined the age of iron meteorites. The application to terrestrial materials stimulated considerable activity in this area of research. Many difficulties confronted researchers, particularly that of carrying out measurements given the very low sensitivity of the SIMS method and the availability of SIMS instruments. It appeared that the only way to progress was to utilize a resonance laser ionization scheme. Several groups started work in this direction. We decided to try using Geoff Blake's laser and a spare ion source to find out how well it would work. The duty rate for the lasers was obviously a barrier. After a year or two, we had still not made much progress. Other groups, trying very hard to move ahead, designed and began construction of selective laser ionization source mass spectrometers—rather complex systems. One day, Heumann from Regensburg visited Caltech, and I arranged for a Lunatic Asylum seminar. He is undoubtedly the world's expert on negative ion production. In his talk, while emphasizing major activities, he mentioned in passing that he had seen some negative Os ion complexes. Heumann left the next day and we picked up on his hint. Rob Creaser got started on it and within a couple of months had produced enormous negative ion beams of all the PGE, in particular OsO_{3^-}, and showed that isobaric interferences were readily eliminated because of the large mass difference between the complex ion species.

The results were spectacular—ion yields of over 10%, clean spectra, and the ability to measure femtomole quantities of an Os isotope at very good precision. When Creaser presented the results to the Lunatics, the chap working on RIMS said, "Well, I guess I am out of a job." All you had to do was to take any old mass spectrometer, reverse the high voltage and the magnet field, put the element in the right paste, and then measure until the cows came home. A letter was written and submitted to *Geochimica et Cosmochimica Acta* (Creaser et al. 1991). A week later, DAP got a phone call from the reviewer asking permission to use the technique. "Well, of course," he replied, "but it would be nice if you sent us the review first." The reviewer did both and, as advertised, found a humongous OsO_{3^-} ion beam that was stable and ran forever. Phones were ringing up and down the East Coast and the word was out. The dragon guarding the Os isotopes was gone and all of us in the field could go ahead and do science with the ^{187}Re-^{187}Os system. However, problems with Os chemistry and contamination remained.

J. Völkening, who was a postdoc in the Asylum at this time, returned shortly thereafter to Heumann's laboratory in Regensburg and also showed that this new approach indeed worked. I think Heumann would have done it first if he had recognized the importance of the problem. The DTM RIMS machine program was then closed out and something done with all of the fancy lasers. DTM did put it in their annual report and posted on their laboratory walls a picture showing before and after. R. Creaser quickly became very well-known and promptly left for a professorial job and gave up the chance to stay around a bit longer to capitalize on his work.

The laboratory went on to help refine Re-Os ages of all classes of iron meteorites, pallasites, and mesosiderites. Anbar and Sharma also tackled the problem of directly measuring Ir and Os in rivers and in seawaters. Researchers had avoided this problem like the plague due to the extremely low concentrations. The work on Os in seawater and in marine hydrothermal waters opened up a vigorous area of research. It is of interest to note that when we wrote a proposal to the NSF to support work on Os, the reviewers stated that we had done some technological things but were not scientifically productive in this area—thus it is with the peeve review system. Good science management requires program managers who are advised, but who themselves have good judgment in identifying imaginative researches of great potential and quality.

TO BRIDGE WITH THEORY

The ongoing discovery of isotopic anomalies in early Solar System material provoked a widening number of theoretical proposals to explain the data. Willy Fowler and Al Cameron were stimulated by the results and would often engage in the sport of making models to explain particular isotopes. For the most part, models of most astrophysicists were ad hoc and created to explain one or two isotopic ratios and did not provide predictions that could be used to test hypotheses. There was a special stellar source for just about every isotope. The more universal explanation involved supernovae (SN) and assumed extra contributions from a given layer for an onion shell model of SN. The short-lived nuclei with disparate lifetimes, produced by fundamentally different mechanisms, were unexplained by any models. Just before Willy Fowler left for Stockholm to receive his Nobel Prize, he called me at home in a festive mood. I had already gone to bed. He proclaimed that things were now settled with the issue of long-term galactic nucleosynthesis on stars. Half awake, I reminded him that there were conflicts in production mechanisms (protons versus neutrons) and very different timescales ($^{129}I \sim 7 \times 10^7$ years and $^{26}Al \sim 10^6$ years). He agreed that things were more complex and promised that they would get sorted out. Although the general principles seem clear, it is simply not obvious how to apply them to reality. We are still working on it.

After the discovery of a circumstellar Al_2O_3 grain in a meteorite by Huss, Hutcheon, and myself, the oxygen isotopic composition of such grains were of key importance. The results of Huss, and particularly the extensive work of Nittler et al., showed that there were extreme enrichments in ^{17}O (up to $7\times$) and depletions in ^{18}O (down to almost no ^{18}O). Consideration of stellar models with A. Boothroyd and J. Sackmann allowed us to show that some of the results could be explained by the standard model of AGB stars, but there was no way to explain the large ^{18}O depletions. The oxide grains revealed that, if they were from AGB stars, then there had to be an added mixing mechanism in these stars taking material in their envelope to near the H-burning shell. This hypothesized mixing mechanism was not in the standard stellar models. It is now! Of course, the new model led off into broader astrophysical studies and then to some cosmologic issues, such as not making ^3He (but destroying it), possibly in most stars. This mixing mechanism then explained both the ^{18}O destruction in the grains and the astronomical observations of high $^{13}C/^{12}C$. This model of "cool-bottom-processing" was proposed to explain a large number of observations on low-mass stars. The theory of cool-bottom-processing with the extra mixing and associated nuclear processing is now considered to be a real process in stellar evolution. Ken Nollett has made great progress in theoretical studies of TPAGBs with cool-bottom processing, particularly including ^{26}Al production. The dynamical mechanisms of transport that cause this small amount of mixing into the hydrogen shell are not well understood, but the concept leads to a substantial improvement over the one-dimensional models of stellar evolution. As things stand now, there is overwhelming evidence of oxides from AGB sources in grains from meteorites. However, the large ^{16}O excess that would be expected, based on the anomaly discovered by R.N. Clayton in CAIs, has not been found. It appears that this ^{16}O excess may be due to Solar System chemical processes as yet unknown (possibly mass-independent fraction, à la M. Thiemans, K. Mauersberger, and maybe the theory of R. Marcus). It apparently does not derive from stellar nuclear sources. What helped to start the search was not to be the key to the problem.

The remarkable observations by T. Bernatowicz (Washington University, St. Louis) of multiphase assemblages in circumstellar graphite grains (TiC crystals inside of graphite) caught my attention. It looked as if the "paragenesis" of stellar condensates could be understood. Indeed, it all appeared to follow the laws of equilibrium condensation (Sharp & Wasserburg 1995, Bernatowicz et al. 1991). The kinetics of dust formation in matter expelled from stars seemed now to be a very efficient mechanism and was not kinetically limited by a host of destructive processes, as was often thought. Phase equilibrium rules again!

The failure of standard astrophysical models has brought about some interesting insights. The remarkable discoveries on the isotopic composition of presolar carbide grains in meteorites by E. Zinner and S. Amari led to an investigation by R. Gallino, a true expert on the *s*-process, who showed that asymptotic giant branch (AGB) stars were a very plausible source of such grains. Stars of this type are the site of the *s*-process postulated by B^2FH (Burbidge et al. 1957) and Cameron (1957). These fellows (and one gal) were the creators of modern nuclear astrophysics. The evolution of AGB stars is reasonably well understood. As the envelopes of AGB stars are well mixed, the output of all stable nuclei is rather reasonably well prescribed. This mixing is distinct from the SN models where there is great heterogeneity in the envelope, and people picked a SN envelope zone for each isotope effect with very few predictions of what woud be seen. Further, the production of heavy nuclei in SN models was and is simply not well understood nor a settled problem.

I approached R. Gallino and M. Busso in Karlsruhe at a Nuclei in the Cosmos meeting and proposed that some effort be made to see what contributions AGB stars might make to produce some short-lived nuclei. The question posed was if you considered an AGB source that could produce the observed ^{107}Pd and ^{26}Al, what would the abundances of all other nuclei be? As ^{129}I was not expected to be produced in an AGB source because of the low neutron flux, such a study might provide a solution to the conflict between the abundances of ^{129}I, which had to be produced much earlier, and ^{107}Pd. The Torino-Caltech collaboration was off and going in all directions.

The results of our joint research showed that a self-consistent model could be made within the framework of standard stellar evolution in which an AGB star polluted the interstellar medium (ISM) and enriched it in certain short-lived nuclei with calculated abundances. There were weaknesses with the model, particularly the justification of a nearby AGB source for triggering the Solar System formation and the mixing efficiency. However, we made very specific predictions for a wide variety of nuclei that would be present in grains formed by the hypothetical AGB star, and what would be present in the Solar System. This model for the source of several short-lived nuclei in the Solar System might be wrong—but it made specific predictions. Our model could, in part, be tested by observations of AGB dust grains or for the bulk Solar System using a fixed dilution volume. The resulting AGB yields were subject to some change with the development of much superior stellar models, but the results were not much shifted. A major review of this part of stellar evolution was presented by Busso, Gallino, & Wasserburg (1999). Some of the predictions of short-lived nuclei were directly tested in presolar circumstellar grains recovered from meteorites. The abundances of many radioactive nuclei for the model bulk Solar System were listed and provided a quantitative basis for comparison. One prediction was of great importance—the ratio $^{182}\mathrm{Hf}/^{180}\mathrm{Hf} = 3 \times 10^{-6}$. The discovery of ^{182}Hf by Harper and Jacobsen, and the later extensive work by Lee and Halliday showed a small discrepancy with the AGB model—$(^{182}\mathrm{Hf}/^{180}\mathrm{Hf})_{\odot} = 2 \times 10^{-4}$ (now 1.0×10^{-4}) (Yin et al. 2002). This factor of 70 unambiguously showed that the solar inventory of ^{182}Hf ($\bar{\tau} = 13$ Myr) could not come from the proposed AGB source, even if the ^{107}Pd did. A little thought (in bed) showed that ^{182}Hf could readily be produced in the canonical *r*-process over galactic history if the production continued to less than 10^7 years before the Sun formed. That was fine except that the ^{129}I required 70×10^6 years for the last “real” *r*-event. The solution was a debacle—unless one assumed that the *r*-process really meant two (or more) *r*-processes. These two processes were proposed to be quite distinctive: One made heavy *r* nuclei and the other made light *r* nuclei. This scenario led Busso, Gallino, and me to propose that early stars in the universe should show distinctive abundance patterns in the *r*-nuclei, with the earliest stars having heavy *r*-element excesses (Wasserburg et al. 1996). C. Sneden and colleagues then discovered such excesses in heavy *r* nuclei coupled with deficiencies in light *r* nuclei in low metallicity halo stars (Sneden et al. 1998) and their results have been extensively confirmed. These workers found that in low metallicity stars, heavy *r*-process nuclei are essentially just a scaled version of the solar values, but heavy and light *r*-nuclei do not track. They had to come from different sources. The solar *r*-process abundance pattern had earlier been found to be typical of all higher metallicity stars, and the golden rule of one standard *r*-process was the guide for all nuclear astrophysical theoretical models. That rule is now changed. There must be at least two *r*-processes, and what constitutes the real *r*-process remains an unopened book.

One day in 1998, I got a phone call from Petr Vogel who said there was a new postdoc in theoretical astrophysics who had read our article (Wasserburg et al. 1996) and wanted to talk to me. We went to lunch at the Athenaeum where I met Yong-Zhong Qian—a theoretical powerhouse. He was full of ideas and I encouraged him to proceed. He invited me to participate, but I told him that I did not know enough theory to be able to contribute. Qian replied that I would be of use and proposed that we collaborate. We did, first involving Petr Vogel and then on our own. Our adventures have produced an extreme personal high that has persisted for me until today. The two of us came up with a phenomenological model for the chemical evolution of the universe considering very massive stars ($M > 100M_{\odot}$), which governs nucleosynthetic activity at high red shifts ($z \geq 4.5$). These stars were followed by normal SN with the new golden rule of *r*-process nucleosynthesis (two types—high-frequency, heavy *r* elements (H) and low-frequency, light *r* elements (L) of occurrence). These SN were then followed much later by SNIe. We laid out a basis of calculating the abundances of essentially all the elements to be found in stars with a particular emphasis on low metallicity stars (Qian & Wasserburg 2002).

To get some serious attention for our model, Qian and I sent out letters saying that if we were given the abundances of Eu and Fe, we would predict all the other elements from O to U in the star of the astronomer's choice. We offered to wager bottles of good wines if we were wrong. At two recent meetings, announcements were made of the observations and the predictions that we had submitted. Good observers and good sports like Tim Beers made the wager most pleasant. We are still drinking the wine and enjoying the exciting discoveries of 0z (zero metals) led by Judy Cohen and Norbert Christlieb. I am sure we will start losing bets.

Our predictions have also led to a concept that regular stars could not form efficiently unless there was a background of metals in the universe—the prompt inventory—made after Big Bang by massive stars and before regular galaxies formed. This inventory was the basis for Qian, Wal Sargent, and me to quantitatively estimate the abundances of baryonic species in the intergalactic medium (Qian et al. 2002). It is, of course, possible that everything we have done here is wrong. We shall see. At this moment I am waiting for Qian to come to our home to spend a week together trying to write a paper on this new cosmological approach–the origins of heavy elements in massive stars.

Well, what to do next? Of course, there is the thin section of a new, low-metamorphic-grade chondrite on my desk that may provide a clue to early planetary differentiation. The petrographic structures are remarkable. Then there is the problem of ground water transport of radionuclides and the matter of PGE in deep sea sediments that may just provide a global measure of sedimentation in anoxic ocean basins.

The accomplishments of the Lunatic Asylum, which are outlined in this chronicle, could not have been achieved without the contributions of Dimitri Papanastassiou to all things, both through his own research and to that of all the inmates, including me. The Lunatic Asylum will, after a time, be closed, and other great games will be played. The achievements of the Lunatic Asylum were dependent on the presence of an extraordinarily accomplished and dedicated staff, and of a few scientists and technicians. This kernel was the basis of development of new techniques and the maintenance of high skills, arts, and concepts. This kernel was the means of training new students and postdoctoral fellows who could then, for a short time, train others in the arts. The financial support of this enterprise was dependent on the understanding and commitment of federal agencies to the program. Many criticisms (some quite bitter) were directed toward the Lunatic Asylum, with the view that "this sort of thing should not be done at a university" or "they have too much money." Remove this basic group of technically and intellectually dedicated individuals and you are left with the Lone Ranger and maybe Tonto trying to do good. So I will have to stop here as it is time to go back to work before I am ejected.

In closing, I must tell the story of a group lunch at the Athenaeum with Hans Bethe. Hans and Rose Bethe would come for winter visits with some regularity, often with Jerry and Betty Brown. On this occasion Hans was 92 years old. "Hans," I said, in his good ear. "You are the youngest scientist at the table." He replied, " Well, Jerry, it is very good to be young, then you have so much to learn." I will try to keep learning.

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