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Report

Oral Histories in Meteoritics and Planetary Science – XV: John Wood

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Abstract–John Wood (Fig. 1) was trained in Geology at Virginia Tech and M.I.T. To fulfill a minor subject requirement at M.I.T., he studied astronomy at Harvard, taking courses with Fred Whipple and others. Disappointed at how little was known in the 1950s about the origin of the earth, he seized an opportunity to study a set of thin sections of stony meteorites, on the understanding that these might shed light on the topic. This study became his Ph.D. thesis. He recognized that chondrites form a metamorphic sequence, and that idea proved surprisingly hard to sell. After brief service in the Army and a year at Cambridge University, John served for 3 years as a research associate with Ed Anders at the University of Chicago. He then returned to the Smithsonian Astrophysical Observatory in Cambridge, Massachusetts, where he spent the remainder of his career. At Chicago, he investigated the formation of the Widmanstätten structure, and found that the process informs us of the cooling rates of iron meteorites. Back in Cambridge, he collaborated with W. R. Van Schmus on a chondrite classification that incorporates metamorphic grade, and published on metal grains in chondrites, before becoming absorbed by preparations for the return of lunar samples by the Apollo astronauts. His group's work on Apollo samples helped to establish the character of the lunar crust, and the need for a magma ocean to form it. Wood served as President of the Meteoritical Society in 1971–72 and received the Leonard Medal in 1978.

VIRGINIA TECH, METAMORPHISM, AND THE ROSEWOOD BOX

DS: Let's start with the question Ursula Marvin always asked, how did you get interested in meteorites?

JW: It's a long story. My undergraduate school was Virginia Tech, that's where all Wood males went to college, and I opted for a geology major for the usual reason, the (seemingly) outdoor character of the work. After my first degree I went for Ph.D. work to M.I.T., where Gordon MacDonald was my thesis advisor. Geology is about history over a vast range of time, but it began to bother me that nothing appeared to be known about the origin of the earth or its earliest history. It seemed like the first chapter in historical geology textbooks was always missing; we knew nothing about the first billion years.

Cross-registration at Harvard was possible, and I rode my bike from M.I.T. up to Harvard Square to take a few courses. At some point, Cliff Frondel showed me Harvard's J. Lawrence Smith meteorite collection, from the nineteenth century. Harvard had made thin sections of every meteorite, over a hundred of them, and these were neatly stored in a small rosewood cabinet (Fig. 2). Meteorites were understood to be very old, and thinking that meteorite petrography might say something about the early history of the Earth, I got quite interested in these sections. I found that Cliff was willing to lend me the box of sections, and I gleefully bicycled it back to M.I.T. It was a somewhat perilous trip for the sections, as on an earlier occasion someone had opened a car door in front of my bike as I was skimming along the row of cars parked on Mass. Avenue, and I went over the handlebars. Fortunately I had only a loaf of rye bread in

Fig. 1. John Wood.

Fig. 2. The "Rosewood Box" of meteorite sections from the J. Lawrence Smith collection, still in the Harvard Mineralogical Museum 55 years after Wood studied them. One of the sections (Tieschitz) appears at right.

the bike's basket that time. This time the rosewood box made the trip okay. (*Note:* I could swear the box was made of an exotic reddish wood, but having just seen it again to photograph it for this article, I must now confess the box is made of an ordinary brownish hardwood. It just shows what a romantic I am.)

So, I went through the sections late in my graduate program (third year), was intrigued by them, and with Gordon's approval I switched my thesis topic to meteorite petrography. I had been mapping igneous ring dikes in New Hampshire for a field/laboratory thesis, and I wasn't really getting anywhere. The sections, mostly chondrites, looked like pyroclastics to me, but they differed from one another in interesting ways. I loved the chondrules, each a unique igneous rock, and tried to picture how they could have formed in the early solar system. I got my degree in 1958, after 4 years; those were the same 4 years that MacDonald was on the faculty at M.I.T. We were in sync.

DS: You said Virginia Tech is where the Wood males went to college. What did your father do?

JW: My father worked for a fire insurance underwriting company. He would estimate the fire risks, which determined the cost of insurance, and he also reported on damage when fires did occur. He was basically an engineer, rather than a business person.

DS: Did you have siblings?

JW: I had a sister, but she has passed away.

DS: You published your thesis work?

JW: Yes, I published my thesis work in 1962 and 1963, arguing (among other things) that the chondrites clearly had been metamorphosed to varying degrees. Gordon had given me a key paper by George W. Bain (Amherst College): this paper showed thin sections of a series of limestone samples from the same stratigraphic unit collected at widely separated places in New England's metamorphic terrain, and these samples had been metamorphosed to differing degrees. At one locality, they had gone all the way to marble, while many miles away, the rock was still limestone. Looking at these thin sections, with fossils in them whose outlines were more or less degraded by recrystallization, was just like what I was seeing in chondrites with more or less degraded chondrules (Fig. 3). That's what put the idea in my head.

DS: This was what a metamorphic sequence looks like even if it's in limestone.

JW: Yes, but the concept of metamorphism in chondrites ran into unexpected resistance, though it seemed so obvious. Most of the people who had begun studying meteorites in those days were chemists and physicists, and the idea that rock could be profoundly changed by solid-state processes operating over long periods of time was unfamiliar and counterintuitive to them. Harold Urey in particular could not accept this idea. We talked about our different perspectives, and he explained to me that he had had to familiarize himself with many new scientific areas when he wrote his book *The Planets* (1952), and that his absorptive capacity was used up before he got to petrology. His background in chemistry did not prepare him for the idea of metamorphic change.

(Thinking about it more recently, there were very high walls between scientific disciplines in those days, and one was expected to stay between his walls. Urey was ahead of his time and courageous to extend himself as far as he did. By now we've found you can't be that narrow and make progress in cosmochemistry. Perhaps this is a unique service cosmochemistry has served, motivating scientists to broaden their outlook.)

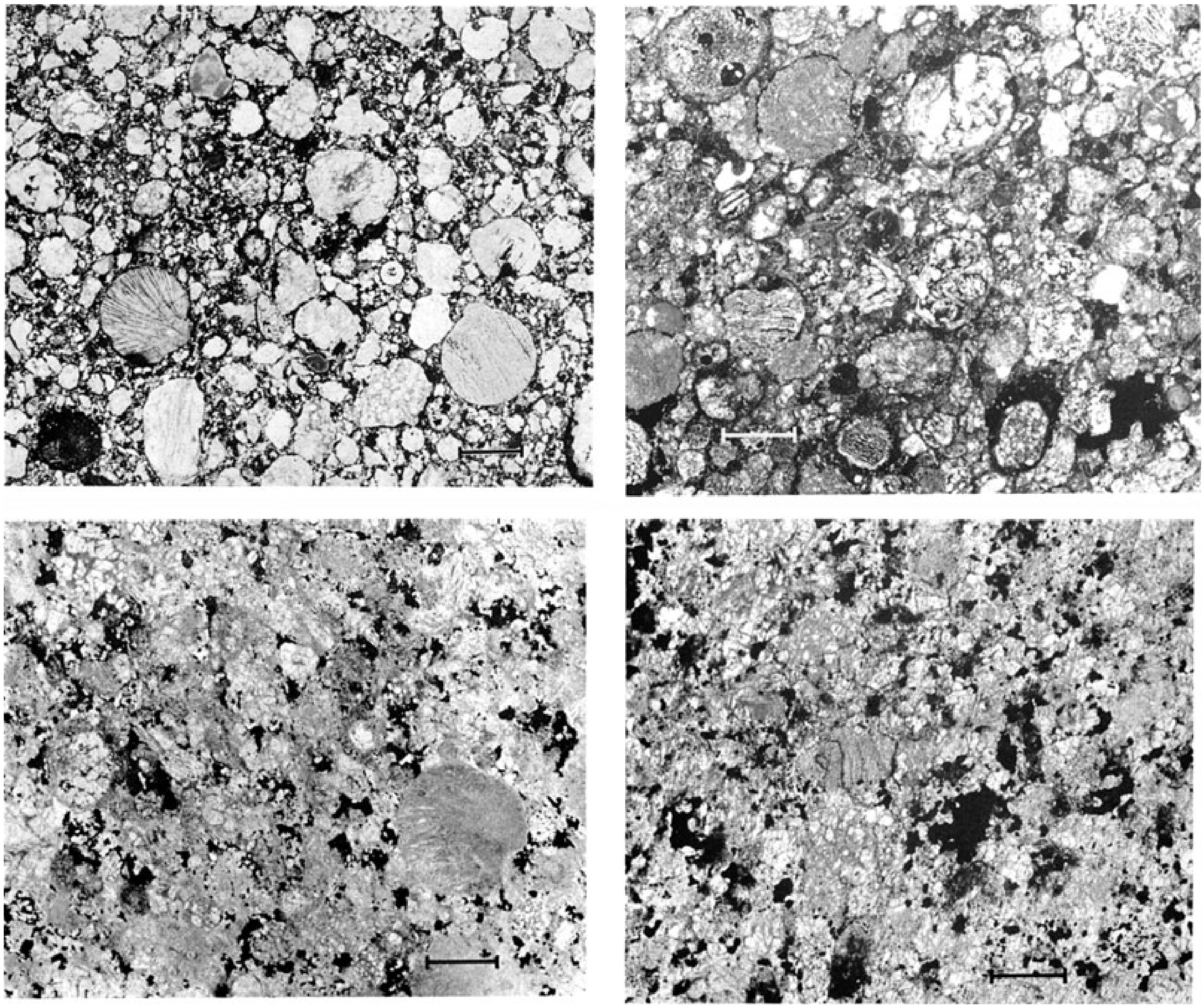

Fig. 3. Figures from Van Schmus and Wood (1967), showing the textures of chondrites metamorphosed to the four grades they recognized.

I showed reproductions of Bain's figures in one of my Gordon Conferences talks. Ed Anders observed Harold Urey's reactions as I showed Bain's slides. All Ed could report was that he looked thoughtful! Ed was on my side; he wanted to see this idea win.

DS: So it was a while before the radical idea of metamorphism in chondrites became accepted. Didn't you do a year in Cambridge? With Stuart Agrell or …?

POSTDOCTORAL YEARS IN THE UK AND CHICAGO: THE COOLING RATES OF METEORITES

JW: I had a postdoc year at Cambridge in the Institute of Geophysics. I didn't know Stuart in those days. I was nominally supervised by Sir Edward Bullard. I wanted to learn geophysics. I wanted to learn better how to do quantitative science. I tried and I tried to master the mathematics, but with little success.

DS: So it was not a comfortable year for you?

JW: Oh, it was a delightful year. Cambridge is a very charming place, I met some very interesting people, I had a good time, and I learned a few things. It certainly was not a year wasted, but I didn't achieve what I had expected to. One of the things the English university system seems not to have is a series of formal courses at advanced levels like the American system does, and that was what I was looking for.

DS: What was the project?

JW: I didn't really have a research project. I wrote a paper on the statistics of meteorite falls (1961), the

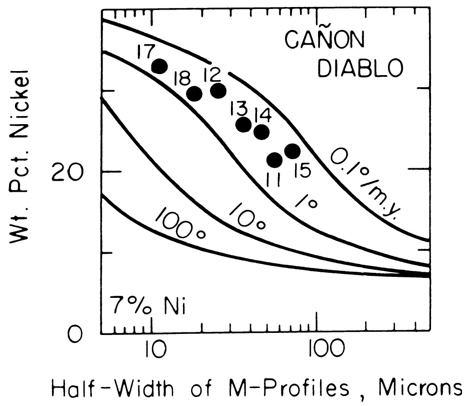

Fig. 4. Nickel profiles in the taenite fields of octahedrites have an "M" shape. Nickel has to diffuse from the Ni-rich surfaces of the plates to their Ni-poor cores as the meteorite cools. The diffusion rate decreases exponentially with temperature. The slower the meteorite cools, and the narrower a plate is, the more Ni can penetrate to its core before the decreasing diffusion rate freezes in the movement of that element. Solid curves in this figure show the final Ni contents of taenite cores as a function of plate width and cooling rate, from computer simulations. The solid points are measurements made in taenite plates of the Canon Diablo octahedrite. From Wood (1964).

prevalence of afternoon over morning falls, and what it means about their source in space.

DS: Then you went to Chicago. How was Ed Anders as a mentor?

JW: He was truly a mentor. I was and remain very grateful for all he did for me.

DS: You were there from 1962 to 1965 and that was where you studied metallurgy?

JW: Yes; I saw a problem that seemed amenable to solution, and I taught myself some metallurgy just like Harold Urey had done. As it turned out, I was doing this work in parallel with Joe Goldstein, but I didn't know it at the time. Eventually, we learned of each other's work, but by then, I was committed to the research. Of course, he was too; it was his Ph.D. thesis project.

DS: He was also at M.I.T. He came just after you? You didn't overlap.

JW: His degree was 6 years after mine.

DS: So you came up with this method of central-nickel-content versus grain-size.

JW: Cooling rates can be derived by numerically modeling the diffusion of nickel through taenite crystals as they cool through the Ni,Fe phase diagram, and the equilibrium Ni content of taenite changes. Diffusion slows with cooling, inhibiting the maintenance of equilibrium compositions, and eventually the crystals are left with frozen-in, non-equilibrium Ni profiles, which differ depending on the cooling rate. One modeled the production of these frozen profiles in a digital computer. It occurred to me that you could find a relationship between the thickness of taenite plates and the central-nickel-content that would be frozen into them, as a function of cooling rate; all you needed to do is measure the central-nickel-contents of a number of taenite grains in a meteorite with an electron microprobe, and their thicknesses, and then use the relationship I just described to derive the rate at which the meteorite had cooled through the phase diagram (Fig. 4). It was much simpler than repeating the computer-modeling process to match the profile found in each separate meteorite. And it later turned out, rather to my surprise, that the same technique could be applied to isolated taenite grains in the silicate fabric of chondrites.

DS: Whereas Goldstein was rigorously modeling the Ni profiles to fit the data.

JW: We both started out doing it that way but I eventually found a different way to do it.

DS: But you came up with similar results.

JW: That's right, happily. I don't like to argue with people!

DS: What I meant was that in order to get the same results you were using the same theory and the same input data, diffusion coefficients etc., so you should expect the same cooling rate results.

JW: That's right. If we hadn't gotten the same results, one of us must have been doing something wrong. The diffusion coefficient I used, by the way, was one that Goldstein found in his thesis work.

DS: Who else was at Chicago at that time?

JW: Dieter Heymann was in Ed Anders' group. I haven't seen him in many years. (We were at the Enrico Fermi Institute for Nuclear Studies.) Of course I knew Bob Clayton, Tosh Mayeda, and Tony Turkevich.

DS: What was the atmosphere like in the group? Was it a big group?

JW: There were never more than two or three people in the group. Ed Anders had postdocs and he had graduate students. There were only a few people, but it was a very stimulating atmosphere. Joe Smith in the Department of Geophysical Sciences was establishing their electron microprobe. He taught me how to use an ARL microprobe. Probes were still new then (1962–65). In doing my M.I.T. thesis work probes were not available, so we had to use optical techniques to learn mineral compositions. That was what people did in those days. Incredibly crude: it worked, but it was imprecise and destructive of the sample.

DS: So at Chicago you had a probe that you used for metallographic research.

JW: And for other things. Ed wanted me to get involved with carbonaceous chondrites. He was homing in on the most primitive materials. I did a certain amount of that on the microprobe. The metallography work turned out to be the most interesting and original thing I did at Chicago.

DS: This was about the time you did the olivine compositions in carbonaceous chondrites. You found the spread in FeO and the large number of very-low-FeO olivines. Bob Dodd found them to be high in Ca. You did get a paper out of it.

JW: Yes, in 1967. I did some of that work in Chicago, but much of it after I came back to the Smithsonian Astrophysical Observatory (hereafter SAO) in 1965.

THE SMITHSONIAN ASTROPHYSICAL OBSERVATORY: CHONDRITE CLASSIFICATION, ALLENDE AND THE RUN-UP TO APOLLO

DS: So you were in Chicago 3 years, during which you did the metallography and had some interaction with Goldstein. Then you went to the Smithsonian?

JW: I spent my entire career at the SAO. While I was doing my thesis work at M.I.T. I was also involved in taking courses at the Harvard Observatory with Fred Whipple, and Fred actually hired me and supported my M.I.T. thesis research on meteorites. So, in a sense, my meteorite research was an SAO project from the very beginning. My immediate SAO supervisor, John Rinehart, was the PI I nominally worked under, but he didn't have a background in meteorites. He was an explosive impact person, he did the sort of experiments Tom Ahrens did. He was administratively my superior, but I was mostly working by myself at the SAO.

DS: You had a research associate appointment?

JW: Not sure what it was called. After my SAO work and my degree at M.I.T. I went into the Army briefly, then I went to Cambridge, England, for a year, then I came back to the SAO briefly, then I went to Chicago for 3 years (where I was technically on leave of absence from the SAO). After I came back to Cambridge, Massachusetts, I stayed at the SAO. I was, technically, at the SAO for my whole career, 1957–2004.

DS: When did you get your own group?

JW: Not until 1968. You can't have your own group until you have funding, and until that time I had not had a grant. Funding became available for me with the lunar program. I actually never wrote a meteorite research proposal before the Apollo program. I think that's right; I forget so many things. They had money at the SAO, and I didn't need to have support as long as I didn't want to hire people, and I was perfectly happy to work by myself.

DS: What were you working on at this point? I guess we are in the run-up towards Apollo.

JW: That's right. I returned to the Smithsonian in 1965, and we started working seriously on Apollo in 1968. Meanwhile, in 1967 I wrote my paper on metal grains in chondrites, my paper on olivine/pyroxene compositions in C2 chondrites, and my paper with Van Schmus on the classification of chondrites by metamorphic grade.

DS: Tell me about Van Schmus, how did you link up with him?

JW: Randy and Bob Dodd were fulfilling ROTC commitments in the Air Force at that time, and they (separately) had managed to get themselves assigned to the Air Force Cambridge Research Center just outside town at Hanscom Field, in Bedford, Mass. (not Cambridge). The Air Force let them study meteorites. Ursula Marvin was organizing the Meteorite Discussion Group meetings at which people from SAO, Harvard, MIT, and nearby labs met once a month to discuss their latest work or listen to distinguished visitors to Cambridge. These included Ed Anders, Brian Mason, Ross Taylor, and even Harold Urey. Randy and Bob would come to the meetings and give talks; it was easy to get them to participate. That must have been where I first met them.

DS: How did you get together to do the 1967 paper?

JW: Randy and I had independently made the observation that the state of preservation of the chondrules in a chondrite was at least a crude indication of the degree of metamorphism the chondrite had experienced, and we each had in mind writing a paper that would set up a chondrite classification based on metamorphic grade. Collaboration seemed like the logical and gentlemanly thing to do.

DS: So the two of you defined the metamorphic spectrum, and combined it with the chemical classes of Urey and Craig, putting the two together into a matrix to form the classification. You reported looking at about 450 meteorites for that paper.

JW: Yes, I had visited a bunch of museums and looked systematically through their entire collections of meteorite sections, recording my subjective impression of the state of degradation of the chondrules in each.

DS: Then you and Randy cross-checked each other.

JW: Yes, he did the same thing.

DS: That's got to be one of the most influential papers we have.

JW: I think it is, yet the concept is so simple and so obvious. The fact that it took that paper, which was considered controversial at the time, to cross the barrier that Harold Urey and others had set up says a lot.

DS: The idea that there was no metamorphism?

JW: Well, the lack of understanding of the effects of metamorphism.

DS: In that paper, you didn't offer new insights into the nature of the metamorphism, you just described it.

JW: That's right, but the concept that the data in most of the chondrites are of questionable value for deciphering events in the early solar system, because the chondrites have been so tampered with, was unsettling at the time.

DS: And you chose to use a classification scheme to make this point.

JW: That's right.

DS: So you were thirty-something years old arguing with people like Urey. Do you think that your understanding of the role of metamorphism was more easily accepted by the meteoritical community when it was presented in the form of a classification scheme, than from your earlier papers where you just described the process?

JW: Randy and I hoped it would be. Then we had a big stroke of luck: in 1968 Joe Zähringer published a paper showing (among other things) that the ^{36}Ar contents of chondrites correlate with the petrological classes of Van Schmus and Wood. More of the primordial ^{36}Ar had been cooked out of the chondrites of higher metamorphic grade. This made the meteorite community sit up and take notice.

DS: Yes, the classification scheme has never since been questioned. It took off straight away.

JW: It did; very gratifying.

DS: Didn't Ursula Marvin appear on the scene at about this time?

JW: I finally did get a NASA grant in the late 1960s, to study moon rocks, so I was able to form a group at the Smithsonian, and we started equipping the lab. I got a MAC electron microprobe and a good microscope and all kinds of things like that. At that time, Ursula was working at her lab in the Harvard Museum, although (unknown to me) Fred Whipple, who was closely allied with Cliff Frondel, had hired her in 1961 in one of the federal slots at SAO. He first assigned her to prepare meteorite samples for Ed Fireman to run on his new mass spectrometer. So, by general agreement, she stayed, for the time being, at the Museum because that's where the meteorites and microscopes, and x-ray machines were. I got to know her well at the Meteorite Discussion Group meetings. Clearly she was going to be one of the people who studied lunar samples so, once I got my lab equipped, I asked her if she would like to move over to the SAO and join my group. I hadn't quite realized that she actually belonged there, but it worked out very well. She moved to my lab in the summer of 1968.

DS: Talk to me about Allende. I know Ursula was involved.

JW: Let it be remembered that the first collection of material from the Allende fall was made by the SAO's Center for Short-Lived Phenomena, whose Director, Bob Citron, was well known by my group. All of us in the SAO group were involved in studying it; Ursula played a key role, but as I recall, I seized the first authorship and submitted the paper to *Science*. Phil Abelson, the Editor, sent it to Kurt Fredricksson to review. Kurt rejected it, because they were studying Allende at the Washington Smithsonian as well. He didn't think we should be the first Smithsonian people to describe it. By the time it got rejected, I had become absorbed by our preparations for the Apollo program, and I thought "To hell with it." I let it fall into the background, but Ursula was not so easily defeated. She took over the paper, saying "We've got to get this published," and she succeeded. It was the first paper on Allende CAIs and it appeared early in 1970.

DS: In *Earth and Planetary Science Letters?*

JW: Yes.

DS: Was this where we first got the notion that the refractory inclusions were high temperature condensates?

JW: It was where we first saw and described them. But there had been a thermodynamics paper by H. C. Lord III in 1965. Lord is the one who made that connection. Lord did the thermodynamics and described what the first condensates from a hot solar nebula should look like.

DS: Then you found the inclusions and linked them to Lord's predictions?

JW: Yes, we cited him in the paper. I don't know whatever happened to him. He didn't stay in meteoritics.

DS: Yes, it sometimes happens that way. Someone comes along, makes a major contribution, and you never hear from them again.

JW: Regrettably.

DS: So that was all going on in the late sixties. Lunar samples are coming along.

JW: I had gotten approved as a lunar sample Principal Investigator. We knew we were going to study lunar samples and we started doing simulation studies. I hired two postdocs, John Dickey and Ben Powell, with fresh Ph.D.s from Princeton and Columbia respectively, and we worked very assiduously. We understood clearly that our position in the lunar program was at the bottom of the totem pole. We figured that the really good samples and the thin sections they were going to make of the lunar rocks were going to go to people higher on that pole.

DS: This was because of your age?

JW: My age, my lowly stature in the field, and maybe the fact that I wasn't on a university faculty.

DS: You had just come out with Van Schmus and Wood and were involved in this metallographic work.

JW: Yes, but that didn't cut much ice. Most of the people that were involved in setting up the lunar science program had not come from the meteorite community. They were workers in the terrestrial geosciences, a very different population from cosmochemists. They didn't know about and would not have been impressed by my background. The people they respected were those that had made reputations in terrestrial mineralogy. So I assumed correctly that we would be at the bottom of the totem pole, and I advised the system that we would settle for studying some of the lunar soil, the lowliest type of material they expected to collect.

DS: So this wasn't a great insight you had, that the really interesting stuff would be in the lunar regolith?

JW: No. It was my realistic estimate of what we could get. I figured we should prepare to learn from the most humble material they could supply. So all of our equipment and all of our work on Allende involved studying tiny mineral grains.

DS: For your simulations, you were using Allende? What were your simulants?

JW: I don't remember in detail.

DS: Everyone says Allende.

JW: That was one simulant. But I seem to remember we made mineral mixtures too. I'm not sure.

DS: So you've got your microscope, your probe, your colleagues...

JW: … and a fine-focus X-ray machine and camera for Ursula to use in studying tiny dust particles.

DS: And you decided to ask for regolith because that is all you thought you could get.

JW: That's the way the proposal was written. I was that insecure.

DS: So then what happened? You got samples of regolith.

APOLLO AND THE HISTORY OF THE MOON

JW: We got lunar soil, and we got the loan of sections of big rocks, which turned out to be pretty much the same as everyone else got. In the event they didn't really discriminate all that much, and everyone got samples of the regolith. They gave much the same suite of samples to all investigators, except for those who had a study that was destructive and required a great amount of material, or that required a special type of sample.

DS: Take me slowly through the process. This is historic. We had landed on the Moon, Apollo 11 has come back, the astronauts went into quarantine, scientists got their rocks, and you had your regolith. Do you remember the samples arriving?

JW: Oh yes, very well. The Apollo 11 lunar samples arrived at the Lunar Receiving Laboratory (LRL) in Houston on July 26, 1969. At that time I was in Houston, attending a workshop on lunar science (described below). The workshop attendees had (very limited) access to the LRL, and there we soon saw our first lunar rock, through several thick layers of glass.

By September, the LRL had made a preliminary examination of the samples and divided them for initial allocation to the teams that had been approved for sample studies. (Cliff Frondel, by the way, was an important member of the Preliminary Examination Team.) For that first sample distribution, every PI had to go to Houston to pick up his or her samples. At that point, NASA wouldn't mail them to you. You had to hand-carry them. So you had to go to Houston and sign all sorts of papers acknowledging responsibility, and then carefully convey the sample back to your lab. Mine came in a couple of small plastic vials and I was so impressed with their importance that I placed the samples in the pocket of my seersucker jacket, then borrowed a needle and thread from one of the secretaries in the Lunar Receiving Laboratory and stitched up the pocket, to wear on my flight home. This was an Eastern Airlines flight that made a stop at Washington DC on its way back to Boston. There were a bunch of other investigators on that same flight that were going back to the U.S. Geological Survey, the Geophysical Lab, and places like that in Washington, and we were all whooping it up on the plane and drinking somewhat; and I got so warm that I tore off my jacket and threw it in the overhead storage, forgetting until much later that I had lost contact with those precious samples. The joke amongst us was that if the plane had gone down and crashed, the news reports would talk not of the people who died, but the amount of Apollo samples lost!

DS: So you got back to Boston.

JW: We got back to Boston. I had a little show for the children in my neighborhood and their parents, then next day at the SAO we put on a bigger and more elaborate show for Observatory people and their families and friends.

DS: This was going on all over the world. It was going on in England.

JW: I am sure it was. That was only human. Then we got to work on the samples. In addition to getting this grungy fine-grained dust, we were given sieved material, in particular some of the 1-to-2-mm sieve fraction, and while any right-minded terrestrial geologist would expect such a size fraction to consist of a lot of mineral fragments, we were surprised to find that wasn't the case. What we found were actually tiny fragments of rocks, which were sufficiently fine-grained for almost all of them to be polycrystalline, polymineralic, and fairly representative of the rock it had been broken from. So we had this huge collection of lunar rock types that we had not anticipated. We thought we would be dealing with single-crystal samples, and we got something totally different. We got rocks, and lots of them. So we set out to do population studies of the rock types, and to make a long story short, we found a minority fraction of little white particles among the dark basalts and glasses and breccias; so did other petrography groups. These particles were tiny rocks that consisted mostly of calcic plagioclase. We were dumbfounded at first, but then John Dickey, scanning the microprobe analysis of a pale greenish impact-melted glass from our sample, declared the truth: "That's an anorthosite composition!"

Other groups that found them, like J. V. Smith's group at Chicago, described them in the papers they wrote, but subordinated their importance relative to the samples of basalt from Mare Tranquillitatis. Everybody (almost) knew that the surface of the Moon was going to be covered with basalts; the terrestrial geologists among lunar investigators thought it their first duty to understand that basalt, and they worked in that direction. My history had been a little different than theirs, and I considered it more interesting to try to understand those unexpected little white particles. In any case, *tout le monde* was working on the basalts and we wanted to do something different.

DS: This is the bit I have always been intrigued by, and I am delighted that I have this chance to ask you about it. There is something I can't understand: you have this regolith sample—you have just given me the new insight that these were little rocks not mineral fragments, but from this 100 mg of regolith you...

JW: No. We had more than that, about a tablespoonful, and not just dust but what they called coarse fines. The good stuff.

DS: Okay, a small handful, and from that your vision gave us the Moon we now understand.

JW: We made an important contribution to understanding the Moon.

DS: Quite remarkable.

JW: Yes.

DS: But how did it happen?

JW: I personally had a whole lot of good luck. I was in the right place at the right time to have access to these samples of a whole new planet, when they became available. My colleagues and I had the right training, equipment, and experience to make the most of the opportunity. And surprisingly, our competition was handicapped in various ways.

One category of competition, those who cared the most deeply about the moon, had pondered long and carefully the question of its properties and origin, and by the time of Apollo they had formed and published strongly held views on the matter, which unfortunately the samples showed to be wrong. These colleagues lacked the geological background needed to understand planets. An example, I'm afraid, was Harold Urey again. I feel badly beating up on Harold as I have been, I had great respect and affection for him, but he had a blind spot when it came to the geological sciences. I and others in my group had the advantage of ignorance of the moon and completely open minds. (The notable exception to the generalization I made earlier was Gerard Kuiper. An astronomer, he nonetheless acquired enough understanding of volcanism and cratering to reach all the right conclusions from what we knew before Apollo.)

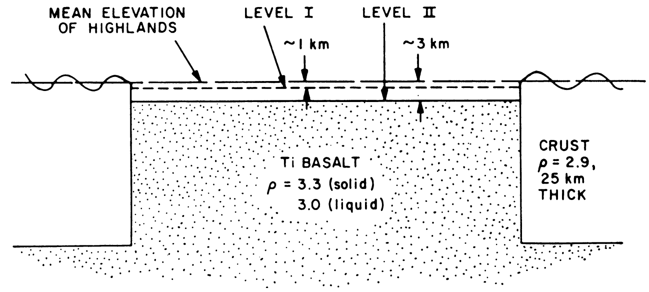

Fig. 5. The SAO crustal model of the moon, from Wood et al. (1970). The "Levels" referenced were an effort to understand the formation of mascons in circular maria.

The other category of competition was terrestrial petrologists, who had the right background but who were constrained by the conservative mores of the geological profession.

DS: At this point, the scientific establishment, by which I mean NASA, had given it to the terrestrial geologists to run with the science.

JW: The logical thing to do. But I believe the U.S. Geological Survey had some of the worst offenders in the kind of conservatism I'm talking about. As I understand it you couldn't just publish a paper if you worked for the USGS, it had to go through many levels of internal review by conservative geologists first, who were likely to say, "You can't print this. You can't say that." The moon, a completely new planet, called for some outside-the-box thinking, and I think this was not encouraged by Survey managers, who had developed a CYA mentality.

As a meteoriticist, I was not so strongly affected by this conservatism. Very little was known about meteorites and the orbiting bodies they came from, and it was necessary and to a degree accepted that risk-taking and some mistakes would have to be tolerated to build a conceptual framework for understanding them. I was willing to adopt unorthodox positions, and Ed Anders was too. We were constantly describing models that terrestrial geologists would brand "speculative," and they considered that a very dangerous thing to do. Their work was very rigorous and unchallengeable. Having a background in cosmochemistry spared me that particular mindset. So I didn't see anything wrong with saying, "Hey, these might be pieces of the white highlands crust of the Moon."

DS: So it was a bit of a cavalier moment for you, you've got this cosmochemistry background, you've got the Ed Anders experience, you have a handful of regolith, and you envisage a planet.

JW: The samples had to have a context and a history.

Fig. 6. The SAO research group in 1973. Seated, postdocs Jeff Taylor and Mike Drake bracket Wood. Standing, *l.* to *r.*, are Marie Hallam (postdoc); Janice Bower (microprobe analyst); Sandy List (secretary); Ursula Marvin; and Arlene Welch (data aide). We chose the photo of John's 1973 group to publish because it includes Mike Drake, who was lost to the planetary science community in 2011.

DS: Instead of saying these white pieces came from this outcrop and the basalts came from that outcrop, you envisaged a magma ocean.

JW: It wasn't quite that simple or quite that cavalier. People forget there was evidence other than the samples. In July 1969, I had been included in a workshop held by the Lunar Science Institute, which had just been founded. The LSI (later to be the LPI) had leased the West Mansion on Clear Lake near the Manned Spacecraft Center, but it was not yet ready for occupancy. To kick off the new Institute, it held a workshop on lunar science, in rented space above the barber shop in the Nassau Bay Shopping Center, in the same time frame as the Apollo 11 mission; it ran for several weeks. The workshop was headed by Gene Simmons, Chief Scientist at the Manned Spacecraft Center. He assembled a crew of experts on what was then known about the moon: he brought in the gravity (mascon) people from JPL, Muller and Sjogren; he enlisted people who had made remote studies of the moon's magnetic field, maps of its surface at optical and infrared wavelengths, and so forth; I was included to represent meteoritics. It was a very interesting group of people, and I learned a huge amount from them. One of the topics that impressed me deeply was the lunar mascons, and we spent many hours talking about them and the gravity signature of the moon generally, which says something about the structure of the crust. John O'Keefe had made the perceptive observation that, apart from the mascons, which are anomalous, the moon's overall gravity map says it is isostatically compensated. Since the lunar surface has relief, there must be a low-density crust of variable thickness. So when my group embraced those little white particles of relatively low-density material from the regolith, and suggested they were samples of the light-colored lunar highlands, we were able to cite the supporting gravity evidence for an extensive, thick, low-density surface layer (Fig. 5).

DS: Oh I see, so you were putting all this petrography data in the context of the global geophysics.

JW: And there was other supporting evidence. In 1968, the Surveyor 7 lander had analyzed material from the ejecta blanket of the Tycho crater in the lunar highlands, using Tony Turkevitch's alpha-scattering technique; I knew Tony from my Chicago days. His instrument found a curious composition. The error bars for the analysis were so large that the composition was consistent with a broad range of rock types, but the one that fitted most comfortably was anorthosite. A committee of terrestrial scientists was charged with interpreting this Surveyor 7 data when it first became available; however, the anorthosite interpretation must have seemed too radical to them. Instead, they stretched the error bars as far as they would go and said the Tycho ejecta blanket was composed of iron-poor basalt. That fit their preconceived notion of what the Moon was made of, but the truth is Surveyor 7 had found anorthositic crustal material. So I had some other information to lean on. Anorthosite had global significance on the moon, its occurrence wasn't restricted to Tranquility Base. Lunar anorthosite wasn't just a wacko idea.

DS: It was a spectacular idea though.

JW: I guess it was, although it sent many geologists and petrologists into a state of shock. No rocks on the Earth consist entirely of anorthite, which contains 90–100% An, the most calcium-rich member of the plagioclase feldspar series. But the name "anorthosite" had been bestowed long since upon huge, coarse-grained metamorphic bodies in Precambrian terraines consisting of labradorite, which contains only 50–70% An. People were asking, who are these folks at the SAO who have given the same name to tiny, white, microcrystalline grains of anorthite from the Moon?

DS: I see the point, but as I remember it, the name was accepted without much dispute.

JW: Yes, it was. Nobody suggested a better term and so it all worked out very well for my group. They put my talk last on the program at the Apollo 11 Lunar Science Conference in 1970, as befitted my stature in the program (but also, maybe, the position of W in the alphabet). But people got very interested in what I had to say, and I found that my position on the totem pole was suddenly inverted. It was the high point of my life.

POST-APOLLO

DS: Now into the seventies. We have a long way to go to bring this to the present.

JW: But as you know, most of us do our best work when we're young, and after that it goes downhill. I have published a lot of research since that time, working with my group (e.g., Fig. 6), describing lunar breccia clasts and so forth, but it wasn't as exciting as what we've talked about. I think it's expected that beyond your postdoc years, you will soon transition into leadership and teaching. Leadership has never been a strength of mine: I take no pleasure in telling other people what to do. I've enjoyed teaching, and I think I was fairly good at it, but even there, age takes its toll. Students don't rap with old profs the way they do with young ones.

DS: You continued with the thermodynamics, changing gas-dust ratios and so on.

JW: I didn't do much of that until the eighties and nineties, but there's nothing I did in that period that compares with metamorphism, the metallographic work, and the happenstance of my lunar experience. I did coin the term "magma ocean."

DS: Since then you have been involved in missions. Weren't you involved in Magellan, and other missions?

JW: I think Magellan was the only actual mission, other than Apollo, that I participated in. The JPL involvement was a unique experience for me. The technology behind scanning the planet's surface by synthetic aperture radar was really fascinating. But I was disappointed not to be more involved in the planetary geology aspects of the mission. I ended up working on phase equilibria as it bears on the surface mineralogy of Venus.

And, I've served on countless lunar and then meteoritic proposal review panels.

DS: Great fun?

JW: I wouldn't call it that. Of course I enjoyed hanging around with my buddies, going out to Maribelle's and things like that, and of course it was a way of maintaining your visibility in the field.

DS: Affecting the field for the greater good?

JW: One hopes so, but I'm glad to be away from that kind of work.

DS: What is it like to be on these national committees? You were involved in the panel that recommended firing Galileo into Jupiter.

JW: I believe that would be Mike Belton's Terrestrial Bodies Science Working Group, in the late 1970s, but I can't confirm it because I haven't saved most of my old records. It was very interesting talking about things like "JOP," Jupiter Orbiter Probe, which was the mission's name before it came to be called Galileo. And we heard about advanced technologies like ion drive and the solar sail. I was fascinated by the solar sail, what an extravagant idea! A huge disappointment that it has never been implemented.

DS: Tell me about the Masursky Lecture in 2000, "Tight Lipped Chondrules."

JW: LPI invited me to give the talk. I pulled a lot of things out of my memory that I thought were colorful and made a nice story, and which I had slides for; it was a chance to relive history, sum things up, and express some of my prejudices.

DS: You and many others said that we know little more about chondrule formation now than Henry Sorby did in 1877.

JW: I have a lot of respect for Sorby, and the other nineteenth-century petrologists who worked on meteorites. It is a remarkable thing that that research area died away in the early twentieth century. There's a huge gap in the record.

DS: Tschermak did some remarkable work.

JW: Yes he did, and there were others too. Daubrée did some crude but interesting melting experiments in the nineteenth century.

DS: We see a lot of papers that rediscover some of these nineteenth century observations.

JW: Sure, I'm guilty of it myself.

DS: Is that a fault with our field, or is that just the way science is?

JW: In the instances we just spoke of, I think it's because of that long, curious gap in the record of meteorite research. It became so easy to just ignore that earlier body of research, maybe assuming it was substandard. But more generally, that's the way science is. A researcher can do a fine piece of science, and it will be forgotten unless he or she constantly stirs people's memories, writing papers that self-reference, giving talks, and generally staying conspicuous. Do a good piece of work and guaranteed someone else will repeat what you've done, and do it better, profiting from your experience and mistakes, and publish it; and unless you're out there reminding people of your work, subsequent references to that topic will be to the second paper, or the third, not to yours. After all, those later papers are more informative and more likely to be correct.

DS: To be a good scientist ….

JW: It helps to be a good publicist. Very few in science, only the likes of Darwin and Newton, have achieved immortality. It occurred to me years ago that to if you're interested in immortality, science is not the way to go: you have to paint like Van Gogh or you write music like Mozart. Anything short of that and you will be forgotten.

Fig. 7. Julie and John Wood, happily married for 22 years, at Tech Night at the (Boston) Pops; June 2011.

DS: So don't take things too seriously?

JW: That's right. There's not much you can do about it.

DS: So when you reached the end of your cosmochemical career in 2004, you retired from the SAO with a sense of frustration that we hadn't made better progress in understanding what chondrules were telling us?

JW: Well, I had spent my whole career trying to understand chondrules, with little success. Others working in that area have done no better, in my opinion. Who wouldn't be frustrated. When I read the current literature (occasionally… I get *Science* and *MAPS* online) I'm disappointed at how rarely the work described has a broad, problem-oriented approach. This is one of the things I was referring to in my Masursky lecture.

DS: Okay, so you had a forty-year career in cosmochemistry and if you could stand aside from your career, but include your own contributions, what are the major advances since 1960?

JW: Oh… a fair understanding of the meteorite parent bodies and processes in them; short-lived radioactivity; the early chronology of the solar system; the accretion of planets; the condensation sequence; cratering and shock; new places to collect meteorites...

DS: Meteorites from Mars, meteorites from the Moon?

JW: Yes, certainly, but I was actually thinking of the vast harvest of meteorites from the world's cold and hot deserts.

DS: But your frustration—please correct me if I am misusing that word—is really that we have made such poor progress in chondrules, not in meteorite research as a whole?

JW: Yes, chondrules and CAIs. They were created in an astrophysical environment, and another thing I was lamenting in my Masursky talk is that there is so little communication between meteorite researchers and astrophysicists—the people who are in the best position to understand the context of the meteorite problem—in spite of all of the well-meant efforts that have been made to bring the two communities together in conferences and publications.

DS: Why is that?

JW: It seems to be too hard to do. The two cultures are just too different.

DS: Do we need more interdisciplinary programs?

JW: We have lots of interdisciplinary programs. They just don't seem to make much difference. We can't get on the same wavelength as astronomers. We don't understand each other. I guess it will always be that way. Maybe my hope that meteoritics was going to help solve the problem of the origin of the planets was naïve, and we are not going to do any better than the astrophysicists have done. The truth is that solar system formation is an extremely difficult problem. It may be unrealistic to think that we simple-minded rock-knockers will solve it.

DS: You have spent a career, at least the second half of your career I guess, trying to integrate astrophysics with meteoritics.

JW: Longer than that. It was a goal of my thesis.

DS: In fact, meteorite people take care of Meteoritical Society business and mission people take care of missions: there is a big disconnect between flying missions and taking samples. Why for example are most of the people who advocate the return of samples from Mars experts on Mars and not Mars meteorite experts? Is this what you are saying?

JW: It's true; the study we did on COMPLEX of quarantine requirements for returned Mars samples stemmed from the fact that we need sample return so we can analyze Mars material in the laboratory, not by remote-sensing with a robot. As wonderful and inspiring as robotic analysis is, it's no substitute for having samples in the lab. And there's a world of Mars material, beyond SNC meteorites, that we haven't seen.

I guess one thing I was trying to say in my Masursky lecture was that we should try to identify the big questions and work to answer them, not be led aside by smaller issues.

DS: Maybe we should work harder to integrate our work with the astrophysical literature and the asteroid literature.

JW: I agree.

DS: How many of us read every asteroid paper?

JW: Not *moi*. In fact, much of the asteroid literature is photometric studies, light curves, which do not greatly reward reading.

DS: But a lot of work has also been put into understanding their history, their diversity.

JW: That work *is* important. But chondrules and CAIs, and an understanding of what they are telling us, lie a step earlier in solar system history than when asteroids were formed. And remember that my obsession, since student days, was getting back to the beginning.

JOHN WOOD ARTIST

DS: Okay, just a few minutes on what you are doing now.

JW: I'm painting pictures. This last year I have been taking a course called "Artist's Professional Toolbox," run by the Arts and Business Council of Greater Boston. It's a series of lectures on the marketing and legal aspects of being an artist. There are 28 of us in this year's program. It's aimed at making us more effective marketing-wise than we were to start with, which wouldn't be hard in my case. It's very interesting, and I've met some excellent people. I went to a session last night where several marketing plans were presented and discussed, though not one of mine. I didn't write one.

DS: So how do you occupy your time now?

JW: Oh, there's plenty to do. Boston is a fine pace for cultural entertainment (Fig. 7). And I'm producing art. I've been in a gallery, though not now.

DS: But you have a website which lists a fair number of shows. You have had three or four-one-man shows and...

JW: I have had several one-man shows, and I've been in quite a few other exhibits. The problem is just that I can paint pictures faster than I can sell them. Selling art is important not so much to make money—the amount of money you can make from selling art you could put in your eye—but because when someone is willing to pay money for your art, it validates what you're doing. My intention, upon retirement, was to take art quite seriously and pursue it as a profession, not as a hobby. There's a big distinction. It makes me see red when a well-meaning person says "you were lucky to have such a nice hobby to fall back on."

DS: Does that put stress on you, thinking of it as a second profession?

JW: Oh sure, but that was in the cards. I'd like to be displayed more, I would like to sell more. As it is our house is filling up with paintings, and I need to move some of them. I've had to slow down production of new work.

DS: From what you have said, this has been a lifelong interest of yours. Were you able to pursue your artwork during your Smithsonian career?

JW: In small bits and pieces. I've taken courses at the Cambridge Adult Center, and a few courses in the sixties at the School of the Museum of Fine Arts in Boston. When I was in the Army at Fort Belvoir even earlier, I took a course at the School of the Corcoran Gallery in DC. Things like that.

DS: Describe your art. You have a website which people reading this can look at (http://www.woodjohn.net).

JW: I'm a realist. I paint pictures as accurately as I can, which as you know is an unfashionable thing to do. You're expected to be loose, emotional, and undisciplined, but I find this hard to do. Perhaps a career in science has ruined me for art; I can't help trying to get things right.

As I see it, paintings have two qualities: their content and their style. To be commercially viable, your work needs to be *branded*: this is one of the things I learned from the Artist's Professional Toolbox. Your brand can be based on pictorial content—one particular subject area and variations on it; or on a distinctive style; or both. My paintings are strong on content, but my subject matter is too variable to constitute a brand. I would be bored to always paint the same subject. And I don't have a really distinctive style. The path to success seems to be to have an interesting, identifiable style that lets people identify your stuff and want to own it.

DS: Is there a single painter you look up to?

JW: Oh lots of them. John Singer Sargent! Turner, Manet, Degas, Whistler, the Wyeths, I could go on and on. I like Hopper's work. He is a realist with content that is distinctive, in that his paintings are rather lonely. The people in them are slightly uncomfortable with their surroundings.

DS: In some technical sense, your work reminded me of Hopper but you are more disciplined than Hopper.

JW: If I am more disciplined than Hopper, that's not a good thing. People like to see artists as emotional creatures, spilling their guts on the canvas.

DS: You work doesn't spill your guts but it has soul.

JW: I'm pleased you think so.

DS: John, thank you for doing this interview.

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