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Report

Oral Histories in Meteoritics and Planetary Science – XVII: Joseph Goldstein

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Abstract–In this interview, Joseph Goldstein (Fig. 1) recounts how he became interested in meteorites during his graduate studies working with Robert Ogilvie at MIT. By matching the Ni profiles observed across taenite fields in the Widmanstätten structure of iron meteorites with profiles he computed numerically he was able to determine cooling rates as the meteorites cooled through 650–400 °C. Upon graduating, he worked with a team of meteorite researchers led by Lou Walter at Goddard Space Flight Center where for 4 years he attempted to understand metallographic structures by reproducing them in the laboratory. Preferring an academic environment, Joe accepted a faculty position in the rapidly expanding metallurgy department at Lehigh University where he was responsible for their new electron microprobe. He soon became involved in studying the metal from lunar soils and identifying the metallic component from its characteristic iron and nickel compositions. Over the next two decades he refined these studies of Ni diffusion in iron meteorites, particularly the effect of phosphorus in the process, which resulted in superior Fe-Ni-P phase diagrams and improved cooling rates for the iron meteorites. After a period as vice president for research at Lehigh, in 1993 he moved to the University of Massachusetts to serve as dean of engineering, but during these administrative appointments Joe produced a steady stream of scientific results. Joe has served as Councilor, Treasurer, Vice President, and President of the Meteoritical Society. He received the Leonard Medal in 2005, the Sorby Award in 1999, and the Dumcumb Award for in 2008.

DS: I am going to start by asking you the same question Ursula Marvin always opened with, what first made you interested in meteorites?

JIG: I acquired an interest in meteorites during Ph.D. studies with Robert Ogilvie, my advisor at MIT. I was primarily interested in some metallurgical phenomena, mass transport, diffusion, but I was also interested in the instrumentation needed to measure compositions and compositional variations on the micrometer scale using the electron microprobe. This instrument was new and Ogilvie was one of the few people pioneering such techniques.

DS: This was the early 1960s.

JIG: Yes. There were no commercial electron microprobes available at this time. People were making their own. So with the instrumentation available and an interest in mass transport, the easiest objects to measure were iron meteorites with their large sized Widmanstätten pattern (Fig. 2).

DS: Everyone comes to meteorites because they are simple then they learn otherwise.

JIG: No, no. They are simple. You can see the structures and you can measure the profiles. So I got involved making measurements with the microprobe. It was very slow collecting data in those days, very time consuming. I set up a program to measure diffusion coefficients appropriate to iron meteorites and determine the Fe-Ni phase diagram (Fig. 3). I was able to measure the diffusion gradients in a couple of iron meteorites (Fig. 4).

DS: Do you remember the meteorites?

Fig. 1. Joseph Goldstein.

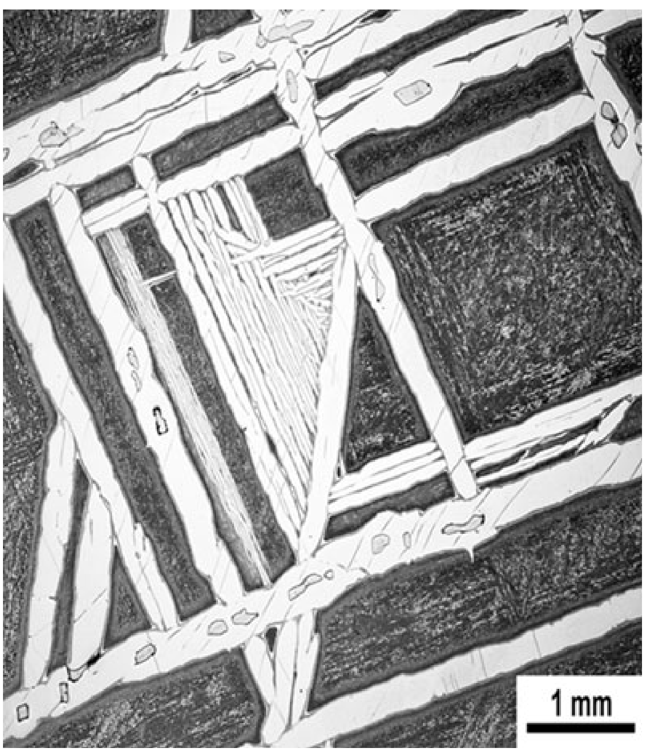

Fig. 2. The Widmanstätten pattern in the Carlton iron meteorite.

JIG: They were Grant and Toluca.

DS: Tell me about how you made the measurements. Now we have electron microprobes that have microcomputers with sophisticated software and elaborate graphics.

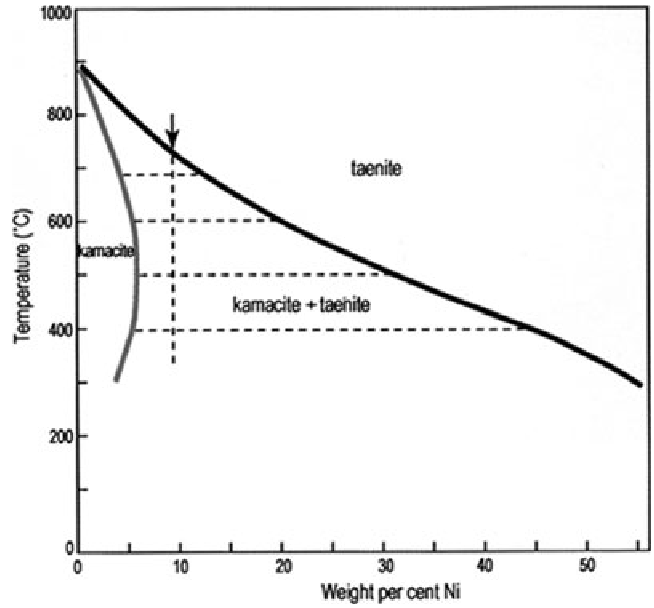

Fig. 3. The iron-nickel phase diagram. The kamacite and taenite phase boundaries are shown.

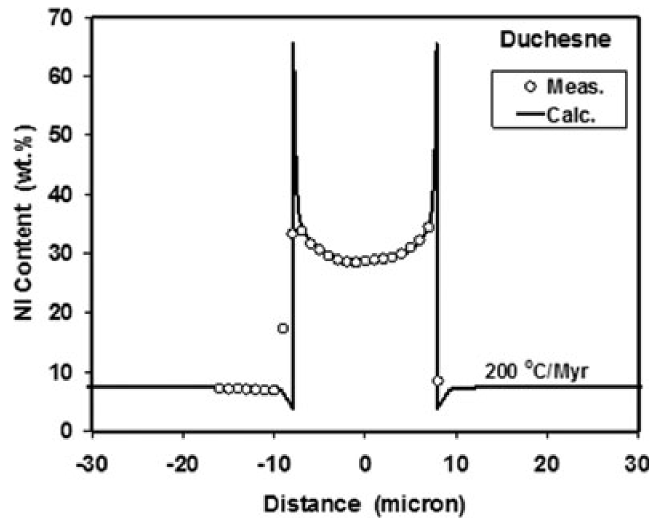

Fig. 4. Comparison between measured and calculated Ni profiles across the taenite phase in the Duchesne IVA iron. The metallographic cooling rate is 200 C/Myr.

JIG: It is the same basic technique but you could only measure one element at a time and you had to move the sample stage by hand with a micrometer stage, one micron at a time.

DS: When I came into this field, sometime later than you, I was taught to use the microprobe by Howard Axon, I was given 10 Fe-Ni alloys and told to make a calibration curve.

JIG. Yes. That's how it was done. International Nickel made up these alloys and I still have some of them.

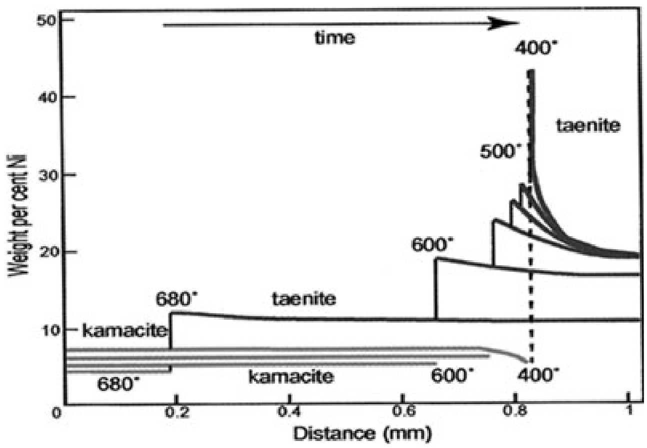

Fig. 5. Growth of the M profile. Ni profiles develop as temperature decreases during cooling in a parent asteroidal body.

DS: There was no ZAF correction program; maybe we had Bence-Albee?

JIG: No, those came later. Tom Ziebold was a graduate student in Ogilvie's group and worked with me on this. We produced calibration curves and fitted them with a quadratic equation and used them to make corrections. With iron-nickel it was pretty straightforward and we had the theoretical corrections set up by Castaing which we used.

DS: You were an undergraduate at MIT. Were you born in Massachussetts?

JIG: No. I was born in upstate New York.

DS: And your Ph.D. thesis was the work you have just described on iron meteorites. Was that where your first cooling rates were from?

JIG: Yes, yes. The problem was finding ways to solve the diffusion equations because time and temperature were constantly changing? The analytical equations assume isothermal conditions and available literature data were all for isothermal conditions. Those data had been available for years. Solving these equations needed a computer (Fig. 5). John Wood did a similar thing.

DS: John was working as a postdoc at Chicago at that time?

JIG: I think so. He was mostly working with the metal in chondrites. He was interested in iron meteorites, but to him they seemed simpler and less interesting than chondrites. He published first published on iron meteorites in 1964 then on metal in chondrites in 1967.

DS: He was mostly measuring central Ni compositions and comparing them with taenite grain size, whereas you were matching profiles?

JIG: That's right. We set up these equations and solved them on the biggest IBM computer we could get our hands on at MIT, but it was still very slow work. Now you could do these calculations on a microcomputer in 30 seconds.

DS: Tell us what it was like at MIT during your graduate years. Was it a large team?

JIG: Yes. That was a magical time for Ogilvie at MIT. He had four graduate students. There was plenty of money, all the students working on different problems using the microprobe. At the same time he was developing new instrumentation, the scanning electron microscope.

DS: This was all in the Metallurgy Department?

JIG: That was at about the time metallurgy changed its name to Materials Science.

DS: Who else was in the group? Were any of the other folks interested in meteorites?

JIG: No. One of the students, Rob Hanneman, was a coauthor on the Fe-Ni diffusion paper. He also measured diffusion coefficients, but worked for MIT Lincoln Labs where they had high pressure apparatus. So we were able to measure the Fe-Ni diffusion coefficients at high pressure. There was also a fellow called Uhlig at MIT, who believed meteorites formed at high pressure.

DS: But these are solid-state processes. Would pressure be important?

JIG: Well he was thinking about pressures at the center of the Moon.

DS: Did you see any pressure-dependence?

JIG: Oh yes. It's a good order of magnitude at high temperatures in our diffusion measurements. So I made those high pressure measurements.

DS: So in your thesis you had data for the two meteorites.

JIG: Before that I did a master's thesis on phosphides in iron meteorites, their compositions and formation details. We relied on a 1935 phase diagram from Germany. That research was one of my first publications.

DS: Then for your Ph.D. thesis work you had the iron meteorite work and cooling rates. The results were not too different from those that Wood was getting for the chondrites, right?

JIG: That's right and he was getting similar values for irons too. There was a Gordon Conference, I think in the summer of 1963, where Wood and Uhlig's post doc were presenting their technique. I was not on the program but I went to the meeting. I arranged my honeymoon in New England so that I could go to this one session of the conference!

DS: Many members of the Meteoritical Society combine their annual holiday with the meeting, but not their honeymoon!

DS: Where and when did you meet your wife?

JIG: A couple of summers before in Syracuse, NY. I'd gone home. We've been married almost 50 years.

DS: Congratulations.

JIG: To her especially.

DS: So you graduated.

JIG: Yes. I had some really nice job opportunities, one at US Steel and one at IBM in Yorktown, both research positions. Then an opportunity came up to work at NASA Goddard Space Flight Center where there was a rather large group headed by Lou Walter. He had five or six Ph.D.-level people in the group.

DS: What was going on in his group at that time?

JIG: He was building up a broad group for meteorite research and it seemed like it would be fun although the salary was about 30% less than the industrial jobs. I spent 4 years at Goddard.

DS: What were your projects at Goddard?

JIG: I could do anything I wanted. I built some heat treatment furnaces. One of my major objectives was to determine the iron-nickel-phosphorus phase diagram. I was convinced that the phosphorus was critical to the formation of the Widmanstätten pattern.

DS: Up to this point we were just looking at iron-nickel phase diagrams?

JIG: Yes. There is also cobalt, carbon, and sulfur in the Fe, Ni alloys of meteorites, but the really critical element is phosphorus. While waiting for the laboratory to be ready, I did some trace element work using the microprobe, germanium and other elements.

DS: You published a paper in *Science* on the Ge in an iron. One iron had anomalously high Ge so you could detect that element with the electron microprobe.

JIG: One percent or so, easy to measure.

DS: So you are working with the furnaces, mixing up various materials, and working your way across the phase diagram.

JIG: Yes. I would heat treat alloys of various compositions and look at the microstructure. We also grew the Widmanstätten pattern and obtained alpha [kamacite] plates in the correct orientation.

DS: What were the other people in the group doing?

JIG: Age dating, oxidation-reduction reactions, mineralogical/petrological work. Bevan French and John Philpotts were in the group.

DS: When I hear Philpotts I think of rare earth elements.

JIG: Yes, he was measuring rare earth elements. There was also Phil Cressy, who was working on cosmic ray exposure ages.

DS: I recognize all these names.

JIG: Yes it was a great time. I had a colleague at the National Bureau of Standards, Harvey Yakowitz, who was a microprobe specialist who worked with me on developing methods for corrections. At that time the Microbeam Analysis Society was formed and I was involved in that effort. I was at Goddard from 1964, when I got my Ph.D. until 1968. All the time I was looking for a teaching job. I knew that was the position I wanted.

DS: The main difference between a NASA base and a university is the students.

JIG: Well, in addition Goddard was not the place to study meteorites. Goddard's job was to build satellites. That's where the long-term vision was. Earth observation satellites, weather satellites, and so on. That was Goddard's specialty.

DS: Low Earth orbit missions.

JIG: Yes. So what we were doing was an outlier and I didn't see how I was going to progress. So I started looking for a teaching position. I taught a couple of courses at the University of Maryland. I taught an X-ray diffraction course, which was fun. They also let me use the golf course. So that was good. It made me feel more productive. We were approaching 1969 and the Apollo era. Everybody was tooling up. I thought the group should move to Houston. That was my personal view.

DS: Houston was building up a pretty big group.

JIG: Yes, but the last thing I wanted to do was to move to Houston. It was just not where my wife and I wanted to live. So I eventually got a teaching position at Lehigh University, which has one of the best materials science departments in the country. I was very pleased to be able to go there

DS: Did you have anything in the way of a laboratory ready for you to use?

JIG: They had an electron microprobe. That was the reason they wanted me. They had a very large NSF grant to build a stronger materials science department. They hired about five or six people bringing the department to fifteen or sixteen faculty members. The electron microprobe was the newest instrument on the market and they were purchasing one.

DS: Straight to meteorites?

JIG: I immediately went to work. I needed some samples to put on the microprobe in preparation for the return of lunar samples. I wanted to get started, and I wanted some funds, so I went to NSF to get a grant to study mass transport and diffusion. I got support to measure the iron, nickel, phosphorus diffusion coefficients.

DS: This is a continuation of the Goddard work.

JIG: Yes. A nice aspect of Lehigh was that they had two or three faculty there who were working on problems related to mass transport. Some of my initial projects involved tool steels and other materials of interest to the metallurgical community.

DS: This is something I have been curious about. We have this whole aspect of meteorite studies, which is the metal phase and it seems to me—and I spent 3 years in a metallurgy department—that there are relatively few people studying metal and yet there is critical information. I don't know that there is any other satisfactory way in which you can get a cooling rate, for example. Maybe chronological techniques with different closure temperatures, thermoluminescence will give you a qualitative idea, but in terms of a direct measurement the metal studies is it.

JIG: You are talking about the irons?

DS: I am talking about meteorites in general, irons and the metal phase in chondrites.

JIG: The advantage of the metal is that it continues to react at lower temperatures.

DS: And it is a huge range, starts at 900 °C and goes down to below 400 °C.

JIG: For the Widmanstätten pattern it's about 650–400 °C

DS: I guess what I am getting at is why are there not more metallographers in the business?

JIG: There used to be a more competitive environment.

DS: There was you, Vagn Buchwald, Ed Scott, John Wasson did the geochemistry, John Wood did some. Were there any others?

JIG: Most of the people who come into this business are geologists, geophysicists, geochemists, astrophysicists.

DS: So you are on the runup to Apollo, you have a working probe lab, you have done the work with the M profiles, you have made the measurements and you have the cooling rates.

JIG: Yes, we looked at the various chemical groups.

DS: You worked with John Wasson at one time?

JIG: No. John was measuring the chemical groups of the irons, looking for the chemical information that told him something about core formation, how asteroids developed and so on. John's major contribution to the study of irons was the determination of the chemical groups and what iron meteorites came from different asteroids. His work really helped set up the experiments I wanted to do, so we could get cooling rates for various groups. His work was extremely important but I never worked with him. From the beginning we never saw eye to eye on cooling rate measurements, and how the irons cooled in their parent bodies.

DS: All right. So are we ready for the Apollo samples now?

JIG: Yes, As I was continuing to work on meteorites to develop the Fe-Ni-P phase diagram, the diffusion coefficients and the microstructures, we worked on the Apollo samples. I decided the Apollo samples were not appropriate for study by students. For the most part, all the lunar work I did myself, with a student to help here and there. And it was always a rush. Take the data, have a paper ready for the meetings, interact with other people, talk about your results. You needed a considerable depth of knowledge to do any work with the soils.

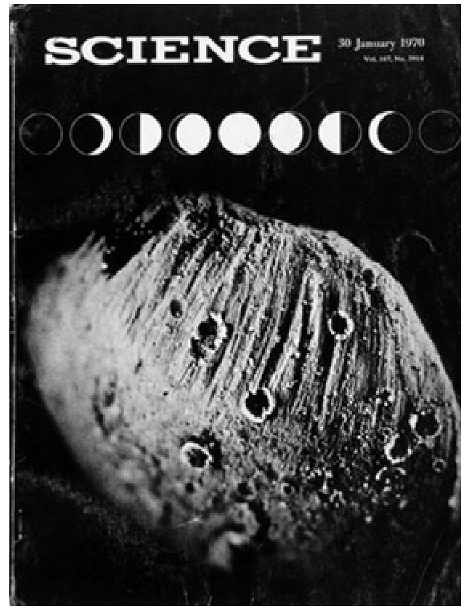

Fig. 6. A large metal particle in the Apollo 11 soils. This picture appeared on the front cover of *Science*, which collected the first papers on the Apollo 11 samples.

DS: There were only about 6 months between Apollo 11 and the first lunar science conference?

JIG: Yes, things were really moving.

DS: Then they were launching the next mission before the scientists had reported on the last; there were only 6 months between missions.

JIG: Yes, I got NASA support for postdocs. This is where we got the money for John Friel, Roger Hewins, and L. Lin. We all worked together. We worked on the lunar soils, I did some experiments, but there were very few students involved. I couldn't see a major problem they could handle. During that period I got a NSF grant to bring out Howard Axon to Lehigh. He spent a year with us looking at the metal in the soils as well as some iron meteorites. This was early 1970s.

DS: I went to work with Howard in 1974, and it was a year or two before that.

JIG: Yes, that's right.

DS: The major thesis of your lunar work was to look for meteoritic component.

JIG: Right. We looked for the chondritic metal component in the soils (Fig. 6). Then we moved to examining metal microstructures. Metal particles had been melted and that was analogous in part to the microstructure of Canyon Diablo micro-spherules. The best man at my wedding and my roommate when I was a

Ph.D. student, Hal Brody, was an expert in solidification. One of his papers concerned the dendrite spacing of Fe-Ni alloys. You can get cooling rates directly from the dendrite sizes. A friendship built up over the years spilled over into science cooperation. We found the dendrite microstructure in melted metal-sulfides in all the soils. In addition soils from Apollo 16 had a large amount of metal to study.

DS: Really. What does that translate to in terms of meteoritic component?

JIG: The amount was still quite small.

DS: Anders and others were using siderophile element abundances in the soil to determine chondritic component and generally came up with about 2 vol%.

JIG: Yes, well there was so much mixing and so much melting it was never clear that you could differentiate meteorite component from lunar component.

DS: Your main criterion was the Ni-Co plot. Howard was still excited with these results when I arrived there in 1974. The last Apollo was 1972 and you were still writing papers with him in 1974. I arrived at Manchester with a suitcase full of TL residues, the ground-up powder and the magnetic extracts, and I showed Howard the metal grains and he said, "Go and do what Joe and I have been doing to the Apollo soils." I don't know how many people would go about studying the metal in chondrites by first removing it! Well I guess it concentrates a lot of material in a single mount so our chances of finding rare grains increased. We found some extraordinary things. There was the incredible range of textures, and the high Co in the kamacite.

JIG: Yes, there was a lot to mine in the chondrites. John Wasson did a paper on the Co contents, with one of his students.

DS: Affiatlab and Wasson picked it up. That's the paper of mine that got away. I did a huge amount of work as a postdoc with Howard, we wrote a letter to *Nature* and two-pager in *Meteoritics* and then...

JIG: … John took it and ran with it.

DS: A lot of good data came out of it. Alan Rubin picked it up later. He is now finding enormous values for Co in the kamacite, up to about 40% for these highly oxidized LL chondrites.

JIG: Right.

DS: So you did the chondritic component of the soils with Ni-Co then you found these particularly interesting microstructures.

JIG: In the Apollo 16 soils there were interesting microstructures containing phosphides and phosphates and John Friel did some oxidation state studies.

DS: Yes with coexisting phosphates and phosphides you can do some real chemistry. So you did all this work on Apollo samples, you published maybe a dozen papers, then what? Back to iron meteorites?

JIG: Right. During that period there was a lot of work trying to move from the electron microprobe to the analytical electron microscope. I wanted to get better chemical spatial resolution. As you get to lower temperatures and diffusion rates become very slow, the profiles become very steep and you need finer spatial resolution. I got very involved in that. A Lehigh colleague of mine, Dave Williams, and I were building our own instrument. When I was on sabbatical in England at Cambridge with Stuart Agrell, I often visited Graham Cliff and Gordon Lorimer at Manchester. I would get on the train and go up to Manchester. We would analyze thin sections, the first thin sections of iron meteorites.

DS: How thin is a thin section of an iron meteorite?

JIG: About 50 nm. Peter Duncumb had developed an analytical electron microscope. I saw it in 1965 after I went to the Paris international electron microprobe meeting. We went to his lab and I saw it. I said, "I have got to have one of those." So when I did my sabbatical in England I used a commercial version of his microscope in Manchester with Cliff and Lorimer. They developed the techniques for how to make the quantitative chemistry measurements. So I learned how to make thin sections in the metallurgy department at Cambridge. Then I could go to Manchester and analyze the samples. I published many papers with my colleagues; technique, quantification, how to run the microscope to get the best data, minimizing continuum backgrounds, and so on.

DS: This is while you were still in England?

JIG: No, by now we had a commercial analytical electron microscope at Lehigh.

DS: I want to get some sense from you of life in Cambridge at that point. Was Ed Scott in Cambridge then?

JIG: No, Ed had just finished his degree there and had gone to UCLA to work with John Wasson.

DS: Stuart Agrell was nearing the end of his career then. Was he winding down or still very active?

JIG: We were working on lunar samples together. My purpose was to learn electron microscopy. Stuart was in geology and I spent most time there but I would go over to metallurgy, trying to learn. That is what sabbaticals are for, changing directions, learning new techniques. I also learned a lot from Gordon Lorimer at Manchester.

DS: Now back at Lehigh, you started up the SEM workshops at about that time?

JIG: We needed very badly to get a scanning electron microscope. I had to figure out an approach to do that. But we needed to train students and Ogilvie at MIT had decided that he could no longer teach a course in scanning electron microscopy and microanalysis there.

We agreed that we would just take that course over and do it at Lehigh. Our plan was to teach the course, get all the faculty involved, and then go to our vice president for research for the money and tell him we have got to have one of these instruments. Then we could write a proposal and he would give us the start-up money. It worked, we got a commercial instrument.

DS: So you are back to working on the M profiles in iron meteorites. Are you now revising those cooling rates?

JIG: These computer models were criticized because there was no ground truth. In other words, you may have phase diagrams and diffusion coefficients, but you have no proof that this is the way the Widmanstätten structure formed.

DS: Agreement between the theoretical curves and the meteorite data is not proof?

JIG: No, that's not proof. You have to assume the model works. What you have to do is see if you can make samples, where you know the cooling rate and you know the chemistry, and measure the gradients. You don't have millions of years, but there is no reason to believe that a kamacite band a micron wide is any different from one that is a millimeter wide, so long as it is made under the same conditions. I think I started this work with an undergraduate doing the optical and scanning electron microscopy.

DS: Now these micron-sized kamacites you are making are they needles or plates?

JIG: They were plates and they were giving nickel profiles that were perfectly analogous to those we were seeing in the iron meteorites.

DS: And again, you had iron, nickel, and phosphorus.

JIG: Iron, nickel, phosphorus and we could measure the orientation and the chemistry with the analytical electron microscope on the nanometer meter scale.

DS: So what was the outcome of this work for the meteorites and their cooling rates?

JIG: It showed that the modeling we were doing was correct.

DS: When did you leave Lehigh?

JIG: 1993. I was there 25 years.

DS: I didn't realize it was that long. You kept the workshop going that long?

JIG: It's still going. It's being offered this summer, although I think it's time to phase it out.

DS: Because?

JIG: Oh, because nobody cares about the theory any more. Nobody cares how the lenses work. That sort of thing.

DS: These machines have become turn-key?

JIG: I wouldn't call them turn-key but they are pretty easy to run.

DS: You don't have to plot your calibration curves by hand …

JIG: You can train students to run these machines; without their having to know how they work.

DS: Do they need to?

JIG: Well, that's the question. I think they do but not if they use in routine. However, anyone who wants to use that instrument seriously should know the details.

DS: So you are saying there is still a need for the workshop but people are just not taking it.

JIG: Most students are just not interested in the theory. In any case, we bring in world class people to teach these workshops but it is not clear that typical students need that level of expertise any more.

DS: I understand.

JIG: So I think in the long run this course needs to change to suit a new audience. In addition, the manufacturers are now running their own courses and they argue that the students don't need all the sophisticated physics.

DS: I learned Algol before Fortran, but I just don't need it any more. At some point these techniques become obsolete.

JIG: The techniques do not become obsolete, they just become routine.

DS: Tell me about why you moved to Massachusetts.

JIG: That's an interesting question. I think the reason was that I was moving from an administrative position back to the faculty.

DS: Oh, so you had an administrative position at Lehigh?

JIG: Yes, I was their Vice President for Research for about 8 years, up until about 1990. When I went back to the faculty it became clear to me that a decade away from research made it difficult to get back into the general field of metallurgy. When I went back to my department I was not as good an engineer as I was at my prime. So maybe it was time to do something different. I decided to compete for a deanship and eventually got a position at the University of Massachusetts and stayed on the administrative track.

DS: How long were you dean at the University of Massachusetts?

JIG: Eleven years.

DS: Eight plus eleven, a healthy chunk of your career. But your meteorite research papers were still coming out during that period.

JIG: Yes, I was still publishing meteorite papers.

DS: I don't know how many deans continue to publish?

JIG: Not many, and my research efforts were not appreciated, at least not by the last Provost I worked for.

DS: You should have been spending the time on administration?

JIG: No, it just didn't matter to her.

DS: What about your department, you were assigned to a department. Didn't they care?

JIG: No, they didn't care at all.

DS: So this was just a labor of love for you.

JIG: And I was lucky enough to keep my grant going. NASA knew my productivity was not as high as it used to be. However I had some good students and several high quality post docs.

DS: At the end of your deanship you went into the department.

JIG: The problem with that is that they do not have a materials science department.

DS: So you went to mechanical engineering?

JIG: They have a polymer science department, which is a branch of materials science, but I know nothing about polymers.

DS: Most departments have closed down their polymer science. What was your research as you went into the department?

JIG: Oh I just expanded my meteorite work. I developed many more collaborations, connected with other research teams so that I could work on some new and exciting problems.

DS: That's an interesting avenue to pursue in its own right, collaborations with others in the field.

JIG: I wanted to do analytical electron microscopy at the highest level. Now we have the focused ion beam instrument to make thin sections in select micron size regions. I developed a collaboration with Al Romig who at that time worked at Sandia National Labs. I did a sabbatical there and now I go two or three times a year with a whole bucketful of meteorites, each one of them with a story that is really important. We did the IVA cooling rates for example and also the IVB irons, and the ataxites, for which you can't obtain the necessary Ni gradients with an electron microprobe.

DS: Is the kamacite big enough?

JIG: You can see it with the electron microscope but you cannot get many analyses when the kamacite is only a couple of microns wide. But with the analytical microscope you can do a lot of good work

DS: Were you able to get a cooling rate out of that.

JIG: Yes, we've just published that research. In recent years I have worked with Ed Scott who is interested in planetary issues. So I often obtain the data and the two of us work on finding out what the data are telling us. We have just finished a summary paper on iron meteorites. We got together with Nancy Chabot to write this review paper that I thought was really needed. It's a modern, up-to-date review of microstructures, cooling rates, and ages.

DS: Talk to me about the history of cooling rate determinations. You started out with really slow cooling rates and at some point in your career you dropped them a couple of orders of magnitude.

JIG: Right, because when you apply the Fe-Ni-P phase diagram and corresponding diffusion coefficients, everything speeds up. The diffusion coefficients are faster and you have a different phase diagram when phosphorus is in the system.

DS: Are they settled down now, do you think?

JIG: I expect we got them right this time! John Wasson doesn't think we have.

DS: You had some exchanges with him on IVAs, didn't you? With John Willis. John was his graduate student and then he was a postdoc with you?

JIG: That's right. Willis and I worked on chondrites as a matter of fact.

DS: Most of what you have been talking about has been subsolidus, but you have been involved in solidification processes? Dendrite formation?

JIG: Yes I had a postdoc and my colleague Hal Brody, the best man at my wedding, had a Ph.D. student and I took him over as a postdoc. We started to do single crystal solidification experiments to see where the Ge, P, and C would go during solidification.

DS: So you could reproduce the patterns you see in iron meteorites.

JIG: That was the idea. At the same time Nancy Chabot and Mike Drake wrote a whole series of papers measuring the distribution coefficients. We were actually doing the experiments. We got a lot of flak from their group. They said that our experiments were not at equilibrium, and I kept saying that the meteorites are not at equilibrium either during solidification. We were actually trying to simulate what the meteorites had experienced.

DS: It's an empirical calibration.

JIG: Yes.

DS: Talk to me in the most general terms about how you think meteoritics has progressed throughout your career.

JIG: Well, it seems to me that as we got more and more sophisticated in our analytical instruments the more we got cooling rates that were more consistent with what people were thinking were reasonable for asteroids. A few years ago when meteoriticists started measuring ages, Hf-W we realized that irons actually formed before chondrites. At that point things started to get really interesting for us. I mean, that was a really big surprise. You know we are getting cooling rates of 1000 °C/Ma and people were saying they cooled in a couple of million years and we had actual examples of an asteroid that had gone through this process. So that was very exciting. It's taken on additional interest. You know, there were some years when I was the only person writing papers on iron meteorites. It was disheartening, let's say.

DS: That was going to be my next point. How do we get more young folk thinking about metallography?

JIG: Well that's a problem. Many of the original scientists in this field are no longer with us. Howard Axon has passed away. Vagn Buchwald has stopped working on meteorites. John Wasson and I will be retired in a few years. Even Ed Scott is approaching sixty!

DS: Okay, so let's press reset on your career and you can start out over again, what would be you vision for the future of extraterrestrial metal over the next four decades.

JIG: Well, we can do more of the same. Instrumentation is constantly improving. But I think that the real success in recent years has to do with developing teams. I think it is really difficult now to be a lone researcher. I may be an expert in metals, but if I don't talk to someone who knows about silicates, I am dead in the water. We also need people who can operate these major new analytical instruments. The groups that have developed in Hawaii, Chicago, Tucson, etc. these are the places where the excitement will take place.

DS: Large, integrated, multidisciplinary, well-equipped groups.

JIG: We need to be training young folk to understand the various materials, sulfides, silicates, metal.

DS: I have been told I can't do TL without making a section first.

JIG: Is that not a good idea?

DS: It's an excellent idea.

JIG: That's the point. People should be more broadly educated. The difference between silicates and metal is that the metal continues reacting to lower temperatures. I have been having this fist fight with Wasson. He talks about blocking temperatures where there's no more mass transport. I keep saying, "It's just not true." With metals it just keeps happening. I have always wanted to measure mass transport of minor elements in iron-nickel.

DS: Is there another phosphorus out there, a minor element we haven't considered that could have a major effect on the formation of the Widmanstätten pattern.

JIG: Carbon. Carbon is the greatest issue now. We are doing some experiments, but I just don't have another 10 years in me.

DS: You don't?

JIG: No. You get to a certain point in life when you realize there are some things you will never do, unless you make time now to do some of those things while you still have a few bucks in your pocket. You realize that you have had a good career. You have made some contributions.

DS: So this is the perfect time for you to be doing this interview?

JIG: Yes, oh yes. Perfect. I have obtained a renewal of my NASA grant and that will enable me to work on some of the last problems that I want to tackle. I now have a new SEM at UMass (Fig. 7). The Magellan field emission SEM is a world class field emission instrument with 1–2 nm image resolution. It is fully outfitted with a large size Oxford SDD EDS and Electron Backscatter Diffraction. We can do orientation relationships, X-ray maps, and high quality X-ray work. I would also really like to write a book on meteoritic metal, probably in conjunction with my colleague Ed Scott. Not as good a book as Buchwald wrote but a book that describes how the metallography of the irons has developed.

Fig. 7. Joe Goldstein at the Smithsonian Institution during the cutting of the Old Woman meteorite.

DS: Something like, "The metal phase of extraterrestrial materials."

JIG: Something like that, with a couple of other people, with really nice pictures. The pictures will need to be taken afresh, now we know what we want to say.

DS: One of my impressions of Howard Axon is that he had the handbook of S. H. Perry and he would look at it with a hand lens.

JIG: Yes, well, Howard was quite an incredible individual but I couldn't get him to use some of the newer techniques.

DS: He had a particular way of going about studying meteorites. For him it worked. He would train his one student at a time and that was the way he functioned.

JIG: I felt he had more to contribute in a more general sense than he did. I mean, he knew so much but he never put it together. We wrote a review article together but it wasn't very long. He just did not want to spend his time in that way.

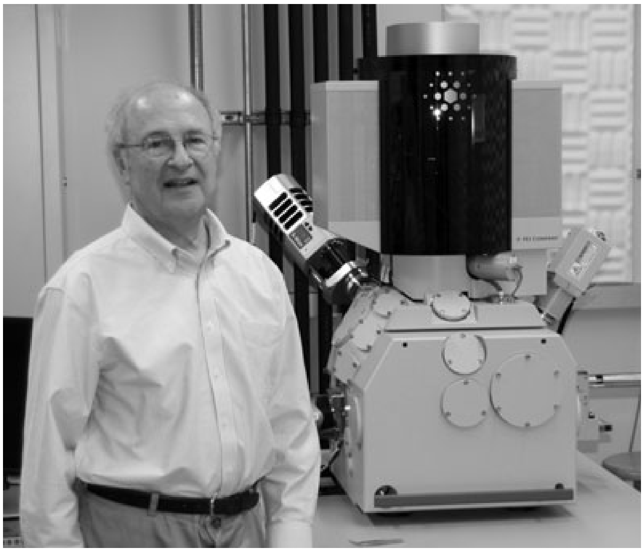

Fig. 8. Joe Goldstein with his recently purchased Magellan field emission SEM.

DS: He was a very happy guy. He didn't have unsatisfied ambitions. He taught metallography, did it very well.

JIG: He was happy doing it, but I always thought he could do more for the field. He had more to say. You know, I am working on haxonite right now.

DS: Really! Is there a goldsteinite anywhere?

JIG: No, no.

DS Tell me about the Meteoritical Society.

JIG: I think that as an officer I get more out of the Society than it got from me.

DS: Really.

JIG: I really enjoyed the relationships. I was a councilor way back. I think in the 1970s. That was when there was a Met Soc Lehigh meeting. I did that with Charlie Sclar. Just after the Apollo missions started. That was really fun. We took everyone down to Bethlehem Steel. You could see how the cores of asteroids behaved, how solidification proceeded. It was a really nice tour. Roy Clarke brought a whole slew of samples from the Smithsonian (Fig. 8). We put them in our library on exhibit all week; really beautiful slices. Then I didn't do anything for decades, besides going to the meetings. Then starting 10 years ago I began to get involved, first as the Treasurer and then 5 years after that as Vice President and President.

DS: You said you got more out of it than you gave.

JIG: Yes. I really enjoyed the people, the accomplishments, being in a society where people really care, it is different.

DS: Different than?

JIG: Well there are many organizations I could mention, but do you know any other professional societies where 10–20% of the membership is on a committee? It is unheard of with larger societies.

DS: Lot of engagement. Lot of ownership. I have a theory that meteorite specialists are loners in their departments so they develop special relationships with the people who share their research interest. I know there are meteorite researchers on the other side of the world I know better than I know people down the corridor.

JIG: In many ways that's true. Absolutely. I remember the first Meteoritical Society I went to was at Arizona State, Carleton Moore hosted that one, 1965 I think. That's where I met Peter Buseck, a relationship which has developed into a lifelong friendship.

DS: Talking of lifelong friends in the field might be a good place to end this interview. Joe, thank you very much.

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