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Report

Oral histories in meteoritics and planetary science – XXII: John T. Wasson

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Abstract–In this interview, John Wasson (Fig. 1) describes his childhood and undergraduate years in Arkansas and his desire to pursue nuclear chemistry as a graduate student at MIT. Upon graduation, John spent time in Munich (Technische Hochschule), the Air Force Labs in Cambridge, MA, and a sabbatical at the University of Bern where he developed his interests in meteorites. Upon obtaining his faculty position at UCLA, John established a neutron activation laboratory and began a long series of projects on the bulk compositions of iron meteorites and chondrites. He developed the chemical classification scheme for iron meteorites, gathered a huge set of iron meteorite compositional data with resultant insights into their formation, and documented the refractory and moderately volatile element trends that characterize the chondrites and chondrules. He also spent several years studying field relations and compositions of layered tektites from Southeast Asia, proposing an origin by radiant heating from a mega-Tunguska explosion. Recently, John has explored oxygen isotope patterns in meteorites and their constituents believing the oxygen isotope results to be some of the most important discoveries in cosmochemistry. John also describes the role of postdoctoral colleagues and their important work, his efforts in the reorganization and modernization of the Meteoritical Society, his contributions in reshaping the journal *Meteoritics*, and how, with UCLA colleagues, he organized two meetings of the society. John Wasson earned the Leonard Medal of the Meteoritical Society in 1992 and the J. Lawrence Smith Medal of the National Academy in 2003.

DS: John, thank you for letting me document your oral history. Let us start with my normal opening question, how did you get interested in meteorites?

JW: My Ph.D. research was in nuclear chemistry at MIT. Until late in my studies I thought I could be a nuclear chemist using the classical scientific method. That is, you gather data on a topic that seems interesting, you look for patterns in the data, and you write an interpretative paper that explains the data. I had learned, though, by going to Gordon Conferences, that this was not the way nuclear chemistry was being done. Nuclear chemists measured gamma ray energies as accurately as they could, they tried to fit these into energy levels diagrams, and then the nuclear physicists took over and interpreted the data. The nuclear physicists looked for the patterns in the energy-level diagrams and made the models. That was not what I had in mind. But while I was at MIT, I heard lectures by Harold Urey, Hans Suess, and James Arnold. These were people whose backgrounds were not that different from mine and all three extolled the virtues of working on meteorites, and how you could learn neat things about how the solar system worked. That's a strength of MIT, exposure to neat ideas, and I credit the institution for doing this. So that was it. I was hooked.

DS: You have talked to us about how you became interested in meteorites, let's go back and talk about your precollege years.

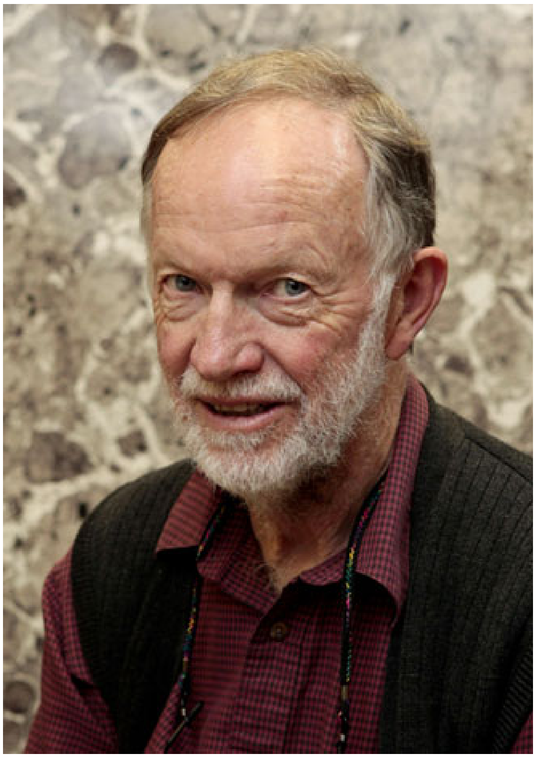

Fig. 1. John T. Wasson.

NORTHWEST ARKANSAS, FLINT CREEK, AND COLLEGE

JW: I grew up in a small town, a town with a population of about 100. I went to a one room school house for my first eight grades; the first year when I was 5 years old the teacher, my mother, lost her babysitter to marriage and took me along. Then I rode a bus to a high school in the nearest town where there was no physics course but there was a chemistry course that I really enjoyed. That convinced me to become a scientist.

DS: Tell me something about your parents.

JW: My father was the youngest child, somewhat shy and spoiled, and never very aggressive. He was content to run a small general merchandise store that he had inherited. He also inherited a farm, and we milked cows by hand. In fact, at age five my mother decided that I should start milking cows too and I did this from age five until seventeen.

DS: You did it well?

JW: My father was better, but I still have the ability to milk cows today.

DS: Was there any doubt as you were growing up that you would go to college?

JW: No. My school-teacher mother had taken college courses and she was convinced that I should go to college from the beginning, and I was soon convinced. I did little farm work, and I read as many books as I could. I did not know what subjects I wanted to study until I took the chemistry course. My father never went to college; in fact he never finished high school. He explained that the young men, "thought it was smart not to finish high school."

DS: You went to the University of Arkansas in Fayetteville?

JW: Yes, that is right.

DS: Was that the default, or were there specific reasons?

JW: The UofA was close, only 27 miles from my home. I had a scholarship offered to me by a local religious school but I knew that the center of intellectual activity for my part of the universe was the University of Arkansas and, in those days, it was quite cheap.

DS: So you went to the university, to the chemistry department. What recollections do you have of your time there?

JW: They are very positive. I had good teachers that I enjoyed and the faculty was very friendly to me and the other chemistry majors.

DS: So you were there when (the meteorite researcher) Paul Kuroda was there?

JW: I was there from 1951 to 1955. Paul Kuroda arrived 1 year before I left. I never took a course from him. Other people were arriving at about the same time, like the organic chemists Art Fry and Sam Siegel. Ray Edwards was the chair and he had a Ph.D. in nuclear chemistry from MIT. I told him I wanted to be a nuclear chemist and he advised me to go to MIT and work with Charles Coryell.

DS: Edwards's recommendation caused you to go to MIT?

JW: I also got a support offer from Harvard, but MIT offered me \$135/month and Harvard only offered me \$125/month; thus Coryell and more money sealed the choice. An advantage of studying in Cambridge (rather than Berkeley or Chicago) was that I could occasionally watch Ted Williams play baseball at Fenway Park (once triple parking in Kenmore Square during the game).

DS: Before we leave Arkansas it says on your web site that one of your interests is Flint Creek (Fig. 2).

JW: Yes, there is a big spring in my hometown of Springtown, Arkansas. It is one of the larger springs in Arkansas, probably in the top 20 in terms of annual flow. There are dry branches of Flint Creek upstream, but downstream of the spring is a strong flow and the water is very clear. It is spectacularly beautiful. Clear water and small sandstone cobbles, and it crosses our 75 acre farm. At one end of our property the creek

Fig. 2. Flint Creek, Arkansas. A portion of the creek that runs through the Wasson farm; throughout his career John has campaigned to make it a protected river as it is threatened by encroaching urbanization in Benton County (the home of Walmart).

creates a wonderful riparian ecology with a canopy of trees overhead and several limestone outcrops along the banks. I would like to see it preserved for future generations. Long after I left Springtown, Walmart became the main employer in my home county of Benton. The population is rising rapidly and developments are coming close to our farm.

DS: Flint Creek runs into Oklahoma?

JW: Yes. It turns green as it runs into Oklahoma because of the algae (too many nutrients added by Arkansas farms). In Oklahoma it is called a scenic river, but in Arkansas it has not yet received this designation.

DS: Okay, you had a satisfying time as an undergraduate at Arkansas, and in the time-honored fashion at the University of Arkansas, you got your name written on the sidewalk. In fact, the students who graduated in your year got their names inscribed in the pathway right outside the chemistry department! Why don't you tell me something about MIT.

JW: It was the first time I lived away from home. I mean well away, the University of Arkansas was only 27 miles from home. I had developed a sense of independence; the transition was fine. I spent the summer at a Dupont laboratory in Newburgh, New York.

In Cambridge I stayed at the MIT Graduate House which housed lots of interesting young men. The team that worked with Coryell were all very pleasant. The problem was that after almost two years I still didn't know what I was going to do. Coryell had broad interests, including political interests, and didn't give me much guidance.

I decided that I would apply for a summer job at Oak Ridge National Lab, and one was offered in a team doing gamma ray spectroscopy. I worked with Dave O'Kelley and his colleagues and knew much about the technique by the end of the summer. They had much better equipment than MIT. In the fall I returned and told Coryell that these guys were doing great stuff using 3″ by 3″ high-resolution scintillation counters and I argued we could do cutting-edge research if we had that equipment. I must have spoken at the right time because there was money available and a physicist name Martin Deustch had just purchased multichannel analyzers for MIT. I soon came up with a couple of short-lived radionuclides I could work on and that became my thesis topic.

DS: What were the projects?

JW: My main achievement was measuring several gamma-rays associated with the decay of 9.3 minute ^{139}Cs produced by the neutron fission of ^{235}U.

DS: What were the samples?

JW: The samples were natural uranium that was bombarded with deuterons in the MIT cyclotron. The deuterons would transfer their neutron to the uranium and cause it to fission; I separated the Cs from the fission products. We would open the door of the cyclotron and as the horizontal central opening reached 20 inches, I would jump through the opening, run over and grab my sample with a metal holder, then run back out and to the radiochemistry area.

DS: Break your ankle on the way?

JW: That didn't happen. But another story that might interest the Meteoritical Society, is that based on a misplaced sense of justice I ended up losing my thesis advisor for much of my last year at MIT. I wrote my thesis without the aid of an advisor.

DS: So how did you get through?

JW: I thought I was going to hand my thesis to the department and say, "Here is my thesis. I do not have an advisor." However, one day Coryell walked in and said. "John, I am going on a trip to Europe and Asia Minor this summer; you should write up your thesis so we can have your defense before I leave." No apology from me. No apology from him. But understand correctly; I loved this man who died far too early.

DS: So after you graduated with your Ph.D. in 1958?

GERMANY, PROMPT GAMMAS AFTER NEUTRON CAPTURE, GUDRUN, THE U.S. AIR FORCE, AND BERN

JW: In September I sailed to Rotterdam on the Statendam. I visited the World's Fair at Brussels, then proceeded to Munich to start my postdoc at the Technische Hochschule. Two months later I met

Gudrun Hanewald, a German woman who was a student at the University of Munich; 1.5 years later she became my wife.

The TH had a little research reactor in the village of Garching, near the banks of the Isar River; I took the bus to Garching every day. But, as I said earlier, I didn't go to the TH with a research project in mind so they assigned me one. In fact, I spent much of my time copying papers on meteorites.

DS: Were you doing neutron activation?

JW: Sort of. I used an apparatus built by a grad student to measure prompt gammas following neutron capture. When neutron capture occurs, the new isotope one mass number higher is in a highly excited state and emits several gamma rays which bring it down to the ground state. In normal analytical neutron activation you measure the gamma or beta radiation that is produced following decay of the new isotope.

DS: Was this a new technique?

JW: The technique was relatively new; I measured the energies of the prompt gammas to five significant figures with a curved crystal spectrometer.

DS: You were in Munich a year. Did you get married that year?

JW: No. We didn't even get engaged. We had a tearful separation in September and I came back to the states. I had to prepare to start my service as a second lieutenant in the Air Force. The Air Force had treated me very generously. They gave me delays in reporting so that I could complete a Ph.D. and again so I could be a postdoc in Germany.

DS: There was conscription in the U.S. at 18?

JW: When I was in high school the Korean War was going on and there was conscription. I think I was actually too young to be drafted during the Korean War, but my high school friends were not. They went off to battle and some of them died. I was patriotic and wanted to do my share. However, I thought I might as well do it as an officer rather than a foot soldier so at the University of Arkansas I joined the advanced Air Force ROTC (Reserve Officer Training Corps). If you went through four years of ROTC you were commissioned a second lieutenant.

While I was studying at MIT I learned that there was a nuclear chemistry group at the Air Force Cambridge Research Laboratories, which were 14 miles west of Cambridge, Massachusetts. This group put in a (successful) request that I be assigned there during my two-year period of active duty.

But I only entered the Air Force at the end of December 1959. Coryell found some money to pay me as a postdoc for a few months. During that time I got engaged and we set the wedding date for February. When the time arrived I had only two and a half days of earned leave.

DS: How long were you at the Air Force labs?

JW: Three and a half years. My contractual obligation to the Air Force was 2 years. However, three quarters of the way through that 2 year period the Berlin wall was built and my tour was involuntarily extended by a year. However, after 6 months they decided that things had calmed down and they let me out. In the meantime Gudrun was making good progress in writing her dissertation at Harvard but needed another year. Fortunately, my boss at AFCRL had enough funds to provide me with a year of postdoc support.

DS: Tell me more about this nuclear chemistry team.

JW: The leader of the team was Ed Martell, a former student of Bill Libby (who received the Nobel Prize for his studies of ^{14}C and was my first boss at UCLA). The *raison d' être* for the AFCRL research group was to use bomb-produced radioactivity to measure stratospheric circulation. So I became an atmospheric chemist too. Because during the first 2.5 years I was free to the group, they let me do anything I wanted to. So I was doing a lot of reading about meteorites, I would attend the Harvard-MIT Meteorite Discussion Group and, as a result, I got acquainted with Fred Whipple. The discussion group included Whipple, John Wood, Ursula Marvin, Ed Fireman, and later, Bob Dodd, who also was serving his tour at the Air Force lab. He started two years after me; I was not yet researching meteorites and I don't think Bob and I ever talked about our own research.

But I had established a strong relationship to Whipple and Fireman and I still had a connection with Coryell and J. Winchester, one of his students, now an assistant professor of geochemistry at MIT, who was an expert at neutron activation.

DS: Gudrun was a student at Harvard?

JW: Before Gudrun came to the U.S. I took her transcripts to Radcliffe College and asked whether she could possibly be admitted as a student and they said, "Yes, no problem." Two days after she arrived she was attending classes at Harvard and was soon working as a TA. In those days Radcliffe and Harvard were separated, Radcliffe educated women, Harvard men, but by the time she had her Ph.D. 3 years later they had removed the bureaucratic segregation and said that she could choose either school for her diploma. She chose Harvard, even though for her entire 3 years she was officially a Radcliffe student.

DS: What was her subject?

JW: Modern German literature, a study of imagery in a novel by Hermann Broch.

DS: What happened after your time at the Air Force labs?

JW: I got another fellowship. This time I went to Bern, Switzerland. It's a bit embarrassing to admit, but this time the fellowship was from the National Institutes of Health.

DS: Why is that embarrassing?

JW: Because I knew that I would not pursue a career in biochemistry or health. In my application I proposed to study cosmic ray effects that might be related to radiation effects on astronauts. I did do some research on cosmic ray products in Bern but I spent little time thinking about astronauts.

During the fellowship my first major paper (of which I am quite proud) appeared; it was entitled, "Radioactivity in interplanetary dust." For a relatively inexperienced and naïve guy, I think it is an interesting paper. A scientist at the Air Force Laboratory sent AgBr emulsions into space on Discoverer satellites that were recovered in midair; he would then look for cosmic ray effects in the materials he put in them. One time we heard that the Discoverer XVII emulsion had come back extremely black and somewhat radioactive because a solar flare occurred when the satellite was in orbit. I had never heard of energetic solar protons, but I was not against learning, but I figured that if the solar protons were making radioactivity in Discoverer XVII they were making radioactivity in interplanetary dust. No one before me had ever discussed this possibility.

Solar protons do not have much energy so they have shallow penetration depths. Meteorites have radioactivity from solar protons on their outer skin but the interiors are only be affected by galactic cosmic rays. But I realized that these 5–100 MeV solar protons particles would make radioactivity in dust and predicted that there should be 720 ka ^{26}Al in the interplanetary dust from the sea floor due to this source. Bill Cassidy, then at Lamont, helped me get a large deep-sea dredge sample and I got the ALCOA Research Lab to separate the Al for me. I took the Al_2O_3 sample to Bern where Hans Oeschger counted it and found ^{26}Al.

DS: You were at Bern for a year?

JW: I was there for 15 months. There were other cosmochemists visiting during that academic year. Ed Anders and Mike Lipschutz were there and Harold Urey visited. Figure 3 shows Lipschutz and me talking to Urey. I did not yet have a beard (but have had it continuously since 1972).

DS: So the dust radioactivity paper was your first publication?

JW: My first important publication. The earlier ones are short nuclear papers. My first Nature paper,

Fig. 3. Michael Lipschutz, John Wasson, and Harold Urey. This photograph was taken in the Physikalisches Institut in Bern in fall 1963 by Norbert Grögler.

"Terrestrial accretion of the solar wind" had appeared, but I was the third author. This one discussed possible solar wind contributions to Na in the upper atmosphere but stated that dust was a stronger source.

DS: What else happened at Bern?

JW: Well, Houtermanns was head of the institute. His health was not good. My real boss was Johannes Geiss and he hoped I, like his students, would carry out mass spectrometric research. Because I was not interested in building a mass spectrometer they left me alone. I had my own money and I did my first meteorite research there. I measured boron in iron meteorites using colorimetric analysis. Very simple. What I found was that my upper limits were lower than other people's "real" numbers. There is not much boron in iron meteorites.

DS: Before we leave Bern, do you want to say something about your interactions with Anders, Lipschutz, and the others?

JW: There was little interaction. Anders was a loner and he spent most of his time alone in his office. He was highly focused and he could write more words in an hour than any cosmochemist of his generation (but his output was puny compared to that of Al Cameron).

Lipschutz and I had quite different interests but we would sometimes have coffee together. We knew each other from AGU meetings and Gordon Conferences. Eberhardt was there. He was not yet a professor. Oeschger of course. He became a very famous man later for his work on gases trapped in polar ices. Then he was just a new associate professor working on natural radioactivities.

DS: Did you see much of Geiss?

JW: No I didn't really. I didn't see much of most people, although I went to seminars and bumped into people in the hallway. There was not much intellectual exchange at the institute, so it was not much of an influence on my later career. It did give me a comfortable office where I could work hard and it gave my wife an opportunity to be in German-speaking Europe not too far from her family.

When I left the U.S. I had only one job waiting for me; Fred Whipple promised me a position at the Smithsonian Astrophysical Observatory. These days the director could not make such a promise because all organizations are running on the edge of their budgets, but Whipple had enough money, or confidence that he could get the money, that he could make such an offer.

DS: This is a topic that has come up a lot in these oral histories. It was relatively easy to get positions in the 1960s.

JW: The SAO offer was great; it assured me of a good scientific position in back in the U.S. I would have been happy as a member of the SAO team (with Wood, Marvin, and Fireman), but I really wanted to be a teacher and I had this sense that my career would be better carried out in a chemistry department.

Very late in our stay in Bern I got an offer from UCLA, half in chemistry and half in the Institute of Geophysics and Planetary Physics then directed by Libby. An interesting aspect of my interview at UCLA is that I was not asked to give a seminar. Libby never got around to organizing one. It was probably a good thing. In those days I was terribly bad at organizing and practicing talks, and if I had given a talk I would probably never have gotten the job! So in the fall of 1964 we came to Los Angeles. In the meantime our first daughter had been born.

DS: She was born in Bern?

JW: That's right.

DS: So let's move to UCLA. What were your research plans as you set up here?

UNIVERSITY OF CALIFORNIA AT LOS ANGELES AND IRON METEORITES

JW: I was well aware of the need to publish in order to get tenure. I saw myself primarily as an experimentalist. I considered trying to study cosmogenic activities in meteorites, similar to what Jim Arnold was doing at UC San Diego, but that required very sensitive counting equipment that I did not have. However, Libby gave me $40,000 that I could spend when I got here but I could also use his rather primitive gamma-counting equipment. I decided to spend $20,000 to hire a technician and spend the remainder on minor equipment and supplies. I could do some neutron activation using Libby's gamma spectrometer and samples activated at the UCLA (homemade!) reactor. In my research in Bern I realized that, because there are fewer elements, it is much easier to work on irons than stones. I did some calculations and found that I could do Ga and Ge better than it had been done at Caltech using emission spectrometry (this is the paper that led to the "Ga-Ge classification" of iron meteorites). Because I could go down to much lower concentrations than the people at Caltech I started with the low Ga-Ge irons.

DS: The Caltech paper is Lovering et al. (1957) and your first paper is Wasson (1967).

JW: Before the Caltech study there was a paper from the University of Chicago, Goldberg et al. (1952), that showed that Ga values clustered in irons. Anyhow, to my delight I was able to measure Ga and Ge in all the meteorites I studied; my data on low-Ge irons formed two tight groups covering a field of about 5% of what had been called Ga-Ge group IV (Fig. 4). It was a triumph of analytical chemistry. It led me to a career in iron meteorite research because I discovered that analytically I was in a good position. The first paper I was the sole author and my super technician Jerry Kimberlin was on the second that came out a month or two later.

DS: Ed Scott joined your group about this time?

JW: Yes, a bit later; based in part on the recommendation of Buchwald, in 1972 I rescued him from a job on the UK Electricity Board. A half-year after his arrival he started to analyze irons by instrumental neutron activation (INAA) using the data reduction program written by our postdoc Phil Baedecker.

DS: There are a couple of papers that caught my eye. You worked with Vagn Buchwald. Can we pause for a few minutes to talk about Vagn.

JW: Sure. The classification scheme in wide use today is really the work of both Vagn and me. The geochemical data are a little harder to obtain than Buchwald's petrographic observations, so our team can take more credit than Vagn. It is a coincidence that Vagn was systematically looking at the detailed structure of every known iron meteorite at about the time we discovered how to classify iron meteorites with precise compositional data. Before we started our work no one knew about the major trends in trace element composition present in each of the groups.

DS: How did your relationship with Vagn work? Did he come here on a sabbatical?

JW: No, he never spent time at UCLA. He started his work in Denmark and then he spent a couple of years with Carleton Moore at Arizona State University and that is when I first met him. We collaborated on about six papers. He provided structural information

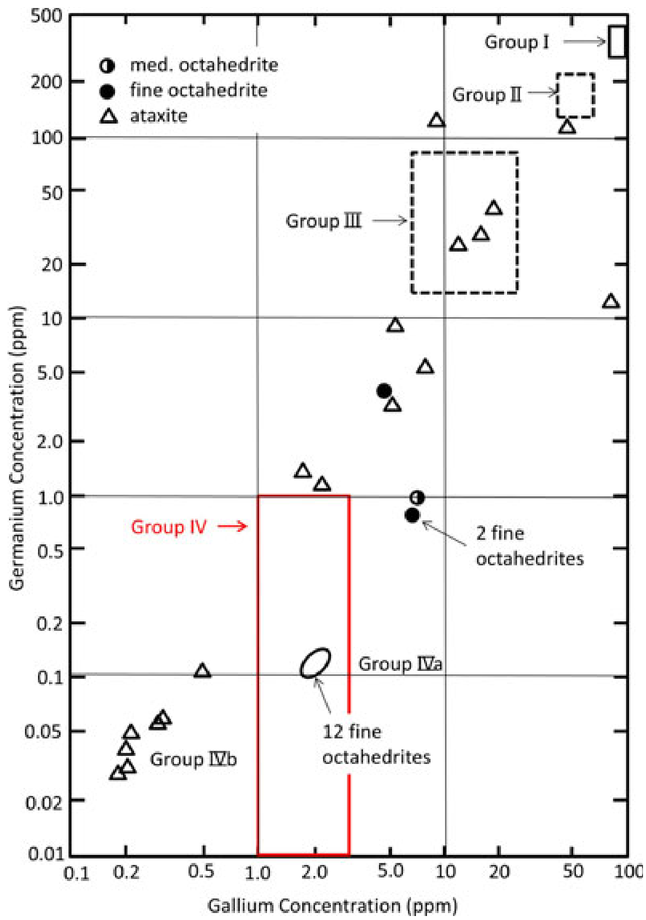

Fig. 4. The relationship between germanium and gallium concentration in iron meteorites. The 34 meteorites for which new data have been obtained are shown by symbols or by the irregular field. The locations of the Ga-Ge groups I, II, and III as previously identified by Lovering et al. (1957) are shown as rectangular fields; their limits on group IV are shown in red, a much larger area than Wasson's group IVA. (From Wasson 1967.)

that went along with our compositional assessments. It was so satisfying that we came up with the same classes with different techniques. It gave both of us confidence in our conclusions. Vagn compiled all sorts of information: major, minor, and trace minerals, metallographic textures, everything. It is not by chance that his handbook on iron meteorites is the easiest book to grasp from my desk chair.

DS: That is your measure of book quality, how close you keep it. Tell me about the publication of that massive book.

JW: Vagn and Carleton Moore did most of the marketing for it, although I wrote a letter of support to the University of California Press. Vagn and Carleton had arranged a subsidy from ASU but ASU didn't have a press so they came to the UC Press.

DS: Is this the most important book on meteorites, besides your own?

JW: My books are out of date; Vagn's is timeless! You have to interview him. Vagn has mostly left meteoritics, but he is active in archeological metallography now (in his early 80s).

DS: Where I was heading when I asked about Vagn was the role of large meteorites. What do we learn from cutting open large meteorites? Every now and then we have a chance to cut large irons. Talk to me about that.

JW: Forty years ago I used to say that the only good iron was a cut iron, but my tune changed for a while when the Old Woman meteorite was found in the desert of California and I wanted it to stay in California, relatively intact. My original view was correct: every large iron meteorite needs to be cut so that researchers can see what the inside looks like. One learns much more about planetary processes from a large surface compared to a centimeter sized research specimen.

DS: The Cape York iron was huge and when cut you could see the elongation of the sulfides.

JW: The morphology of the sulfides in the 20 ton Agpalilik specimen of Cape York was an important scientific discovery but the very large abundance of FeS in Agpalilik compared to other Cape York irons such as Savik was an equally important and independent fact that was revealed by the cutting. When Buchwald discovered this iron in Greenland he thought he could see the orientation in the surface sulfides so he had it cut parallel to the long axes of the sulfides and confirmed that they are ellipsoidal and that they are parallel. The direction of the gravitational field is marked by the locations of phases more and less dense than FeS.

DS: Tell me about the Old Woman meteorite.

JW: It was found by prospectors. I heard about it from Old Man Bekins, who grubstaked these particular guys. He had started to visit us because he wanted to help us find new meteorites by "dousing" or "divining" maps while holding a meteorite. I would not let him into my office because he wanted to talk about divining for much more time than I had to spare. He always had a thick stack of papers most of which were announcements about divining club activities. We would stand together in the lab and he would flip through these and tell me why they were important. One day, in the middle of a stack, was a picture of what was obviously a large meteorite. I could see that it was large because there was a boot for scale. I calculated from the estimated boot size that the meteorite must weigh about three tons, and I was fortuitously right.

I tried to get the prospectors to agree to a moderate price (I think I suggested $20,000) but this only made them angry at me. They then made the mistake of contacting Roy Clarke of the Smithsonian in hopes that he would buy it from them. However, when they took him to the site (in a mountain ravine too remote for them to be able to move it), he realized it was on federal land so instead of buying it he offered them a small reward and they would not take it because they felt it was far less than the value.

There were soon several legal suits by organizations trying to keep the meteorite in California. The Los Angeles County Museum of Natural History, the San Bernadino Natural History Museum, the Board of Supervisors of San Bernadino County, myself, and others were all making publicity about why the Smithsonian should not take a Californian meteorite and put it in Washington. We got the entire California Congressional delegation to write to the Smithsonian. Eventually, Dillon Ripley, the secretary of the Smithsonian, wrote a letter to Senator John Tunney, saying, "If you want it so damn bad you can have it." The bulk of the meteorite is now in a small museum of the Bureau of Land Management in Barstow, resting on the cut face.

DS: So, it is exhibited in the small desert city of Barstow, in a federal museum?

JW: Yes. Just off I-40. Far from the population centers, but easy to get to if you are heading to Vegas. When my wife and I visited it on a Saturday afternoon a few years ago there was one other family there.

REORGANIZATION OF THE METEORITICAL SOCIETY

DS: We have wandered into the 1970s. Before we leave the 1960s, I wanted to ask you about the insurrection in the Meteoritical Society in 1966. It was described by Ursula Marvin in her history of the Meteoritical Society (Marvin 1993). You were involved in that?

JW: Yes, I attended the Gordon Conference on Nuclear Chemistry that year which had interesting cosmochemical topics on the program and attracted a number of meteorite researchers. Several of us were aware that it was an election year for the society and thought it would be good if the society could be run by professionals rather than amateurs. It took relatively few signatures to nominate persons for various positions. We decided to do this and our candidates won every contested position except that of editor (which is now an appointed position), for which the incumbent editor, Dorrit Hoffleit, received one vote more than I did.

DS: What were the consequences of that event, as far as you can see?

JW: Having professionals take over the running of the Meteoritical Society has had many positive consequences for meteorite research and researchers. Foremost have been the meetings which now (together with the LPSC) provide the foremost forum for presenting our papers. And, in contrast to the LPSC (which, because there are too few sessions, frequently does not allow oral presentations of some very good papers) the Meteoritical Society provides slots for all almost all senior first-author presenters and oral slots are also available for a sizable fraction of student attendees.

Thanks to several Europeans who came to the early North American meetings we soon (1971 in Tübingen) started to meet in Europe. And a few years later (mainly thanks to the efforts of Paul Pellas) we started to provide travel funding for students. These annual meetings and the increasing high reviewing standards of our journal under Carleton Moore and later editors have led to meteorite research becoming dominated by hard science.

One of the most important tasks the new Meteoritical Society started to play was in meteorite nomenclature. You probably know that Vagn Buchwald and I played an important role in founding the Meteorite Nomenclature Committee, in part in reaction to Glenn Huss's suggestion that the old requirement of naming meteorites after towns with post offices should be expanded by using letters after the names if more than one meteorite was found. Vagn and I argued that unique mnemonic names were much more useful, and that it was fine to choose any landmark that was found on well-archived modern maps. Our current regulation of nomenclature is sophisticated but still being improved. We must be grateful to the many members who have devoted large amounts of time to this enterprise. I am constantly impressed that the whole world of meteorite dealers and collectors acknowledges the importance of this system and makes so much effort to make it work.

All scientific societies have a political role to play, especially in keeping their members informed about proposed changes in government funding. We have done this on occasion, but to a relatively minor degree because of our international nature.

Do I think that the total impact of four decades of research would have been much smaller if we had not taken over the management of the society in 1967? No, I don't; clever people would have been making their discoveries, but some of these would have occurred later. And, if we had not taken over the Meteoritical Society another organization would eventually have been created to do the good things that were done by the Meteoritical Society, just not as quickly and probably not as well.

SAMPLES OF THE MOON

DS: We are in the late 1960s now, the run-up to Apollo. When did the Apollo program impinge on your work, or did it?

JW: My first NASA money came to study Apollo samples. Earlier proposals to NASA for studying meteorites were unsuccessful but our team was able to obtain a share of the influx of funds made available to study Apollo samples. I got money in 1968 to gear up for the Apollo samples.

DS: How did that go? New people? New machines?

JW: All of these. We hired one or two new postdocs and got better at radiochemical neutron activation analysis by adding a few new elements to our repertoire. Unfortunately, I was so wrapped up in studies of chondrites and irons that I did not properly do my homework for the lunar samples. So when I went to the first conference in Houston I had lots of data on lunar rocks and soils but I did not have anything interesting to say. I was a bit jealous when Ed Anders started to talk about the meteoritic component of the lunar soils. However, we were fast learners and published lots of lunar papers in the 1970s.

Paul Warren joined my group in 1976, and Paul used his expertise in petrology to become an expert on rocks from the lunar highlands. Paul did not learn petrology from me of course, but there are many papers where I played the role of the advisor, smoothed rough edges, sometimes played devil's advocate, but for all those papers where Warren is first author he should get 80% of the credit.

DS: He came in 1976 and is still here.

JW: He was a postdoc with Keil in Albuquerque for a couple of years.

CHONDRITES, CHONDRULES, AND CONTROVERSY

DS: You still worked on chondrites during the return of lunar samples? I see Tandon appearing on your early chondrite publications. Who was he?

JW: Tandon was my first postdoc; he was supported by NSF money. We made this very important discovery that highly volatile In was strongly correlated with the petrologic type of the L chondrites, with a range of a factor of 1000 from the lowest L6 to the highest L3. My postdoc Chen-Lin Chou obtained similar data for H chondrites and failed to find the dramatic trend.

DS: It was about this time that you started to carry out INAA on large numbers of chondrites?

JW: Yes. Probably our biggest discovery was work with Greg Kallemeyn where we found that refractory lithophile abundances were uniform (Fig. 5) within chondrite groups, different between groups. The three ordinary chondrite groups all have the same refractory element abundance, normalized to CI chondrites and Mg, but the carbonaceous chondrite groups fall into separate clans, CV high, CO and CM similar but intermediate, CI and CR similar but low. It was already known that Al varied in this fashion, but it was not known that if you know Al and the appropriate factor you could calculate the abundance of any refractory lithophile element, even the rare earths. This trend had already been hinted at by the South African group under Louis Ahrens.

DS: They used XRF.

JW: Yes.

DS: That was the refractory elements. What about the moderately volatile elements?

JW: That is an interesting story. Ed Anders had promulgated a two-component model to explain the distribution of moderately volatile elements in chondrites. He proposed that chondrules are volatile free, whereas the fine matrix is volatile rich with a CI-like composition. You can explain bulk compositions of chondrites by mixing the two. Thus the moderately volatile elements form a plateau with, for example, CM chondrites having volatile contents half those of CI chondrites because chondrules accounted for 50% of the rock. I was always ready to play the devil's advocate, especially for Anders. As Chen-Lin Chou and I gathered more and more data on ordinary chondrites we were able to show that the volatile elements slowly decreased in abundance with increasing volatility (Fig. 6); Chien Wai and I introduced the concept of 50% condensation temperature and calculated these for several moderately volatile elements. We showed that the fractional retention of a volatile element decreases as its volatility increases. Anders sent a critique of our conclusions and we countered it by publishing a comparison of ordinary chondrite abundance ratios with nine data sets based on random numbers. We invited the reader to search for the plateaus predicted by the two-component model and identify the ordinary chondrite set.

DS: There is a role for controversy?

JW: I think some controversy is good for the field. It can get out of hand at times and people can get nasty. There are people that say any controversy is good for the field, but I am not convinced. On the other

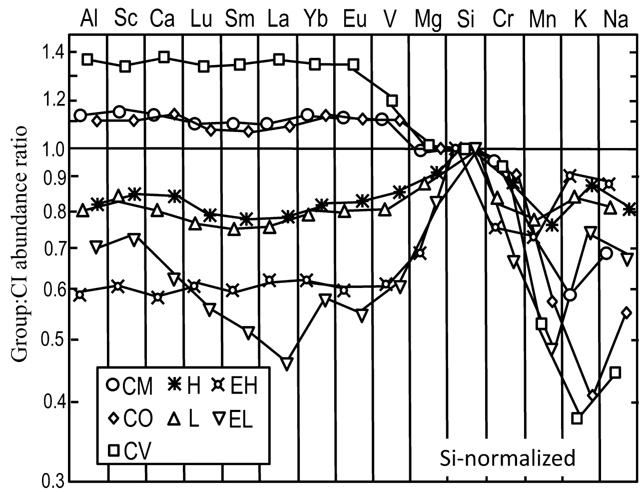

Fig. 5. The abundances of lithophile elements (normalized to Si and CI chondrites) for all the then-known chondrite groups except LL chondrites. The elements are arranged into rows, with volatility increasing to the right. The horizontal line represents CI chondrites. All groups, except EL, have a flat refractory lithophile element pattern, implying that these elements were in the same nebula component. (From Wasson and Kallemeyn 1988.)

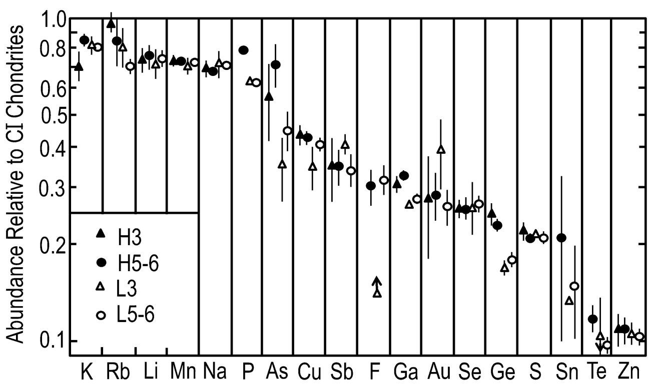

Fig. 6. Fractionation of moderately volatile elements in ordinary chondrites. Abundances of moderately volatile elements (normalized to Si and CI chondrites) for H and L chondrites decrease with increasing volatility to the right. However, there is no resolvable difference between unequilibrated (type 3) and equilibrated (type 4–6) chondrites. (From Wasson and Chou 1974.)

hand, I am convinced that there is too much bandwagonism, too much following of the herd (the consensus) in cosmochemistry.

DS: Enlarge on that. Maybe you have some examples.

JW: I do. The chondrules clearly formed by melting; most agree that these formed in the solar nebula. We do not know the heat source (my best guess is lightning) but that's a detail. The cooling rates that are recorded in the petrographic structures of chondrules have been simulated by furnace experiments by Roger Hewins and Gary Lofgren and the cooling rates are quite low, too slow for a transparent nebula environment. They require an opaque environment with dimensions of a 1000 km. The furnace simulations are called fast cooling, but they are approximately 0.1 K s^{-1} whereas a chondrule radiating into black space cools at approximately 100 K s^{-1}. The furnace experiments do not do a good job of simulating the complex set of heating events that occurred in the solar nebula. For one thing they try to explain phenocryst formation with a single heating event.

Chondrules were initially melted by one heat pulse, then partly melted again by additional heat pulses; the fraction of melt varied from event to event. My best guess is that each chondrule experienced ten to a hundred heating events that brought them to incipient heating. I don't want to go into my work in detail but overgrowths on olivine grains suggest much faster cooling rates than furnace experiments that try to explain phenocryst formation in a single event. However, when modelers need chondrule cooling rates they invariably choose the low rates obtained in furnace experiments.

Another example is, "What has melted the asteroids?" There are two possibilities, ^{26}Al and impact. The community has rejected impact, partly because of a 1997 paper from the Keil group where they used terrestrial analogs and were trying to melt whole asteroids instead of just part of them. In fact, impacts into porous asteroids can produce melts as modeled by various researchers, the higher the porosity, the more efficient the conversion of impact velocity to heat and thus the greater the amount of melt. What Kunihiro, Rubin, and I confirmed, although the writing was already on the wall, is that no studied chondrite has enough ^{26}Al to bring the temperature above 1400 K, too low to melt chondritic silicates (Kunihiro et al. 2004). Yet most meteoriticists assume that melting was caused by the decay of ^{26}Al.

DS: Ernest Pik wrote an autobiography with the theme, "Dogma in Science." He lists five or six examples, from astrophysics mostly, but one is that all iron meteorites come from cores. You would not go along with that.

JW: Of course not! The community has largely accepted my model for the nonmagmatic iron meteorites. In a paper that we published in 1981 we called attention to the fact that all the strange features of the IAB meteorites could be due to impact.

DS: What the tight compositional arrays indicates is that each of the iron meteorite groups came from a single magma but not necessarily a core?

JW: Most iron meteorites, IAB and IIE are the exceptions, underwent fractional crystallization which means that the residual fluids must be well mixed and cooled very slowly. I think this requires a core with an insulating mantle.

DS: Why don't you say a little more about the irons that did not undergo fractional crystallization?

JW: It was recognized soon after the large Campo del Cielo meteorite, El Taco, was cut in Mainz, and after Gene Jarosewich obtained compositional data, that the silicates were essentially chondritic. Maybe they had lost or gained some plagioclase, or had lost or gained a little bit of a minor amount of metal and sulfides, but they were basically chondritic.

DS: You have moved us into the 1980s. In the interests of disclosure, we should mention that I was in your group from late-1979 to mid-1981.

It seems to me that one big event of the 1980s was the interest in the K/T boundary.

THE CRETACEOUS-TERTIARY BOUNDARY, LEADERSHIP, POSTDOCS, AND STUDENTS

JW: Yes, of course. There is another story there. At a late-1979 luncheon meeting of the professors of the Institute of Geophysics and Planetary Physics a young professor showed us a preprint he had just received. A Berkeley team was claiming that there is iridium at the Cretaceous-Tertiary boundary and that it was caused by a supernova exploding near the Earth. I thought, "Oh my God! What a great discovery! Why didn't I make that discovery?" My team and I are especially good at measuring Ir. How did we miss this opportunity? But my second thought was that they were giving the wrong interpretation. So I went upstairs and told my graduate student, Frank Kyte (a guy who ultimately won the Barringer Award for his work on Ir and other features at impact horizons), and I said to him, "Frank, we have to get some samples of the K-T boundary and try to publish quickly so that we can give the correct interpretation, that it is an accretionary event." I could not convince Frank on the first day but within a week he was convinced and, being savvier than me, he found samples of the K-T boundary here at UCLA in the collections of a senior member of the geology faculty. In no time he was also getting samples from elsewhere. We went to work, and sure enough we got our paper published in the last issue of *Nature* for 1980. We were the fourth paper that year. By the time Luis Alvarez and the Berkeley team had published their paper, they had also concluded that the Ir was deposited in an impact event. Our main remaining claim to fame is that we were using radiochemical neutron activation and could determine more platinum-group elements than the Berkeley team, which used INAA; we interpreted the unfractionated elemental pattern to indicate that the bolide had a chondritic composition.

Within a year Frank had found a second boundary with Ir enrichments in sediments from the Eltanin Site near Antarctica, Jim Crockett had reported minor Ir enrichments in a core collected by the Eltanin Research vessel. Frank obtained samples, measured them at closely spaced intervals, and found a large Ir anomaly. Frank fortuitously met Rainer Gersonde, a German scientist who was organizing a new sampling trip to the region and during several trips was able to collect many samples and map out this area containing extraterrestrial materials.

DS: Your name is synonymous, at least in my mind, with RNAA and INAA, especially INAA. Could you recap the history of this technique and say something about its future.

JW: I will only say a couple of things about the technique. It is very good for some elements such as Sc, Co, La, Eu, Ir and Au and, in contrast to techniques that involve the dissolution of the sample, it is free of reagent blanks. There are fewer and fewer practitioners of neutron activation, mainly because the number of research reactors is decreasing. The UCLA team consisting of P. Warren, F. Kyte, and me is still quite productive, one of the most productive in the geochemical/cosmochemical world.

DS: You've mention Paul and Frank, and the individual directions their careers have taken. Alan Rubin has also been with you a long time and covers another distinct field of chondrite petrology. Do you want to say something about that?

JW: During the past 30 years I have had frequent collaborations with Alan, much more than with any other scientist. Alan has taught me much about petrology, especially chondrule and chondrite petrology. We have given each other a great deal of cross stimulation.

My skills at understanding chondrite textures took another quantum leap when Sasha Krot was here; Sasha taught me the value of BSE images by making these into slides and dragging me to our Red Table to see them projected. Sasha presented his interpretations and my personality demanded that I challenge some of these. After three decades as a chondrite researcher I started to love to stare at chondrule/chondrite textures. As you know, we now have our hallway decorated with BSE images of primitive chondrites. Alan and I frequently look at these and come up with new possible interpretations and research projects.

DS: You have talked about the various contributions of your postdoctoral coworkers, how does this fit in with your philosophy as to how to run a research group. Launch a postdoc career and let them run with it.

JW: While they are at UCLA we collaborated closely. But we all want our students and postdocs to become successful independent scientists with productive careers.

DS: It is not the Germanic tradition of building up a group around them where everybody contributes their skills to a single line of work.

JW: Well, I wish I could claim to be nobler. I sometimes argue that my projects are more worthwhile than those my students have found. I think most cosmochemists establish their independence when they are postdocs.

DS: Do you have a deliberate hands-off policy after your students have the Ph.D.?

JW: I do, once it is clear they do not need me. I haven't published a paper with Paul since about 1985, or Frank since about 1987. Paul recently asked me to join him on a paper but when I tried I found our styles were just too different and we gave up the attempt.

DS: You mentioned different skill sets too. You have managed to encourage these colleagues to each develop different skill sets.

JW: What is wrong with the older Germanic system was that there were too few top jobs. Good people would become trapped in minor roles helping test some of the professors' ideas.

DS: Well, it is arguable that there are still too few faculty positions in our field. There are too many career-long researchers on soft money.

JW: It's chicken and egg. The reason we have so many soft money researchers is that there is so much money available.

DS: Some prefer it that way?

JW: No, I wouldn't say prefer. Most would like to be a big shot faculty member. It's partly that NASA is willing to support soft money positions; NSF tends to mainly fund faculty members.

DS: There is a big difference in the missions of NASA and NSF. You told me when I came here that NASA funds missions, NSF funds science.

JW: It's gotten better. When you came here NASA's cosmochemistry funding was going up and down depending on the latest mission. Lunar research was winding down and it was not clear that they were going to fund meteorites to a comparable degree. It has now leveled out. At this time it appears that no high NASA official questions the value of meteorite research. Cosmochemistry now has a more-or-less level budget from year-to-year.

DS: You have talked about postdocs, but you said you came to UCLA rather than SAO because you wanted to teach.

JW: I wanted to have students. In fact, one of my first courses here was "Physical Chemistry for Nurses." I didn't take the job so I could teach nurses. No, what I wanted was to have a research team. I wanted to have students, postdocs, the whole works. A mini empire.

DS: What is your philosophy of teaching graduate students? Have you had many? Are they essential for your work?

JW: I have had less than twenty Ph.D. students. Postdocs and technicians produce results at a faster rate than graduate students. Graduate students have many distractions. You have probably had similar experiences.

DS: Well, it amounts to how you see your role in life. Is your role to mentor students or get research done?

JW: Both, but the larger role is to produce important research. I enjoy interacting with young people, both as a lecturer and as a mentor. It is a great pleasure to think that I have helped them mature. But what gives me most pleasure is pushing back the research frontier.

DS: Have undergraduate students factored large in your career?

JW: No, not as researchers. But many very talented undergrads have helped us with sample prep, data reduction, and the like.

DS: Undergraduate research?

JW: I have had relatively few undergraduate researchers; some have accomplished publishable research, but most have not.

DS: Back to the historical narrative. What other developments were there in the 1980s? Jeff Grossman came along.

JW: Yes, his interests enabled me to learn more about chondrules. I had been interested in chondrules for 10 years, and Jeff was a fine experimenter and collaborator. He separated chondrules, analyzed them by INAA, and then carried out petrological studies. We obtained bulk compositions but focused especially on volatiles. As a result of Jeff's research, and similar work by Jim Gooding and Klaus Keil, we learned that chondrules in ordinary chondrites had retained moderately large fractions of most volatiles; in terms of bulk compositions they are miniature chondrites. They seem to have largely retained the compositions of their precursor materials.

DS: Of course, not all chondrules are equal.

JW: Vive la difference.

METEORITICS AND MEETINGS

DS: In the late 1980s you became editor of *Meteoritics*, the journal of the Meteoritical Society. Do you want to say a few words about that? I recall that you took over from Carleton Moore after he had done it for 25 years.

JW: For years several of us had felt that *Meteoritics* needed to be upgraded in a way to make it a proper modern scientific journal with higher editorial standards and a more professional format. The council invited me to take on this task which I did with mixed feelings. It was time-consuming but I had the chance to choose the new standards. I proposed and the council agreed that it should closely follow the *Geochimica* style. I chose associate editors who would bring prestige to the journal. Thus the look on the inside was very similar to *GCA* including the unusual *GCA* reference style. An initial problem was making sure that we had enough papers to make it thick enough to print on the spine! As you know, there was eventually a major policy disagreement with the president of the society, Ed Anders, and I resigned. In the final issue that I edited I added an editorial clarifying the issues from my viewpoint. Ed demanded that I withdraw the editorial and I would not. He, with the help of the new editor (you) would not permit the printer to print and distribute the issue. However, after a delay of a couple of months, you persuaded him to give in.

DS: I think I wanted to wait until I was formally editor; your piece appeared in my first issue. What are your views on the journal and its history?

JW: I am generally happy with the present journal. It has achieved a prestige level close to what we wanted when we changed the style.

DS: I have heard it said, mostly while I was editor, that there are too many journals chasing too few good articles and this is driving the quality of published papers down. What is your view of this?

JW: *MaPS* gets many good articles but the mean quality in *GCA* and *EPSL* is higher. The role of *MaPS* is somewhat different. It can and should publish papers that are more descriptive.

DS: What about meetings? You have hosted two meetings of the Meteoritical Society. Could you say something about that? How important they are to the society and the impact they have on organizers and their careers?

JW: I like the fact that Meteoritical Society meetings are organized by our members, and I especially like it when organizers take advantage of institutional infrastructure to offer low registration fees and lots of opportunities for social interactions. At the 2002 UCLA meeting we offered food and drink to our attendees every evening, and we received much praise for this.

DS: The next papers that catch my eye on your publication list concern Antarctic meteorites. Can I get you to talk about that?

METEORITES FROM HOT AND COLD DESERTS AND TEKTITES

JW: Our data showed that ungrouped irons comprise a larger fraction of the Antarctic set than in the whole-world set. It was Roy Clarke who first pointed it out, that 30% of the Antarctic meteorites are ungrouped. Then, about 2 years ago there was a follow-up paper showing that the same pattern was found in the hot-desert iron meteorites from North Africa. What is now clear is that there are two kinds of ungrouped irons, those that formed in cores and those that formed in impacts. The reason the desert population differs is that the mean size is smaller and that impact melts are more common among small meteorites.

DS: So the Antarctic and North African meteorites are bringing new insights. It is not just large amounts of material, but there are fundamental differences.

JW: You know as I do that these large collections have yielded many new kinds of rare meteorites.

DS: Is this leveling off?

JW: Maybe. I am receiving fewer NWA irons now and irons from Antarctica have been quite rare in recent years.

DS: In the 1990s you became interested in tektites.

JW: They are much more fun than I expected. Early in my career I went to the Washington AGU meetings and there were always tektite sessions with John O'Keefe arguing that tektites were from the Moon. He was obsessed with this idea, despite compositional evidence to the contrary. We recovered samples from the Moon and they were nothing like tektites. Everyone's prejudices were confirmed, tektites were not from the Moon, and the world lost interest in them. It seemed that we knew everything we needed to know.

I gradually got interested in tektites because we had a visitor from Heidelberg, Otto Mueller, who had worked on tektites. He told me that tektites from different fields were compositionally different. He gave us some samples of what were called Muong Nong tektites (but I follow Virgil Barnes and use the descriptive term "layered tektites"). Mueller had shown that these had larger contents of volatiles such as B than the splash-form tektites. In the late 1980s I gathered my own data and confirmed the difference in volatile contents. (Fig. 7).

DS: These are the tektites that are just fragments of glass; they don't have nice teardrop or dumbbell shapes.

JW: Yes, the layered tektites are all fragments and some of them are quite large, up to 24 kg, far larger than the largest splash-form tektites. Thanks to my

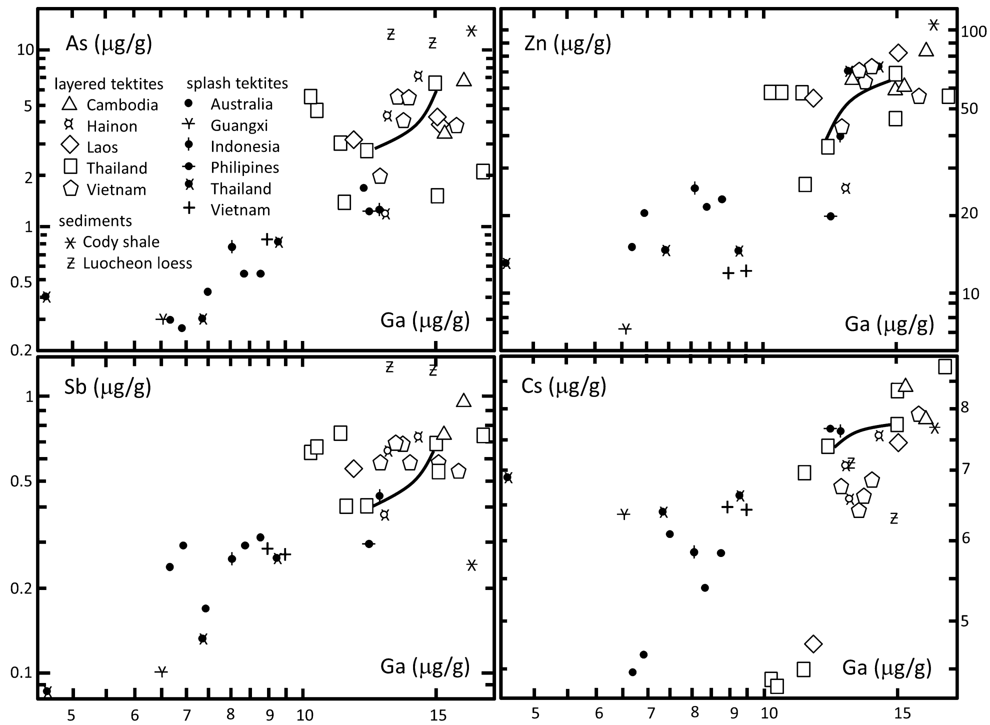

Fig. 7. Diagrams showing differences in volatile element concentrations between splash-form Australasian tektites (on the lower left) and layered tektites (on the upper right). (From Wasson 1991.)

efforts UCLA has the best collection of layered tektites from known locations.

DS: They have sedimentary compositions.

JW: Yes, the compositions of mean continental sediments. They may have formed by melting highly porous deposits of loess, windblown dust. The volatile contents of layered tektites are very similar to those in loess and shales; although they formed as melts they have retained their volatiles.

DS: Why are the layered tektites so different from the splash forms?

JW: Although Koeberl and others still believe the layered tektites were ejected from craters there are major problems with this view. There are no craters in Southeast Asia and it is impossible for a melt to retain its integrity during 500 km of flight as needed to explain the 1000 km length of the strewn field. I initially thought that they must have formed in a myriad of small craters but I gradually realized that a more plausible model is that they formed as a melt sheet heated by radiation from a Tunguska-like airburst. The layering reflects downslope flow; the thickest tektites are just puddles that formed in depressions. My estimate is that the whole area was covered by a mean depth of 3 mm, but it was thicker in some places and thinner in others.

I have made three field trips to NE Thailand. It is not hard to find small tektites. You look for a layer of laterite (a weathered soil) hardpan and you find them. In my main field area S. of Ubon Ratchathani all the small tektites are layered. There are no splash forms. I can think of only one explanation, which is that the splash forms fell into the melt sheets. The final nail in the coffin was the discovery by Greg Herzog and others that every Australasian tektite contains 1.5 Ma ^{10}Be at levels similar to those in local soils, proving that they are cannot have originated in a crater which mainly melted materials at the crater floor.

DS: How does your proposed airburst compare with Tunguska?

JW: It was similar but the mean fluence of heat was $>10\times$ higher and the area underneath the fireball was $>100\times$ greater in area.

DS: I would call your next decade, the decade of oxygen isotopes (e.g., Fig. 8).

OXYGEN ISOTOPES

JW: In my opinion, Bob Clayton's discovery of non-mass dependent oxygen isotope fractionations is the greatest cosmochemical discovery of the last 60 or 70 years. It totally changed our view of what could be found in nebular materials. The origin of these mass-independent fractionations are, however, still unclear.

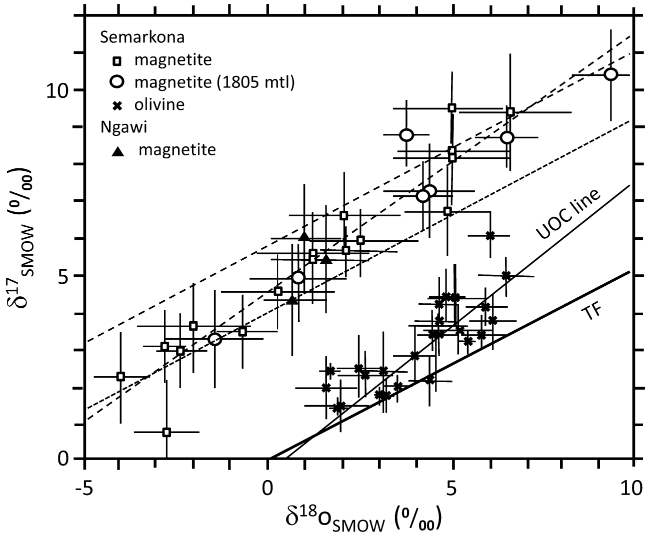

Fig. 8. Oxygen isotope measurements for magnetite and olivine from Semarkona and Ngawi expressed as a plot of $\delta^{18}O$ versus $\delta^{17}O$ with 1σ uncertainties; the terrestrial fractionation line, and a regression line through the UOC are also shown. The difference of approximately 4‰ between the $\Delta^{17}O$ values of magnetite and those in olivines are interpreted to mean that the magnetite formed by oxidation of metallic Fe by H_2O having $\Delta^{17}O$ >4‰ higher than mean values in the chondritic silicates. (From Choi et al. 1998.)

Fig. 9. Above, the Wasson group during a search for meteorites around Needles. (Left to right, John Wasson, Gudrun Wasson, Greg Kallemeyn, Gisla Wasson, Andrew Sears, Kerstin Wasson, John Willis, Willis child, Lauri Willis, Jeff Grossman, Hazel Sears, Clare Marshall, Zhiming Zhou). Below, John Wasson excavating a large mesosiderite at the Vaca Muerta site in the Atacama Desert of Chile.

My view is that they were inherited from the presolar molecular cloud, i.e., that the solar nebula formed heterogeneously. The newer data, especially ^{54}Cr mass-independent anomalies, demand such a model.

DS: This is a great point to stop the historical narrative part. I don't want to go any further because I don't want to touch on current and unpublished research. That's for the peer review process. I do have some general questions though. Close your eyes, forget about your own career and you contributions, what are the five major events in what we now call cosmochemistry since you entered the field?

CLOSING THOUGHTS

JW: Well, I have already mentioned oxygen, which goes to the top of the list. Certainly, the discovery that short-lived isotopes and especially ^{26}Al were alive in some meteorites is important. The age data of various sorts that tell us just how old the solar system is provide major constraints. The more recent data for Hf-W has provided for a lot of stimulating details about how the asteroids and planets evolved. It is great that we have a chronologic system that dates the fractionation of a lithophile parent from a siderophile daughter. Then, of course, presolar grains are important. They have provided many important insights into the way isotopes performed and they demonstrate that appreciable matter accreted to the nebula without evaporating. On the other hand, I make relatively little use of the isotopic patterns found in presolar grains because I am primarily interested in rock-forming processes going on inside the solar nebula, not in the nucleosynthesis processes in stars.

DS: If you look at planetary science as a whole what are the major advances? I am thinking particularly of space missions. What has been the impact of missions on cosmochemistry and your work in particular?

JW: Well, cosmochemistry, broadly defined, has profited enormously from the Apollo missions to the Moon. The recent Mars landers, especially those that demonstrate the presence of water, have been quite important. The photographic missions to asteroids have helped understand space weathering and, together with radar observations, have confirmed that low-density, rubble-pile asteroids are common. Until now I would say that these missions, except for those to the Moon, have not had much impact on my research, but I hope this will change when we have sample return from asteroids. I hope I am still active when the first asteroid samples start coming back in reasonable amounts. We really need ground-truth rocks.

DS: Talking of bringing back extraterrestrial samples, I wanted to end on mentioning that among my memories of our first few months in the U.S. is a trip into the desert to search for meteorites. Meteorite hunting is something you have a taste for (Fig. 9).

JW: I did once find a Canyon Diablo iron with a metal detector. The image you provide was made on one of our annual "Meteorite and wildflower hunts" to Neenach in the Antelope Valley north of Los Angeles. We never were able to find a piece of Neenach but we found square kilometers of California poppies.

DS: Well, John, thank you again for doing this. It has been a hard-worked few hours on a day when you had plenty of serious matters to attend to.

JW: Thanks for coming by, Derek, and I am truly grateful—on behalf of the society—for your efforts.

Acknowledgments and notes—This interview was recorded on February 6th, 2013, and edited by the author and JW. I am grateful to NASA for financial support and to John Friedrich and Hazel Sears for reviews, and Hazel also for proofing.

Editorial Handling—Dr. A. J. Timothy Jull

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