Report

Oral histories in meteoritics and planetary science:
V. Brian Mason

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Abstract—In this interview, Brian Mason describes the sudden awakening of his interest in meteorites during his student days at Canterbury College in New Zealand when he read a paper on the cosmic abundances of the elements by Victor M. Goldschmidt. Subsequently, he won a scholarship for graduate study abroad and wrote to Goldschmidt asking if he could do a thesis with him in Norway. Shortly after he began his research in Oslo, he fled the city, ahead of the German invasion of Norway, and completed his doctorate in Stockholm with a thesis on the iron-manganese minerals of the Långbana Mine. After the war he taught for 3 years at Canterbury College where he gave courses on mineralogy and geology (into which he inserted lectures on geochemistry) and led students in extensive field studies. In 1947, Mason accepted a professorship of mineralogy at Indiana University. While there, he wrote the landmark book, Principles of Geochemistry, which appeared in 1952. The following year he moved to New York City where he served as the Curator of Minerals at the American Museum of Natural History and an adjunct professor at Columbia University. He became fascinated with the museum's meteorite collection and discussed meteorites in his lectures, which inspired some of his outstanding students to enter the field. During a sabbatical year he spent as a Fulbright Professor in Japan, he gave an advanced level seminar on meteorites and based his book, Meteorites, on his lecture notes. Mason developed a rapid method of optically classifying chondritic meteorites that he applied to major collections in many countries, thus enabling curators to replace uninformative labels such as "stone" or "chondrite" with species names, and to recognize which of their meteorites were rare types demanding serious study. In 1965 he moved to the Smithsonian Institution in Washington, D.C. where he remained for the rest of his career. Early in 1968, he collected specimens from the spectacular fall of the Allende meteorite in Mexico, which proved to be a carbonaceous chondrite containing rare types of inclusions enriched in calcium and aluminum. His analyses showed how these inclusions could be divided into groups on the basis of their differing rare earth element patterns. Mason's studies of Allende continued while he investigated lunar samples returned by the Apollo missions and coauthored a book on them. Beginning in the latter 1970s, he applied his rapid classification of stony meteorites to the large numbers of specimens collected each year by U.S. teams on the Antarctic ice sheet. In 1992 he capped his career with a biography of Victor M. Goldschmidt. In recognition of his many fundamental contributions, The Meteoritical Society honored Brian Mason with its Leonard Medal at its meeting in 1972 at the University of Chicago.

UBM: Brian, my first question to you is what was it that first aroused your interest in meteorites.

BM: In 1937, when I was a fourth year student at Canterbury College at Christchurch in New Zealand, I read Victor M. Goldschmidt's lecture to the Chemical Society of London titled: The Principles of the Distribution of the Chemical Elements in Minerals and Rocks. It opened up an entirely new field of interest to me—one that never had been mentioned in any of my courses in chemistry or geology. Goldschmidt showed that chondritic meteorites hold the key to the absolute abundances of elements, so if you're interested in cosmic abundances you don't look at terrestrial rocks, you look at chondrites.

UBM: Did you write to Goldschmidt immediately?

BM: No, but I checked Chemical Abstracts for more of his papers and found that most of them were in German periodicals that were not available in New Zealand. I couldn't have read them anyway. I did find one in English on Rare
Elements in Coal Ashes, and that also fascinated me. But I had no hopes of going abroad back then.

UBM: When did you graduate from Canterbury College?
BM: In 1938, with a double Master's Degree in geology and chemistry. Then I took a job exploring for oil.

UBM: In the outback of New Zealand?
BM: Yes. In the very rough, remote outback. But in March of 1939, I got a letter forwarded to me from the University of New Zealand offering me a Graduate Fellowship with a generous stipend to study abroad for 2 years.

UBM: So then you wrote to Goldschmidt?
BM: Yes, I did. Students from New Zealand usually went to England, to study at Oxford or Cambridge, or maybe London, but I wrote to Goldschmidt and asked if I could come to study at his institute in Norway. He wrote right back that he never had had a student from the Antipodes, and I would be very welcome. Those were simpler times; he didn't ask for transcripts of my record or any letters of recommendation.

UBM: Being on an island half way around the world from Norway, did you sail westward or eastward?
BM: I sailed east across the Pacific to San Francisco. My ship embarked 2 months after the start of World War II in Europe. On the way to Hawaii, I noticed a young woman sitting on deck reading a book by the famous geologist, Reginald A. Daly. I introduced myself, and she turned out to be the daughter of T. Wayland Vaughan, a well-known American paleontologist. When she left the ship in Honolulu she contacted her mother in Washington who invited me to visit the Vaughans when I arrived there.

UBM: Did you stop off in California or move right along?
BM: I stopped for several days in California. I visited with Howell Williams, the Professor of Volcanology at Berkeley, whom I had met once before. He planned a bus trip across the country for me with introductions to geologists along the way. I also met Ian Campbell at Caltech. I then paid $52 for a month's fare on Greyhound buses and rode to Washington, where I stopped for a week. There, Dr. Wayland Vaughan took me to the U.S. Geological Survey and introduced me to the Director, Dr. Mendenhall. Later that day I shared sandwiches and recited limericks with three leading geologists, Jim Gilluly, Hugh Misner, and T. Foster Hewitt. I had Christmas dinner with the Vaughans at their home before hopping a bus to New York. From there, I sailed across the North Atlantic to Bergen and arrived in Oslo on the 10th of January, 1940.

UBM: Were things quiet in Norway then?
BM: Yes, that was the time of the "phony" war, with no sign of serious action. The next morning, Goldschmidt greeted me cordially, showed me around, introduced me to his assistants, and assigned me an office. He told me that my research subject would be on tellurium—the least known element in geochemistry. I started out making standards by precipitating known amounts of tellurium with lead sulfide for use on his emission spectrograph.

UBM: But your stay in Norway didn't last long, did it?
BM: It lasted until the morning of April 9th, when the Germans invaded Norway. My leg was in a cast from a skiing accident, but when I heard the news I went out after breakfast to cash a check. Money always comes in handy in an emergency. Then I sat in the early spring sun wondering what would happen next. Out of the blue, came Ragnar Kristoffersen, an English lecturer, who said he had gotten his car out of winter storage the day before and we should leave Oslo then and there. I grabbed my stuff and we drove out of the city about 2 hours ahead of Hitler's troops.

UBM: Did you go directly to Sweden?
BM: No. First, we drove to Hamar, a short distance north of Oslo. Then we stopped and debated what to do. We could go east to Sweden, which was nearby, or to the west coast of Norway and try to make our way to England by boat, or we could go to a cottage in the mountains Ragnar knew about, and wait a few weeks until the British expelled the Germans. We couldn't decide between Norway and Sweden, so we tossed a coin. Sweden won.

UBM: That must have been one of the most fortunate coin tosses of all time.
BM: It certainly was, as things turned out. We arrived in Stockholm 3 days later.

UBM: Did you have any chance to say "goodbye" to Goldschmidt?
BM: None at all. I was sorry about that. I had learned a lot from him in the 3 months or so that I was there. He used to come to the laboratory every day and discuss things. I picked up a lot of information and ideas from him.

UBM: What did you do in Stockholm?

BM: I arranged to have my grant from New Zealand pay for graduate work at the University of Stockholm. Nobody there was doing research on geochemistry or meteorites so I had to change my thesis topic. I began working with Professor Percy Quensel on mineralogy and crystal structures. As a non-Swede, I had to get permission from the King of Sweden, himself, to pursue a doctorate there. It finally came as a beautifully autographed parchement all in Swedish.

UBM: What was your new thesis topic?

BM: It was on the complex chemistry and mineralogy of the iron-manganese minerals at the Långbans Mine in Sweden.

UBM: You did meet Goldschmidt in Stockholm later on, didn't you?

BM: Yes. One December evening in 1942, while I was driving myself crazy trying to calculate the structure of the mineral braunitte (Mn₂SiO₄), I received a phone call inviting me to come to a professor's house to meet a visitor. There sat Victor Goldschmidt! At first, he had tried to continue his work in Oslo, but he had been arrested twice and interned in a concentration camp in Norway. Then, just as he was about to be deported to a camp in Poland, he was abducted by Norwegians dressed in purloined Gestapo uniforms and spirited to Sweden.

Goldschmidt's knowledge of materials science was of such strategic value that the British, at no small risk, flew him to England in March of 1943. Once there, he told them about a bright young scientist in Sweden (me) who also might be valuable in the war effort.

UBM: Did you have your Ph.D. by then?

BM: Not quite. I got it in May of 1943. Then I put my name on a list to fly from Stockholm to Aberdeen aboard one of the SAS DC3s that were clandestinely ferrying ball bearings from neutral Sweden to Scotland. I had to wait for months but I finally flew there in August, 1943, on just a few hours' notice.

UBM: Did you see Goldschmidt in Britain?

BM: Yes, but only once. He had been moved from London to Aberdeen, at the northeastern tip of Scotland, to work at the Macaulay Institute for Soil Research. I think it very likely that they sent him out of London because he was talking too freely about developments in atomic energy research. This topic was commonly talked and written about in Sweden but it was top secret in England. I visited him at Aberdeen in December, 1943, and we spent a pleasant evening watching his coal fire burn to bright yellow ash. I recalled that in that second paper of his that I had read as a student, he explained the yellow color as due to the presence of 1–2% of germanium oxide.

UBM: What did you do in England?

BM: I had several short assignments, including x-raying minerals in the British Museum. Then I met Dr. Marsden of the New Zealand High Commission who urgently wanted me to return to New Zealand and set up an x-ray laboratory in the Dominion Laboratory at Wellington. In fact, he hired me then and there so that I would be ready to return to New Zealand as soon as they could arrange transport for me. It didn't work out that way, though, because I arrived in New Zealand way ahead of the x-ray equipment, which, for all we knew, might not come until after the war. I made myself useful on various projects at the Dominion Laboratory and then I got a message from Robin Allan, the Professor of Geology at Canterbury University College in Christchurch, urging me to apply for the lecturership in geology that had opened when he was promoted to his full professorship. I asked to be released from my responsibilities to the Dominion Laboratory and took up the lecturership in October, 1944.

UBM: So did you discuss meteorites in your courses in geology?

BM: Not in any detail, but I started upgrading the traditional geology courses with lectures on the new science: geochemistry. I also taught mineralogy and hand-specimen petrography for beginning students, and advanced tutorial-type sessions on many topics, including engineering geology. Arranging field excursions for students had its difficulties in wartime, but we managed to visit some spectacular geological exposures on both islands. I was with my advanced students at a station called Island Hills in August, 1945, when we heard about the Japanese surrender and the end of the war.

UBM: How much longer did you stay in New Zealand after that? I know you were at Indiana University in 1947.

BM: I stayed at Canterbury just about 3 years altogether and arrived in Indiana in September, 1947. Late in 1946, to my great astonishment, I had received a letter from Indiana University asking if I would be interested in a professorship of mineralogy there; and if so, would I please cable them what I would want for my starting salary? (Later on, I learned that I had been recommended by Ian Campbell at Caltech, who had turned down the offer himself.) I quickly polled friends in the states about an appropriate salary and asked for one that would be a little more than double what I was earning at Canterbury.

UBM: With all the post-war crowding you must have found IU to be a very different scene from Canterbury.

BM: It certainly was, what with lots of new students plus veterans on the GI Bill. I had 36 students in mineralogy in 1947 and 72 in 1948. Incidentally, it was in 1947 that I gave a lecture in the Department of Geology at the University of Chicago and met you there.

UBM: I remember that very well. You came to Julian Goldsmith's laboratory where I was working as his assistant and then I went to your lecture on geochemistry. It was especially appropriate because at that time the department was awash with Scandinavian geochemists, including Tom Barth, Kalervo Rankama, and Hans Ramberg. Were you already writing your book on geochemistry?
BM: No. But I was giving a graduate seminar on geochemistry which came to the attention of the publisher, John Wiley & Sons. I told them I doubted if a book on such an out-of-the-way subject would sell, and they said: "You write it; we'll sell it."

UBM: And sell it they did! Your Principles of Geochemistry was, after all, the first textbook on geochemistry in English and it was written in a clear, logical style that made the subject interesting to undergraduates as well as to graduate students and to full professors. It was a new field for almost everybody. It went through three editions, didn't it?

BM: Actually, it went through four: the first in 1952 and three more in 1958, 1966, and 1982. It still remained a new field for some while after 1952, but geochemistry has since become an accepted subject in most universities.

UBM: Your book has been translated into several languages, hasn't it?

BM: Yes, Japanese, Russian, German, and Portuguese.

UBM: Speaking of Portuguese reminds me that in 1953 I bought your book in Rio de Janeiro. My copy was in English, but I bought it in a Portuguese-speaking country and carried it to another one, Angola, where I read it at our field camps near the source of the Zambezi River. I told you about this after I got home thinking you would be pleased to know how far your book had traveled. But you replied: "I would much rather you had bought it in the States; I would have gotten a much bigger royalty."

BM: Well, I apologize, in retrospect, for having been so ungracious.

UBM: No problem. While you were at Indiana, Ross Taylor joined you there, didn't he?

BM: Yes, in 1947 Ross had been a student in the last mineralogy course I taught at Canterbury. In 1949 he wrote me saying he wanted to do graduate work in geochemistry in the United States and asked me where he should apply. I started out suggesting Berkeley or Chicago or Penn State and then it occurred to me that he could come to Indiana. We had excellent facilities and I told him I could get him a fellowship to pay his fees. So Ross came to Indiana University in 1950.

UBM: But you must have moved to New York soon after that.

BM: I moved about 3 years later, in May, 1953, to accept a position as Curator of Mineralogy at the American Museum of Natural History with an adjunct professorship at Columbia, to teach geochemistry.

UBM: This move must have been unexpected to both you and Ross.

BM: It was. I had no idea of leaving until I got the offer late in 1952. Ross completed his thesis after I left and I went back for his oral examination. He did an excellent job and I am proud of him as my only Ph.D. student.

UBM: In New York, you had your own meteorite collection.

BM: When I came to the museum I was confronted with the whole meteorite collection stacked in boxes in my office.

The Mineral Department had transferred the collection to the Hayden Planetarium when the Planetarium opened in 1937. Now, in 1953, the Planetarium had sent it back, and there it was.

UBM: So you couldn't even see what you had until you unwrapped all the specimens.

BM: That's right. I spent the summer of 1953 unwrapping each specimen, sorting them out, checking them against the catalog, and at once I became entranced. I hadn't quite realized the tremendous variety of meteorites. I knew them vaguely as chondrites and a few other types of stones, but I was not prepared for the large range of compositions I found. The first time I saw a carbonaceous chondrite, I said: "This can't be true. This can't come from outer space."

UBM: Your enthusiasm must have been contagious because Ed Anders has testified that when you brought some meteorites to your geochemistry class at Columbia in 1953 he was so excited by them that he resolved on the spot to pursue a career in meteoritics.

BM: I'm delighted to hear that. Billy P. Glass also was one of my students and he wrote his first paper on tektites for the course.

UBM: Back to carbonaceous chondrites, hadn't you seen any of them in Goldschmidt's lab?

BM: No, carbonaceous chondrites were one type of meteorite Goldschmidt never studied.

UBM: And meteorites simply hadn't been available for most people to see and compare.

BM: Exactly. And in going through the correspondence at the museum, I was appalled to see that my predecessor, when asked for meteorite specimens, usually chose the dirtiest, most weathered, most miserable-looking objects in the collection. He supplied these to people like E. B. Sandell, who did analyses for some elements, I forget which ones. Then, too, I found the catalog full of meteorites classified simply as "stone", or possibly as "chondrite", but nothing beyond that. This upset my natural curatorial sense—I do have such a thing—I like to classify things. And having all these meteorites just labeled "stone" struck me as a very, very bad situation. What we needed was to supply meteorite researchers with clean, well-documented, unwreckered meteorites, and I was stimulated to do just that.

UBM: How old was that collection? Who had started it?

BM: Oh, it started right back at the time the museum was founded in 1872. For the most part the collection was originally put together by a man called Bement. Bement was a keen collector of minerals but he also collected meteorites, and it was his collection, really, that was the basis of the American Museum collection.

UBM: Did he give the collection or sell it to the museum?

BM: If I recall correctly, J. P. Morgan bought the collection for the museum. That gave it a sound basis, but nobody at the American Museum worked on meteorites. There once was a curator named Reeds, who made a catalog in 1937 when the collection was transferred to the Hayden Planetarium. By that
time they apparently decided they'd better at least have a catalog. It actually was quite a good catalog, although, as I say, a lot of the material was simply called "stone".

Shortly after I came to the American Museum, I had a visit from Birger Wiik. Wiik was a chemist with the Geological Survey of Finland who had spent a year at the University of Chicago analyzing meteorites for Harold Urey. He had gotten interested in meteorites through Walter Wahl, in Finland, who had done some very good work on meteorites way back before the first World War, and who published a paper in 1950 in the first issue of Geochimica et Cosmochemica Acta pointing out that some of the published analyses of meteorites were highly improbable. They just didn't fit the mineralogy. And Wahl had Wiik redo some of them. Apparently, Harold Urey then got interested in them and managed to hire Wiik to work at Chicago for awhile. As a result, Wiik published a paper in 1956 in Geochimica in which, among other things he distinguished three types of carbonaceous chondrites: types I, II, and III. He was on his way back to Finland after Chicago, when he stopped off in New York to see me and said, look, we should do some more of this. I agreed heartily. I said I would be glad to select meteorites in good, unweathered condition from observed falls that would provide us with good documentation. I would do the mineralogy and petrology, and he would do the chemistry.

UBM: An ideal arrangement!

BM: Yes. So, as I recall, I made an application to the National Science Foundation for a grant for systematic analyses of stony meteorites. I think it was a modest request for $3,000, or something in that order. I got back a reply that, no, they had so many other important fields to cover that they were forced to reject mine. Then I heard about the J. Lawrence Smith Fund of the National Academy of Sciences. I got $3,000 from that fund, which paid for me to get thin sections made of the meteorites we were going to analyze and paid for Wiik's chemicals and some platinum ware, and so on. (I might add that after Sputnik-I went into orbit in October, 1957, I dusted off my NSF proposal, asked for a much larger grant and got it without delay.)

UBM: Did Wiik do the analyses at the Geological Survey in Finland?

BM: No. Once he was back in Finland, he set up a little laboratory in his own backyard where he analyzed meteorites for me. So, I went all through the museum collection, selecting meteorites which were not well analyzed but which were large enough so that we could characterize them and then supply samples of them to other people. I think that between 1956 when we began and 1965 when I moved to the Smithsonian, we must have analyzed 30 or 40 stony meteorites and published their descriptions and analyses, mainly in the American Museum's Novitates series.

At the same time, I was concerned because of the very large number of meteorites that remained unclassified. I had Hey's Catalogue from the British Museum, the standard one used in those days, and it listed just lots and lots of unclassified stony meteorites throughout the world. I realized that while complete chemical analyses are very useful, we couldn't possibly analyze all these uncharacterized stones. It was just too time consuming and too expensive, and so we had to find a simple way of classifying stony meteorites. Thinking this over, I realized that the common constituent of virtually all stony meteorites, except enstatite chondrites, is olivine. Now, it's relatively easy to determine the composition of olivine from its refractive indices because these vary with the iron content. So I started doing a little simple procedure of just grinding up a little chip of a stony meteorite, sieving the powder to a uniform size...

UBM: Putting a pinch of powder onto a glass slide, adding a drop of index oil...

BM: ...and looking at it under a polarizing microscope! You only have to check how the indices of refraction of the olivine grains match that of the index oil. Harold Urey and Harmon Craig had just published their paper establishing the high, low and low-low (H, L and LL) iron groups amongst the chondrites, and I found that I could classify a stone into one of those groups in less than 5 minutes by using the immersion method. A single oil with a refractive index of 1.710 would suffice: the refractive indices of the high-iron olivines cluster around 1.705; those of the low iron group cluster around 1.715. So you check whether the olivine indices are higher or lower than that of the oil and you've classified your H or L chondrite. The indices of the LL group olivines are higher—1.72 to 1.73—so they clearly stand out from the others. This technique works very well, even with weathered meteorites. Weathering doesn't affect the olivine composition.

UBM: The immersion method is wonderful. It's rapid, reliable, and cost-free, once you have a microscope and a set of index oils. But today few students are taught how to use it.

BM: Right, and it's a great loss. For many years now, most people have required expensive equipment such as electron microprobes or x-ray fluorescence to analyze olivines, as well as every other mineral. These instruments are essential for all sorts of other studies but not for chondrite classification.

UBM: So I suppose that you quickly classified all the stones in the American Museum collection.

BM: Yes, I did. Then, in 1960, I went out to Arizona State University and classified all the stones in the Nininiger Collection at the Center for Meteorite Studies. I carried my little bottles of oil with me.

UBM: Isn't that about when you went to Japan?

BM: I went there the next year, 1961. I was due for a sabbatical leave and went to the Geological Institute of the University of Tokyo as a Fulbright Professor. They asked me to give an advanced level seminar and I chose meteorites for my subject. I found that they had a very good library with many of the sources I needed. I wrote out my lectures in some detail in an effort to make them as clear and understandable as possible to a Japanese audience. Then I expanded on them during my free time in Tokyo and submitted a manuscript to
John Wiley & Sons, who published my book, Meteorites, the following year.

UBM: That was another first! The first text of its kind and exactly the book so many people needed in the early Space Age! I know that you attracted one of your Japanese students, Akiho Miyashiro, to do some research on meteorites.

BM: Yes. I was very pleased about that. Of course, he eventually went on and made a name for himself as an expert on the petrology of island arcs, and such.

UBM: And he spent a large part of his career teaching at the State University of New York in Albany. In 1999, when Ross Taylor and I were planning the symposium in your honor at that year's Goldschmidt Conference at Harvard, I invited Miyashiro to come and give a talk. He thanked me graciously and sent you his warmest regards but explained that his health would not allow him to make such a trip.

BM: I would have felt greatly honored to see him there.

UBM: So, when you left Japan, did you go back to the museum in New York?

BM: Yes, but I spent 3 months in Europe in the summer of 1962 classifying stony meteorite collections in Helsinki, Stockholm, Copenhagen, Bern, Heidelberg, Göttingen...

UBM: Wonderful!

BM: Also at the ETH in Zurich, the Natural History Museum in Paris, and the British Museum. In all, I classified something like 800 chondrites that year. Actually, just at that time, Ture Sahama in Finland had established a means of determining olivine compositions using small samples of powder on the x-ray diffractometer. I had carried home a gram or two of each of the chondrites I classified optically, so I ran a bit of each of them again on the diffractometer and they checked out nicely. Then in 1963, I was able to publish a paper in Geochimica et Cosmochimica Acta, on the compositions of olivines in chondrites.

UBM: As a result, by 1963 researchers in every one of the places you visited, and others who had specimens of the same meteorites, finally knew what types of chondrites they had available to work on. Also, as I remember it you added ~60 meteorites to those in the catalogues.

BM: That's right. And the optical method proved to be very useful again in the 1970s when we started to get all the Antarctic meteorites. How would we ever have classified all those thousands of stones if we hadn't had some simple, straightforward technique?

UBM: We were fortunate that you volunteered to do it, and that you still had some liquids around. I wonder if you and I just possibly may be the only two people who still have sets of index oils.

BM: Probably not, quite. Lots of oils are gathering dust in mineralogy labs. Yet it's such an elegant technique. So I suppose if I were to state one thing that I've done that has been the most useful to meteoriticists it was to sort out the classifications of chondrites and to classify so many of them.

UBM: Which no longer are listed as "stones"!

BM: Right. But some people may say that an effort like that is like stamp collecting.

UBM: People only say that when they haven't a clue what they would do if they were faced, as you were, with totally unclassified research materials.

BM: In a way, though, chondrites are a bit like stamps in that there are lots of very common ones and there are some very rare ones. And of course you learn more from the rare ones than you do from the common ones. So, not only did I classify a lot of stones, but I established which ones are rare and deserve more study.

UBM: I never realized that you went through all those European collections.

BM: There were more. In 1964 I spent a couple of weeks in Moscow, and from there I went to Calcutta and did the Indian collection. I got to do a lot of traveling on that project.

UBM: How about collections in Australia and New Zealand?

BM: There was not much of a collection in New Zealand, except for a beautiful Canyon Diablo iron in the Christchurch Museum. But of course I did Australian meteorites on various visits.

UBM: During all your years in New York did you continue your collaboration with Birger Wiik?

BM: Yes, we worked together from 1957 to 1965, when I came to the Smithsonian. After that I did a lot of work with the excellent Department chemist, Gene Jarosewich.

UBM: Wiik went to Arizona at the time of the Apollo 11 mission, didn't he?

BM: Yes, he did, in 1969. I remember that because in July or August that year, he stopped off to see me on his way west. I urged him to wait until it would be cooler. He said that after Finland, an Arizona summer would be delightful. He reported later that his wife said she never felt better than when she was in the Arizona sunshine.

UBM: You moved to the Smithsonian in plenty of time to get ready to receive the lunar samples.

BM: Yes. The presence of an electron microprobe that had been acquired at the Smithsonian proved to be crucial in attracting me here. I had 4 years to get ready for the lunar rocks.

UBM: Meanwhile, in 1969, the Allende meteorite fell in Mexico and changed history.

BM: Yes, Yes! What a year that was!

UBM: Let's count the Space Age marvels: in February, Allende, a rare type of carbonaceous chondrite with conspicuous pink and white inclusions strewed more than two tons of fragments over northern Mexico; in July, Apollo 11 splashed down into the Pacific Ocean with the first precious cargo of lunar rocks and soils; in September, Murchison, another carbonaceous chondrite that contained molecular hydrocarbons not known on Earth, fell in Australia; and in December, nine meteorite fragments were collected from a small patch of bare ice by a Japanese team in Antarctica!
BM: In all of history, there never has been a year for meteorites like that one.

UBM: How did you first hear about the fall?

BM: I always get the Sunday New York Times and on that Sunday, which must have been February 9th, the paper reported that a brilliant fireball had been seen in southwestern Texas and northern Mexico on the early hours of February 8th. When I got to the office Monday morning, there was a message to call the Smithsonian Astrophysical Observatory in Cambridge. They told us that the fall had taken place at ~1:05 A.M. on Saturday, February 8th, and Dick McCrosky at the Prairie Network had established the site of fall to be near Parral in Chihuahua. I felt that we couldn't just go dashing down there without clearing it with the Mexicans, so I arranged for one of our Spanish-speaking curators to call up the Director of the Geological Survey in Mexico City. He authorized us to go and search but he wondered if it would be worth our while. He believed that one or two specimens had been picked up but held out no hope of finding much more. So that was Monday. On Tuesday, the 12th, Roy Clarke and I flew to El Paso, and on Wednesday we hired a car and drove south to Hidalgo del Parral.

The realization of what we were onto dawned on us when we arrived in Parral, the nearest city to Allende. As we were driving down the street to get to a hotel we saw a specimen displayed in a window. We took a close look and saw that it quite clearly was a meteorite, quite clearly a carbonaceous chondrite, and quite clearly an odd one, because it had great big white chondrules in it. We had hit the jackpot. We stayed there about a week. February was early spring in northern Mexico and just a delightful time with the fruit trees just coming into bloom. Fortunately for us, the ground was pretty bare. There was plenty of wide open country and they had just started plowing. The only other rocks around were limestones, so if you saw a piece of black rock it was almost certainly a meteorite.

UBM: And they were so common that you just kept seeing them?

BM: Yes. In Parral, I had an introduction to the manager of the American Smelting and Refining Company which had several mines in the Parral district. He told us he was still doing some office work when he saw the flash of light and thought something had blown up. He walked through his house and then he heard the sound. Anyway, he arranged for us to take one of his Mexican engineers, Manuel Gomez, who spoke good English, as an interpreter and guide. Allende is ~20 miles east of Parral, so the next morning, we set out with Gomez. He suggested that we should go to the local school and hire a sixth grade class to act as searchers. We got a company truck and loaded in some cases of Coca Cola to satisfy them, planning to go to where meteorites had been found and walk the fields from there.

When we approached the headmaster of the school, he was quite happy to let his sixth-grade class go out with us for the day. We lined up 30 boys side-by-side with some distance between them and started them marching across a field. We hadn't been gone more than 2 or 3 minutes before one of them found a meteorite. It was a beautiful crusted stone just sitting there waiting to be picked up. I don't remember how many we got that day, but we walked ~3 miles and must have found ~20 or 30 in that time. During the week we were there, I think we got ~150 kg of them. That was one of the great experiences of my life; going out into the field and picking up meteorites everywhere.

UBM: Did you also buy specimens?

BM: We did. We put out the word that we were interested in buying meteorites, after consulting with Gomez about what would be a reasonable price. He said, well, the farm laborers around here get about a dollar a day. So, if you offer a dollar a kilogram, I think they will find it to be fair, without getting the idea that they are of too much value. But perhaps we would pay a little more for fine specimens.

UBM: Good.

BM: So, we did buy quite a number. And by noting where the bigger and smaller ones were we got a very fair idea of the configuration of the strewn field—with the help of some observations that came in from farther to the southwest. That was certainly the most fruitful meteorite hunt I've ever been on. Besides being beautiful weather, the people were all very nice to us. Incidentally, Elbert King had been on the ground just ahead of us.

UBM: Elbert had only to go down there from Houston; he was NASA's lunar sample curator at that time. I first heard of the fall on Tuesday when Charles Lundquist, our Assistant Director for Science, came into my office at the Smithsonian Astrophysical Observatory and told me I was to fly to Houston later that day to receive a specimen of the new meteorite from Elbert. Elbert was back in Houston by then and had agreed to give me one. I left for Houston toward evening on a flight that was delayed en route and did not land until way after midnight. Nevertheless, Robert Greenwood, from the curatorial facility at the Manned Space Flight Center, was at the airport to meet me. He took me home and his wife gave me a quiet bedroom in which to catch a bit of sleep before meeting Elbert the next morning.

"This hurts me terribly to give this to you," said Elbert as he handed me a nice specimen about four inches across. No offense taken or intended; Elbert and I had been good friends during his years in graduate school at Harvard. That is the piece of Allende, in which I described a suite of minerals resembling the condensation sequence that had been predicted for nebula processes by Ed Anders and his group. It also is the one in which I found spinel crystals embedded in an isotropic phase that gave no x-ray pattern so I identified it as glass. This was weird because glass doesn't belong with minerals like spinel, and no additional glass has been found in Allende. But I think that in meteorites you have to get used to finding the one and only example of this or that.
BM: You certainly do. In one of the specimens I brought back, there was a very unusual chalky-looking chondrule about a centimeter across. I dug it out and found it was just loaded with sodalite. So I sacrificed about half of it for chemical analysis thinking well, I found one, we'll find more. But we never found another one like it. You know how that goes.

UBM: Was the sodalite coarse-grained?

BM: Not at all. It was very fine-grained. I think the sodalite is sort of a metasomatic product. I've only seen meteoritic sodalite in x-ray diffraction patterns, never microscopically.

UBM: So that is the story of the fall of the Allende meteorite which still is yielding research results that are reported every year whenever and wherever meteoriticists gather. Did you play any role in recovering the Murchison fall?

BM: No. I had been in Australia earlier in 1969, but had left before the fall. I've never really worked on Murchison. But, with more than 100 kg to work on, meteoriticists everywhere fell to and did a bang-up job on the characterization of Murchison.

UBM: Meanwhile, in 1969, we received the first lunar samples. And shortly thereafter you and Bill Melson wrote a book about them.

BM: I thought that inasmuch as we had to do all this work for the first Lunar Science Conference in January, 1970, we might as well put it all together in book form so people could read about it. Ross Taylor and Al Levinson were writing their book at the same time, quite independently.

UBM: That was a good thing. Members of the public kept asking me where they could read about the lunar samples, and there was no easy answer until these books came out. What kind of samples had you asked for from the Apollo 11 collection?

BM: I'm not sure now what I asked for. What I got were two vials, each containing one gram of soil.

UBM: In our lab, we actually had asked for soil in the first place at a time when most people were asking for rocks or thin sections. John Wood, who led our group, reasoned that soil would be collected if anything was and it might well include impact fragments from many parts of the Moon.

BM: Very good; very good reasoning. I remember coming back from Houston with my two vials and when I arrived at my house at dinner time I threw the two vials on the table saying: "Here's $24 billion of your taxpayers' money."

UBM: Yes, but I must say I never cared for that line of reasoning. Some newspaper headlines were shrieking: "Moon Rocks cost $24 billion." Well, the Apollo program as a whole cost about $24 billion up to then, and its principal purpose was to reestablish the national prestige of the USA. I would argue that it was that picture of the US flag planted on the Moon that cost $24 billion; the rocks were free of charge.

BM: (LAUGHTER) Of course they were.

UBM: How long did you keep on getting lunar samples?

BM: I got samples from all the lunar missions. But, actually, when Apollo 17 brought back what was essentially the same as Apollo 11, I dropped out of the program.

UBM: So in the early 1970s you went back to meteorites. Do you remember, what were the main things you took up then?

BM: All the while I was working on lunar samples, I still was taking Allende apart. I was interested in the very curious rare earth patterns I was getting with ytterbium anomalies and also thulium anomalies which I didn't really understand but which obviously meant something. I decided it would be important to have a standard meteorite that would be analyzed by a number of laboratories, so as to establish a reference sample.

UBM: Like G-1, the standard sample of granite from Westerly, Rhode Island, that was used by chemists everywhere?

BM: Yes, exactly like that. Of course, it would have to be a stone. But how do you homogenize a stony meteorite with all its metal and sulfide grains? It is very difficult, but Allende provided an excellent solution because it had essentially no metal, so you could grind up a large sample of Allende and make it homogeneous.

UBM: And you had a large supply of Allende in the National Museum.

BM: We did have a lot of it. So, I chose one of our big stones and carefully supervised its crushing. As chondrules fell out, I saved them and when I saw an inclusion of interest, I picked it out. I was convinced that these small particles were not going to change the bulk analysis of such a large sample. So I acquired a considerable collection of chondrules and inclusions. I had thin sections of them made and did the microscopic work here at the Smithsonian. Then I took them out to Canberra where Ross Taylor and I analyzed them on the spark-source mass spectrometer. That is a sort of a universal instrument that hasn't received the credit due to it. On a single plate every isotope will show up if it's there in parts per million. Undoubtedly it is the most useful tool for analyzing rare earth elements because you can get all of them using lutecium as an internal standard. In the 1970s, I went to Canberra several times to established several different groups of Allende inclusions on the basis of their rare earth distributions. Doing that, I found lunar rocks to be less interesting than meteoritic materials.

UBM: But the main mass you crushed up was used to establish the standard meteorite of which samples were available to whomsoever asked for them?

BM: Yes, absolutely.

UBM: Was there much demand for it?

BM: There was great demand. Gene Jarosevich curated it, and he sent splits to dozens of universities and other laboratories all over the world. It's been very widely used. I have been interested to see the extent to which different analytical methods gave very consistent results. Although it was not so for some elements.

UBM: Which ones?

BM: For one example, most people didn't get zirconium right. But the spark-source mass spectrometer did. That was right on the mark. Ross and I were very pleased about that and
published a small memoir discussing what made some results differ. We prepared the standard sample of Allende in 1970 and began distributing it in the early 1970s. I think we finally worked up the results for publication around 1980.

UBM: Meanwhile, in 1971, you had edited the *Handbook of Elemental Abundances in Meteorites*, a title a bit reminiscent of the work of V. M. Goldschmidt.

BM: It does reflect his basic interests. It seemed, back then, like high time to get together in one place everything we knew about the subject.

UBM: A couple of years later we first learned at the Meteoritical Society meeting at Davos in 1973 that meteorites seemed to occur in concentrations on the Antarctic ice sheet.

BM: That was when Makoto Shima reported finding four different types of meteorites among the nine fragments that had been collected in 1969 by a Japanese team on a small patch of ice. And Bill Cassidy in the audience realized that there must be something special about ice motion in Antarctica.

UBM: He surely did, but Bill didn't get his proposal for a USA search team funded by the NSF until his third try in 1976.

BM: I remember that the NSF sent me Bill's first proposal and I reviewed it very favorably. I said it was a great idea, not very expensive, and likely to produce great results. Apparently, I was the only favorable reviewer because it was rejected that first time around.

UBM: And the second time around. And it would have been rejected the third time except that by then the Japanese had just returned home with more than 600 meteorite fragments collected during their third season of searching.

BM: So Bill first went to McMurdo Station in 1976 and took Ed Olsen, of the Field Museum, with him. At McMurdo I gather that they teamed up with Keizo Yanai who had been sent there from the Institute of Polar Research in Tokyo.

UBM: Yes, an arrangement was made, on site, for the three of them to search together and share whatever specimens any of them found. So at the end of that season, they went to the grinding room in the Earth Sciences Laboratory at McMurdo and simply sawed open the stones. Yanai got one-half of each one and Cassidy and Olsen shared the other half. Back home, Cassidy began thinking that this couldn't be the right way to handle meteorites from the cleanest environment on the Earth, so he polled the members of The Meteoritical Society for opinions on how to collect and curate Antarctic meteorites without contaminating them.

BM: Then the NSF arranged for an one-day meeting of people to discuss this. You and I were both there.

UBM: Yes, we were. That was a very *improptu* conclave held on Armistice Day, November 11th, 1977, at NSF headquarters in D.C. I got a call, a week or so before that, from Mort Turner at NSF who invited me to attend but quickly added that they had no money for anyone's travel expenses. Mort didn't know me nor I him, but he explained that he was calling people whose names were suggested by other people as being interested in meteorites.

BM: That was the meeting at which we all began to take seriously means of avoiding contamination in collection and curation procedures.

UBM: And we accepted the proposals by Don Bogard, who came fully authorized to offer the clean room facilities and experienced personnel of the lunar curation team at Houston. We also agreed to the principle that the NSF would support the field teams, NASA would carry out the curation, and the Smithsonian, would classify the meteorites and, in its role as the national curator, act as their final repository. This led to the formal three-agency agreement between the NSF, NASA and the Smithsonian which must be unique in the U.S. government bureaucracy. It has some troublesome clauses that are way out of date but it has served its purpose very well over the past 25 years.

BM: I believe the new sterile techniques were in place in time for the 1977–1978 field season.

UBM: Yes, they were. They called for photographing each meteorite *in situ* on the ice, collecting it using clean Apollo-surplus tongs and bags, and shipping the season's crop still frozen to Houston to be opened in sterile glove cabinets filled with dry nitrogen. The three-agency Meteorite Working Group, with a rotating membership, on which we both have served, was formed to advise on the distribution of samples to research labs around the world. Since the 1976–1977 season, U.S. teams have collected more than 12,000 meteorite fragments.

BM: And when you add in the ones collected by Japanese and European teams you get ~25,000 of them, which at a rate of ~4 specimens to one fall, would be more than 6000 different meteorites. I remember that a year or so after the program began, I was at the Canterbury Museum in Christchurch talking to Margaret Bradshaw who was the Curator of Geology there. She asked me: why do you want to collect more meteorites every year, and why do you take them all, why don't you leave some?

UBM: She asked me the same thing when I visited her museum on the way to my first Antarctic field season in 1978.

BM: It struck me as a very curious attitude. But people who are not interested in meteorites just don't understand why we would want to collect more of them.

UBM: They seem to think that if you've seen one, you've seen them all. Perhaps we are not being quite fair, though. Anything resembling a mining operation violates the Antarctic treaty, and Margaret may have felt that by taking every sample we found we were close to mining them out. Others shared that attitude.

BM: But, we need to take them all in order to find the rare ones.

UBM: We surely do. But another type of problem arose when large numbers of the meteorites began arriving in Houston. "Who is going to look at all these stones?" asked Klaus Keil at an early meeting of the Meteorite Working Group. He knew most of them would be ordinary chondrites and neither he nor his students would want to give much time to them.
But you saved the day. I knew you already had volunteered to classify them, and, in fact, I was carrying a copy of your first description of one so I read it to the group. It was, indeed, of a very dull, uninspiring ordinary chondrite. Klaus said: "If it looks like that, I don't want to see it." He was extremely pleased, though, that you were willing to classify them for us.

BM: But perhaps not everybody was so pleased. I got the distinct impression that some of our colleagues were thinking that the Smithsonian was going to hog this stuff.

UBM: They wouldn't have thought so once they learned about the rules you agreed to. You were not allowed to keep any specimens for yourself. If you wanted to work on one you would have to request it from the Meteorite Working Group, just like everybody else. And you were allowed to do only the minimum of study needed just to classify them. Mainly, I suppose, by the immersion method.

BM: Yes, and now and then I could do a bit of microprobe work if I needed to settle a question. In the preliminary go-around though, I left all the real analyses for others to do.

UBM: And, as usual, you thought the classification was fun.

BM: I did. And, it was true that I did get the chance to see all the thin sections first. I remember the thrill seeing that first lunar meteorite. It took perhaps 30 seconds for me to see that it looked like an Apollo 16 rock.

UBM: In your description, though, I think you wrote something more circumspect, such as: "The large white clasts are highly reminiscent of anorthosites from Apollo 16."

BM: Yes, yes, I thought I shouldn't be too positive. But I was quite certain it was lunar. That was probably the most exciting thing to come out of the program, for me. The realization that meteorites could come from the Moon and be recognized as such.

UBM: I don't think we would have recognized it as lunar if we hadn't been to the Moon. We were very surprised to find anorthosites in the highlands.

BM: Yes. And now we have meteorites from Mars. To me, the physics of getting something from Mars to the Earth is black magic.

UBM: Some of the dynamicists admit that it is almost black magic to them, too. They say that we really do not understand how to eject a piece from Mars that is large enough to have anything left of it when it gets to the Earth. However, whether or not we understand how they get here, we now have an astonishing 126 kg or so of meteorites from Mars!

BM: And they are providing us with lots of unexpected information. There are many questions, though, remaining to be answered. It is rather like the situation with australites. I've worked on that for decades, but now I have dropped it because there are things I don't understand. It is like a jigsaw puzzle with some of the pieces missing. I don't quite know what to look for.

UBM: Is the problem mainly the discrepancy between the isotopic and the geologic ages of australites, with both the potassium-argon and fission track methods dating them as being ~700 000 years old, while the stratum in which they are found is only ~10 000 years old?

BM: That's part of it. I know where to go out into the field and find australites in a horizon which I believe, and all the Australian geologists I've discussed this with agree, is ~10 000 years old. I've done it time and again. I realize that Gene Shoemaker studied the occurrence of australites at Port Campbell about 3 years ago and concluded that they were derived from a horizon of middle Pleistocene age. That could match well enough with their 700 000 age, so he thought he had resolved the problem.

UBM: Most other people did too.

BM: But I still don't agree. This is the one subject on which I differ strongly with Ross Taylor. And there are other types of problems. If the isotopic vs. geologic age were not enough of an enigma, there are australites with different chemical compositions which occur in streaks across Australia and Tasmanina. These were described in 1969 by Dean Chapman and L. C. Scheiber, of the Ames Research Center in California. I did some additional work on them and reported on it at the Meteoritical Society Meeting in Perth in 1990. There is a streak containing all of the high alumina tektites that runs down through New South Wales to eastern Tasmania. A streak of high calcium ones goes from the middle of Australia down through South Australia to western Tasmania. Then there are high magnesium ones that extend from central Australia to the west coast. And most of those in Western Australia are what Chapman called "normal" australites. So the compositions are geographically restricted within a very large strewnfield of tektites with K/Ar ages of ~700 000 years. Adding to the confusion is the small group Chapman described of six to eight high potassium tektites with argon ages of 14 Ma. These were found in the midst of the large strewn field but they have to belong to a separate event.

UBM: No wonder many people regard tektites as inexplicable!

BM: I concluded at Perth that the fall of the australites seems to have been a unique event (except for those few K-rich ones), but that a satisfying explanation for the geographic localization of chemical groups eludes me. There's more to be done but somebody else will have to do it.

UBM: But they aren't doing much of anything about it at present, are they?

BM: Probably not, but these problems will come up again. Perhaps in another 30 years or so, somebody will rediscover them. Right now, we still have the whole question of the relationship of tektites with microtektites.

UBM: What do you think about that?

BM: The first I ever heard about microtektites was in 1967 when I stopped in Adelaide on my way back from hunting australites. An old friend there, Reg Sprigg asked: 'What's this about microtektites?' I said I'd never heard of them. He held up a copy of the Scientific American with an article about microtektites by Billy Glass, of the University of Delaware. I
read the article and said to Reg: "The only thing they have is common with tektites is the same name. I think it is prejudging to call them microtektites."

UBM: Since then, Billy has depicted the North American strewn field as extending all the way to the Philippines.

BM: But there are compositional differences. The funny thing was that when I gave my geochemistry course at Columbia, Bill Glass was in the last class I taught. I had each student write an essay on some subject and I gave Bill the topic of tektites. Bill said later that if he hadn't had to think about tektites in that class he probably never would have recognized these glassy objects in deep sea cores. But tektites have a much more restricted composition than microtektites. Tektites are silica rich—almost by definition. All of them have values between 65 and 76 wt% SiO₂. But microtektites range down to 40 or 45 wt% SiO₂. And there are distinct differences in their Fe to Mg ratios. I have pointed out these things but Bill has been unhappy with me because, having introduced him to tektites, I am slightly skeptical about the tektite—microtektite relationship.

UBM: I think I see why you say there are pieces missing to the puzzle. Several types of reproducible data on tektites may all be good and solid, but they don't fit together.

BM: My point, exactly. Well, I think we have just about gone through the highlights of my career.

UBM: Leaving me with the impression that you enjoy field work above all else.

BM: I surely do. The happiest days of my life were spent chasing around the outback of Australia. It is so beautiful and such a nice country. In the winter time it is not going to rain. It is going to be nice and warm in the day and cool at night, and the Aborigines are friendly, and they speak English. It is an ideal place to do fieldwork.

UBM: Brian, you retired in 1984; I remember that because I was in Washington at the time so, of course, I came to the big party for you in the gem and mineral gallery of the National Museum of Natural History. But since then you have kept your office at the Smithsonian and worked pretty steadily in Washington with periods in New Zealand.

BM: Yes, but now I work mostly "banker's hours": in late and out early.

UBM: Before we finish, I would like to observe what a key role Victor M. Goldschmidt played in both the earlier and the later periods of your career. With his paper of 1937, he inspired you to study meteorites, and now he is the subject of one of your latest books, a biography of him published in 1992.

BM: Yes, that was a fascinating project, going through his letters and other papers in the archives in Norway. We could wish that the book had come out earlier when more of his contemporaries were still alive. However, I can assure you that Goldschmidt inspired numerous students besides myself.

UBM: More recently, in 2001, you co-authored a short autobiography, From Mountains to Meteorites, published in 2001 by the Geological Society of New Zealand. It tells much the same story as this interview in somewhat more detail, and is full of amusing snapshots.

BM: It should tell the same story. It is taken from a tape recording and conversations with a young New Zealand colleague, Simon Nathan. I try not to change my tales too often.

UBM: Thank you very much, Brian. I learn new and interesting things every time I talk with you.

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