

Report

Oral histories in meteoritics and planetary science: VII. Alastair G. W. Cameron

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(Received 2002 April 9; accepted in revised form 2002 August 28)

Abstract—In this interview, Alastair Cameron recounts how he started his career as a nuclear physicist but taught himself astrophysics after he read a paper that required an astrophysical explanation for the presence of technetium in red giant stars. Subsequently, as new analytical data became available, he periodically updated the Suess–Urey tables of elemental abundances to enhance the value of the systematic approach they provided to understanding individual processes of nucleosynthesis. Since many of these new data were based on analyses of carbonaceous chondrites, he taught himself meteoritics. In recent decades, Cameron has focused his research interests on problems such as the provenance of certain components of meteorites (calcium-aluminum-rich inclusions, FUN (fractionated and unknown nuclear) anomalous inclusions, amoeboid olivine aggregates, and presolar grains) that he believes to have formed in the supernova envelope prior to formation of the solar nebula, the origin of chondrules in the primitive solar nebula, and the origins of the solar system and of the Earth–Moon system. To investigate these subjects he has pioneered the use of advanced computer technology to make lengthy calculations of nucleosynthesis in complicated networks. After teaching courses and advising graduate students at several research institutes and colleges, Cameron served as a Professor of Astronomy at Harvard University from 1973 to 1997 when he was appointed to the Donald H. Menzel Research Professorship of Astrophysics. In 1994, The Meteoritical Society honored him with the Leonard Medal at its meeting in Prague, the Czech Republic.

UBM: Al, as I understand it you started out as a nuclear physicist and you might have remained one except for a transforming moment that occurred early in your career.

AGWC: That's right, there was one.

UBM: Would you describe it to me?

AGWC: After I got my Ph.D. at the University of Saskatchewan, working with a betatron doing photonuclear physics, I accepted an assistant professorship at Iowa State College (as it was then named) at Ames, largely because they had a new 70 MeV synchrotron there. The betatron I used was only 28 MeV. So my idea was to continue my research using these higher energy photons. Once I got to Ames I discovered that, in fact, they weren't really doing any significant amount of physics research with the synchrotron. They were mainly trying to get a better beam current. That kind of engineering tinkering was of zero interest to me. So there I was with time hanging heavy on my hands. Ames had a smallish research laboratory of what was then the Atomic Energy Commission (AEC), mainly dealing with some solid state things for reactors. They had a reading room, and I occasionally went browsing through the magazines there. On this occasion I was looking



FIG. 1. Alastair Cameron, Professor of Astronomy at Harvard University, ca. 1995, when he was presented with the Leonard Medal.

into what was then called the *Science News Letter* (now *Science News*). There I found a report on a paper by the astronomer, Paul Merrill of Caltech, on the discovery of technetium lines in class-s red giant stars. It immediately popped into my mind: "Where's he going to get the neutrons for that? He would need great floods of them in the interiors of the stars!" This was very exciting to me, but it was an astrophysical problem and, at that point, I knew essentially zero astrophysics. So I decided on the spot to repair that omission rapidly. I bought a whole bunch of basic textbooks in the field and subscribed to the *Astrophysical Journal* and was on my way to a new career. That was in 1952.

UBM: So your knowledge of astrophysics is entirely self-taught.

AGWC: Well, so is my meteoritics.

UBM: I suppose the meteoritics came later, after you mastered the basics of astrophysics.

AGWC: Yes, it did. In some respects I think it's better to learn on your own. There is a big difference between being passive and active about it. When you sit in a lecture course you hear all that is presented to you passively, but you don't really remember it and take it in in quite the same way as when you struggle through it yourself. Being problem oriented is a great motivation.

UBM: At least you started with a good grounding in nuclear physics.

AGWC: Yes, it would have been very difficult without it. The first problem I looked into was how to produce neutrons after a star starts evolving. The star begins by burning hydrogen and the first thing that could possibly produce neutrons are deuterium–deuterium reactions, but the deuterium present would have only a small abundance, and the hydrogen present would capture the neutrons so quickly that this was not a promising source at all. Next would come helium burning and reactions in carbon. Very shortly you discover that the reaction of ^{13}C with alpha particles producing neutrons is exothermic—so that will give you neutrons in whatever numbers you want. I published my first article in astrophysics on this process in 1955 and very soon afterward Martin Schwarzschild invited me down to Princeton to give a talk. At Princeton I suggested that this is a way to get lots and lots of neutrons.

UBM: How did they view this idea at Princeton?

AGWC: In fact, it's sort of funny because I came up with a notion that maybe a star, having gone through hydrogen burning and commencing helium burning would actually mix hydrogen down with the carbon it was forming by the triple-alpha reaction and make some ^{13}C and produce lots of neutrons that way. But Schwarzschild said: "Oh, no, no, no. The hydrogen and carbon won't mix. They can't. There is a big entropy barrier between them." So I put that aside for some time. Some years later, of course, the models evolved to the point where, yes, they do start mixing. So that was a bit of *post facto* justification for me. It's too bad I was discouraged from pushing that idea a little harder.

UBM: So, as your first foray into astrophysics you began working out the basics of star evolution.

AGWC: At first, I really was working on star formation rather than evolution, because we had to begin with hydrogen. I started thinking that here we have this cloud of molecular hydrogen and to wonder how it makes stars. We begin by heating hydrogen until it leads to dynamic collapse of the interstellar cloud. What we mean by collapse is simply that the adiabatic exponent, gamma, becomes less than four-thirds and down it goes. It's as simple as that. That was the actual route that I followed in thinking about the problem and getting to the point of asking: when does gamma get to be less than four-thirds? So, first we have dissociation and ionization of hydrogen, then ionization of helium. These things happen at certain amounts of compression and lead to the collapse of the interstellar cloud, which, in turn, leads to making a star.

UBM: Were you still at Ames when you were working on this?

AGWC: Yes, but I was ready to move on. I no longer had much interest in photonuclear reactions but I still thought I needed a nuclear physics environment to pursue what I began to call "nuclear astrophysics". So I applied to the Canadian Atomic Energy Project at Chalk River, Ontario, and was accepted. (I forgot to say earlier that I was a Canadian, born in Winnipeg, Manitoba.)

UBM: What problems did you work on at Chalk River?

AGWC: I spent a lot of time working out nuclear cross-sections from known data and calculating or estimating what was not known. Then I concentrated on improving semi-empirical methods for calculating nuclear masses, nuclear level densities, and nuclear radiation widths, all of which proved to be useful for tackling astrophysical problems. I also was interested in elemental abundances in the solar system. It was clear from the presence of technetium that there were active nuclear reactions involving heavy elements going on in stellar interiors and in the advanced stages of nuclear burning. The notion of tying all of this together into sort of a galactic evolution of chemical abundances, and so on, wasn't really there at the time. That tended to come along as we started to investigate things in more detail.

UBM: The first version of the Suess–Urey table of elemental abundances must have been published about that time. Was that important to your research?

AGWC: The Suess and Urey table was published in 1956 and was critically important for me, because it gave a systematic approach to understanding the individual processes of nucleosynthesis. They took some abundance data from astrophysical sources but much of it came from analyses of meteorites. At that same time, the astrophysicists were determining that the elemental abundances relative to hydrogen in stars were variable. The older stars tended to have smaller abundances of heavy elements in them, and this implanted the notion that there was a chemical evolution of the galaxy. So these things were starting to fit together in a very exciting way.

UBM: Were you aware at that time of the information on elemental abundances in meteorites published in 1937 by Victor M. Goldschmidt?

AGWC: I was completely unaware of it.

UBM: It inspired Brian Mason, as a student in New Zealand, to go to Norway and do his thesis with Goldschmidt.

AGWC: But that table wouldn't have affected me because it didn't involve nuclear physics and the formation of the elements. Goldschmidt's abundances were basically a chemical matter, of importance for understanding meteorites but not for the physics of element formation.

UBM: You did a lot of work updating the Suess–Urey table, didn't you?

AGWC: I spent two or three decades doing that. Suess started the whole thing by making interpolations among the heavy elements that were purely empirical. He said, in effect, that it looks as though the odd mass numbers have abundances that vary smoothly. Therefore, when he didn't know experimentally what a given abundance was, he interpolated it by making the curve smooth. In general, that approach, which was also adopted by Urey, is fine. But what happens when you come to a closed neutron shell? Then, certainly, the abundances do not vary smoothly across that shell. That means that you have to start doing some fine-tuning to get away from the empiricism and make sure that things are consistent with what you know about nuclear cross-sections.

UBM: So did you adjust the tables as new data came along?

AGWC: Yes. I tried to update the abundance table every few years. As new measurements accumulated I would readjust things. But the principles of making the adjustments generally tended to remain the same. You have to do it quite differently in the lower atomic number range, say between neon and calcium. There you can actually start using semi-equilibrium theory to do it. But the data kept improving and I began to rely more and more on elemental abundances in carbonaceous meteorites. Initially it had been from ordinary chondrites, for the most part. Then, gradually, the details of measured abundances improved. Ed Anders, as I recall, wrote a paper at some point justifying the use of carbonaceous chondrites as primary meteorites representing abundances in the primeval solar nebula.

UBM: Was that Ed's paper of 1989, coauthored with N. Grevesse? They published an abundance table based almost entirely on Ed's own analyses of carbonaceous chondrites with practically no interpolations.

AGWC: No, it was an earlier paper by Ed alone. I had been doing the same sort of thing all along but using published data. When you get away from the heavy elements, there was a controversy in which Harold Urey was strongly involved, on whether you should take the solar abundance of iron or the meteoritic abundance of iron. A lot of people oscillated between taking one or the other. Harold made a big deal out of the difference in the apparent abundances. Every time I made a new abundance table, I rethought all that and chose the

meteoritic abundance for a variety of reasons. In the end, of course, the determination of the solar abundance changed and began to agree with the meteoritic abundance of iron. So that controversy disappeared. I found that, fortunately, I had made the right choice all along.

UBM: How did the paper published in 1960 by John Reynolds, at Berkeley, announcing his discovery of ^{129}Xe in meteorites affect your thinking? It implied the existence of an extinct parent radionuclide, ^{129}I .

AGWC: I was very excited about it. That paper provided another turning point in my self-education by refocusing my attention on meteorites. For some years prior to John's actual discovery, it had been recognized that ^{129}I would be a possible extinct radionuclide, which would be produced in supernovae by the *r*-process. So one should look for evidence of it in meteoritic materials. John was actually following this line of thought to see if such an anomaly could be detected. At that time, I was spending a year at Caltech, among the astronomers. We all were impressed.

UBM: In one of your papers you wrote that the ^{129}I discovery introduced the first clear evidence of the *r*-process in nucleosynthesis.

AGWC: To put it another way: when ^{129}I was discovered one could recognize that it was an *r*-process product, but that didn't establish the *r*-process *per se*. Evidence of the *r*-process was implicit in the Suess–Urey abundances. They may not have said it that way. But how else could you interpret that stuff? When you look at the processes of neutron capture in the region of the heavy elements, it's clear that there had to be a process that operated on a fast timescale. In fact, it generated some interesting nomenclature. I called it the "neutron capture on a fast timescale process", and Burbidge, Burbidge, Fowler, and Hoyle called it the "*r*-process". Guess who prevailed!

UBM: How did the ^{129}I discovery influence your own ideas?

AGWC: The abundance of the ^{129}I relative to the stable isotope, ^{127}I , provided important evidence about both the galactic environment and the formation of the solar system, so I was inspired to think seriously about the factors that led to the formation of the solar system. Up until then, people began with what they could observe of the present solar system and worked backward to try to figure out its origin. In attempting to account for the regularities of spin and orbital motions that cause all the planets to revolve about the Sun in the same plane at radial distances roughly in a geometrical progression, they hypothesized scenarios such as having a passing star strip off matter from the Sun, or a contracting cloud of gas shed rings of matter occasionally. These ideas were almost devoid of real scientific content and tended to make solar system origin seem not quite respectable. I set out to learn about the prior states of matter in the galaxy and see what sorts of events would lead to the evolution of a solar system.

UBM: Again, your background in nuclear physics must have been essential to this approach.

AGWC: No doubt, it was. The presence of ^{129}I in the early solar system, considered in the light of the general theory of stellar nucleosynthesis as it had developed by the early 1960s, indicated that 4.6×10^9 years ago nucleosynthesis was occurring and contaminating the interstellar medium with new elements. Clouds containing these elements were contracting to form new stars.

UBM: After Reynolds' discovery, evidence of additional isotopic anomalies kept coming to light, including some really big ones.

AGWC: Yes, so I wound up writing papers about them as a whole. I was especially interested in the so-called FUN (fractionated and unknown nuclear) anomalies, which were reported in inclusions in the Allende carbonaceous chondrite by Jerry Wasserburg and his group in 1977. My analysis of the FUN inclusions is that you have to get them by changing the mix of the *s*- and *r*-process products a little bit in other stars. I continue to believe that the FUN inclusions were inherited by the solar system as the inclusions were coming in with grains that were a lot larger.

UBM: Then, in the 1990s we learned about interstellar grains, or presolar grains, as we now call them. They reveal their presence by isotopic anomalies that only could be created in other stars. I think it is a marvelous advance in our science that we can separate out presolar grains from meteorites and actually see them and analyze them.

AGWC: It certainly is that. And we also got interplanetary dust particles (IDPs) which are relatively unaltered collections of materials from comets, so they include a heterogeneous collection of interstellar grains that were present at the time the solar system was formed.

UBM: You remarked a moment ago that most people talking about the origin of the solar system or of the formation of planets within the solar system usually start with what we have at present and try to work backwards, instead of starting from nuclear physics and working forward, as you like to do.

AGWC: Well, I like to begin with nuclear physics and observation. But good observations of young stars and planetary systems had not been done at the time I started worrying about these things. Today, of course, observations provide a much more firmly based starting point for people to take that approach.

UBM: To what extent were you alone in your approach? I think in our meteoritical community, you may have been quite alone.

AGWC: That may be the case. But I think the notion of a solar nebula goes back—I don't know how far that goes back.

UBM: Many people date it to Immanuel Kant, ca. 1755.

AGWC: From the meteoritical point of view, that would not be meaningful in terms of what the nebular composition was. The idea that there was gas is what dates back to Kant. But the thought that the gas would interact with solids and produce chemical changes is much newer than Kant. That's not really where I started from, though. I started by wondering

how the Sun could form—the astrophysics of it. What did we have out there? We had molecular clouds. At least nowadays we've got molecular clouds, but in the 1950s and 1960s when I began thinking about solar system origin, the molecular clouds were not considered to be involved.

UBM: When did they first begin to gain recognition?

AGWC: Much more recently than that, but I can't suggest any major thing that suddenly launched molecular clouds onto the stage. Back in the earlier days we used to describe the interstellar medium as consisting of ionized gas and neutral gas. These are just two of the components that we now recognize, which include with them a molecular phase and a rarified cosmic-ray phase, making four in all. It was clear all along that some parts of the interstellar medium were a little denser than others, to explain the heavy obscuration of starlight. My thinking, as I recall, was to try to divine whether they were ionized, meaning that they were bathed in a big flood of ultraviolet radiation, or neutral, with not nearly as great a density as we now think about for molecular clouds. At any rate, the question in my mind in those days was OK, we've got this cloud. How does it collapse to make a star? In order to collapse, it's got to shrink enough to start heating up. Eventually, when molecules start dissociating in it, gamma (the adiabatic exponent) becomes less than four-thirds and collapse occurs. The idea of gamma being less than four-thirds as a condition of collapse is, of course, directly stolen from the then prevailing thoughts about what happens in a supernova core—its the same basic physics.

UBM: Supernovae are said to have triggered the collapse of molecular clouds to initiate the solar system.

AGWC: That's a different kind of collapse—if it occurred. At first, the question is: how does a star collapse to make a supernova? That's the core-collapse aspect of the gamma-less-than-four-thirds problem. Later on, the idea of the supernova triggering the collapse of a molecular cloud came about when we started seeing all these isotopic anomalies in early solar system material. How did the anomalies get there; especially the short-lived, radioactive ones? We said, well, there must have been a supernova nearby. But if there was a supernova nearby, maybe that triggered the formation of the solar system in the first place. That's how the train of thought developed.

UBM: Al, would you be willing to reduce what you have called an incredibly complex affair and give me a brief account of your favored scenario of the origin of the solar system?

AGWC: Well, you have to start with a molecular cloud complex; these are forming all the time in the galaxy and live for a few tens of millions of years before dispersal. Regions within the molecular clouds start to contract under the influence of their self-gravity, forming what are called cores. Some of these are very massive and form associations of massive stars that evolve quickly in just a few million years to the supernova stage. When a supernova explodes, the outgoing shock wave sweeps up the molecular cloud gas. If the shock encounters a core within ~ 4 parsecs or so, it will be strong enough to shred

that core. But if the core is farther away, then the swept-up gas wraps around the core and compresses it, thus triggering an accelerated collapse of the core and injecting fresh radioactivities from the supernova into it. This has been numerically simulated by Harri Vanhala, one of my graduate students. The total elapsed time between the supernova explosion and the completion of the formation of the solar nebula is ~ 1 Ma. That supernova explosion may be responsible for lots of triggered core collapses in the molecular cloud complex, because it should be able to trigger those collapses out to distances of a few tens of parsecs. The cores not triggered into collapse will usually collapse anyway, but this will take a significantly longer time and the resulting stellar nebulas will not have freshly synthesized radioactivities within them.

Of course when the solar nebula was formed, a lot of further somewhat complicated processes occurred before we got the Sun and planets. I still believe most of what I wrote about these processes in my 1995 paper in *Meteoritics* prepared in connection with the award of the Leonard Medal. The major modifications that I would make concern the formation of chondrules and calcium-aluminum-rich inclusions (CAIs). Recent work at Los Alamos by Stirling Colgate, Hui Li, and others has shown that there should be large-scale vortices always present in the nebula and driven by what is called the Rossby Vortex Instability. There are standing shock waves trailing these vortices and this appears to be just the right environment to form dust balls into chondrules in very large numbers within the nebula. The CAIs are a different matter; it now appears to me that these were produced in the expansion of the envelope of the triggering supernova when the temperature in the inner region fell to a small enough value so that the more refractory minerals could condense. Subsequently, once the solar nebula was formed by the compression of a molecular cloud core, the CAIs would be injected into it while chondrules were forming.

The solar nebula is very opaque to transmittal of radiation from the Sun, so it cannot be directly heated by that radiation. But all young stars develop bipolar outflow jets as they accrete material, as this prevents the accretion from putting too much angular momentum into them; the jets are powered by magnetic fields embedded in their stellar nebulas. The magnetic fields accelerate the ions, and the ions carry along the neutral atoms with them. The resulting ion-neutral friction then ionizes the neutrals. But at the same time there is recombination of the ions, and this produces ultraviolet light that shines down on the top surfaces of the nebula, thus providing the heat. Some rough calculations of this are in my Leonard Medal paper.

So at that stage we have formed the solar nebula. In the same paper I tried to address the next stages in which the planets are formed. When you start with a huge collection of interstellar grains in the nebula, how do they stick together? Consider the collision of a pair of grains. The kinetic energy in the collision is converted into the energy required for the deformation of the grains and into internal modes of oscillation, and it becomes

thermalized. At that point the grains cannot separate because it is statistically very unlikely that the thermal energy can be concentrated into the kinetic energy required for a rebound, and the thermal energy is quickly radiated away as infrared radiation. As other grains collide with this pair they will stick for similar reasons, and we get a clump. It will be of very low mean density and likely to have a fractal structure. When clumps collide they will partially interpenetrate, thus forming bonds between many of their constituents and gluing the clumps together.

Now consider the newly formed solar nebula, which will have a vertical thickness of order $1/10$ of the radial distance from the Sun. At every point in a vertical column in the nebula the predominant force acting on the matter is the gravitational force of the Sun which keeps the matter in orbital motion. This force will have a downward component pointing toward the midplane of the nebula, and it will be strongest at the top of the nebular column. The grains at the top of the column will then be accelerated downward more rapidly than those lower down in the column, so the clumping of grains goes fastest at the top of the column. These clumps are then biggest and are least affected by friction with the gas. What follows is what I call a "vertical avalanche"; a wave of increasingly larger bodies starts down from the top of the column and sweeps up most of the material in its path, just like an avalanche coming down a mountain slope. My simulations of this indicated that the largest bodies could grow to ~ 0.1 km by the time they reached midplane. So now we have planetesimals at midplane, and they will start colliding with one another in the manner so well simulated by George Wetherill, and we are on the way to making planets. We get to Mars-sized objects in $\sim 10^5$ years in the terrestrial planet region, but the formation of the Earth is not complete until $\sim 10^8$ years, because the time interval between fewer but larger bodies becomes greatly lengthened.

UBM: From the molecular cloud to CAIs, to chondrules, to the Earth in 7 minutes! *A tour de force*, Al. Thanks very much. Now that we have a solar system, let's turn to the origin of the Earth–Moon system. What first aroused your interest in this problem? Was it your considerations of how the system got the angular momentum it has?

AGWC: Well, yes, except that's actually a more logical statement of how I proceeded than what I really did. When I first got curious about the origin of the Moon we had three leading theories. I went through all three at one point and never was satisfied with any of them. So then I began wondering how we could account for the very real problem of the angular momentum in the Earth–Moon system. It seemed to me that something would have to have hit the Earth and spun it up and put a little angular momentum into it. So I asked myself how big an object I would need if I wanted to get the known value of angular momentum. That's a high school physics problem.

UBM: The first I heard about a collision was in your talk to the Meteorite Discussion Group in Cambridge in 1976. You

said that none of the three extant theories of lunar origin—fission of the Earth, capture of a passing body, or accretion in Earth orbit—were satisfactory, so you asked, "How about a collision?" That was a new idea to most of us, but you must have been thinking about it for some time?

AGWC: I had done the calculation and found that you would need to strike the Earth with something at least the size of Mars, as the extreme lower limit.

UBM: I remember that your first publication on the collision appeared in 1976. Incidentally, you were a professor of astronomy at Harvard by then, but the last we heard of you you were at Chalk River, Ontario. What happened in between?

AGWC: Well, in 1957 Sputnik-I went into orbit and the Space Age began. NASA was established in 1958 and it showed an interest in all sorts of problems relating to space physics—missions to planets, studies of particles and fields throughout interplanetary space, and the establishment of space observatories for astronomy—while a committee of the Canadian National Research Council, on which I served as a member, thought that Canada might be able to afford to probe the upper atmosphere with rockets. That persuaded me to move south of the border, where, in the spring of 1961, I was hired by the newly established Goddard Institute for Space Studies (GISS) adjacent to Columbia University in New York City. At GISS, I organized scientific conferences and taught courses and/or supervised students at several universities—Columbia, New York University, Yeshiva, and Yale. In 1962, I began a visiting lectureship at Yale, where I went every other week to give courses and oversee three physics students, Dave Arnett, Carl Hansen, and Jim Truran, who wanted to do theses in astrophysics. I spent 6 years as a visiting lecturer at Yale, where I became involved in several branches of science—nuclear physics, astrophysics, geophysics, planetary science, and meteoritics—each of which, traditionally, had stood pretty much on its own. I undertook to describe the universe—meaning everything from the center of the Earth to the cosmological horizon—through the eyes of a physicist, which, incidentally, required me to learn a great deal of new physics. In 1965 I also accepted an adjunct appointment to teach space physics at the Belfer Graduate School of Science of Yeshiva University in New York City. A year later my appointment advanced to full time, although I continued to spend a couple of days a week at GISS where I still had students. In the midst of all this, I had become an American citizen. In the latter 1960s, government funding of science began to decline and, at the same time, I noted a waning of the intellectual promise of Belfer. I began to want to leave behind the grime, crime, and congestion of New York City. I was, in fact, among the first to leave Belfer, from which a general exodus soon took place and Yeshiva closed it a few years later.

UBM: This must bring you close to the time you joined Harvard.

AGWC: Yes it does. In 1972, George Field, at Harvard, invited me to join the Harvard-Smithsonian Center for

Astrophysics, a new umbrella organization he was forming to include the Smithsonian Astrophysical Observatory and the Harvard College Observatory—with some members serving part time on the faculty of the Department of Astronomy. I accepted, and went to Harvard in 1973 as Associate Director for the new Planetary Sciences Division.

UBM: So you joined the faculty and stayed until your retirement in 1999—26 years, although not always as an Associate Director, a duty which rotates in each division.

AGWC: That's right. The ADs change every few years. Later I did a second tour of duty, but this time as Associate Director for Theoretical Astrophysics.

UBM: Now we can go back to where we left off talking about your collision model of lunar origin, which you first published in 1976.

AGWC: Well, the first thing I did when I arrived at Harvard was to hire Bill Ward, who had done his thesis at Caltech, as a postdoc. Bill was on hand at the time I did my first calculation of the collision, which required striking the Earth with a large object. Bill and I started to think about all the things that would be involved in such a collision. In 1974 I went to a meeting on planetary satellites at Cornell where I listened to Bill Hartmann present a paper, coauthored with Donald Davis, suggesting that a collision with the Earth might throw up enough material to form the Moon. As soon as he finished, my hand went up in the audience. Bill has said that he was quaking a bit thinking, 'Oh, Gee, now I am going to get shot down for good'. Instead of that I agreed with him and told him the impacting body would have to have been at least the mass of Mars. Bill hadn't considered that the angular momentum would put a lower limit on the amount of material involved in the collision. So our approaches were different: Bill wanted to get some material up from the Earth, and I wanted to get it into orbit.

UBM: By that time the lunar rocks had been analyzed, and you knew from the oxygen isotopes that both the Earth and the collider formed in the same neighborhood—at ~1 AU from the Sun.

AGWC: Yes, the oxygen isotopes indicated that the collider was local, but a dynamical consideration would not tell you that. In our early papers Bill and I looked at what would happen if the impacting body struck the protoEarth at different velocities. We reached the conclusion early on that if you increase the velocity of the impact, you will get a larger fraction of the material escaping into space, but it still was just a small amount. Other than that, the velocity didn't affect things very much. In any case, the issue of where the collider came from did not play any significant role in the dynamics.

UBM: It was very important to planetary scientists, though.

AGWC: Of course it was. Looking at this as an overall property of the Earth–Moon system in the context of forming the planets, then of course it plays a major role in your thinking, and should.

UBM: In 1984 you wrote that the material of the Moon came mainly from the impactor.

AGWC: Yes, I did. I stated that at the meeting in Kona, Hawaii, in 1984. It caused a stir at that time because this idea upset the geochemists' apple cart. They were convinced that since the lunar bulk composition closely resembled the mantle of the Earth, you had to get most of the Moon from Earth's mantle. My conclusion to the contrary was not well received. Nevertheless, that was the conclusion I had reached from the very simple calculations that I presented at Kona.

UBM: What do you think about it now?

AGWC: We always get the same number. It's that about two-thirds, or maybe three-quarters of the mass of the Moon comes from the collider.

UBM: And the rest comes from the mantle?

AGWC: Yes.

UBM: To please the geochemists?

AGWC: That's a bit of a complicated story in itself, because one argument used to be that the Moon has to come from the mantle because you need a large, differentiated body as a source, and, geochemically, a small body would be different from the mantle. But, the size of the impactor has been growing over the years as I have tried to get more realistic simulations. Right now the impactor in my calculations is 2× the size of Mars, but the total mass of the system is smaller. The protoEarth prior to the impact is down to 0.65 of the Earth's present mass.

UBM: So this means that the collider struck when accretion of the Earth was not quite two-thirds complete?

AGWC: Yes, those are the numbers I've used in all my simulations.

UBM: That's something fairly new, isn't it?

AGWC: No.

UBM: I thought that in your early models the impact occurred when the Earth was just about complete.

AGWC: That's true, we did make that assumption when we started out in this game. But the problem was that in order to get a decent amount of mass into orbit, I always had to assume that the angular momentum in the collision would be 2 or 3× the angular momentum of the present Earth–Moon system. That left a dozen unsettled problems. What happened to the rest of the angular momentum? How did we get rid of it? Eventually in coming to grips with that, it became clear that I had to cut down the total mass involved in the giant impact. Because to optimize the amount of mass in orbit, you have to have a nearly tangential impact. The yield of material in orbit goes up as the impactor has a larger and larger impact arm. Finally, of course, you get to the point where the encounter is so tangential that the "impactor" misses the target altogether, so there is a sudden drop in the amount of material that goes into orbit. It's zero. I would say that for the last 3 or 4 years that conclusion has been basic to what I have been doing.

UBM: You do say that the accumulation of the Moon was very rapid.

AGWC: That's what comes out of the simulations. The trouble is that the simulations I do using the so-called smoothed

particle hydrodynamics (SPH) method, contrasts with the way in which people have done accumulation calculations in the past, which are various types of *N*-body simulations. Basically, I was running these particle hydrodynamics simulations for periods many times longer than I had intended to do in the beginning, because we were trying to get materials into orbit. Then, we were letting the people take over who were doing the *N*-body calculations and they would tell us how the materials would accumulate. This is where I benefited from my collaboration with Robin Canup. I would do a simulation on these high-resolution calculations involving 100 000 particles. After a while I would send Robin the locations of those particles that were in orbit. She would do an *N*-body calculation of what would happen to them. Then we would compare her results to what I would have later on in my simulation. They didn't agree at all.

UBM: Not at all?

AGWC: That's right.

UBM: Why?

AGWC: There are several things that people who are doing *N*-body calculations do not take into account. One thing is that the gravitational potential is enormously flat in the protoEarth. Therefore, its departures from spherical symmetry cause particles in orbit to precess rapidly around the equator. The bodies sweep out a much bigger actual volume of space if the orbits are a little bit elliptical than they would if their orbits were strictly circular and stationary in place. In that case, they would sweep up what was there to begin with, but then they wouldn't sweep up anything more. Another problem is that the *N*-body calculations do not take into account tidal stripping, where the bodies will come inside the Roche lobe and be torn apart. You can see that happening in the simulations. When you're accumulating bodies in orbit, the massive ones, when they have a close encounter—not a collision, but just a close encounter with others—tend to deflect the less massive ones either directly inside the Roche lobe or directly away from it. This means that either way the minor particles will go through apogee, come back, and then go inside the Roche lobe. Once a particle is inside the Roche lobe it gets torn to pieces, or at least partially torn to pieces. So the biggest bodies in orbit dominate; they don't get deflected and don't get destroyed. They destroy their lesser competitors, thereby spreading out the materials of the lesser competitors and so they accumulate material a little more rapidly. Furthermore, you find that collisions are not always permanent. Pieces can come apart even if the collisions are very gentle, so maybe a collision with a single particle would knock at least part of it out. But the gain and loss of particles, tidal stripping, and rotating ellipses are the three main things that the *N*-body simulations do not take into account. That is why Robin's calculations differed from mine and this encouraged me to carry on the SPH simulations for about 4 or 5× longer than I had originally tended to do. That's the significance of the accumulation aspects of these things.

UBM: According to your present models, the impactor strikes the Earth then loops around it, comes back, and strikes it a second time. Isn't that something new?

AGWC: Not entirely. Right at the very beginning in the late 1980s, it was clear that an impact with a relatively small angular momentum, would destroy the impactor and that would be that. Whereas if you had an impact with a rather large angular momentum, you damaged the impactor and caused it to slow down a bit. Then it would be captured into orbit and would come back and hit the main body again from the other direction. The second strike would destroy the impactor. For a number of years, I used to describe this process as being either one or the other of these two cases, and I didn't know which. Since then my simulations, which determine the amount of material in orbit and how it varies with the angular momentum, have made it clear that the actual impact had to involve the double collision. So, yes, this conclusion was fairly new within the last 3 years or so.

UBM: I thought I had followed all this fairly carefully, and I have borrowed colored pictures from you now and then, but only the latest ones show the second impact.

AGWC: Anybody who borrowed my slides always would pick out the clearest cases and these showed the single collision.

UBM: Oh, I see. I'm sure I was guilty of doing that. However, I do remember that you always cautioned me that your pictures represented just calculations; you were not saying this was how the Moon actually formed.

AGWC: Yes, it always was a project in progress.

UBM: Since 1999, when you retired from Harvard and moved to Tucson, you have taken a fresh look at the r -process in supernovae undergoing core collapse. In your paper of 2001, you concluded that this process forms certain products—including deuterium, lithium, beryllium, and boron—which astrophysicists never have believed could form in stars. What persuaded you to such a radical new view?

AGWC: Well, you can blame it all on the r -process. Even though the necessity for having an r -process was evident right from the beginning of work on stellar nucleosynthesis, for almost a half century now the models suggested for this process have conspicuously failed to fill the bill. You have to start with seed nuclei, and for each such nucleus you need to have available at least 100 free neutrons. For many years the effort was to find a situation in which you could produce those neutrons as you needed them in some astrophysical situation, but all those attempts failed. So in the last couple of decades the efforts have focused on very high-density scenarios like neutron stars in which the neutrons are there waiting for you to use them. A very promising start was made by considering a neutron star newly formed in a supernova core collapse, in which the high flux of energetic neutrinos out of the center would drive a neutron wind off the surface. This material has to build up seed nuclei as it cools and then capture neutrons on them. But it turned out that this could drive an r -process that only builds up to about mass number 80 or so. What was needed

to repair this was a much more energetic event (technically, a much higher entropy), but nobody could find one. Then it was suggested that you could smash a couple of neutron stars together and look to see what happened in the debris. This actually works somewhat better, but such events must be extremely rare, and there was no way you could transport the radioactive nuclei that are r -process products across immense distances and implant them into the solar nebula. So what to do?

At this point I decided to see what would happen if you added rotation and magnetic fields to the supernova collapse and neutron star wind scenario. Adding rotation forms a disk around the neutron star equator, and putting a magnetic field in it forms a pair of bipolar jets rooted close to the neutron star equator. I call this an accretion–extrusion disk because dissipation causes matter to flow inward in the disk, and wrapping the magnetic field due to the neutron star rotation builds up a magnetic toroid that pushes matter out in the equatorial plane. Both of these streams of matter feed into the jets and are violently ejected at about half the speed of light, or ~ 140 MeV per nucleon. The first thing that must promptly happen is that the jets have to blast their way through the expanding supernova envelope surrounding everything.

Now, in the outer fringes of the neutron star envelope the nuclei build up into statistical equilibrium in the presence of a sea of free neutrons. If nothing more happens these nuclei will sit there and cool off by emitting neutrinos and antineutrinos (this is called the URCA process). But equilibrium is upset when they start moving toward lower density; the r -process sets in, but it runs more slowly than traditionally thought. They probably get to decay back toward the valley of beta stability as they start shooting up in the jet. When these nuclei encounter the surrounding envelope, they cause spallation in the various layers processed by energy generation in the supernova, and they suffer spallation themselves.

Meanwhile, the material at larger radii in the disk also come to statistical equilibrium, but without the free neutrons and because of the high density they have an abundance peak around mass number 90 or a little more. These nuclei also get shot up in the jets and give their own contribution to the spallation, both of themselves and in all the presupernova layers. This accounts for the production of p -process nuclei in the mass range 90 to 100, whose origin has also long been a mystery.

So, to answer your question (finally!), the deuterium, lithium, beryllium, and boron will be products of spallation, especially when these high-speed beams irradiate the carbon–oxygen layer of the presupernova. Of course, for a very long time stellar evolution people have been saying that stars cannot do this. So it is very pleasant to have found an alternative to cosmic rays to do this.

UBM: In your paper that is now in press, you argue that r -process jets probably formed ^{10}Be in competition with cosmic rays and injected it into the primitive solar nebula. When we

measure ^{10}Be in a meteorite can we discriminate between these modes of origin, and, if not, what does this do to calculations of terrestrial ages?

AGWC: Beryllium-10 will be one of the spallation products made in the supernova envelope. Couple this with the fact that I have long argued that a core collapse supernova was responsible for triggering the accelerated collapse of a "core" in an interstellar molecular cloud and injecting fresh radioactivities into it, and this fits right in with the supernova origin of all the other extinct radioactivities. Such a core collapse supernova is the only astronomical object that can produce all the extinct radioactivities, including ^{10}Be . Of course, in the present-day solar system the cosmic-ray produced ^{10}Be can form an active ingredient of meteoritic material, but that is different from finding boron condensates as part of the primitive material itself.

UBM: You also have worked out modes of formation and a chronology of various "mysterious" components of meteorites. Have you, in fact, assigned the condensation of CAIs to a prenebular stage rather than having them form in the early solar nebula?

AGWC: Yes, this was prompted by another extremely important discovery by Chaussidon, Robert, and McKeegan, who reported at the 2002 Lunar and Planetary Science Conference that they had found evidence in a CAI that the short-lived nucleus ^7Be was an extinct radioactivity in that CAI. Not only did its abundance vary over distances of 50 to 100 μm but these abundance variations were correlated with abundance variations in ^9Be . But ^7Be has a half-life of only 53 days, which is but the blink of an eyelid in the lifetime of the solar nebula. And it cannot be a product of solar flare activity for several reasons, of which the most important are these: (1) any Sun-centered particle accelerator will irradiate material close to it much more intensively than that farther away, contrary to observation. (2) My simulations of planetesimal formation indicate that objects with dimensions large compared to the range of such energetic solar particles will be formed in just a few years, which would also generate large variations in the local abundances of irradiation products, again contrary to observation. (3) The T-Tauri solar wind will carry away the solar nebula, but it will take a few million years to do it, and in the mean time the wind must divide to go around it, carrying the magnetic lines of force with it, and thus carrying Sun-generated energetic particles away from the nebula.

So this tells us that we must form the CAIs within a few months of the supernova explosion, long before the supernova trigger can have formed the solar nebula. How is this possible? It occurs in the expanding supernova envelope. It takes several months for the expansion of the envelope to cool it to the point where the temperature is low enough to allow the condensation of refractory minerals. In the meantime we know that the supernova envelope has become well mixed, because (1) recent three-dimensional simulations of supernova core collapse have shown that local neutrino energy deposition blows bubbles off

the core, like steam bubbles in a pot of vigorously boiling water, thus inducing all kinds of chaotic motions, and (2) in the case of Supernova 1987A, gamma rays were observed from the envelope many months earlier than they were expected to be seen. So the materials are there for the condensation of minerals in the expanding envelope (but at lower densities than would be the case in the solar nebula), and because of the vigorous stirring these condensates should quickly aggregate into a range of objects from millimeters to centimeters in size: CAIs! They will be injected into the collapsing molecular cloud core along with the fresh radioactivities (maybe even more efficiently since CAIs will move relatively freely through the low density gas). Note that CAIs usually have an enrichment of 4 to 5% pure ^{16}O , which will be a consequence of mixing of the carbon-oxygen layer in the presupernova into the rest of the envelope before the CAIs are formed.

UBM: When and where do you propose to form the amoeboid olivine aggregates, the FUN inclusions, the interstellar nanodiamonds and other presolar mineral grains?

AGWC: In the supernova envelope. The amoeboid olivine aggregates (AOAs) also have a pure ^{16}O enrichment of 4 to 5%, which seems characteristic of the envelope after mixing. These are Mg and Fe condensates which come after the main refractory elements such as Al and Ca in the condensation sequence as a gas cools. Because of the relatively long timescale here the first main generation of condensates (CAIs) has had time to aggregate, so that the next generation (AOAs) must usually nucleate independently.

I think the FUN inclusions come from deeper into the envelope. Inclusions EK1-4-1 and C1 have enrichments of *r*-process isotopes relative to *s*-process ones, and among the lighter elements the heavier isotopes tend to be enriched, and this is not surprising in this environment with so many *r*-process nuclei in the jets. The interstellar nanodiamonds are probably generated in the outer layers of the helium shell where helium burning makes primarily ^{12}C in the early stages of the helium burning. Presolar grains of varying composition can come from all over the envelope, especially where mixing is incomplete. Sorting that all out is an interesting problem that I have not had time to tackle yet.

UBM: Above all, Al, what have you to tell us now about chondrules: when, where, and how did they form?

AGWC: This certainly has been a perennial mystery and source of debate among meteoriticists. Most of the candidate mechanisms have severe problems sufficient to rule them out. The mechanism that has survived best is making chondrules in giant shock waves in the primitive solar nebula. The central problem here is that the chondrule factory must be very efficient because so much material has been processed by it, and we have not known how to set up large-scale standing shock waves in the nebula. But that has now been solved, at least in principle. As I mentioned earlier, Stirling Colgate at Los Alamos and various colleagues have been studying Rossby waves in large disks such as galactic and stellar disks. Such waves are

responsible for the large-scale weather patterns in the Earth's atmosphere. Well it turns out that there is a Rossby Wave Instability, alternatively called the Rossby Vortex Instability, that generates large vortex motions in the nebula. The interesting thing about this is that in both azimuthal directions from the vortex, large-scale standing shocks are generated. Just what we needed! They need to be studied in more detail, but I think that they will prove to have the right properties to do this job.

This also has a bearing on the discussion about the injection of the CAIs from the supernova envelope. The CAIs are frequently described as the oldest objects formed in some "unknown reservoir" in the solar nebula. In CAIs the $^{26}\text{Al}/^{27}\text{Al}$ ratio is usually 5×10^{-5} . But there are recent measurements in ferromagnesian chondrules that yield ratios between 2.28×10^{-5} to 4.5×10^{-6} . The lower limit of these measurements is about what I would expect to be produced when supernova debris is injected into the molecular cloud core, because it represents a dilution of the supernova aluminum by pure ^{27}Al , and a factor of 10 seems reasonable for this. But even more interesting, I think the intermediate values indicate that chondrule formation in the nebula took place faster than the supernova debris could be thoroughly mixed into the nebula, so the dilution was not complete when these other chondrules were made. That is a very efficient process for which these Rossby vortices are certainly needed. The other important thing is that this does not indicate that the CAIs are something like a couple of million years older than the chondrules, and the transit time from supernova site to the molecular cloud core a few parsecs away will be in the range 10^4 to 10^5 years.

This supernova jet—supernova trigger hypothesis has proved to be very fruitful in suggesting answers to many puzzles, and I think that people will have a lot of fun pursuing it.

UBM: Thanks, Al, for giving me detailed answers to some of the most refractory problems in meteoritics. You have challenged most of our customary approaches, but you have proposed a possible mechanism for forming chondrules in the solar nebula and sent us back to the prenebular supernova to look for CAIs. This will take some thinking about.

And now for something completely different, please recall to me the comparisons you drew between the approaches of scientists and historians in your autobiographical article of 1999 in *Annual Reviews of Astronomy and Astrophysics*.

AGWC: The first introduction I wrote to that autobiographical thing was a little more scathing than the one that appeared. I showed it to Owen Gingerich at Harvard and asked if I was going overboard with it. Owen said "Yes." So I toned it down.

UBM: What was it you said, approximately?

AGWC: I said that historians of science tend to concentrate on what people were thinking in the past, but not why. They may report such-and-such a development which changed so-and-so's mind about something, but they will not explain the logical development of thought on why one theory had to be

bad and another one better. That is the approach I have followed all my life.

UBM: You have written that your main purpose is to seek consistency.

AGWC: True. The business about trying to make things consistent, probably has generated half my papers, simply because something quite new and different has been discovered that has changed the logical chain of thinking. Therefore, it requires a new vision that sometimes is revolutionary. For best results, however, being consistent requires an interdisciplinary approach. It's a question of understanding how the consequences of one particular fact that may have been discovered will interact with a logical chain of thought, which not only pertains to that particular topic but probably also to many others.

UBM: The long trail of consequences of one true but ugly fact?

AGWC: Yes, except that I never have voluntarily thought of a new fact as ugly. A new fact, if it is something dramatic, may be ugly to the people who are desperately trying to hold onto some favorite theory, but to me if it's a new fact it means that you're gaining new insights into something. It's not ugly by any means. It's thrilling. It's exciting. But the fact of it being there and prompting you to rethink what you've done is always part of the approach of trying to get things right. A historian may claim that there is no such thing as "truth" so I never can get things right. That historian is probably correct, but I won't be intellectually satisfied unless I try. In the end I think we will arrive at a pretty good approximation.

UBM: Historians, for their part, complain that scientists write as though science were one long series of successes, as judged by present standards, with no account taken of the social and intellectual milieu in which the scientists lived and worked, and none given of the byways and dead-ends they took *en route*.

AGWC: Some scientists do write that way, but, of course, most publishers of a technical article wouldn't print your thought processes anyway, with all the weird bypasses you took in trying to understand a subject. I try to describe them in talks, though, and in review articles. I think we need to have more oral histories, actually. Because among other things you get an inside version which may not be in the written word. That's important from the point of view of understanding what the historian wants to know about what were people thinking, and this is rarely clear from the written word because the written word always is *post facto*.

UBM: For these reasons I believe it is important for scientists to put their unpublished notes and correspondence into archives.

AGWC: Long ago I put most of my correspondence and such into the Harvard archives. But once I stopped having a secretary and started using e-mail the record ended as far as written materials are concerned. I guess that's a general problem for historians.

UBM: The whole archival profession is concerned. One of the archivists at the Smithsonian Institution in Washington told me they are working on possible solutions, but they really do have serious problems.

On another topic, I know that over the years you have mastered each new development in computer technology and applied it to your research. You have written about how the very act of computing can make a difference in the development of ideas.

AGWC: Yes, but the best recent source of information on that is in a paper that Don Clayton, of Clemson University in South Carolina, published in *Meteoritics and Planetary Science* about 2 years ago. Don describes the influence of computing on the development of ideas about the synthesis of iron in stars and compares it to the approach at Caltech where they were working just with observations.

UBM: And he shows how computing itself made a difference?

AGWC: He surely does, and he also discusses how I pioneered monster calculations of nucleosynthesis in complicated networks.

UBM: You have broad interests, Al.

AGWC: In a sense, yes, I have broad interests across many disciplines, which I have developed over the years. As you know I am self-taught in astrophysics and self-taught in all aspects of meteoritics and planetary science.

UBM: And self-taught in geophysics, mineralogy and geology.

AGWC: Yes, but I am still very deficient in geology.

UBM: Besides continuing your research apace, are you also giving courses at the University of Arizona?

AGWC: No, I have given a small number of lectures at the Lunar and Planetary Laboratory and in some of Mike Drake's and Jay Melosh's courses to graduate students, mainly telling them about the Moon and stuff like that. That's the extent of it so far. They may involve me a little more heavily in a few things. We'll see.

UBM: In addition to mastering computer technology and using it for research, you have arranged many conferences, particularly interdisciplinary conferences where people can meet and argue out their problems. You've also served on numerous policy-making committees, both national and academic, and have done a lot of teaching. You have been busy.

AGWC: I am still busy. I may have retired from teaching at Harvard, but it has made essentially no difference to my

research practices other than that I now have to cope with a different location and environment.

UBM: But you still retain your title as Donald H. Menzel Research Professor of Astrophysics at Harvard University.

AGM: Yes. That stays with me for up to 5 years, then it will be replaced by Menzel professorship emeritus. Here in Arizona I go into my office at the Lunar and Planetary Laboratory every day and try to get things done. As usual, it always takes much longer than I think it should. So nothing really has changed.

UBM: Thank you very much, Al. I have had a chance to hear your newest ideas and have learned a lot about the breadth of your interests and your approach to science, both of which strike me as quite unusual in the community of meteoriticists and planetary scientists.

Acknowledgments—I wish to thank the Council of The Meteoritical Society for their support of this effort. This interview has been edited in consultation with Dr. Cameron.

Editorial handling: D. W. G. Sears

SELECTED REFERENCES

- ANDERS E. (1975) On the depletion of moderately volatile elements in ordinary chondrites. *Meteoritics* **10**, 283–286.
- CAMERON A. G. W. (1955) Origin of anomalous abundances of the elements in giant stars. *Astrophys. J.* **121**, 144–160.
- CAMERON A. G. W. (1995) The first ten million years in the solar nebula. *Meteoritics* **30**, 133–161.
- CAMERON A. G. W. (2001) Some properties of *r*-process accretion disks and jets. *Astrophys. J.* **562**, 456–469.
- CAMERON A. G. W. (2003) Some nucleosynthesis effects associated with *r*-process jets. *Astrophys. J.* (in press).
- CHAUSSIDON M., ROBERT F. AND MCKEEGAN K. D. (2002) Incorporation of short-lived ⁷Be in one CAI from the Allende meteorite (abstract). *Lunar Planet. Sci.* **33**, #1563, Lunar and Planetary Institute, Houston, Texas, USA (CD-ROM).
- CLAYTON D. D. (1999) Radiogenic iron. *Meteorit. Planet. Sci.* **34** (Suppl.), 145–160.
- MERRILL P. W. (1952) Spectroscopic observations of stars of class S. *Astrophys. J.* **116**, 21–36.
- REYNOLDS J. H. (1960) Determination of the age of the elements. *Phys. Rev. Lett.* **4**, 8–10.
- SUESS H. E. AND UREY H. C. (1956) Abundances of the elements. *Rev. Modern Phys.* **28**, 53–74.
- WASSERBURG G. J., LEE T. AND PAPANASTASSIOU D. A. (1977) Correlated O and Mg isotopic anomalies in Allende inclusions. II. Magnesium. *Geophys. Res. Lett.* **4**, 299–302.
- WETHERILL G. W. (1990) Formation of the Earth. *Ann. Rev. Earth Planet. Sci.* **18**, 205–256.