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Report

Oral Histories in Meteoritics and Planetary Science—XIX: Klaus Keil

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Abstract–Klaus Keil (Fig. 1) grew up in Jena and became interested in meteorites as a student of Fritz Heide. His research for his Dr. rer. nat. became known to Hans Suess who—with some difficulty—arranged for him to move to La Jolla, via Mainz, 6 months before the borders of East Germany were closed. In La Jolla, Klaus became familiar with the electron microprobe, which has remained a central tool in his research and, with Kurt Fredriksson, he confirmed the existence of Urey and Craig's chemical H and L chondrite groups, and added a third group, the LL chondrites. Klaus then moved to NASA Ames where he established a microprobe laboratory, published his definitive paper on enstatite chondrites, and led in the development of the Si(Li) detector and the EDS method of analysis. After 5 years at Ames, Klaus became director of the Institute of Meteoritics at the University of New Mexico where he built up one of the leading meteorite research groups while working on a wide variety of projects, including chondrite groups, chondrules, differentiated meteorites, lunar samples, and Hawai'ian basalts. The basalt studies led to a love of Hawai'i and a move to the University of Hawai'i in 1990, where he has continued a wide variety of meteorite projects, notably the role of volcanism on asteroids. Klaus Keil has received honorary doctorates from Friedrich-Schiller University, Jena, and the University of New Mexico, Albuquerque. He was President of the Meteoritical Society in 1969–1970 and was awarded the Leonard Medal in 1988.

DS: Well, Klaus, thank you very much for doing this. As you know, the Meteoritical Society has a long-standing interest in recording the oral histories of some of its leading members. I want to begin this oral history by asking the question Ursula always asked, which is, how did you become interested in meteorites?

KK: My interest started in 1953 when I became a student of Professor Fritz Heide at Friedrich-Schiller University in Jena, Germany. Fritz had worked on meteorites for many decades, off and on and, most importantly, he taught a semester-long course in meteoritics. I took that course and I was hooked. I decided there and then that I would do a dissertation on meteorites and become a meteoriticist. This was a somewhat risky career decision, because in those years, meteorites were viewed by most geoscientists as little more than scientific curiosities: no one really knew where they came from, no one really knew what one could learn from their study, and so the prospects of getting a job in this field, at least in East Germany, were not very high. Nevertheless, I decided to become a meteoriticist. Fritz assigned me a dissertation topic. I was very fortunate that the results of my work became known at the University of California, La Jolla, and particularly to Professor Hans Suess.

CHILDHOOD, JENA, AND MAINZ

DS: You were a graduate student with Heide or an undergraduate?

KK: There was no distinction between undergraduate and graduate students in Germany, at least not then. In

Fig. 1. Klaus Keil at the University of Hawai'i at Manoa, 2007.

1953, at the age of 18, I went to the university to study physics, chemistry, geology, and other supporting classes, but to major in mineralogy and petrology. I started my dissertation in early 1958, so I had 4 or 5 years of what you would call undergraduate training. I didn't do any non-science subjects, except the required Russian language and political classes, because I had done everything else in high school. I first was required to get a master's degree, called the diploma, and my advisor insisted that it was not on meteorites in order that I had a broader experience. So he assigned me a project in a potash mine (Solveyhall-Friedenshall). That was a lot of work. It took 2 years, so I did not get that finished until early 1958. I spent about 1 year underground collecting samples, mapping, and in the laboratory preparing thin sections. I did a lot of optical microscopy and chemistry, analyzing for bromine, boron, and trace elements. It was a useful but also a terrible experience in many ways. The salt mines in those days in East Germany were unbelievably primitive. We were bathing in river water, there was no cleanliness, and the salts were affecting me terribly. I had all sorts of health problems. I was keen to get back to meteorites.

DS: But you did some meteorite research in East Germany?

KK: Yes, I started in 1958 and the work was my doctoral dissertation.

DS: You chose to work on meteorites when it would have been so easy to go into ore microscopy, mining geology, or economic geology.

KK: Yes, it would have been, and that is what most students did. It was a risky proposition to go into meteorite research in those days.

DS: You published your thesis research in Germany?

KK: Later, once I had gotten to the United States, I published in *Chemie der Erde* and in the *Journal of Geophysical Research*. While I was in Jena, the results of my dissertation work had become known to Hans Suess through conversations he had with West German colleagues, who knew about my work. You may recall that Professors Harold Urey and Harmon Craig had by then also moved to La Jolla. They had discovered, on the basis of total iron content of what they called "superior" literature bulk chemical analyses, that the ordinary chondrites could be divided into the H (high-iron) and L (low-iron) groups. I was able to confirm the existence of the H and L groups on the basis of the content of metallic Fe, Ni and of the meteorite densities. I also published in 1960 an 80-page paper called "Fortschritte in der Meteoritenkunde" ("Progress in Meteoritics") in a rather prestigious West German journal called *Fortschritte der Mineralogie* (*Progress in Mineralogy*). That paper, and the results of my dissertation, prompted Hans Suess to try to get me to La Jolla.

DS: How did that process go?

KK: Well, it is a fascinating story. Hans and his wife Ruth came one day in 1960 to Jena under the pretense of wanting to visit Fritz Heide, but they really wanted to see me. That was not an easy proposition, because the authorities tried to keep people like me from having contact with Westerners, let alone with Americans. But Hans was very clever. He pretended that he could not find his hotel without my help. He asked that I get into their car and guide them to the hotel, a proposition the authorities could hardly refuse. In the 20-minute ride, with a few detours, Hans made me an offer to come to La Jolla. We worked out all the details, all on a handshake. However, I pointed out to him that I had not finished all my research. The experimental work was done, but I had to write my dissertation, submit it, and defend it. That process should not take more than 6 months or so, so I suggested that I do this in Jena and then come to La Jolla. But Hans, rather mysteriously, said he would let me know. He said he would send me a picture postcard from La Jolla that would include a code word. That code word was "meteorite." If the code word was on the postcard I should take off and go to Mainz right away. A few weeks later, the postcard arrived, and the code word was in it, so I left Jena for the West. Six months later, the Wall went up. Had I tried to complete my dissertation in Jena I would have never made it out of East Germany. I think you can now understand why Hans and Ruth Suess are the great heroes in my life. When I arrived in Mainz, Professor Heinrich Wänke had a job waiting for me at the Max-Planck Institute for Chemistry. He made arrangements with the mineralogy professor, Ernst Beier, at Gutenberg University in Mainz, that I could submit and defend my dissertation, with Fritz Heide's approval, after a 6-month stay in Mainz.

DS: These were very special circumstances.

KK: Yes, these were very trying times. When I left Jena, only my mother knew what I was doing. I thought at the time that I would never see her again. But like mothers are, she wanted me to have the best opportunities in life and La Jolla was an unbelievable opportunity for me. And I had a wonderful time in Mainz. Actually, Heinrich was in La Jolla during most of this time, on sabbatical, so I collaborated with one of his colleagues, Dr. Hans König, and we did some interesting work on what then were called primordial noble gases in the so-called gas-rich chondrites.

DS: Before we go too much in that direction, you mentioned your mother. Tell us a little bit about her and your childhood.

KK: Well, my mother was extremely important in my life: She was my first mentor and supported all the scientific interests I had in high school. She made it possible for me to have private lessons in fields that interested me, particularly mathematics, where I was not all that good, and Latin, which I felt I needed as a future scientist. She was a master hat maker by profession and had a store that was doing well, but then there came a time when women were not wearing hats anymore and she had to struggle to make a living in those difficult post-World War II years. I had always been very interested in chemistry and I wanted to become a chemist, but then I met Professor Fritz Heide, through his daughter, and he took me to his institute and persuaded me to become a mineralogist/petrologist. I think he was impressed by the chemistry I knew; I had been studying university level chemistry books during my high school years.

DS: There was a time when mineralogy was a branch of chemistry.

KK: Yes, absolutely (or the other way around!) and as a person knowledgeable in chemistry you have an advantage as a mineralogist. So, my mother provided the funding, little bits here and there, and even let me have a little chemistry laboratory in the apartment. I once started a curtain fire and she thought I would burn the place down—all those things that kids do—and so I got into mineralogy this way.

DS: No brothers or sisters?

KK: No, I had no one besides my mother. Fortunately, the government in East Germany had a policy that older people could leave East Germany legally once they had reached retirement age, so they did not have to support them anymore. So they were allowed to go to West Germany where that government would pay for their retirement. My mother was born in 1910 and she could retire when she was 60, so she applied and was given permission to move to West Germany in 1970. She kept her apartment in West Germany, but spent a lot of time in New Mexico when I was at UNM and on Kauai once I had come to the UH-Manoa. When she died in 1999, she was buried here on the island of Kauai. She had a very good life after leaving Jena and that was very gratifying to me.

DS: Have you ever returned to Jena?

KK: I indeed returned to Jena as soon it was possible after the reunification of Germany and, as you can perhaps imagine, it was very emotional. My first return to Jena was after the German reunification in 1993. I was invited to give a plenary lecture on the occasion of the 100th birthday celebration of my first teacher, Professor Fritz Heide. The auditorium was full of my former fellow students that I had not seen in over 30 years. I also gave a public talk in the famous Planetarium of Jena on "The US Space Program and the Geological Exploration of the Solar System." The place was sold out, with many old friends from elementary and high school present. The highlight for me was a wonderful brief but very emotional speech that, after my talk, was given by my now well over 80-year-old former soccer coach, who coached me from about 1947–1949; it brought tears to my eyes. The most astonishing thing to me about this first visit was that nothing had changed in over 30 years: the houses and streets looked just like then, and my old friends all lived in the same places; there was very little opportunity in East Germany for people to move, and those who had a nice apartment would hang on to it. And I returned in 2002 to receive an honorary doctorate from my alma mater, Friedrich-Schiller University. It was very festive, with all university dignitaries in academic costumes, and some 300 people filling the auditorium maximum, including many old friends. As associate editor of *Chemie der Erde-Geochemistry*, which is based in Jena, I have returned since then almost every year for the meeting of the editors.

DS: So you were collaborating with König in Mainz. Tell me a little more about that research.

KK: Yes, he was measuring noble gases in meteorites with light–dark structures such as Tabor and Pantar, and so were others in other parts of the world, but that was a frustrating business because every sample he and others measured, even samples from the same meteorite, gave different answers. One sample would give a high level of noble gases, one would give a very low value, and another would give values in between. So, I told Hans that he was doing this all wrong. I argued that the gases were concentrated in the dark portions, and none in the light portions. He looked at me disbelievingly, but we proceeded to separate the dark from the light material and he measured these fractions separately. I was right, because I had this hypothesis that was half right and half wrong. I thought the meteorites started out completely light, and that was the correct part, but I also thought that when the meteorites accreted, carbon, sulfur, the noble gases, and other volatiles accreted into the interior of the body and were driven out during reheating and metamorphism, converting part of the light into the dark, veined material; a similar suggestion had earlier been made by Ed Anders. That was the part that was wrong: I had never heard of solar wind at the time, and it was up to Suess, Wänke, and Wlotzka who later proposed that these meteorites were regolith breccias and that the gases were incorporated by the solar wind. But, at least I had the satisfaction that we published these results in the *Zeitschrift für Naturforschung* in 1961 and 1962, and that from that time on, noble gases in chondrites and achondrites with light-dark structures were always measured in separated light and dark materials.

DS: That was all done in the 6 months you were in Mainz writing up your thesis? A very productive period.

LA JOLLA AND NASA AMES

KK: Yes. Then I came to La Jolla in 1961. I flew from the U.S. military airport in Frankfurt with MATS (Military Air Transport Service) to Philadelphia and then I took a commercial flight to Los Angeles-San Diego. I arrived at the U.S. airport in Frankfurt and waited to be called to board, but my English was so poor—a few weeks of a Berlitz course—that I missed my call and nearly did not make it onto the plane. I sat next to a man who was flying to Austin, Texas, to watch a college football game. I could hardly believe it. Here was someone flying across the Atlantic and half-way across the United States to watch a football game! I thought, "What a country!"

Once I got to La Jolla, I was introduced to Professor Gustav Arrhenius and his coworker, Dr. Kurt Fredriksson. Gustav had just purchased the first commercially available ARL electron probe microanalyzer (EPMA) for a geoscience laboratory, and Kurt had been working on meteorites, and they welcomed me to join their group. Those 2 years in La Jolla were wonderful: every time I put a new polished mount of a meteorite into the EPMA, I made a new discovery. Those of you who work with EPMA today may be amused to learn that all our work was done on polished, thick mounts: Polished thin sections had not been invented in 1961, and it took a few more years before enlightened and inventive technicians like Grover Moreland at the U.S. National Museum invented these. This was a critical step forward for petrologists, because we could now study our samples in transmitted and reflected light, while analyzing the constituents, and this opened up a new world for us. You might also remember that when Raimond Castaing had invented the EPMA, he suggested that pure elements should be used as standards, and that differential matrix effects between sample and standard should be corrected theoretically. This was barely possible for simple binary alloys, but it was impossible for complex minerals containing many elements, many of which with low atomic numbers and, hence, long wavelength-low energy characteristic X-rays, where differential matrix effects were most pronounced. In those days, lack of knowledge of mass absorption coefficients, fluorescence yields and atomic number effects made use of pure elements for the analysis of minerals impossible. So Kurt and I had to first develop empirical correction procedures using calibration curves and certifying natural and synthetic phases of well-known composition as standards. As a result, our analytical data from 1961/1962 were good data that are still being quoted today, and for many years to come, these and similar procedures were used successfully for mineral analyses in laboratories around the world.

DS: When I learned to do electron microprobe analyses in the early seventies we were still using empirical curves to correct our data. I worked on metal grains from chondrites and used a suite of iron-nickel alloys for my corrections.

KK: That was the only way to go in those days. Kurt and I made some important discoveries during these years: we confirmed the Urey-Craig H and L groups of ordinary chondrites on the basis of the FeO contents of coexisting olivines and orthopyroxenes. We also discovered a third group, which we called the LL (low iron-low metal) group, as well as what today are referred to as the unequilibrated ordinary chondrites. We also worked extensively on chondrites and achondrites with light–dark structures, such as Pantar and Kapoeta, and Kurt actually introduced me to the thought that these structures were the result of impact brecciation and gardening. That ignited my life-long interest in the effects of impacts on meteorite parent bodies. And I also worked on some rare minerals in the Norton County aubrite, and that started my interest in differentiated meteorites, an area of research that has also stayed with me throughout my career.

DS: Did you work much with Hans Suess?

KK: Hans was very supportive. He paid my salary, talked to me daily, and had lots of new ideas and suggestions, but he let me do what I wanted.

DS: Urey was there, too.

KK: Harold Urey was there, so was Gordon Goles and Jim Arnold. It was an unbelievably exciting and stimulating environment. We would have lunch down on the Scripps Pier and everyone was there. But I didn't have a permanent position. My position with Hans was that of a glorified postdoctoral fellow, so I had to look for a permanent position. I was interviewed at a number of universities, but I really liked the idea of working at NASA Ames Research Center at Moffett Field, California. NASA had been founded in 1958 and Chuck Sonett and Don Gault were busy establishing the Space Science Division at Ames. I was invited there, gave talks, and became an NRC-NAS Fellow for 1 year in 1963, and then another minor miracle happened: In 1964, I was offered a NASA Civil Service position at Ames. Incredible, here I was, a young guy fresh out of East Germany, working on a U.S. Naval Air Station as a NASA Civil Service employee, and I was not even a U.S. citizen at the time! How the world has changed: I do not think that would be possible today. I had the opportunity to set up a first-class, state-of-the-art EPMA laboratory for the study of meteorites and in preparation for the return of lunar samples. I was also allowed to hire NRC-NAS fellows and I had three working with me, Dick Schmidt, Ken Snetsinger, and Ted Bunch. Ted is probably the best known of the group, because he stayed at NASA Ames and continued to work on meteorites after I had left. We did some extensive work on ilmenite and chromite in ordinary chondrites whose compositions also reflected the H, L, and LL group classifications. We always kept in mind to not just study one meteorite but to study whole groups. That is where I did my early work on all the enstatite chondrites known at the time, published in my 1968 paper which is still quoted today. And I also began to think about the origin of aubrites, which many assumed resulted from melting and differentiation of known enstatite chondrites on their parent body(ies). However, I concluded, on the basis of the Ti contents in troilite and the different abundances of the mineral in the two rock types, that aubrites could not have formed from known enstatite chondrites. I also discovered and published in *Nature* that a relationship existed between the classification of ordinary chondrites into H and L groups and their U-He and K-Ar ages: L group chondrites had on average younger gas retention ages than H group chondrites. And my co-workers and I also discovered the first of several new extraterrestrial minerals, sinoite, Si_2N_2O, with Chris Anderson and Brian Mason, named after the composition Si-N-O, and the second, niningerite, (Mg, Fe)S, with Ken Snetsinger, named after Harvey Nininger, the early pioneer of meteoritics. And I also was the first to discover, but did not understand the true significance of, what today are known as the CAIs. I found them in the Leoville CV3 chondrite, then called a Type III carbonaceous chondrite. Together with Glenn Huss and Birger Wiik, I presented a talk at the International Symposium on Meteorite Research held August 7–13, 1968 in Vienna at the International Atomic Energy Commission. I showed pictures of these large, up to cm-sized inclusions (Fig. 2) and I published EPMA analyses of their constituent minerals (spinel, anorthite, perovskite, and gehlenite). I was preparing a research paper on this topic when the Allende meteorite fell in 1969. This preempted our work and we did not publish our results until 1985.

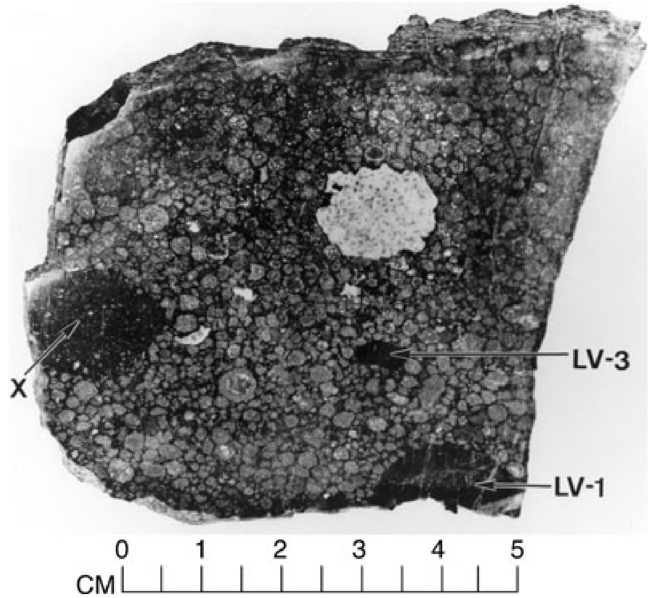

Fig. 2. Polished slab of the Leoville CV3 chondrite shown in the talk at the 1968 Vienna meeting. It contains a large, 17 mm in diameter CAI, and several smaller ones to its left. The dark inclusions marked LV-1, LV-3, and X are xenoliths related to CM chondrites.

DS: Do you think that if we dug back into Tschermak's work we might find that he saw refractory inclusions?

KK: I do not know about Tschermak, but I am quite sure no one had described the CAI "inclusions" before us, but there are reports in the old literature of finding individual mineral grains that we know today are typical constituents of CAIs. For example, the Hungarian scientist J. Török, in the original description in 1858 of the Kaba CV3, mentioned "white spots" which the Hungarian mineralogist K. I. Sztrokay in 1960 recognized as consisting of Mg-Al-spinel. And Brian Mason in 1962 mentioned grains of spinel occurring in Vigarano, and Madam Christophe Michel-Levy in 1968 (at the same meeting where I spoke about Leoville) noted the occurrence of grains of melilite and spinel in the CV chondrite Lance. Also, none of these authors recognized the true significance of these minerals. It was up to

Ursula Marvin, John Wood, and John Dickey in 1970, after the fall in 1969 of Allende, to make the revolutionary suggestion that these inclusions may represent early, high-temperature condensates from the solar nebula. In fact, it is amazing to me that generations of scientists must have seen CAIs in meteorites, long before Leoville and Allende came along, but they had been ignored. As a case in point, on my way to Vienna in 1968, I stopped at the British Museum and was given permission to go into the collections: I wanted to see if there were other CVs that had inclusions like the ones I had worked on in Leoville. It was absolutely amazing: As I opened the draws of the CVs, there were all these beautiful meteorites with cut and broken surfaces, abound with light-colored inclusions, the CAIs. Many generations of curators must have looked at these meteorites, but simply paid no attention to these objects!

DS: Was it at Ames that you were involved in developing the first solid-state detector for use in the EPMA?

KK: Yes, that's also an interesting story. The reason the EPMA worked so perfectly at La Jolla, with very little down time, was that Gustav had hired Ray Fitzgerald. Ray was the senior engineer at ARL who had developed the EPMA, and after I had moved to NASA Ames, I hired him as a consultant to maintain our EPMA. On one of his visits in 1965, Ray attended a talk at Stanford University where they described the use of a solid-state ionization chamber in conjunction with a multichannel analyzer. That inspired Ray to think about developing and using a Si(Li) drifted solid-state detector capable of measuring the relatively low energy X-rays (when compared to γ-rays) in the EPMA. He worked with engineers from ORTEC to develop the detector and spectrometer with an initial resolution of 600 eV (Fig. 3a); I supplied the 1024 channel multichannel pulse height analyzer and assisted in the measurements and, with Kurt Heinrich, we published the description of the device and the results of our measurements in *Science* in February 1968. This resolution allowed us to detect characteristic X-rays of 3–30 keV and to discriminate between elements adjacent in the Periodic Table with atomic numbers $Z > 20$ (Fig. 3b). This devise revolutionized the detection of X-rays not only in EPMAs, but also in SEMs, TEMs and XRDs, by allowing semi-quantitative analysis of many elements simultaneously and at very high speed.

DS: So, in 1968, you moved to New Mexico.

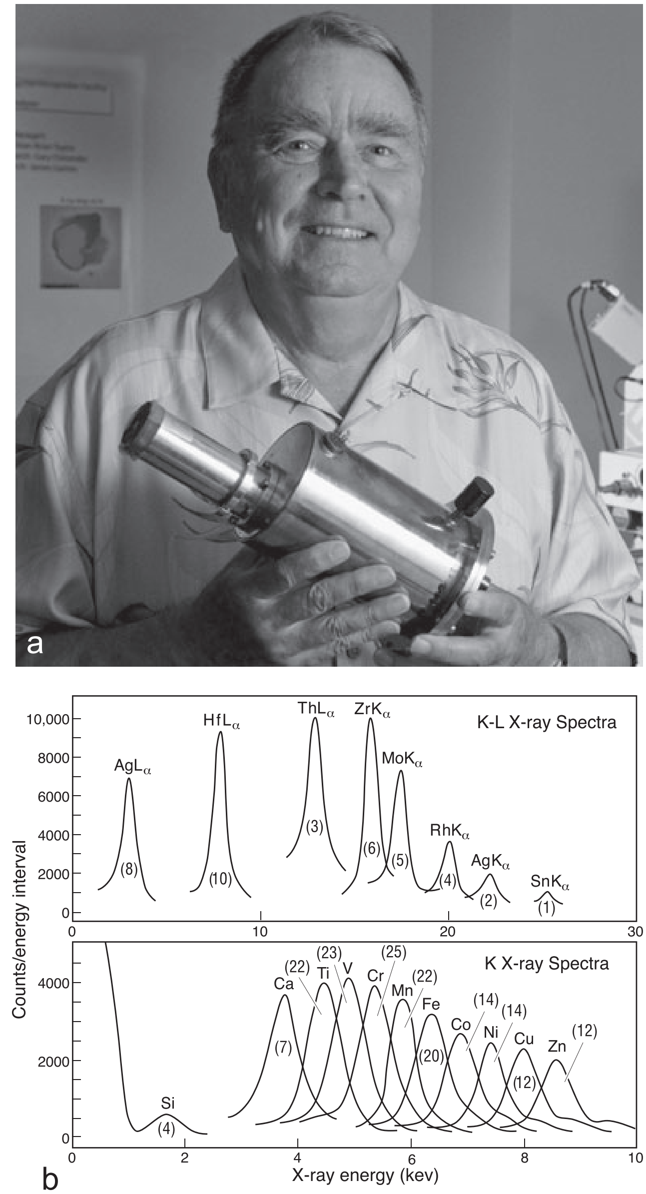

Fig. 3. a) The original first Si(Li) EDS detector and spectrometer that we developed for use in the EPMA (picture taken December 2008 and the detector is preserved at HIGP). b) Composite of pure element K and L X-ray spectra obtained in an ARL EPMA with the solid-state Si(Li) energy spectrometer with a resolution of 600 eV of Fitzgerald et al. (1968). Peak to background ratios are given in parentheses, and integration time was 100 s. The characteristic X-ray lines of elements adjacent in the Periodic Chart are clearly separated.

UNIVERSITY OF NEW MEXICO AND THE INSTITUTE OF METEORITICS

KK: Yes, I did. My work at NASA Ames was going well, I liked living in the San Francisco Bay area, and I really liked the working environment at Ames. So I was very happy there, and I could have stayed there and been a civil servant all my life, but I missed university life. I missed working with students. I missed teaching. And I missed intellectual intercourse with colleagues from other fields. So, when the offer from the University of New Mexico came along to be the director of the Institute of Meteoritics and professor of geology, I took it. That was another risky career decision I made. The Institute of Meteoritics had been a dormant organization for a number of years. Even in its heyday under Lincoln LaPaz, it was just a one-man operation, mostly devoted to collecting and describing meteorites, and there was no research infrastructure.

DS: LaPaz was publishing *Meteoritics*.

KK: Yes, if you look at an old copy of *Meteoritics* you will see on the cover the building they occupied before I arrived. It was a little adobe building and that was where the collection was. LaPaz was still in Albuquerque when I arrived, but he had retired from teaching in 1962, but remained as director until 1966.

DS: Was there a director between him and you?

KK: There was an interim director, a mathematician, Dr. James Wray [1966–1967]. During extensive discussions since 1963, the university finally decided to attach the institute to the Department of Geology and to provide matching funds for the purchase of an EPMA and the hiring of an operator, a research assistant, and a director. With the assistance of excellent collaborators such as Marty Prinz, Jeff Taylor, Ed Scott, Hort Newsom, Adrian Brearley, Rhian Jones, and many outstanding post-docs and students, and with generous support from NASA, we built up a wonderful organization, one of the best centers for meteorite research in the United States. So within a year of the return of lunar samples, I moved to UNM.

DS: Was the Institute of Meteoritics still in the adobe building when you were hired?

KK: No, as I arrived the institute was moved to the Geology Department building, including the entire meteorite collection with the one ton and rather friable and fractured Norton County meteorite. This was no easy task. I made friends with the person in charge of lighting in Popejoy Hall, our concert hall at UNM, and he helped me design a wonderful meteorite museum, with a dark room, spot lighting, and so on. It was and is very important for educating students and the public.

DS: You were hired at the full professor level?

KK: Yes, I was probably one of the youngest full professors they had ever hired. I was 33 at the time.

DS: La Paz was apparently hard to get on with, but the university appreciated his contributions.

KK: Yes, they did. But that was all before my time. I have always made an effort to work in peace with my colleagues and be as constructive as possible. That's very important. The Institute of Meteoritics was a part of the Department of Geology and I always made the point that I was first and foremost a professor of geology and director of the institute second. It worked out well.

DS: Now it's 1969. The year of Apollo.

KK: I wrote proposals to work on lunar samples and became a PI and then the lunar samples came along. It was an experience I will never forget. They did not mail the samples to investigators. We had to go to Houston to pick them up. Some colleagues actually flew back with their briefcases containing the samples handcuffed to their wrists. I did not go that far, but I was met at the Albuquerque Airport by campus police with their lights flashing. We had this little vial with lunar dust and we had a piece of basalt. Those were exciting days. One mission was followed by the next, and we had hardly begun work on the Apollo 11 samples when the samples of Apollo 12 came along; these were hectic but also very productive years.

DS: What was it like? You had the samples in the safe. You came in the next day. You were working with Marty?

KK: Yes, I was, and there was a contract with NASA safeguarding the samples, which we all had to sign, saying that we would look after the samples and treat them properly. We were very careful. We kept a log, detailing every action.

DS: So how did you start? You put them under a low-powered binocular microscope?

KK: Under a low-powered microscope we could not see a thing, because the mineral, rock, and glass fragments were covered with a submicrometer dust layer that obscured everything. We had to wash them in acetone and then a whole new world opened up.

DS: I want to know about the goose bumps.

KK: There were goose bumps. You would hold the vial up and look up at the Moon and say that is where this came from. It was unreal.

DS: You had students involved in this?

KK: Yes, and post-doctoral fellows and visitors like Gero Kurat, and we also tried to explain our work to the public: we had, for example, an exhibit of moon rocks and dust with enlarged color photomicrographs of lunar rocks and soil for the state legislators in Popejoy Hall, and then we opened this exhibit to the public. There was a line of some 6000 people going several times around the hall, people waiting to get a glimpse at the moon dust and rocks. I gave a talk explaining the things we had found, and then we had an exhibit in a trailer at the New Mexico state fair that was visited by over 18,000 people!

DS: But to take you back to the lab. What was a day like?

KK: We cleaned the samples, hand-picked fragments, made thin sections, put them under the microscope, and then in the EPMA. We analyzed hundreds of glass spheres, glass fragments, mineral fragments, and minerals in lithic fragments, and there were the anorthite fragments that John Wood focused on. He actually proposed that they were pieces of the distant lunar highlands that had been transported to the Apollo 11 landing site by an impact. That was a very imaginative suggestion, typical of John's work. Then we went to the First Lunar Science Conference, which was by invitation only. There were almost as many journalists there as there were scientists. There were all these petrologists (including myself) standing up there giving similar talks and showing similar quadrilateral pyroxene diagrams, rather boring after a while. But we, together with others, also made some interesting discoveries, such as of the new minerals, tranquillityite, $Fe_8(Zr + Y)_2Ti_3Si_3O_{24}$, named after Mare Tranquillitatis, and armalcolite, $Fe_{0.5}Mg_{0.5}Ti_2O_5$, named after the Apollo 11 astronauts Armstrong, Aldrin, and Collins. And we also worked on some rather unique lunar rock types, such as the spectacular spinel troctolite clast in an Apollo 16 microbreccia, the Very High Alumina basalts, which we interpreted as a mixture and not a magma type, the Very-Low Ti mare basalts, the pyroxene-phyric basalts, which we suggested formed from supercooled melts, and we discovered the ferroan anorthosite suite as a widespread and distinctive lunar rock type. And we coined the term ANT suite rocks, for anorthosites–norites–troctolites from the lunar highlands, and we studied rock types such as the alkali norite troctolites and VHK mare basalt clasts from lunar breccias.

DS: Were you involved in the early NASA advisory boards?

KK: Yes, I was. In fact, that was the time when many of us started working on NASA advisory boards, review panels, and committees. I would like to mention just one of the many committees that I worked on. This one has almost been forgotten but, in my view, was very important to the community: Larry Haskin, who was the chief scientist at JSC at the time, asked me to chair a committee made up of NASA engineers and scientists from the scientific community (including Jerry Wasserburg and Dimitri Papanastassiou) commissioned to supervise the design and construction of the pristine lunar sample vault at JSC. The reason, of course, was the recognition that the lunar samples were a national treasure that had to be protected from terrestrial contamination and natural disasters. It is a wonderful facility that is still there today. I cannot tell you how much work that was. All the different experts had their requirements, the geochemists, the organic geochemists, everyone, it was very difficult. The truth is that 40 yr after the lunar landings, most of the samples are still in their pristine state, an incredible achievement! They will long be available for research by new generations of scientists with new analytical tools. We are very proud of that. We were also commissioned to find a remote site in case that, against all odds, the JSC vault would be destroyed. Actually, 83% of the samples are still pristine, 70% in the pristine vault, 13% at the remote site, first at Brooks Air Force Base in San Antonio, TX, and now at White Sands Missile Range, NM.

DS: The remaining 17%?

KK: The remaining 17% are with PIs or in the returned sample facility at JSC where they can easily go out again for more research, if appropriate.

DS: You mentioned working on Apollo 11 samples, you worked on others?

KK: Yes, we worked on samples from all the Apollo sites and from the three Russian Luna sites. Jeff Taylor still continues that work today in our group at UH.

DS: You were at New Mexico quite a while?

KK: I was there 22 years.

DS: Tell me something about Linde. When and where did you meet her?

KK: We met at the UNM summer school in Taos, New Mexico, where she was a Teaching Assistant and I a guest speaker. We got married in 1984.

DS: Somewhere along the line you had children.

KK: Not with Linde. Linde is my second wife. I have two children with Rosemarie, my first wife, a son who lives in Honolulu and a daughter in Baton Rouge.

DS: In terms of meteorite history, what are your major memories from New Mexico?

KK: Simultaneously with our work on the lunar samples, and after that time, we continued our meteorite work. This work was much stimulated by the discovery of large concentrations of meteorites in Antarctica and, more recently, in hot deserts. For me, these collection efforts are for cosmochemistry and meteoritics what the Apollo program had been for lunar science.

DS: I would like to pursue a little more the meteorite research you did at New Mexico. I want to get some sense of your work, the field in general, and progress in meteoritics during your time there.

KK: We did a lot of work on the composition and origin of chondrules, combining the experimental work of Harry Planner and Milt Blander with INAA analyses of single chondrules, which we then sliced and studied microscopically and with the EPMA. Jim Gooding was the leader of this effort. We concluded that chondrules must have formed from pre-existing solids, and we also did a lot of work on the relative abundances of chondrule textural types and on compound chondrules, in an attempt to estimate chondrule number densities during their formation. And, with Addi Bischoff, I worked on Ca-Al-rich chondrules and inclusions in ordinary chondrites and we speculated on the relationship between chondrules in ordinary and carbonaceous chondrites. We also wrote papers on how chondrules did not form, for example, with Jeff Taylor and Ed Scott arguing that chondrules could not have formed by impact into solid materials.

DS: You also worked extensively on differentiated meteorites.

KK: Yes, I did. I spent a few weeks in the summers of 1972–1978 at the University of Sao Paulo, Brazil, working with Celso Gomes on the many poorly described stone meteorites of Brazil made available to us through our collaborator, Walter da Silva Curvello, the Curator of Meteorites at the Museu Nacional in Rio de Janeiro. This work resulted in many research papers on 17 ordinary chondrites and a book summarizing that work. Most importantly, the Museu Nacional had four rare achondritic meteorites, the angrite Angra dos Reis, the nakhlite Governador Valadares, and the eucrites Ibitira and Serra de Mage. These meteorites we studied in greater detail. In fact, I organized a consortium consisting of 10 international research groups to study Angra dos Reis, at the time the only angrite in captivity, and the results were published in *EPSL* in 1977. We did the mineralogy and petrology, and concluded that the rock is a cumulate. Most notably, the Wasserburg group determined its age at 4544 ± 2 Ma. And I had also organized a Consortium study of the Kenna ureilite, and we worked on other ureilites, which we interpreted as ultra-mafic cumulates. We also began studies of a number of rocks which are now considered to have originated on Mars, such as Governador Valadares, Nakhla, and Chassigny; in the latter, we discovered melt inclusions containing hydrous amphibole, I believe, a first at the time. And we had this huge Norton County aubrite in our collection. We only sampled the 1 ton stone for cosmic ray work, but there were literally thousands of smaller fragments that were a treasure trove of fascinating materials: We found clasts with typical igneous textures, some akin to granitic textures clearly of deep-seated melt origin, and that eliminated the thought that aubrites might have condensed from the solar nebula: they are clearly igneous rocks. We also discovered clasts that contained euhedral, cm-sized cubes of oldhamite, CaS. Their size, their occurrence in igneous-textured assemblages, and their REE patterns clearly indicated that they formed from a melt. That had previously been questioned, because of the high melting point of the mineral, and oldhamite in enstatite chondrites and aubrites had been interpreted as relicts from condensation from the solar nebula, a proposition clearly incorrect for Norton County. We also studied Shallowater, at the time the only non-brecciated aubrite and showed, based on its extraordinary and unusual three-stage cooling history, that it must have formed by the collision of a completely or partly molten and a solid asteroid of enstatite composition. We also concluded that Shallowater must have formed on a second aubrite asteroid, different from the one from which all the other aubrites came from. [This article was corrected on September 24th 2012 after online publication. The previous paragraph was shortened for clarity.]

DS: Any other broad topics you worked on at UNM?

KK: Yes, we continued mineralogical–petrological research begun in La Jolla and Ames on brecciated and impact-heated and/or -melted meteorites, their lithic inclusions, and the origin of these objects. And Brian Mason had discovered that the Antarctic meteorite Allan Hills A81005 appeared to be a lunar rock, and he had sent me the polished thin section that he had worked on for confirmation. Sure enough, the rock was a dead ringer for a lunar regolith breccia. While we were studying this section, we had Keizo Yanai, the Curator of the Japanese Antarctic Meteorite Collection at NIPR visit us at UNM, and we showed him the meteorite in the microscope. To our great surprise and delight, he said that he had seen similar rocks in the Japanese meteorite collection, and that prompted him, after his return to Japan, to describe lunar meteorites from their collection.

DS: I assume graduate students were involved in this work. How many graduate students have you graduated?

KK: I can immediately think of 12 Ph.D. students, some of whom first did M.S. theses, and about the same number who finished with terminal M.S. degrees, and I also had many very excellent post-docs.

DS: You said earlier that you left Ames for UNM so you could interact with students. Tell me about your views on students and their role in your research.

KK: Students were assigned to do experimental work rather than just speculate and theorize and one thing I did at UNM was that I always tried to have my students employable after they graduated. Most of my students did a master's before they did a Ph.D., and most of them did two separate projects, one terrestrial and one extraterrestrial. This was why I got involved in work in Hawai'i, because I wanted them to do first rate work on a terrestrial project then they could sell themselves as petrologists. You know even in the United States it is very difficult for a meteoriticist to get a job in a conservative geology department.

DS: The point being to make the students acceptable to a geology department, not that you cannot do good meteorite research without a geology background.

KK: No, no. Just to make them employable.

DS: What is your idea of the relative role of master's and Ph.D.s?

KK: Well it was very good for me to have done a master's very different from my Ph.D. Now, here at Hawai'i, it is possible to go straight into a Ph.D. from your B.S. degree but the Ph.D. simply takes much longer because we expect so much from our Ph.D. candidates. Most of my students publish while they are still in the program, for instance. Some master's programs are pro forma, but in my group, they are research-based and good experience. The master's is also a landmark for students who are not sure they want to carry on. It's an exit point with a degree. I always think that the master's is an excellent way to assess likely success in the Ph.D. I have advised some of my master's students to not carry on in academe. There is another thing I have always stressed. I have always wanted my students to be good analysts and to learn as many analytical techniques as possible. That is another way of ensuring employability. I have many students who, for example, are now running electron microprobe labs.

DS: Do you want to say any more about your New Mexico years? Shall we talk about the 1976 Viking missions?

KK: Yes. I was the fifth PI to be appointed to the Inorganic Chemical or X-ray Fluorescence team. It was a very exciting time. The instrument worked beautifully. But you must remember that the instrument was added late, in August, 1972, 2 years later than the other instruments of the lander scientific package, which left us less time for the design and construction of the XRF unit, and that we had severe weight and power constraints. And yet, the instrument had to be strong enough to survive launch, a 1-year flight, and landing on the planet. That was very difficult to achieve. We had gas-filled counters to detect the X-rays produced by radioisotope sources, and weight constraints did not allow us to have a multichannel pulse height analyzer on board. Nevertheless, we got wonderful data, and the instruments performed perfectly both at the Viking 1 and 2 landing sites. The data on the compositions of the soil that we got are not too different from the data obtained more recently, and our interpretation that the Martian soil consists of a mixture of basaltic weathering products and salts is pretty much still accepted today.

DS: Who built the instrument?

KK: Martin Marietta. The principal scientists in the design of the instrument were Ben Clark and Alex Baird. I also had my graduate students, Terry Steinborn, Jim Gooding, and Gary Huss working on the project. This was the only time I had been on a flight team, and this was quite an experience, very intense and hard work. I spent nearly a year at JPL, which ran the mission. The interesting thing was NASA wanted to land the Viking 1 Lander on July 4, 1976, and we would have met this goal, but the landing site—which was chosen on the basis of Mariner 9 images which had insufficient resolution—was not smooth enough. So they had to look around and find a smooth area, safe for landing, which they did, but we landed not until July 20, 1976. But that was fine with me, because that was a very appropriate date, the 7th anniversary of the landing of Apollo 11 on the Moon!

DS: Did you get invited to be on the team or did you have to compete?

KK: I was invited. As I recall, I had to write a brief proposal and work plan, but NASA and the project felt that they needed someone with extraterrestrial rock experience. They had no one on the team with meteorite or lunar sample experience so I was invited.

DS: One problem that has concerned me from time to time is that because the first samples are remote sensing-driven, so over the last 40 years we have developed quite sophisticated capabilities at remote sensing, and there are now several generations of people who have dedicated their careers to remote sensing, so there is a strong tendency to keep doing remote sensing with every increasing sophistication.

KK: Yes, remote sensing has become so sophisticated that we sometimes seem to forget that laboratory equipment is vastly more sophisticated and flexible. Also, returned samples can be stored and curated for future generations of scientists. In my view, there needs to be a delicate balance between remote sensing and sample return missions. Remote sensing must come first, and it should be followed by returned samples. Of course, there have been several very successful sample return missions such as Apollo, Stardust, Genesis, Hayabusa, and now OSIRIS REx is in the line-up. Let's hope it flies.

DS: Is Viking the last mission you were involved with?

KK: Yes, the one and only.

DS: Was that deliberate, or did it just turn out that way?

KK: I never proposed and I did not want to propose for a mission. It's one thing to be a participating scientist, but to be an instrument PI, that is very tough. I would not want to do that. You can spend many years developing an instrument that may never fly.

DS: What about the terrestrial research you did in Hawai'i while at New Mexico?

KK: I went to a conference at the East-West Center at the University of Hawai'i in Honolulu in 1968 organized jointly by Japan and the United States and dedicated to the development of quantitative analysis with the EPMA, and I fell in love with Hawai'i. I had left Ames but I still had my colleague Ted Bunch there who I could approach to support work in Hawai'i. After all, I had worked for years on lunar and meteoritic basalts, but I had never ever studied terrestrial basalts, where you have the great advantage over extraterrestrial samples that you could study their occurrences in the field. I wanted to study the mineral compositions of the classical basaltic suites of rocks of Hawai'i. While bulk rock chemistry had been carried out for many of the basalt suites, there were very few mineral chemistry data, since the UH had no EPMA, and our work thus filled a large gap in the knowledge of Hawai'ian basalts. I worked with Ted Bunch, Ron Fodor, and Glenn Bauer, and also had a number of students working there. Talking about Hawai'i, we also worked extensively on palagonite and basaltic glass and its weathering products from Hawai'i and elsewhere as possible analogs of a major constituent of the Martian soil.

DS: What were the main science issues you were addressing in this work?

KK: From bulk chemical analyses and excellent field work by eminent geologists like Harold Stearns and Gordon Macdonald, it was known that Hawai'ian volcanoes started with voluminous tholeitic basalts, followed by the alkalic and, finally, the nephelinic suite of rocks. I wanted to explore how these bulk chemical changes were reflected in the mineral chemistries of the rocks, and we started with a big project on a suite of basalts of known bulk compositions from Haleakala Volcano, Maui.

HAWAI'I AND THE UNIVERSITY OF HAWAI'I

DS: How did you decide to join the faculty of the University of Hawai'i?

KK: My wife Linde had lived in the tropics, in Panama, for a number of years, and when she visited Hawai'i with me for the first time, she very categorically told me that she wanted to live here! So, in 1984, I approached Tom McCord, who was the head of the Planetary Geosciences Division (PGD), his colleague Pete Mouginis-Mark, and Chuck Helsley, the director of the Hawai'i Institute of Geophysics (HIG) at the University of Hawai'i at Manoa, and told them that I was movable. Tom had a lot of other interests, and so in 1989, Chuck offered me the position of head of PGD and professor of geology, and we moved there in the summer of 1990. In 1994, with strong support from Dean Raleigh, I merged PGD with HIG to form the Hawai'i Institute of Geophysics and Planetology (HIGP) in the School of Ocean and Earth Science and Technology (SOEST), with me as director. When the SOEST dean resigned in 2003, I became interim dean, a position I held until we found a new dean in 2006. When Chuck offered me the position, he also gave me two additional professorships, as I had requested, so that we would immediately have a strong cosmochemistry/meteoritics program. Fortunately, Jeff Taylor and Ed Scott decided to take these positions, also in 1990, and in 1994, Sasha Krot joined our group. Sasha is an incredibly productive researcher, and his presence added much to our research group. Finally, I had always been interested in the ion microprobe and in its enormous potential for cosmochemical research. In fact, in 1967 or 1968, I visited Raimond Castaing and his graduate student, Georges Slodzian, who had invented the instrument, in their laboratory at Orsay, France, but the time was not ripe for us to get into this field. However, it was in 2002, when Peter Englert became chancellor of the UH-Manoa. I approached him and the Keck Foundation, and we obtained a good part of the funding for the acquisition of the instrument. Most importantly, Peter also made available two professorships for individuals to run the instrument and the laboratory, and we were very fortunate to attract Gary Huss and Kazu Nagashima to join us. Gary took over the planning for the laboratory and which instrument to purchase, and he wrote a proposal to NASA for the rest of the funding. The arrival of the CAMECA ims 1280 has opened incredibly exciting new research opportunities for our group and for the visiting collaborators that come to use the instrument.

O.K., so much for my administrative activities at the University of Hawai'i, after which I happily returned to my current faculty position!

DS: You said at one time that the Antarctic meteorites were analogous to the Apollo samples. Why don't you elaborate on that?

KK: Yes, these and the many meteorites found in hot deserts really stimulated our work at Hawai'i, as they have the work of the entire cosmochemistry community. In one way, it's their sheer abundance that makes them so important: The more you collect, the greater are your chances of finding some new and important types. Before these programs, we had about 10 or 20 new meteorites a year to work on. The Antarctic program has now yielded some 26,000 meteorites. And the collection contains many precious lunar and martian meteorites, and rare asteroidal meteorites such as unequilibrated, enstatite and carbonaceous chondrites, impact-melt rocks, acapulcoites, many achondrites such as aubrites, angrites, brachinites, and ureilites. If we not had the Antarctic program, meteoritics/cosmochemistry would not be where it is today. The significance of the work on Antarctic and hot desert meteorites is incredible. Just take the dating of differentiated meteorites, for example, the angrites: Their ages are within a few Ma of the ages of CAIs, meaning that the parent body of the angrites accreted, was heated, melted, differentiated, and cooled all in a few Ma of time zero! That is exciting stuff!

DS: Is there ever going to be a case when we have enough Antarctic meteorites to terminate the program?

KK: No, I certainly hope not. As I said, the more meteorites we find, the greater are our chances that we will discover some new or additional rare types. In spite of all the material we have, I still feel our sampling of the asteroidal belt is very poor. This, of course, is also a dynamical problem: It is just very difficult to get meteorites from certain parts of the belt. And some samples from the outer parts of the belt that may be full of carbonaceous and organic material, may be so friable that it will take very special circumstances for these to survive entry through the Earth's atmosphere. Stan Love, who was my post-doc and who later became a shuttle astronaut, and I wrote a paper about how to recognize meteorites from Mercury. After all, we have some 79 lunar and some 63 Martian meteorites in our collections, and while it is dynamically more difficult to get impact ejecta off of Mercury to Earth, the probability is not zero. We estimated that the chances of getting to Earth a meteorite from Mercury is approximately 1/100 of getting one from Mars, i.e., once we have 100 Martian meteorites in our collections we should find one from Mercury! In fact, more recent calculations by Gladman and Coffey are even more optimistic and suggest that several percent of high-speed ejecta from Mercury reach Earth. This is only factors of 2–3 less than typical launches from Mars. Thus, we soon should discover a meteorite from Mercury, and I predict that will be in collections from Antarctica or hot deserts! Stan and I also thought about how one would actually recognize a meteorite from Mercury. Clearly, none of the meteorites in our collections today are from Mercury. For example, rocks from Mercury should contain silicates essentially free of FeO, and the strong magnetic field of Mercury should have prevented the solar wind from reaching its surface. Thus, Mercurian meteorites should not contain solar wind implanted noble gases. And regolith breccias from Mercury should contain very abundant agglutinates, perhaps exceeding in abundance those in the lunar regolith. In my mind, the prospect of finding a meteorite from Mercury is reason enough to continue the ANSMET program!

DS: The truth is that we still have very few chondrites of low petrographic type. The old favorite falls are still in high demand.

KK: Correct, and this is because they are fresh and not contaminated by terrestrial weathering. There is no question that there is certain research that can best, and only, be done with fresh falls, and even the freshest of the Antarctic meteorites might not be suitable. But for the petrologist, many are very suitable for research. I am actually amazed how uncontaminated some Antarctic meteorites are. Take, for example, the angrites: Floss, Crozaz, and co-workers explored the alteration of minerals by measuring by SIMS techniques lanthanides and selected other minor and trace elements and found no evidence for major terrestrial alteration in the Antarctic angrites LEW 86010, LEW 87051, and Asuka-881371. I think this is because angrites have not been shocked and are essentially free of cracks which could serve as pathways for terrestrial weathering. A shocked meteorite, lying in the desert or Antarctica, will be more susceptible to weathering. On the other hand, Lugmaier and Galer found that LEW 86010 is contaminated with terrestrial lead, and that its Rb-Sr system is disturbed.

DS: The Antarctic meteorite weathering process is very different from the desert weathering process. From my TL work, I know that you can easily wash away weathering products from an Antarctic meteorite, bring back the fresh grey color, but it is impossible for desert meteorites.

KK: I am learning something.

DS: Tell me your views about the processing of the Antarctic meteorites at the NASA Johnson Space Center: They have learned a lot from the handling of lunar samples.

KK: Yes, they have, and they are doing an excellent job. I cannot say enough about the importance of the fine work that is being done in the "Astromaterials Acquisition and Curation Office," as the facility is officially called. They not only preserve and curate, but also prepare and distribute Apollo and meteorite samples from the Moon, meteorites from asteroids and from Mars, dust from comets, and samples of the solar wind. In my view, this facility is absolutely essential for the well-being of the cosmochemistry and related programs; we would not be today where we are if it were not for this incredible facility and the fine work our dedicated colleagues do there. I should add that I am also immensely impressed by the high quality of the NSF-NASA-Smithsonian Institution meteorite collection program that our colleagues have been carrying out in Antarctica for 32 years: Every sample is meticulously documented, photographed in the field, cleanly packed, and shipped frozen to JSC.

DS: What about the finds of large numbers of meteorites in hot deserts. They are also important?

KK: Yes, they certainly are. These meteorites have mostly been purchased by private collectors in places like Morocco, and a real effort should be made by collectors and scientists, whenever possible, to involve local individuals and scientists not only in the discovery but also in the scientific study of these meteorites. A wonderful example of local and foreign cooperation is the recent discovery and study of the Almahata Sitta meteorite! The scientific significance of the meteorite finds from hot deserts is very high, considering the many rare types that have been recovered. And while most of these meteorites remain in private hands and are traded and sold for ever increasing prices, most of these collectors realize that their samples are nearly worthless unless they are well-characterized by competent meteoriticists, Thus, at least small amounts of these samples usually get into the hands of professional scientists, and have much contributed to progress in our science. You may say what you want, and you may think what you like, but if it were not for these private meteorite hunters, these samples would never have been discovered, and they would have been totally lost to science!

DS: Let me shift the conversation to your asteroid volcanism papers as something that resulted from your work in Hawai'i.

KK: Yes, as Lionel Wilson said in one of his recent talks: "Klaus and I have invented a new field of asteroid science, asteroid volcanology." I do not think he is exaggerating. This all started over lunch one day in 1990 in Manoa Gardens at UH-Manoa when Lionel was visiting Hawai'i. I told him that I was puzzled by the absence in meteorite collections of enstatite-plagioclase basalts complementary to the aubrites: If the aubrites formed by melting and differentiation of enstatite chondrite-like parent lithologies, which contain approximately 10% plagioclase, then the first silicate partial melts should form enstatite-plagioclase basalts. I told Lionel that the lack of these rocks had been explained in the past by eradication by impacts of the basalt flows on the surface of the aubrite parent body, or that the parent lithology had no plagioclase. These proposals seemed unlikely to me. Instead, I suggested to Lionel that, if the early partial melts contained volatiles, these may have driven pyroclastic volcanism. Considering the small size of the atmosphere-less aubrite parent body with a very low escape velocity, I suggested that the eruption velocity of the pyroclasts may have exceeded the escape velocity and, hence, these were lost into space and, thus, no basalts. Lionel is a leading theoretical volanologist with a mathematics background and had for years worked on volcanic processes on Earth, Mars, and Moon, and I asked him if he could not calculate and model if this idea had any merit. Sure enough, he found that if early basaltic partial melts had a few hundred to thousands of ppm of volatiles, then the eruption velocity of pyroclasts on asteroids less than about 100 km in radius would exceed the escape velocity (Fig. 4) and they would be lost into space by spiraling into the Sun or being captured by the accreting planets.

DS: Can you be sure that there were volatiles available to drive the pyroclastic volcanism?

KK: I believe, the answer is yes. We have measured, with David Muenow, with high-temperature mass spectrometry the degassing of ordinary and enstatite chondrites as a proxy for the parent lithologies of differentiated meteorites and found well in excess of 3000 ppm of CO and N_2.

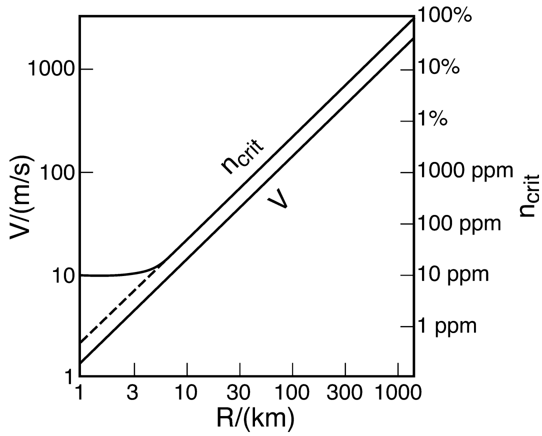

Fig. 4. Escape speed, V, for asteroids of various radii, R. Also shown as a function of R are the critical values of magma gas content, n_{crit} at which the eruption velocity, U, is equal to the escape speed, V (Wilson and Keil 1991).

DS: Are you continuing your work with Lionel?

KK: Yes, the aubrite work was the beginning of a very fruitful collaboration that is still going on today, and we have addressed many volcanological problems of heated and melted asteroidal parent bodies. We were always somewhat puzzled by the nearly 100% efficiency of loss of pyroclasts from the aubrite parent body, as only Bobby Fogel had described three millimeter-sized clasts he called "aubrite basalt vitrophyres" (ABVs), and interpreted these to be the elusive pyroclasts. Our conclusion was and is that the aubrite parent body must have been sufficiently small and the volatile contents of the pyroclastic melts sufficiently high that the small pyroclasts, which we had estimated to be approximately 40 μm to 4 mm in size, were indeed nearly quantitatively ejected off the aubrite parent body. Recently, however, we described with Tim McCoy and others in the Larkman Nunatak 04316 brecciated aubrite, a composite Fe,Ni-FeS/enstatite-diopside-forsterite-glass vitrophyre clast approximately 20 mm in diameter that had cooled at a rate of approximately 25–30 °C s^{-1}. Our modeling showed that this clast, by virtue of its relatively large size, fell back onto the aubrite parent body, which must have been between 40 and 100 km in diameter and must have contained between 500 and 4500 ppm of volatiles in the melt. We also explored the loss of FeS-Fe,Ni partial melts (the first melts to form when heating a "chondritic" lithology) as pyroclasts to explain the low sulfur contents of the melts from which the magmatic iron meteorites crystallized. We also worked extensively on the sizes of pyroclasts that may have formed on asteroids of various sizes and either have been ejected into space or retained on the parent body (Fig. 5), and we considered in detail the nature of volcanic eruptions and intrusions, as well as the fate of pyroclasts, on the largest of the asteroids with current evidence for basalts on its surface, 4 Vesta. And we worked on the internal structures, densities, and shapes of asteroids and proposed and modeled, with Lauren Browning and others, that, under certain circumstances, asteroids containing water/ice could actually be blown to bits by early aqueous alteration through the build-up of internal gas pressures. We are currently working on an invited review for *Chemie der Erde-Geochemistry*, where we summarize our work. And we also show in that paper that, against previous suggestions, because of the physics of melt formation and migration, it is impossible to form magma oceans on small asteroidal-sized bodies!

DS: What other significant work have you been doing in Hawai'i?

KK: There are a lot of different research topics that we have addressed in the 22 years that I have been at the University of Hawai'i. Here are a few examples. I continued my work on enstatite chondrites and aubrites and concluded that these meteorites originated on at least five different parent bodies: the aubrite, Shallowater, H and L enstatite chondrite parent bodies, and the parent body of Northwest Africa 2526 and Itqiy, which are partial melt residues of enstatite chondrite parentage. With Stan Love and Ed Scott, we continued our work on the origin of chondrules and showed that they did not form by lightning. We also worked on the mineralogy and petrology of lunar and Martian meteorites. And Dieter Stöffler came to visit us several times, and Ed Scott and I worked with him on the shock classification of ordinary chondrites, which is probably our most cited publication, as well as of carbonaceous chondrites, and with Alan Rubin on the shock classification of enstatite chondrites. We also continued our work on the effects of impacts and shock on meteorites and their parent bodies and concluded that some asteroids must have been catastrophically fragmented and gravitationally reassembled, so that they are rubble piles. We also concluded that impacts are not possibly the heat sources for the melting of the small parent bodies of the differentiated meteorites, for many reasons, one of which is that the total impact melt volume formed during the typical life time of an asteroid is a small fraction (<0.001) of the volume of impact-generated debris. There is also the wonderful work of Tim McCoy on acapulcoites–lodranites, which are the residues of partial melting of chondritic parent lithologies. And, with Akira Yamaguchi, we also did a great deal of work on the metamorphic history of the eucritic crust of 4 Vesta, and with Anders Meibom on primitive metal grains in CH carbonaceous chondrites that formed by condensation from a gas of solar composition. Finally, there are numerous papers by Sasha Krot on many topics, such as on the astrophysical

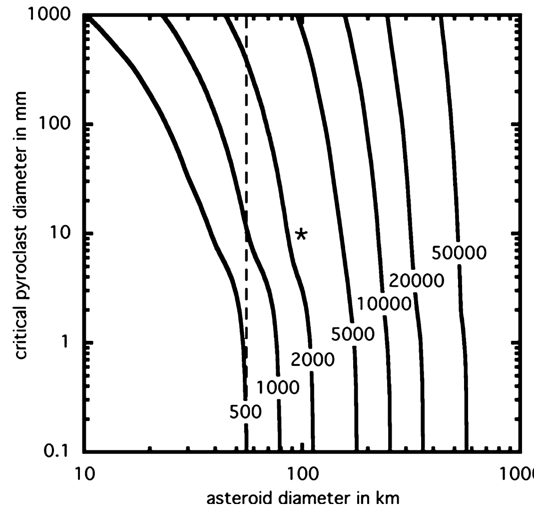

Fig. 5. Variation with asteroid diameter and erupted gas mass fraction, labeled on curves in ppm, of the critical size of silicate pyroclasts that distinguishes smaller pyroclasts that are ejected at greater than escape velocity from larger pyroclasts that fall back onto the asteroid surface. The broken vertical line and the asterisk are used to illustrate specific relationships. For example, the vertical broken line is the asymptote to the 500 ppm gas curve and crosses the abscissa at an asteroid diameter of approximately 56 km. This implies that, on an asteroid of this size, essentially all pyroclasts, irrespective of their size, will be retained on the surface if the gas mass fraction is 500 ppm or less. However, the line crosses the curve for 1000 ppm at a pyroclast size of approximately 15 mm. Thus, on this 56 km diameter asteroid, all pyroclasts will be ejected into space, unless their diameter is >15 mm (Wilson et al. 2010).

setting of chondrule formation, anorthite-rich chondrules, CAIs and their accretionary rims, amoeboid olivine aggregates, aqueous alteration of chondrites on their parent bodies and not in the solar nebula, low-temperature growth of fayalite, and studies of CVs and CRs, of which I am a co-author, but the credit for this work really belongs to Sasha.

DS: Talk to me about finding new minerals.

KK: Discovering new minerals is something that has occurred throughout my career, at Ames, UNM, and UH-Manoa. They were not discovered in a concerted effort hunting for new minerals, rather, they were the by-products of our petrologic, problem-oriented research. With my various collaborators, we described the four that I have already mentioned, and there is also suessite, Fe_3Si, named after my mentor, Hans Suess; caswellsilverite, $NaCrS_2$, named after a very important sponsor of the Department of Geology at UNM, Caswell Silver; schöllhornite, the terrestrial weathering product of caswellsilverite, $Na_{0.3}(H_2O)_1[CrS_2]$ named after Robert Schöllhorn, to honor his contributions to the study of synthetic hydrated chalcogenides; heideite, simplified $FeTi_2S_4$, named after my first teacher, Fritz

Heide; and chladniite, $Na_2CaMg_7(P04)_6$, named after E. F. F. Chladni, the founder of the science of meteoritics. Notice that many of these are phases that occur only in highly reduced enstatite chondrites and aubrites, which formed under very low oxygen fugacities, and they were discovered because of my long-standing and extensive research on enstatite chondrites and aubrites. I should add that my interest in new minerals has always been petrogenetic: what does this mineral tell us about the origin of the rock in which it occurs? A good example is keilite, $(Fe_{>0.5}, Mg_{<0.5})$ S, the cubic Fe-dominant analog of niningerite. This mineral was named and described by a Japanese-Canadian group, but they did not discuss the petrogenesis of the mineral. I found that keilite occurs only in enstatite chondrite impact-melt rocks and impact-melt breccias (Fig. 6). Skinner and Luce had shown in 1971 that MgS and MnS have "extensive and strongly temperature-dependent solid solutions towards FeS," i.e., at high temperatures, MgS can accommodate high amounts of FeS in its structure, but the high-iron phase (keilite) is only stable if quenched from high temperature; during slow cooling, it will exsolve into MgS and FeS. Thus, keilite occurs only in quickly cooled, quenched enstatite impact-melt rocks and breccias and is a petrogenetic indicator, usually of an impact-melt origin of the rocks in which it occurs.

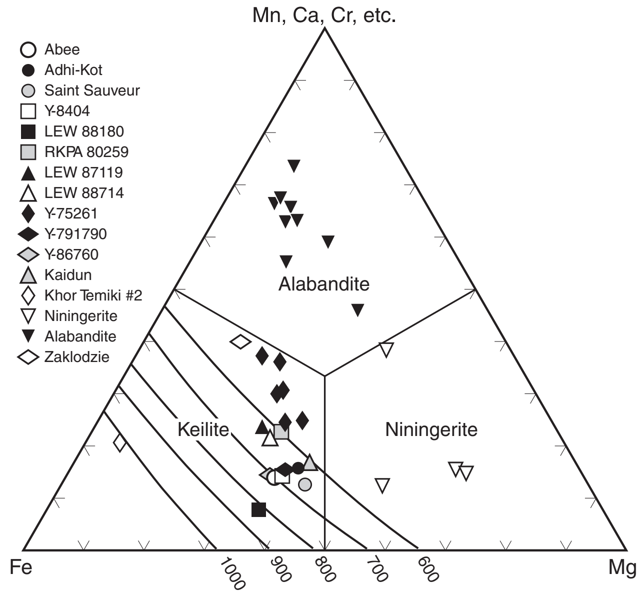

Fig. 6. Electron microprobe analyses of keilite from enstatite chondrite impact-melt rocks and impact-melt breccias and from the vitrophyre clast from the aubrite Khor Temiki (KTI#2), plotted in a ternary diagram in terms of atomic % on the basis of 1 S for the endmembers Fe, Mg, and Mn + Ca + Cr + all other cations. The experimentally determined ternary solvus lines for five temperatures from Skinner and Luce (1971, in °C) indicate that the minimum formation temperatures for keilite were approximately >500 °C (Keil 2007).

DS: We are up to the time of your retirement on July 1, 2012. Have we covered all the highlights of your career?

KK: Yes, I think we have.

DS: Set aside your own work, you have been in the field since 1958, give me your top five events in meteoritics.

KK: Number one, the incredible increase in the number of sophisticated analytical tools that meteoriticists and cosmochemists have had at their disposal, and for which they have in part been responsible for developing and spearheading of their applications. I am thinking of the electron probe microanalyzer, secondary ion mass spectrometer (ion microprobe), solid-source mass spectrometry, instrumental and radiochemical neutron activation analysis, and others. Number two, the discovery of the isotopic anomalies by Clayton and others. Thirdly, determination of the ages of CAIs, chondrules and differentiated meteorites to a degree of precision that is absolutely astonishing. Number four, the very successful attempts that have been made to cross the boundary between meteoritics/cosmochemistry and astrophysics, so that meteorite studies have contributed to our understanding of nucleosynthesis and stellar evolution and how stars and planet systems form. Number five, the discovery of the abundant and often rare types of meteorites from asteroids, the Moon and Mars in Antarctica, and hot deserts.

DS: You have been in the field of meteoritics/cosmochemistry for over 50 years.

KK: Yes, and it was and is a wonderful experience. You may remember that John Wood gave a talk in Houston a few years back about how little progress has been made in really understanding the origin of chondrules. This, to a degree, is probably true, but we have made tremendous progress in almost every other aspect of meteoritics/cosmochemistry. I do not know how chondrules formed, and I cannot evaluate a lot of the models that have recently been proposed, but when I think back to the one semester course in meteoritics I took from Fritz Heide back in about 1954 and compare it with what we know today, it is unbelievable how much progress has actually been made. It has been an incredible adventure for me to have been involved in meteoritics and cosmochemistry. I have seen a lot of excitement, a lot of great discoveries, and I am so grateful that I was able to be part of it.

DS: Would you like to add anything?

KK: Yes, I would. I would like to acknowledge the enormous support that all of us, I, my collaborators and students, and so many meteoriticists/cosmochemists working elsewhere in this field have had from NASA over so many years. Without this support, our field would not be what it is today, and meteorites would have remained nothing but scientific curiosities, as they were when I entered the field in the fifties. I personally owe a great deal to the agency and to all the officers that have administered our programs at NASA Headquarters. To all of them, my most sincere "thank you!"

DS: Klaus, many thanks for agreeing to this interview and many thanks for sharing your perspectives on meteoritics and planetary science during your wonderful career.

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This article was corrected on September 24th 2012 after online publication. Skinner et al. was removed from the Selected Bibliography.