

Report

Oral Histories in Meteoritics and Planetary Science—XVI: Grenville Turner

Derek W. G. SEARS

NASA Ames Research Center, Mountain View, California 94035, USA.
E-mail: derek.sears@nasa.gov

(Received 03 January 2012; revision accepted 17 January 2012)

Abstract—In this interview, Grenville Turner (Fig. 1) recounts how he became interested in meteorites during postdoctoral research with John Reynolds at the University of California, Berkeley, after completing a DPhil with Ken Mayne at the University of Oxford. At Berkeley, he worked on xenon isotopes with fellow students Bob Pepin and Craig Merrihue, but Reynolds' insistence that they analyze all the inert gases in their samples meant that they also made important contributions to Ne isotope studies and potassium-argon dating leading to the Ar-Ar technique. In 1964, Grenville obtained a teaching position at the University of Sheffield where he developed his own laboratory for inert gas isotope measurements. After the return of samples from the Moon by the Apollo program, he became involved in determining the chronology of volcanism and major impacts on the Moon. In 1988, Grenville and his team moved to the University of Manchester as part of a national reorganization of earth science departments. During the post Apollo years, Grenville's interest turned to the development of new instrumentation (resonance ionization mass spectrometry and the ion microprobe), and to problems in terrestrial isotope geochemistry, particularly the source of inert gases in fluid inclusions. He received the Leonard Medal of the Meteoritical Society in 1999, and he has also received awards from the Royal Society, the European Association of Geochemistry, and the Royal Astronomical Society.

DS: Grenville, thank you for doing this. Let me start the way Ursula Marvin always started, how did you become interested in meteorites?

GT: I was doing my DPhil degree in Oxford with Ken Mayne, who will be known to meteoritists by the paper with Paneth and Reasbeck (Paneth et al. 1952). Paneth and Reasbeck extracted helium from iron meteorites and Mayne measured the $^3\text{He}/^4\text{He}$ ratios. This demonstrated that the helium was cosmogenic in origin and not from uranium decay, which was previously assumed. When you assumed that the helium was radiogenic you got ages of 6 Gyr or so. I was there doing a nuclear physics degree with the job of building a very early ultrahigh vacuum mass spectrometer. At that time, there was no link with meteorites. The idea was to measure ^3He and ^4He from 14 MeV neutron capture reactions on various elements. I realized, after struggling with UHV systems for 2 years

that the neutron source at Oxford was not going to produce much ^3He , so I decided unilaterally to go to Harwell and use their cyclotron and study proton-induced reactions. Ken Mayne made a critically important contribution to my career by suggesting that I apply for a job in John Reynolds's laboratory at Berkeley and at Oliver Schaeffer's lab at Brookhaven. Well, John was the first to reply, offering me a post as an acting assistant professor at what seemed, considering my minuscule student grant, an astronomical salary.

DS: So you went for the money!

GT: That was incidental and I suppose it would have been the same or more at Brookhaven, but the prospect of going to Berkeley and California was particularly exciting. Also the prospect of getting experience teaching undergraduates was an added advantage. As it happened, I taught a different course in each of my four

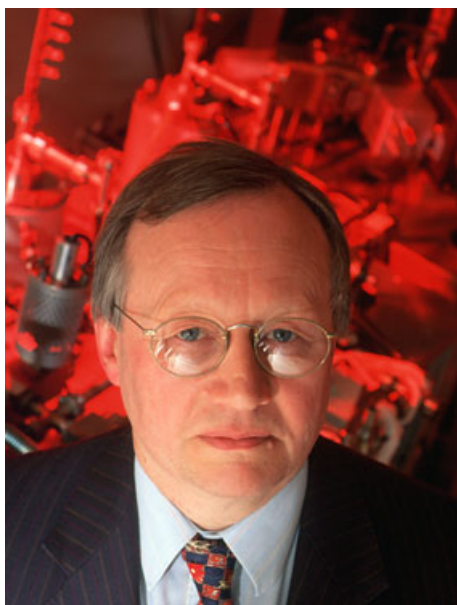


Fig. 1. Grenville Turner.

semesters in Berkeley. In those days, it was quite common after a doctorate to spend time in the United States, known as a BTA degree, Been To America.

DS: So when did you handle your first meteorite?

GT: At Berkeley. Reynolds had suggested a project based on work of Kurt Fredriksson and Paul DeCarli. They had shocked Bjurbole, and had implanted terrestrial argon into it. Bob Pepin had measured the amount of argon implanted and found high concentrations. I was given the job of working on a chondrule that had not been shocked for comparison and to determine the diffusion properties of the argon. The paper, my first, was published, with a companion one by Fredriksson and DeCarli on April 1st (Fredriksson and deCarli 1964; Pepin et al. 1964)! It was particularly interesting from Bob Pepin's point of view because 20 years later, he was looking at the concentrations of Martian atmospheric gases in EETA79001, which was critical in establishing the Martian origin of SNC meteorites, having previously made the very measurement that demonstrated the effectiveness of shock implantation.

DS: So would it be fair to say that you became interested in meteorites because of the unique nuclear chemistry possibilities?

GT: I was interested because I was working in Reynolds's lab where he had just made the ground-breaking discovery of excess ^{129}Xe in the Richardton meteorite caused by the presence in the early solar system of now extinct ^{129}I . I also went there because not only had he just discovered the first extinct nuclide, but in 1956, he had designed and built, in glass, the world's

first ultra high vacuum static mass spectrometer, capable of analyzing noble gases with unprecedented sensitivity. In Oxford, I had just built one out of stainless steel based on the same principles.

TODMORDEN, CAMBRIDGE, AND OXFORD

DS: Now let's take you back to how you started in life.

GT: I was born in Todmorden, which was a semirural cotton town on the Lancashire/Yorkshire border. The cotton has now completely gone. I don't know what people do for a living there now. I think many may commute into Manchester, the nearest big city. My mother was a weaver and my father was a tackler, which is the name for the person who keeps the looms running. The official title was a loom over-looker. I went to Todmorden Grammar school, an architecturally imposing building built in 1912, which sadly closed in 1978 when a new, unimposing, Comprehensive School on another site replaced it. In its 66-year history, it produced two Nobel Prize winners; one was John Cockcroft who, with Ernest Walton, developed the Cockcroft Walton high-voltage generator, and used it to accelerate protons onto a lithium target, "splitting the atom" artificially for the first time. The other was Geoffrey Wilkinson, a chemist awarded the prize for his work on organometallic compounds. I went from there to St. John's College in Cambridge on an exhibition, more or less survived the culture shock, and after I got my degree there, went to Oxford to do my DPhil with Ken Mayne.

DS: What do you mean by "exhibition."

GT: It's a scholarship. "Exhibition," £40 per year; "Minor Scholarship," £60 per year; and "Major Scholarship," £100 per year. Coming from my school, that was fine. Incidentally, the government clawed back the £40 from my State Scholarship.

DS: What time are we talking about now?

GT: This was 1955.

DS: So you did your first degree in physics?

GT: Well, the last year was all physics. In the other years I did some mathematics, geology, and chemistry as well.

DS: So you enjoyed your undergraduate career?

GT: Yes, it was an idyllic time. In the Physics Society, I remember hearing Fred Hoyle describe his new theory for the origin of the elements, and Martin Ryle argue against the Bondi, Gold, and Hoyle steady state universe. Then I moved to Oxford for the DPhil and I had to build this dratted machine. It was early days for ultra high vacuum technology and leaks were a perpetual problem. Gaskets were a major source of leaks and using the correct grade of high purity copper, or gold wire, was

key to success. Nowadays, everything is off the shelf and works first time. A major curse which lots of people in that era experienced was the need to fill liquid nitrogen traps at unsociable hours. The nitrogen in mine lasted only 12 hours so every evening was interrupted by the journey to the labs to “fill the traps.” I was married in my last year and my wife Kate frequently accompanied me on my motor bike. In the end, it took 4 years to get the thing working and get some data. Using the cyclotron at Harwell made it a lot quicker to get results in my final year once the mass spectrometer problems were overcome. Fortunately, I had easy access to the Harwell cyclotron and lots of help from the staff there.

DS: Is there anything interesting you remember about your graduate school, any interesting colleagues?

GT: None who went into meteoritics. Oddly I shared my very small lab with a low temperature physics student, Tony Anderson, and his liquid helium (and liquid hydrogen and liquid oxygen) cryostat. My main recollection is of Tony regularly bending down tapping the mercury manometer vigorously with his finger to take a reading. Technology has changed. Later, I shared the lab with a Canadian, Henry Glyde. After I finished, he used the mass spectrometer to study diffusion of helium in metals and then went on to a distinguished career in solid state physics in Canada and the United States.

Ken Mayne would certainly be described as interesting, also charming and witty and had a fund of stories, which he would deliver in a uniquely conspiratorial manner. He was an Ulsterman, with an accent that many including myself became quite good at imitating. He had a bad habit of cycling into the lab in the evening as I was about to leave—“sure you’re not about to leave this early are you, Grayanville?” The solid state physicists in the neighboring lab used to rag him about this peculiar sounding journal he took, *Geochimi-something* or other they called it! Ken Mayne had another student in the Geology Department, Derek York, who many will know from his later work on terrestrial Ar-Ar dating in Toronto. Mayne did not get on with Lawrence (Bill) Wager, head of Geology and after a major row, Derek’s supervision was transferred to Steve Moorbath, who was setting up the Oxford Geochronology Unit at the time. One outcome of that was the development of a lifetime competition between Moorbath and York as to who could produce the most outrageous wisecracks.

BERKELEY

DS: So you completed your graduate degree in 1962 and then went to Reynolds’s lab. What happened at Berkeley?

GT: Reynolds was great. He gave me a desk in the corner of his office so I was privy to all the things he was

doing. He seemed to be frequently on the phone arguing with the university administration. In my second semester, he stoically attended Portuguese classes 3 days a week at 8 am prior to coming into the lab. This was in preparation for him setting up a new dating lab at the University of Sao Paulo in Brazil in his forthcoming Sabbatical year. It was a good place. There were also his students, Bob Pepin and Craig Merrihue. Pepin seemed to spend his time in his windowless office with a cup of cold coffee trying to work out the various noble gas components that might be present in meteorites. Essentially, it was what he is doing now, working on theoretical explanations of noble gas abundances and isotopes. What Pepin said became gospel. Craig was a healthy looking individual, tanned, a mountain climber. He had led expeditions everywhere. He was an interesting chap. He once had a visit from Salvador Dali who wanted some Bruderheim chondrules which he had read were older than the Sun. Craig had written an essay concerning ^{129}Xe in Bruderheim, which got him a prize, half of the Ninninger Award, that year. I believe the essay included a reference to the chondrules being older than the matrix. In *Time* magazine, this became translated into “Berkeley student discovers meteorite older than the Sun” and Dali wanted some of the action. He wanted some of the meteorite to meditate on. Craig got a letter from Dali, from the St. Regis hotel in New York with a picture of a prancing horse, which Dali had sketched on one side.

DS: Did he get his chondrules?

GT: Yes, Craig visited Dali in his private railway carriage in Oakland. He had come all the way across the country in this private carriage. He spent the evening talking to Craig and then gave him a book (on Dali) in exchange for the chondrules. I never found out whether he produced a picture featuring Bruderheim chondrules.

DS: I have a similar experience with a member of our art faculty. He found something in the desert of northern Arizona and gave it to a student suggesting that it was a meteorite. The student used it as subject for his MFA thesis. It turned out to be one of the manganese ores you find in northern Arizona. What was it like to be in Reynolds’s lab?

GT: It was great. Reynolds made suggestions, but he let people get on with things. If they had a good idea, he just let them get on with it. After the first experiment, Reynolds suggested I look at Renazzo. He had managed to get this sample from the University of Bologna that had the main mass and I went to town on it measuring all the inert gases using stepped heating on neutron irradiated and unirradiated fragments. We published the results in *JGR* (Reynolds and Turner 1964). The authors of papers from Reynolds’s lab were always in alphabetical order. There were never any arguments about authorship, so he usually came last.

DS: Bob Walker did the same. He was always last, until Dave Zimmerman came along.

GT: Reynolds was last until I came along! I wrote that paper when he was in Brazil, I did the work and wrote it up, and he was very pleased with it. He very generously said it was the most thorough rare gas analysis of any meteorite so far published. I have the impression that papers were typically much shorter than now, but this was a long paper, and a lot came out of it. One point about this work that has major ramifications later on, is that John always insisted that we measure all the noble gas isotopes in every sample we had. We did the helium, neon, argon, krypton, and the xenon, and it was a pain.

DS: Could all these be measured on in the same machine?

GT: Yes, but it was a very slow process. Very tedious. You could do one extraction in a morning, then adjust the temperature and do another in the afternoon. It took a long time. But he always said it should be done. "You never know what you'll find."

DS: So it was a fishing expedition, measuring isotopes without knowing why?

GT: Not really. The immediate aim was to locate the xenon and to understand the properties of the xenon, but it would be stupid to throw away potentially interesting information in the other gases. The neon was interesting. I decided to plot $^{20}\text{Ne}/^{22}\text{Ne}$ against $^{21}\text{Ne}/^{22}\text{Ne}$. This has since become the standard way to plot terrestrial and extraterrestrial neon data, but was first applied to Renazzo and showed that in this case it was a mixture of planetary neon and cosmogenic neon. My reason for choosing ^{22}Ne as the denominator was that it kept the abscissa conveniently between 0 and 1 and the ordinate between 0 and 10 or so. There are two points at 900 °C and 1000 °C that lie slightly to the left of the line and possibly contain some neon-E. I thought that the scatter was simply my measurement error. The neon-E component was discovered a few years later by Black and Pepin and is typically released around these temperatures.

The major thing was to understand the xenon. There were obviously more than two components so I constructed this 3D plot (Fig. 2), to try and pin them down. The three components were, what is now called Q-xenon, terrestrial atmosphere, and the H part of what is now called HL xenon. I enjoy devising informative ways of plotting data and use 3-D plots all the time, but nowadays with a computer rather than welding rods! The discovery of xenon HL in Renazzo was the first example of a presolar isotope anomaly, though that wasn't known at the time. It led the Chicago group under Ed Anders to two decades of painstaking work, which culminated in the isolation of nano-diamonds, the carrier of HL. I like to contrast the Berkeley and

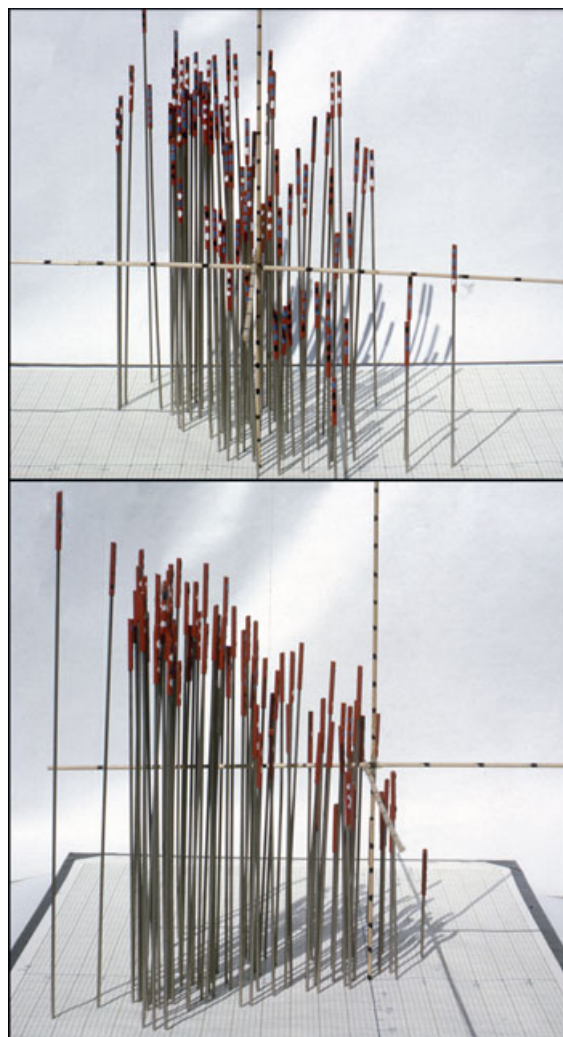


Fig. 2. Three-dimensional plot for the isotopes of chondritic Xe (data obtained at Berkeley in 1963). Axes are: $\delta^{130}\text{Xe}$ (vertical), $\delta^{131}\text{Xe}$ (horizontal, lower view, left is positive), $\delta^{136}\text{Xe}$ (horizontal, top view). Colors represent different meteorites and/or temperature steps. Black bars on axes are in units of 10 permil. Reference isotope is ^{132}Xe .

Chicago procedures. Being physicists, we heated things up, being chemists, they dissolved them in acid.

DS: Is this (Fig. 2) Renazzo?

GT: That was all the available data at the time, Renazzo is in there, so is Murray, so are others, each point is a sample, a temperature step, and so on. We had to use chart recorders, rulers, a sharp pencil, and a Marchant electro-mechanical calculator and as I said earlier, we produced two data points a day. Another of my jobs, aside from three lectures a week, was to design an automatic chart reader that would put the data onto punched cards. Berkeley had excellent electronic and mechanical workshops where the reader was made and it worked very well. This was the beginning of digitization.

Unfortunately, a few years after I left, they ran into trouble when a batch of slightly wider chart paper was purchased. I'm told that the workshop came to the rescue by sanding them down to size!

It was a great atmosphere as well. I have to tell you this story. It's 1963, we're on the west coast, and we wanted to take a vacation touring the national parks. Back in England, we had our "elderly" parents; they were after all in their early fifties! If there was a problem they would need to contact us. So, we gave John a list of post offices that we would visit to collect any mail on "general delivery." John had a great sense of humor and assigned to send something so that our journey to the post office would not be wasted! At the first post office I received a postcard from a fictitious lover, with a painting of a nude with a basket of pears. The "lover" lamented my touring the United States with "her" and had consoled herself with a basket of pears which Carl had purchased from the Coop prior to painting the picture on the front of the card. The writing was remarkably similar to John's!

DS: You were married by this time?

GT: Oh yes! Kate recognized the artist, Carl Hofer. Later, there was a can of kangaroo tail soup, left in Berkeley by Peter Jeffrey, a book on the design of nineteenth century water closets, and a book on how to make a killing on the stock exchange that was published in September 1929! At the final post office in Stateline, Arizona, we received a badly packed, broken Reynolds mass spectrometer, which had previously hung on the wall of the lab. The postmen looked at each other sheepishly as we enquired for any post and then brought out this badly wrapped tinkling package marked "Fragile."

DS: When did you marry Kate?

GT: 1961. We had been married a year when we went to the United States. In fact, she supported me for a while. She bought a typewriter to help prepare my thesis and wanted to use hire purchase but I, the penniless student, had to sign the agreement because she was a woman.

When John was away in Brazil from 1963 to 64, Bob Pepin and I had charge of the lab and one thing we had to do was to interview a potential Ph.D. student. We interviewed someone called Charles Hohenberg. Obviously, we did a good job. Without Charles, the Ar-Xe literature would be much poorer.

Another thing to come out of the Berkeley lab was Ar-Ar dating. Wänke had developed a technique to measure potassium-argon ages using neutron activation followed by counting decays of ^{39}Ar and ^{41}Ar to measure the potassium and ^{40}Ar , respectively, but he could not determine ^{36}Ar and hence make air corrections. This was not known to us at the time, although it was published. Another publication by Naughton (1963), had proposed

a method of using neutron activation of potassium salts to use as a ^{39}Ar spike for use in conventional potassium-argon dating. Clearly, he came very close to proposing Ar-Ar dating! One day, also when John was in Brazil, Craig came into my office with a chart and said, "What do you make of this?" There was a little blip in between ^{40}Ar and ^{38}Ar . He had realized it was ^{39}Ar from neutron reactions on ^{39}K and he suggested we look at all our data to see if we could produce some potassium-argon ages. The only problem at that time was that Heidelberg had just published a paper with potassium-argon ages for meteorites (Kirsten et al. 1963). There were about thirty of them.

DS: Using chemistry for the potassium?

GT: Yes. On reflection, they were not very precise since they assumed a nominal 800 parts per million for potassium in chondrites.

DS: That was the classical way of doing potassium-argon dating.

GT: The point was that they had analyzed a lot of meteorites and we had only irradiated a few. Craig submitted an abstract to a conference in Moscow (Idaho I presume). But we left it at that. I went back to England and struggled to get set up. I didn't have a machine then, but the department financed the building of a machine, and while that was going on, I analyzed the data from Berkeley.

SHEFFIELD

DS: You were then at the University of Sheffield?

GT: Yes. I got a lectureship there in 1964. I met an English visitor in Berkeley who said that they were looking for lecturers at Sheffield. In fact, they were looking for lecturers everywhere. The government was pumping money into the universities. There were many new universities and the old universities were expanding. So I wrote to Sheffield and got a letter by return offering me a job.

DS: The thirty-somethings reading this will find it most strange.

GT: Well if the thirty-somethings look at the cover of *Nature* for January 10th, 1975, they will see a graph of university jobs advertised in *Nature* per 3 month period from 1960 to 1974. There is a big spike in September 1963—when I wrote my letter to Sheffield—followed by a steep plunge. By 1974, it was zero.

DS: Uneventful move to Sheffield? You were the first person in Sheffield to work on isotopes?

GT: Yes. There was a Space Physics group and there was an idea that we would work on mass spectrometry from rockets. I flirted briefly with that, but decided that competing with Al Nier would be a waste of time and I had other urgent things to do.

DS: You were a junior lecturer?

GT: I was a lecturer, on the scale assistant lecturer, lecturer, senior lecturer/reader, and professor.

DS: How long did it take you to get a working mass spec?

GT: Probably about a year and a half. I started building a machine and working through the xenon data. I published a paper on I-Xe ages also including some neutron activation measurements for chlorine, bromine and iodine (Turner 1965), which I later returned to in some fluid inclusion work which is still ongoing. I also corresponded with Craig Merrihue about the argon work and I decided that we should plot the data on an isochron plot. Data from one or two of his Bruderheim samples with very few extractions looked all right, but for one of them with many more points, the data went around in a circle. The last letter I got from Craig said, "The enclosed Ar correlation plots are baffling because IB2 correlates and IBC21 and IB1 do not. Your correlations (on different meteorites) look better. What about Renazzo? Perhaps the poor correlations are due to the small sample size, more air, but IB1 is perplexing." Well, I put the graph in a drawer and carried on with the xenon work and then I got a letter from Bob Pepin to say that Craig had been killed in a climbing accident. It was on Mount Washington. He was roped to a friend and they were both blown from the mountain near the summit and Craig and his companion were killed. That was March 14th, 1965. Craig had been working at the Smithsonian Astrophysical Observatory and had recently made the first measurements of cosmogenic ^3He in ocean sediments, numbers which help determine the influx of extraterrestrial dust and the sedimentation rate on the ocean floor. It was a great loss.

At some stage, I finished the xenon work and published it and got the argon plot out of the drawer. I had just received a preprint of some work by Dieter Heymann and Ed Anders on K-Ar and U-He ages (Heymann 1967). Heymann found that a lot of He ages for shocked L-chondrites came out close to 500 Myr, implying that there was some major event involving the L-chondrite parent body at that time. I took the lowest three points in Craig's Bruderheim data and put a line through them and it came out at 500 Myr. Then the penny dropped and I realized that we had something very interesting here. My interest in argon dating went from close to zero to it becoming my major activity.

DS: There is a paper with Craig at about that time.

GT: That was accepted in December 1965 (Merrihue and Turner 1966). I wrote and submitted this Merrihue and Turner paper to *JGR* and they sent the reply by surface mail so it took a month to reach me in Sheffield. I was a bit annoyed about that. The editor did not think it was worth publishing. One of the referees had asked

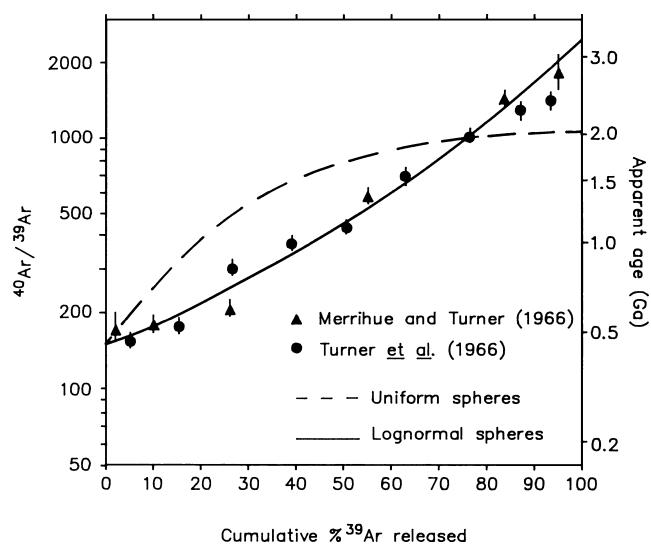


Fig. 3. Release pattern of argon from the Bruderheim meteorite with theoretical diffusion models (Turner et al. 1966; redrafted in McDougall and Harrison 1988).

whether ^{39}Ar was not being produced from calcium during the irradiation. However, because of the way ^{39}Ar release peaked at low temperatures and ^{37}Ar (produced from (n,α) reactions on ^{40}Ca) peaked at high temperatures I could show that the maximum amount of ^{39}Ar coming from the calcium was 10^{-3} of the ^{37}Ar in any extraction. So I resubmitted the paper and it was accepted.

At this stage, I needed to get on urgently and do some more analyses, but I didn't have my own machine yet. However, I did manage to find a machine, in Cambridge, in Jack Miller's K-Ar lab. He used an Omegatron, a peculiar machine, but it could measure argon isotope ratios and, with a bit of fiddling about, the rough amounts released. I took the residues from Craig's samples and ran them on that machine to check that I got the same results that Craig had. Sure enough, they were the same. However, I still had to figure out the best way to plot the data. I decided that the way to do it was to plot the $^{40}\text{Ar}/^{39}\text{Ar}$ ratio, or the equivalent K-Ar age, against the cumulative amount of ^{39}Ar released.

To try and account for the resulting curve, I did some simple diffusion calculations, assuming uniform spheres. This predicted a curve that went the opposite way to the measured curve. I thought that this must be telling me something! Of course, it was telling me that the minerals are not uniform spheres. However, if I assumed a log-normal distribution of spheres with sigma of 0.3, the theoretical curve fell right on top of the observed data (Fig. 3). I wrote to John Wood to see if he had any sections of Bruderheim from which I could determine the actual size distribution of the feldspar. He sent back some Polaroid prints and I got my ruler out, laid it across the

prints, measured the feldspar grains and they were indeed log-normal with sigma equal to 0.3. I published that together with the people in Cambridge, and it turned out to be an important paper (Turner et al. 1966).

DS: So you have been in Sheffield about 2 years, the mass spec comes on line, the Ar-Ar technique is basically working. Did you have students at this point?

GT: Now there's another story. I got elected to the Royal Society not long after my first student got her Ph.D., there must be a message there. But that was some time later. I was a one-man band in the department in 1969, doing things that no one else was doing. We had a space physics group, but students funded by the research council were funneled into projects on the ionosphere and magnetosphere. Even after the Apollo program came along, I didn't get a student.

DS: I wanted to ask you about your work on the Apollo samples. You have the Ar-Ar technique working for you.

GT: Yes, I wanted to run more meteorites. So I went to see Max Hey, the meteorite curator at the British Museum, who was very helpful. I had a selection with a spread of K-Ar and U-He ages to check this 500 Myr loss business. In fact, I got far too many meteorites to ever analyze. The silly thing I did was to publish these early results in conference proceedings—a volume from a workshop in Vienna in 1967 on the origin and distribution of the elements, and others, volumes, which are now hard to find (Turner 1968, 1969, 1970). I assumed I couldn't then publish them again in standard journals.

DS: You raise two interesting points about doing science at this time. One is that you can build up a successful university career without students and the other is attitudes to proceedings volumes versus journals.

GT: I guess, on the latter point, students are a lot more clued up now; I was rather naive.

DS: I joined the field in 1971/1972, and there was a plethora of proceedings being published, but there was a bias against them; you only got half a point in the merit evaluation process.

THE LUNAR SAMPLES

GT: Well, the Apollo program is coming along. In Britain, Keith Runcorn, Tom Kaiser, and others thought that they would put together a combined UK proposal to analyze the lunar samples. A committee would then distribute the samples within the UK. So they invited people to submit proposals. This idea was quickly rejected by NASA who wanted to deal with PIs not national committees. Tom Kaiser was head of my department at the time and he had me look at some of the proposals in my area. I looked at one from someone with no track record and saw his proposal was word-

for-word from one of my papers. I was furious. But the whole idea sank without trace.

In the end, NASA appointed about a dozen PIs in the UK. They were very generous.

DS: That's a good number.

GT: I was one of them. John Reynolds said I should apply directly and concentrate on ^{40}Ar - ^{39}Ar . So I did and, with his support, I was appointed a lunar sample investigator in 1968.

DS: The arrangement then was that if NASA approved your proposal, the UK would fund it.

GT: Unfortunately not! I applied to SRC [Science Research Council], but did not have any luck. They were heavily into funding other things, magnetospheric research, shooting rockets into the ionosphere, and they did not want to know anything about rocks from the Moon. I suspect that many of the senior people believed that the United States would not succeed in bringing these rocks back anyway. The best I was able to get was money for a data logger from an equipment support program, but nothing in the way of post doc funds.

DS: So with Reynold's help, you were authorized to get the samples, but you had no funds to support the research.

GT: It meant that I had to analyze them myself. There were a lot of late nights. Later, in the midseventies, NERC [Natural Environment Research Council] and SRC got their act together and agreed that anything below the surface of the Moon was NERC's responsibility and anything above was SRC's. So I then applied to NERC and I got support from them. This was after I came back from Jerry Wasserburg's lab. Colin Pillinger, who was dealing mainly with implanted solar wind carbon from above the surface was deemed to be SRC's responsibility.

DS: Tell me about your first lunar samples.

GT: Well, the first samples were just dust, the so-called soil. I was absolutely afraid of putting that in the machine. I knew before they came back, they would be full of solar wind and the preliminary analysis showed that they were. I figured that this could seriously compromise this important application of the Ar-Ar dating method.

DS: You were worried about contaminating the machine with these gases?

GT: Yes, the potential problem of dust in the lines was a worry and I concluded that putting very fine dust into the extraction furnace at this stage would have been stupid. So, for about a month, I held my breath and waited until the rocks came, in September I think. We did put the dust on display in a local museum for several days. Like most places, the queues to see this tiny bit of dirt and to look through a microscope at some tiny mineral fragments were huge. They snaked all around

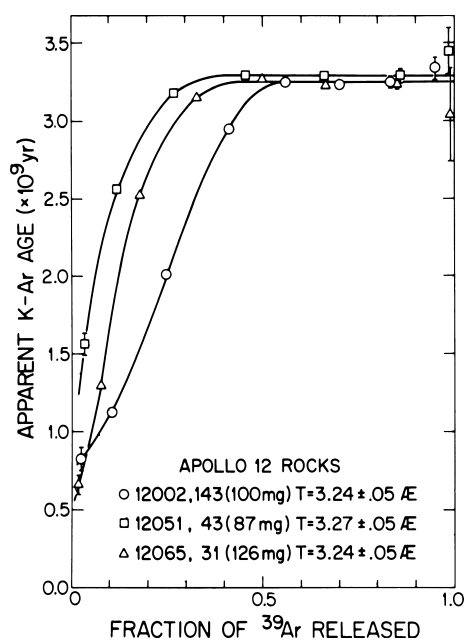


Fig. 4. Ar-Ar release patterns for Apollo 12 mare basalts indicating ages based on high temperature plateau releases. In some early plots like this, points are used to indicate the center of the release steps (Turner 1971).

the museum, out of the door, down the steps, and outside all around the park.

DS: Splendid, and you were the hero for the day!

GT: Yes indeed. A month later, the rocks arrived and with the help of the Atomic Weapons Research Establishment, Aldermaston, who had a reactor, we got the samples irradiated and I analyzed them. We had a digitizer in the Space Physics group so that speeded up reading the charts, and Computing Services were very helpful in running the punched cards through the computers. In those days, they had a batch process, you took the cards in, you waited until the next day, and you got reams of computer paper back, with an error message saying you had left out a comma on page three or some other minor infringement. So you had to do it all again. Stan Wardle, who was the manager of the computer center speeded things up by allowing me to take things in whenever I liked for a rapid turnaround.

DS: Because this was exciting research.

GT: Yes and because there was a deadline of January 1st for the Apollo 11 Conference, which in 1970 and 1971 was held in downtown Houston.

As I said before, I still was not getting any support from the department in the form of students and at the conference I asked Jerry Wasserburg if there was any chance of visiting Caltech for a year. I can't remember his exact words, but I seem to remember that he said something like, "Sure, write your ticket, bud!" I hope I haven't imagined that line but I remember being very

pleased. So on August 7th, we set off for Caltech. By that time, Apollo 12 had come back and I had a couple more papers out or on the way (Fig. 4). In one of them (Turner 1971), I had a stab at what later developed into the subject of Thermochronology. I noticed that ^{39}Ar was released at lower temperatures in those Apollo 11 samples, which experienced the greatest loss of radiogenic ^{40}Ar on the Moon. In fact, the proportion of ^{40}Ar lost on the Moon in all the samples was the same as the proportion of ^{39}Ar released in the lab by a 1 h heating at around 670°C . Using the ^{39}Ar release to estimate an activation energy for argon diffusion, I showed that this was equivalent to the heating experienced over the diurnal cycle on the lunar surface for a time equal to the cosmic ray exposure age. So, the lowered K-Ar ages were caused by solar heating not as I previously thought, by impact.

DS: We are now into 1970.

GT: Yes. So I spent a year at Caltech. The atmosphere was different from Reynolds's lab.

DS: Is this because of your professional growth?

GT: That is certainly true; in Berkeley, I was little more than a student searching for a future, by the time I went to Caltech, I had found it. But there were real differences. Jerry's group was bigger for a start, and being in the middle of the Apollo program made things more intense. But between missions and conferences, it was quite relaxed. We did things together. We did a lot of planning of the next experiments as the samples kept coming in.

DS: He was there the whole year?

GT: He was there the whole year. I read Jerry's oral history (Marvin 2004) about what was going on at NASA and I am surprised we ever saw him because of the way he was struggling and fighting for the continuation of the program and with setting up the LRL.

DS: But by then, that would have been done? Didn't they have the LRL set up before Apollo 11?

GT: In theory. To read Jerry's account, at the time of Apollo 11, the whole thing was nearly a disaster. They had this totally impractical vacuum chamber, the F-201, for handling the samples under vacuum, that almost sucked a technician into the chamber. Jerry struggled with other people, as one of the so called "four horsemen" to get the LRL functioning as a first rate science lab. Sample analysis was low on NASA's priority list. I saw Jerry the scientist in Pasadena, but in the background, he was beavering away with the LRL and NASA, persuading people to do things correctly; to use nitrogen glove boxes instead of the F-201, to build proper clean room facilities and procedures, including for example, getting rid of organic matter, lead in paint and solder, talcum powder, to document everything including sample collection, and so on. He even took on

the job of equipping the LRL with his own selection and design of clean stainless steel and aluminum tools.

DS: So what else was he involved with in the group?

GT: Well, a major topic was the dating using Rb-Sr and U,Th-Pb and the related isotopic implications for the geochemical evolution of the lunar crust and mantle. Price Russ was analyzing samarium and gadolinium isotopes modified by neutron absorption in the lunar regolith. Later on, at Apollo 15 and 17, these effects in the lunar drill cores were used to work out the depositional history of the regolith, so-called neutron stratigraphy. With Jack Huneke and Frank Podosek, I set up an Ar-Ar extraction system which was used first on Apollo 14 samples.

DS: Who else was in the group at that time? Papanastassiou was there.

GT: He had been there since 1962. Don Burnett was on the faculty, he was working with Dotty Woolum on the design of the lunar neutron probe, which was used in Apollo 17 to measure the neutron profile in the hole left by the drill stem. Jack Huneke and Frank Podosek were the people I most interacted with. Jerry referred to his group as “whitewashing the fence,” Tom Sawyer style, in his acceptance speech for the Crafoord Prize.

DS: So you had a good year at Caltech.

GT: Excellent, yes. I analyzed Apollo 14 samples there, but also on the interpretative front, I worked on various projects. There was a publication by the group in Cambridge, UK, that said the Ar-Ar technique would be of use only for samples older than a million years. I thought this was nonsense, so I spent an evening at Frank’s apartment with Frank and Jack playing cards while I scribbled equations on scraps of paper. I showed that you could, in principle, determine any age so long as you chose a fluence appropriate for the age and K/Ca ratio, maximizing ^{39}Ar production, consistent with minimal corrections for interference from Ca interactions. For a young age, you use a low fluence, so you do not produce too much ^{36}Ar from Ca (compromising the atmospheric correction in terrestrial samples), but you need to use a correspondingly larger sample to generate enough ^{39}Ar to measure. So I produced an outline of that paper overnight. The figures I published in the paper are used routinely to estimate optimum irradiation times and sample sizes for Ar-Ar dating.

DS: With your coauthors playing cards!

GT: Yes! Well I was irritated by the Cambridge paper. The other thing I did at Caltech was to produce some calculations on the effects of recoil. The argon-39 is produced with enough energy to give them considerable recoil. So it was a very productive year.

Another strange anecdote. Before leaving for Pasadena, Kate’s sister had asked us to try to find the brother of a friend who was last heard of in Los Angeles

and had not kept in touch with his family. They had tried without success to locate him. One day in the lab, Jerry referred to “the silent Englishman.” I thought he was referring to me, but in fact, he was referring to his electronics technician. You’ve guessed it; his silent technician was the long lost brother—almost as improbable as some of the models of the early solar system.

DS: Okay, so you return to Sheffield and you have been there for 6 or 7 years. You have developed the Ar-Ar method, you have done the background theory, the various corrections, and you have been dating the lunar samples. What was the major thing to come out of this work, the idea that there was a terminal cataclysm?

GT: Well, the chronology of the maria was a major finding, that mare volcanism was going on as recently at 3.0 Gyr ago, maybe a little bit after that. We also dated the time of the major impact basins and concluded that several of these basins formed in a restricted time interval. Equally important is the observation that the basin-forming impacts were accompanied by “countless” smaller impacts on their ejecta blankets, decreasing significantly in rate over this same restricted time interval. A lot of this depended, and I guess it still does, on assigning the ages to specific impact basins. I guess, that is where the argument still is. There is actually something else that came out at that time, using ^{38}Ar and ^{37}Ar to measure cosmic ray exposure ages. In the anorthositic samples, in particular, calcium is the major target for production of ^{38}Ar from cosmic rays. The ratio of ^{38}Ar to ^{37}Ar produced from Ca in the neutron irradiation can therefore be used to determine exposure ages while at the same time you are measuring $^{40}\text{Ar}/^{39}\text{Ar}$ ages.

DS: Did you do a lot of that?

GT: Oh, you can’t avoid it. You get it incidentally as an adjunct to Ar-Ar dating.

DS: The basic finding there is that exposure ages are typically 100 Myr?

GT: Yes, typically a range of a few 100 Myr for an arbitrary location on the regolith. However, if you target rocks brought from depth by a particular crater, you can date the crater. The first such was Cone Crater at Apollo 14 (Turner et al. 1971). Rocks collected near the crater rim have exposure ages close to 26 Myr. Since then, five other recent craters have been dated, ranging in age from 2 Myr for South Ray at Apollo 16, to around 100 Myr for Camelot Crater at Apollo 17. A comparison of morphology with age shows very clearly the slow but steady rate of impact erosion on the Moon. The numbers of even smaller craters on the rims of dated crater indicate the recent influx rate of small meteorite-sized objects. I should add that other methods, in particular Kr-Kr, can produce more accurate exposure ages, but as

I said, the argon numbers come for free along with the $^{40}\text{Ar}/^{39}\text{Ar}$ dates.

DS: So we can do a pretty fine impact stratigraphy.

GT: Yes.

DS: So at Sheffield, did you continue with lunar studies?

GT: We returned briefly to meteorites. I started to get some NERC money and Peter Cadogan was my first postdoc in 1973. He worked on his Ph.D. with Geoff Eglington in Bristol and with Colin Pillinger who was there as a research fellow. I met him at various conferences and he was interested in coming to work with me. He is an imaginative person, he has written a book about the Moon and he had this idea of looking at cherts to measure the ancient atmosphere. He was with me for 4 or 5 years. He eventually left for a job in computing. Claire Enright was my first student around this time, also on NERC support.

DS: So the last Apollo mission was 1972 and you were publishing on this topic until 1978.

GT: We also measured the Russian Luna samples, but the data were not great. Years later, after we moved to Manchester, I was clearing out filing cabinets and I found some empty sample phials labeled Luna 24. On closer inspection, I discovered that they were not empty, but contained tiny specks of sample. With our improved techniques and better sensitivities we found that we could measure them! Ray Burgess divided each speck into three, used one for a polished section and two for Ar-Ar measurements. With that, we increased the number of Luna 24 ages in the literature by a factor of three (Burgess and Turner 1998).

DS: So during the 1980s in Sheffield, what was happening?

GT: We were changing gears slightly. I published a paper with Claire Enright and Peter Cadogan on unshocked chondrites (Turner et al. 1978). They had a narrow range of ages, about 50 Myr, and I decided to try to interpret these in terms of cooling rates and temperatures for argon retention. I deduced an expression in the paper based on a short argument that significant Ar loss ceases when the diffusion coefficient changes by a factor of “a few” on cooling in a time equal to that characteristic of significant loss ($t \sim a^2/2D$). I sent the manuscript to Proceedings of the 9th Lunar Science Conference and got the reviews back pointing out that Martin Dodson had derived and published this expression a few years previously! I kept my hand waving derivation in but credited Martin for the more rigorous derivation and used his numerical constant. I had an inkling later that the reviewer was Martin who I knew well and for whom I had great respect, though I hadn’t read his key paper.

DS: At this time, there was a lot of discussion on whether meteorite parent bodies were onion skin

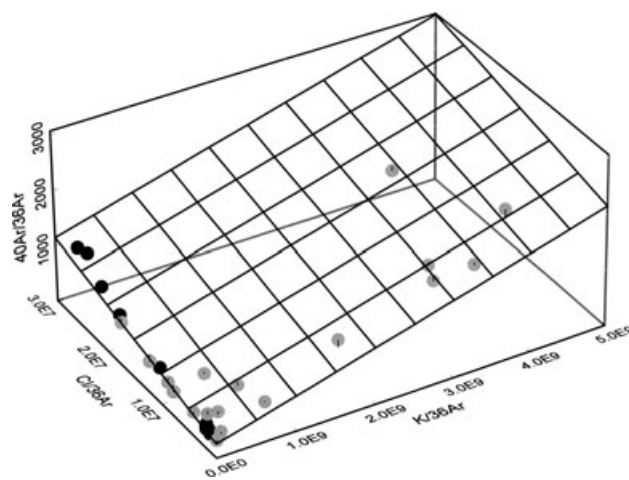


Fig. 5. Three component mixing plane showing K-correlated radiogenic argon, Cl-correlated excess argon, and atmospheric argon in quartz veins from a porphyry copper deposit. Crush data—black, stepped heating data—gray (Kendrick et al. 2001a).

structures or rubble piles. You were involved in that weren’t you?

GT: Only to the extent that we produced those ages on unshocked chondrites. Paul Pellas was always urging me to measure more ages. He strongly favored an onion skin structure.

DS: I seem to recall you showing a plot of Ar-Ar age against petrographic type, with the older types being younger suggesting earlier accretion.

GT: I don’t think we ever had enough ages to do that kind of thing convincingly. Paul used to get annoyed that I didn’t measure more of his samples.

DS: Is this the point at which you started to get involved with fluid inclusions?

GT: That happened in 1983. I met Tom Shepard of the British Geological Survey at one of the annual British informal gatherings on isotope geology and he asked whether we could date fluid inclusions. I said I would give it a go. Simon Kelley came to work with me so he got the job of working on Tom’s samples. What we found was that they were full of excess ^{40}Ar , i.e., over and above that which could be accounted for by potassium decay. We demonstrated that the excess is brought in by the mineralizing fluids. These are saline, lots of chlorine, and so generate ^{38}Ar when irradiated. A correlation of excess ^{40}Ar with ^{38}Ar indicated its presence in the mineralizing brines. We were able to fulfill the original aim of dating the mineralization in one case from the additional correlations with ^{39}Ar and ^{36}Ar . Another was totally dominated by excess (Kelley et al. 1986). Figure 5 shows a more recent example (Kendrick et al. 2001a), illustrating how crushing breaks open inclusions releasing the Cl-correlated excess ^{40}Ar but not

the ^{39}Ar and radiogenic ^{40}Ar , which are held in solid phases.

DS: What is the source of this gas in the fluids?

GT: It's just coming from the crust. Fluids go through heated rocks; the gases diffuse out of the rocks and are dissolved by the fluids.

DS: So they could have a variety of isotopic compositions?

GT: Yes. That is a whole area that terrestrial noble gas people work with all the time. The key signature isotopes are ^4He and ^{40}Ar from radiogenic production in the crust, primordial ^3He trapped in the Earth when it formed and coming out of the mantle, and finally ^{36}Ar from the atmosphere, dissolved in rain and entering the crust as air saturated groundwater. ^{40}Ar is present in all three. There are also characteristic signatures from neon and xenon isotopes. This is a very large research field. What we began was relating argon to chlorine in trapped fluids. One of the nice things you can do with this is to measure the chlorine abundance in the fluids thermometrically using the methods of fluid inclusion microscopy and then work out the concentration of Ar and the other noble gases in the fluid from the Ar/Cl ratios. Measuring absolute concentrations of noble gases in the fluids can be very useful, for example, in the case of ^{36}Ar , which starts out with a fairly limited range in air saturated water, but can change dramatically during processes associated with mineralization such as boiling or the formation of immiscible liquids. So that is something that was started by us and quite a few papers have followed linking the full range of noble gases and halogens. A good picture of our present state is provided in a number of publications by one of Ray Burgess's recent students, Mark Kendrick (e.g., Kendrick et al. 2001b).

MANCHESTER

DS: So I think we are approaching 1988, and your move to Manchester. Tell me what prompted the move. At Sheffield you have risen through the ranks to Reader?

GT: I had been a Reader for a year in 1980 when I was elected to the Royal Society. Then in short order I was promoted to Professor.

DS: Sometimes, the outside evaluators are more on the ball than your departmental friends. Was it the same procedure as the 17th century, six nominations posted on the board?

GT: Steve Moor bath and Keith Runcorn nominated me along with four other undisclosed signatories in 1977. They told me the chance of election was quite small so just to forget it, which I more or less did until I had this surprise phone call from Drummond Matthews in March 1980 and a letter through the post next day.

DS: It got you a promotion in the department, but has it made any other difference?

GT: There was one nameless official at NERC who said, "Now you can stop working," which showed a very strange view of the Royal Society. Fortunately, there were still many things to do. Also, it might be what made it possible to stay in my present job part time, 10 years after retirement.

DS: Talk to me about moving to Manchester.

GT: That happened as part of the Oxburgh Review of Earth Sciences. This was a review of the state of university earth science departments and it was meant to be a precursor to similar reviews of chemistry and physics. However, it turned out to be so expensive that it would break the bank if they tried it with the big departments. Some departments got additional funds, and there was a lot of movement of people.

DS: Didn't a lot of Canterbury people move to the Open University?

GT: Yes. Manchester did very well out of it. I just thought I would be better off in an earth science department with more suitable analytical facilities and access to experts and so on. I had tried to develop closer relations with geology at Sheffield, but it never really worked.

DS: You took several colleagues with you to Manchester?

GT: Jamie Gilmour and Ian Lyon came, and also Dave Blagburn, our technician who became our Senior Experimental Officer. Ian was a "new blood" lecturer, the name of a scheme introduced to counterbalance the top heavy age spectrum resulting from the sixties expansion. Jamie was still a postgraduate student when we moved to Manchester. Later, he became a postdoc and then a staff member. Manchester agreed to support all of us. Ian set up the first resonance ionization mass spectrometer for Xe and got it working in Sheffield. I had put in three proposals to SERC before we finally got funds to build it. When we moved it to Manchester, Jamie developed the cold finger, which greatly increased its sensitivity. Ian moved on to develop the ion microprobe and now is working on TOF-SIMS. Jamie stayed with the Xe instrument, RELAX [Refrigerator Enhanced Laser Analyser for Xenon].

DS: Okay, so now talk to me about RELAX. You have just given me an overview of how it was put together. How did it compare with other mass specs measuring xenon?

GT: Well, I was looking for something to do in the mideighties. There were papers being published by Sam Hurst and his group at Oak Ridge about resonance ionization, indicating that it could be a very efficient method for element specific excitation and ionization. I

wondered whether we could make use of this. I thought we would start with xenon because it is easiest, requiring the lowest photon energies. Recently, with the help of Ilya Strashnov, a Russian postdoc, Jamie has got a second instrument, RIMSKI, working with krypton. The initials stand for Resonance Ionization Mass Spectrometer for Krypton Isotopes. Resonance ionization is even harder for argon. But xenon is easiest, and it has nine isotopes with all these interesting processes at work, ^{129}I decay, ^{244}Pu fission, U fission, neutron reactions, spallation reactions, primordial compositions, lots of exciting science. So after three attempts, we got funding and Ian built the prototype and got his first spectrum while still in Sheffield. When we got to Manchester, there was money from Oxburgh to buy an ion probe, which Ian then took charge of. My main interest in the ion probe was to measure the carbonates in Martian meteorites, which we did with John Saxton's measurements on oxygen isotopes in ALH 84001 and Nakhla (Saxton et al. 1998, 2000). You know the rest.

DS: Yes, that was when I appeared on the scene. You have hosted me in your group on many occasions and I have thoroughly enjoyed it. Let's talk about diamonds.

GT: Ray Burgess came to work in Manchester, initially as a postdoc. There are two aspects to his diamond work. There are the so-called coated stones, or Zaire cubes. The "coat" consists of bands of microinclusions parallel to the growth planes of the diamond. Minoru Ozima had published some data on these diamonds that gave potassium-argon ages of 6 Gyr or so. Based on what we had seen with fluid inclusions and brines and excess argon, I thought these things probably contained excess ^{40}Ar , nothing to do with a K-Ar age. I met Ozima at a meeting where he showed me his Ar data, and it was obvious that there was indeed a correlation with chlorine. Unfortunately, the data were not very precise; these were irradiated samples and I believe that for health and safety reasons, the gas was extracted in a different lab from where the mass spectrometry was carried out, so I decided we should take a detailed look at them. We found a very strong correlation between ^{40}Ar and chlorine and not with potassium (Turner et al. 1990).

DS: This means that fluids were involved in forming the diamonds?

GT: Yes, mantle fluids are trapped in the diamonds. From the compositions inferred from this and other work and in different diamond localities, the argon is enriched by up to four orders of magnitude over the estimates for the ambient mantle, implying a correspondingly low concentration of the actual diamond-forming fluid. That started a detailed study of these inclusions and the halogen geochemistry, tying it in

with MORB measurements and with iodine at the surface, where iodine operates very differently from chlorine and bromine. Most of the iodine is in the crust while the chlorine is on the ocean.

The other diamond question was could we date them? For that, we need potassium, which means using diamonds with potassium bearing inclusions, which in turn leads to eclogitic suites containing clinopyroxene. The diamonds were cut to expose the inclusions, which were then melted with a laser. Typically, ages close to the eruption age were obtained, but with some showing evidence of pre-eruption argon (Burgess et al. 1989, 1991). It occurred to us that argon would readily diffuse in the pyroxene at mantle temperatures, possibly collecting at the interface between diamond and inclusion and be released uncorrelated with ^{39}Ar in the laser extraction. It might also be lost when sectioning the diamond, so the current procedure is to drill down to the inclusion with the laser.

Well, this is getting terribly detailed. One of the problems with the diamond work is acquiring samples. You are handed these precious mystical things wrapped in paper packets, rather like children being given sweets. I have to say, it is a lot simpler dealing with meteorite curators!

DS: So you are at Manchester, you have RELAX working, you have the ion probe working, you have done the work on Martian carbonates and diamonds. We are up to the late 1990s.

GT: We did a lot of work on terrestrial noble gases in the 1990s, principally by Finlay Stewart, Pete Burnard, and Darryl Harrison, with a peripheral contribution from me. One problem that interested me was an attempt I made to use $^3\text{He}/^{36}\text{Ar}$ in black smoker sulfides to calculate helium/heat ratios of the paleofluids. I also made a contribution to work on samples from the Iceland hot spot trying to work out the noble gas components coming from crust, mantle, and atmosphere. In the 1970s, Peter Cadogan had had the idea of looking for ancient atmospheric argon in chert and published some measurements on the Rhynie chert, a Devonian hot spring deposit from Scotland containing some of the earliest land plants. The object was to measure the rate of change of $^{40}\text{Ar}/^{36}\text{Ar}$ in the atmosphere, which would provide a measure of the outgassing rate of ^{40}Ar , an important parameter in Earth evolution models. As we now know from fluid inclusion studies, there are lots of minerals containing ancient atmospheric ^{36}Ar , the problem is to find samples with ancient atmospheric ^{40}Ar uncontaminated by crustal ^{40}Ar . As a surface hot spring, Rhynie was a good candidate. Following Cadogan's work, it became a joke within the group that every new arrival had to measure a few samples of Rhynie chert. These were eventually published in a paper on Rhynie

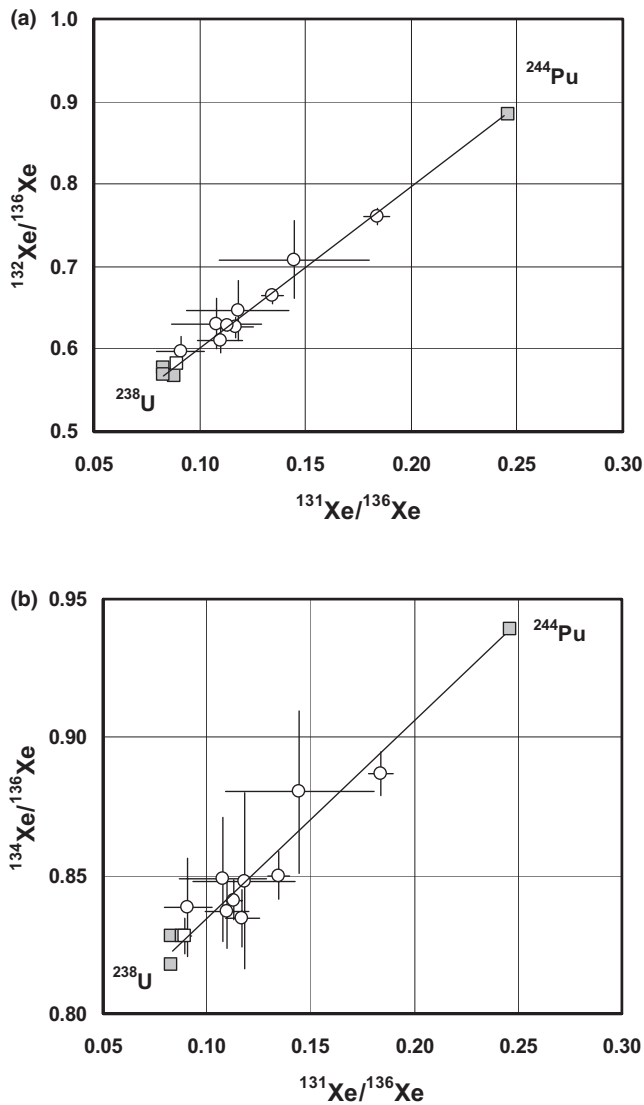


Fig. 6. Xe isotopes in Hadean zircons indicate that the Xe is a mixture of fission Xe from extinct ^{244}Pu and ^{238}U (Turner et al. 2004).

chert (Rice et al. 1995). I understand that Finlay Stuart has continued the work and produced a new value for the Devonian $^{40}\text{Ar}/^{36}\text{Ar}$.

One of the last topics I have been working on is xenon trapped in Hadean zircons. At the Goldschmidt conference in Davos in 2002, I spoke to Mark Harrison who was hoping to detect fission products from extinct ^{244}Pu in 4.3 Gyr old zircons from Western Australia. The major problem was the sensitivity required to analyze single zircons. I offered to try out RELAX on the problem and subsequently, he sent a number of zircons that had been dated on the ion probe by U-Pb and Pb-Pb. Sure enough, we found a mixing line between uranium fission and plutonium fission that we published in *Science* (Fig. 6, Turner et al. 2004). We then took it a stage further

realizing that if we irradiated the zircons, we could produce neutron fission from the uranium and could measure U-Xe ages. We published this and the underlying theory in *EPSL* (Turner et al. 2007). We are currently working with Kevin McKeegan at UCLA on lunar zircons, which have already been exposed to neutrons on the lunar surface. $^{235}\text{U}/^{238}\text{U}$ has decreased by a factor of around 40 over lunar history making the effects of that neutron exposure time-dependent; another interesting problem to determine when the exposure occurred.

My latest project has been to devise a new method, involving low fluence neutron activation, to search for evidence of ^{36}Ar from the decay of extinct ^{36}Cl in sodalite from Allende. Published evidence for excesses of ^{36}S contrasts with the absence of a corresponding huge excess of ^{36}Ar . I have devised a method to distinguish monoisotopic ^{36}Ar from ^{36}Cl decay from ^{36}Ar accompanied by ^{38}Ar from the absorption of cosmogenic secondary neutrons by chlorine. The measurements indicate a small contribution from ^{36}Cl decay (Turner et al. 2011).

DS: So this brings us up to date. So what has been the most exciting event in this long and successful career?

GT: Well, it was taking that graph out of my drawer with the data plotting in a circle and realizing what it was telling me and how it could open up a whole new field of dating; and linked to that the development of a successful theoretical explanation (Fig. 4).

DS: If you think about the last 34 years other than your own work, what are the most exciting few developments?

GT: Well, there are such a lot. When I went to Berkeley, the electron probe was just coming into general use in petrology and has totally changed the field of meteoritics and planetary science. When I began research, B²FH had only been published a few years previously, explaining the origin of the elements. Because of limitations in early mass spectrometers, it was believed that isotope abundances were quite uniform in meteorites and the Earth, an indication of thorough mixing in a gaseous nebula. The discovery of extinct isotopes and presolar isotope anomalies changed all that and led on to the isolation of presolar grains and the detailed interpretation in terms of nucleosynthesis. Our understanding of planetary science has changed almost out of recognition. Plate tectonics arrived in the 1960s and explained why I could never understand the theories propounded by my geology tutors in the 1950s. Apollo brought the Moon from the telescope to the lab and gave us another planet to test our understanding on. Now Mars is doing the same. Isotope geochemistry has evolved into a subject without which it is now impossible to have a true understanding of planetary and solar system evolution. As in all fields of science, the development of computing power has changed how we can even think about

problems. These major developments have all been brought about by a host of minor developments of instrumentation, techniques, and ideas, such as, for example, in my own case, Ar-Ar dating.

DS: Well, if somebody new was coming into the field, what direction would you point them in?

GT: I'm not sure, but one thing I would stress is that the difficult question facing them is not how to solve problems, but how to pick problems worth solving. With so many new areas being developed in the 1960s and with Apollo on the horizon, picking first-order problems was probably more straightforward than now. I suppose, it's just that the front line has moved and maybe now is on Mars, or on the ocean floor, or in rare types of meteorite being found in the Antarctic. After all, the story of Allende demonstrates how whole careers can be based on understanding one particularly interesting piece of rock. Many of today's problem areas require very large collaborations with huge data sets and interactions between people using different scientific languages. The days of one man and his mass spec are well and truly over. You should read a lot of reviews in the area you think you are interested in. Based on my own experience, I would conclude that a key decision very early on is pick the right person to work with. In this, I was very lucky. So, be lucky.

Acknowledgments—This interview was recorded on August 8th, 2011, and edited by the author and G. T. As of November 23, 2011, a CV, publication list, and other information appear on <http://www.manchester.ac.uk/research/Grenville.turner/publications/>. I am grateful to NASA for financial support.

Editorial Handling—Dr. A. J. Timothy Jull

REFERENCES

- Burgess R. and Turner G. 1998. Laser probe ^{40}Ar - ^{39}Ar age determinations of Luna 24 basalts. *Meteoritics & Planetary Science* 33:921–935.
- Burgess R., Turner G., Laurenzi M., and Harris J. W. 1989. ^{40}Ar - ^{39}Ar laser probe dating of individual clinopyroxene inclusions in Premier eclogitic diamonds. *Earth and Planetary Science Letters* 94:22–28.
- Burgess R., Turner G., and Harris J. W. 1991. ^{40}Ar - ^{39}Ar laser probe studies of clinopyroxene inclusions in eclogitic diamonds. *Geochimica et Cosmochimica Acta* 56:389–402.
- Fredriksson K. and de Carli P. 1964. Shock experiment and petrology of sample. *Journal of Geophysical Research* 69:1403–1406.
- Heymann D. 1967. On the origin of hypersthene chondrites: Ages and shock effects of black chondrites. *Icarus* 6:189.
- Kelley S., Turner G., Butterfield A. W., and Shepherd T. J. 1986. The source and significance of argon isotopes in fluid inclusions from areas of mineralization. *Earth and Planetary Science Letters* 79:303–318.
- Kendrick M. A., Burgess R., Patrick R. A. D., and Turner G. 2001a. Halogen and Ar-Ar age determinations of inclusions within quartzveins from porphyry copper deposits using complementary noble gas extraction techniques. *Chemical Geology* 177:351–370.
- Kendrick M. A., Burgess R., Patrick R. A. D., and Turner G. 2001b. Fluid inclusion noble gas and halogen evidence on the origin of Cu-Porphyry mineralising fluids. *Geochimica et Cosmochimica Acta* 65:2651–2668.
- Kirsten T., Krankowski D., and Zähringer J. 1963. Edelgas- und Kalium-Bestimmungen an einer größeren Zahl von Steinmeteoriten. *Geochimica et Cosmochimica Acta* 27:13–42.
- Marvin U. B. 2004. Oral histories in meteorites and planetary science: XII. Gerald J. Wasserburg. *Meteoritics & Planetary Science* 39:A177–A197.
- McDougall I. and Harrison T. M. 1988. *Geochronology and thermochronology by the $^{40}\text{Ar}/^{39}\text{Ar}$ method*. New York: Oxford University Press. 212 p.
- Merrihue C. M. and Turner G. 1966. Potassium-argon dating by activation with fast neutrons. *Journal of Geophysical Research* 71:2852–2857.
- Naughton J. J. 1963. Possible use of argon-39 in the potassium-argon method of age determination. *Nature* 197:661–663.
- Paneth F. A., Reasbeck P., and Mayne K. I. 1952. Helium 3 content and age of meteorites. *Geochimica et Cosmochimica Acta* 2:300–303.
- Pepin R. O., Reynolds J. H., and Turner G. 1964. A comparison with natural argon in its diffusion. *Journal of Geophysical Research* 69:1406–1411.
- Reynolds J. H. and Turner G. 1964. Rare gases in the chondrite Renazzo. *Journal of Geophysical Research* 69:3263–3281.
- Rice C. M., Ashcroft W. A., Batten D. J., Boyce A. J., Caulfield J. B. D., Fallick A. E., Hole M. J., Jones E., Pearson M. J., Rogers G., Saxton J. M., Stuart F. M., Trewin N. H., and Turner G. 1995. A Devonian auriferous hot spring, Rhynie, Scotland. *Journal of the Geological Society of London* 152:229–250.
- Saxton J. M., Lyon I. C., and Turner G. 1998. Correlated chemical and isotopic zoning in carbonates in the martian meteorite ALH 84001. *Earth and Planetary Science Letters* 160:811–822.
- Saxton J. M., Lyon I. C., Chatzitheodoridis E., and Turner G. 2000. Oxygen isotopic composition of carbonate in the Nakhla meteorite: implications for the hydrosphere and atmosphere of Mars. *Geochimica et Cosmochimica Acta* 64:1299–1309.
- Turner G. 1965. Extinct iodine 129 and trace elements in chondrites. *Journal of Geophysical Research* 70:5433–5445.
- Turner G. 1968. The distribution of potassium and argon in chondrites. In *Origin and distribution of the elements*, edited by Ahrens L. H. Oxford: Pergamon Press. pp. 387–398.
- Turner G. 1969. Thermal histories of meteorites by the ^{39}Ar - ^{40}Ar method. In *Meteorite research*, edited by Millman P. M. Dordrecht, The Netherlands: D. Riedel. pp. 407–417.
- Turner G. 1970. Thermal histories of meteorites. In *Palaeogeophysics*, edited by Runcorn S. K. London: Academic Press. pp. 497–507.
- Turner G. 1971. ^{40}Ar - ^{39}Ar Ages from the Lunar Maria. *Earth and Planetary Science Letters* 11:169–191.
- Turner G., Miller J. A., and Grasty R. L. 1966. The thermal history of the Bruderheim meteorite. *Earth and Planetary Science Letters* 1:155.

- Turner G., Huneke J. C., Podosek F. A., and Wasserburg G. J. 1971. ^{40}Ar - ^{39}Ar ages and cosmic ray exposure ages of Apollo 14 samples. *Earth and Planetary Science Letters* 12:19–35.
- Turner G., Enright M. C., and Cadogan P. H. 1978. The early history of chondrite parent bodies inferred from ^{40}Ar - ^{39}Ar ages. Proceedings, 9th Lunar and Planetary Science Conference. pp. 989–1025.
- Turner G., Burgess R., and Bannon M. 1990. Volatile-rich mantle fluids inferred from inclusions in diamond and mantle xenoliths. *Nature* 344:653–655.
- Turner G., Harrison T. M., Holland G., Mojzsis S. J., and Gilmour J. 2004. Extinct ^{244}Pu in ancient zircons. *Science* 306:89–91.
- Turner G., Busfield A., Crowther S. A., Harrison T. M., Mojzsis S. J., and Gilmour J. D. 2007. Pu-Xe, U-Xe, U-Pb chronology and isotope systematics of ancient zircons from Western Australia. *Earth and Planetary Science Letters* 261:491–499.
- Turner G., Burgess R., Kelley S. P., and Wasserburg G. J. 2011. Evidence for extinct ^{36}Cl from excess ^{36}Ar in Allende sodalite. *Meteoritics & Planetary Science* 46:A240.
-