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Report

Oral histories in meteoritics and planetary science: X. Ralph B. Baldwin

Ursula B. MARVIN

Harvard-Smithsonian Center for Astrophysics, Cambridge, Massachusetts 02138, USA
E-mail: umarvin@cfa.harvard.edu

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Abstract–In this interview, Ralph Baldwin describes how he earned his Ph.D. in astronomy and then, early in his career, became interested in the Moon and the origin of its craters. When he concluded that the craters were formed by meteorite impacts rather than by volcanism, he faced great difficulties in finding an audience or a publisher. During World War II, he helped to design and develop operating specifications for the radio proximity fuze which has been credited with shortening the War by at least one year. Subsequently, he joined the family firm, The Oliver Machinery Company, in Grand Rapids, Michigan. He pursued his lunar studies on nights and weekends and wrote his first book, *The Face of the Moon*, which was published in 1949. Sales were poor, but the book was read by Harold C. Urey, who sought out Baldwin for discussions about the Moon, and by Peter M. Millman, in Ottawa, who prompted Dr. Carlyle S. Beals, the Dominion Astronomer, to begin the highly successful search for impact craters on the Canadian Shield. With his second book, *The Measure of the Moon*, published in 1963, Baldwin became recognized as a leading authority on the Moon and on planetary processes in general. He is the only scientist other than Eugene M. Shoemaker to whom the Meteoritical Society has presented both its Leonard Medal, in 1986, and its Barringer Medal, in 2000, and who also received the G. K. Gilbert Award, in 1986, from the Planetary Sciences Division of the Geological Society of America.

UBM: Ralph, ordinarily I would begin this interview by asking you how you got interested in meteorites, but I won't ask that because I know you never did get interested in them—except as cosmic projectiles.

RBB: That's true. I admit that I would not know a meteorite unless it made a hole in my front yard.

UBM: You earned your Ph.D. in astronomy at the University of Michigan in 1937 with a thesis on the spectra of a bright nova, Nova Cygni III, which occurred in the constellation Cygnus in 1920. But, not long afterward, you began a serious study of the topography of the Moon. How did that came about?

RBB: That didn't come about until four years later. On receiving my degree, I spent a few months job hunting in the depths of the depression, and then landed a position as an assistant to Dr. Charles P. Olivier at the University of Pennsylvania's Flower Observatory, where I stayed for one year. Then Dr. Oliver J. Lee, the director of Northwestern's Dearborn Observatory, hired me as an instructor. Three years later, I also was hired to give public lectures at the Adler Planetarium on Chicago's Lakeshore Drive. The pay per lecture began at $4.00 and later went up to $5.00. It may not sound like much today, and actually it wasn't very much in 1941, but to me this was a very welcome supplement to my salary at a time when there were few opportunities for astronomers. Over the next year or two, I gave 312 lectures.

UBM: Didn't you have to drive some distance to get to the Planetarium?

RBB: Yes, I had to drive 16 miles, and I always left early so as not get tied up in traffic. Once there, I spent any spare time I had looking at the beautiful, illuminated transparencies of astronomical objects that were mounted in the halls at Adler.

UBM: Including pictures of the Moon?

RBB: Yes, remarkable pictures. I had looked at the Moon through the telescopes at the Flower and Dearborn Observatories but never had paid much attention to it. These high magnification pictures showed features that puzzled me.

UBM: Which ones in particular?

RBB: The numerous valleys and ridges surrounding Imbrium. The valleys had steep sidewalls, and some of them

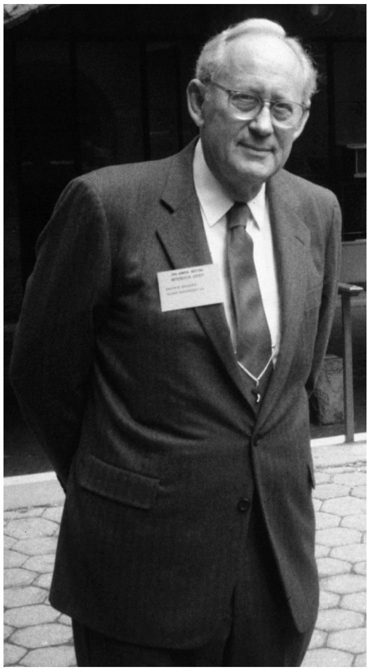

Fig. 1. Ralph B. Baldwin at the meeting of the Meteoritical Society in New York City in 1986, when he was presented with the Leonard Medal. Photo courtesy of John A. Wood.

were empty while others were filled with dark material. I searched the library at Dearborn for interpretations of these structures and found only a single reference to them. It was an article called, "Lunar furrows," by an English astronomer, W. H. Steavenson, published by the British Astronomical Association in 1919. Steavenson said the furrows were formed by the tangential impacts of a swarm of meteorites.

UBM: What did you think of that idea?

RBB: I thought it was just plain wrong. A swarm of meteorites, if there were any such thing, should leave a more localized pattern of parallel grooves. These occurred over a vast area and were far from parallel. So, I got a big photograph of the Moon from the Lick Observatory and inked in projections of the valleys to look for any kind of a pattern. And there it was! The valleys followed great circles that crossed one another in the central part of Imbrium. Imbrium, obviously, was the site of a gigantic explosion.

UBM: A gigantic volcanic explosion? Most people would have thought so in the 1940s.

RBB: Yes, many assumed that for all the lunar craters, but to me that was impossible. The largest volcanic explosion pit on Earth is Tambora in Indonesia, and it is about 4 km across. Giant Imbrium is more than 1200 km across, and it is on a body that is much too small to produce volcanic energy on such a scale. To me, nothing but the impact of an asteroid could produce such an explosion and excavate a crater like Imbrium. I concluded that the radial grooves on its circumference were gouged out by fragments of bedrock flying outward from Ground Zero.

UBM: So, mainly by looking at pictures, you had discovered a major new geological process! After that, you recognized radial grooves around several other circular lunar features, didn't you?

RBB: Yes, I found them around Crisium, Humorum, and Nectaris. These were all smaller than Imbrium, but each one had the radial grooves and also had at least two concentric raised rings around it. Serenitatis and Humboltianum were similar structures. I concluded that these large craters were due to giant impacts and the smaller lunar craters, with their similar forms and random distribution must also be due to meteorite impacts. In that case, the Earth must have undergone the same bombardment, but much of the evidence of it has been lost because of erosion and tectonic changes. Since there is little erosion on the Moon, I concluded that the lunar craters must be very old.

UBM: Speaking of rings around craters, I believe that these were rediscovered and described in 1962 by William K. Hartmann and Gerard Kuiper, when they obtained spectacular views of the lunar surface by projecting rectified photographs of it onto a large lunar globe at Kuiper's Lunar and Planetary Laboratory in Arizona. They recognized and named "basins" as distinct lunar features and also noted rings around some of them.

RBB: Yes, they thought no one had seen these features before, so they named them "multi-ring basins," which is a very good term for them. I called Bill's attention to my description of ringed craters in *The Face of the Moon*, so he checked back and conceded my priority. Actually, I also had mentioned them in two earlier papers of 1942 and 1943, but perhaps I never emphasized them enough to catch a reader's attention.

UBM: You must have been excited by your new evidence for explosive impacts.

RBB: I was. I thought I had demolished all previous hypotheses—those that claimed the craters were volcanic craters, maars, or calderas, or collapsed laccoliths, or coral atolls, or even holes melted in the icy crust of the Moon. Of course, by the 1940s most people believed only in a volcanic origin.

UBM: And believed in it totally. Volcanism is the familiar crater-forming process so it is the one people intuitively applied to lunar craters.

RBB: Yes, and to many, the very term "crater" seems to imply a volcanic origin. I had supposed that Galileo had called them "craters" when he first drew diagrams of them in 1610. But then you and Owen Gingerich traced back through the literature for me and found that Galileo had called the largest one a "cavity" and the rest "spots." In 1620, Kepler called them "pits and sunken cavities," and in 1665, Hooke called them "pits" and "dishes"—all good, non-genetic terms. A century later, in 1791, J. H. Schröter called the lunar depressions "craters," and everyone immediately took them to be volcanic. Then in 1837, Beer and Mädler published their detailed map and described the lunar surface as being covered by a multitude of volcanic craters. Without repeating such labors, nobody was inclined to challenge their conclusion.

UBM: But, now, a little more than a century later, you had independently worked out a new mode of origin for them.

RBB: Yes. And I felt certain I had found evidence to prove it. So, feeling jubilant, I went to Dr. Lee and announced my discovery.

UBM: How did he respond?

RBB: He laughed. And brushed me off with the statement that the study of the Moon did not fit in with the programs at Dearborn. That was in 1941.

UBM: Obviously, you didn't give up.

RBB: No, I continued classifying red stars and helping to take photographic plates at night while fitting in research on the Moon at odd hours. Lee hadn't told me I couldn't work on the Moon, but I knew I would better not do it conspicuously.

Soon after that, I rode up to Lake Geneva, Wisconsin, with Lee to attend the monthly symposium at the Yerkes Observatory. While there, I mentioned my idea of the impact origin of lunar craters to the director, Otto Struve. Struve immediately responded: "Ralph, you are giving next month's symposium."

UBM: How did you feel about that?

RBB: I was delighted. This would give me a chance to introduce my ideas to a group of eminent astronomers. The next month, I went to Yerkes well prepared with photographs and slides and presented what I thought was a good paper, with clear, persuasive arguments for the impact origin of lunar craters. Dr. Lee was there, as were several world-renowned astronomers, including Jesse Greenstein, Philip Keenan, George Van Biesbroek, Gerard Kuiper, and Otto Struve.

UBM: How was your talk received?

RBB: I struck out completely. They showed not a flicker of interest, except for Keenan who jumped up and said: "I just can't conceive of this impact process being right."

UBM: But Struve knew you would be talking about lunar impact craters.

RBB: Yes he did. He must have thought it would make an interesting program, and he listened politely, but he didn't believe a word of it.

UBM: Do you think this was because the Moon, itself, was of no interest to them? I recently reread a paper published in 1935 by Frank Wright, the director of the Geophysical Laboratory in Washington, who decried the prevailing attitude of astronomers toward the Moon. He said astronomers and astrophysicists resented the presence of the Moon in the night sky because its light interfered with photography and analysis of faint celestial objects. To them, the Moon was just an inert mass with a history of long-dead volcanism of no conceivable scientific value.

RBB: I remember that attitude. In fact, I shared it for a while when I was first doing astronomy. There probably was a lot of that feeling in their response—or lack of it. In any case, the astronomers at Yerkes were not even interested enough to ask me a few questions.

UBM: What did you do next?

RBB: I wrote a paper expanding on what I said in my talk. Struve refused to publish it in *The Astrophysical Journal*; Lee refused to include it in *The Annals of the Dearborn Observatory*; and the editor of *The Astronomical Journal* refused to accept it. In 1942, I finally got it accepted by *Popular Astronomy*, a publication of the Goodsell Observatory at Carleton College in Minnesota. A year later, I wrote a second article that went through the same rigmarole and appeared in *Popular Astronomy.*

UBM: So those two were your first publications on the Moon?

RBB: Yes. And by then, I realized that I was preaching heresy. In a way, though, I felt I was in luck because nobody else was mining this lode so I had the study of the Moon almost to myself. That situation continued for nearly 20 years.

UBM: How long did you stay at Dearborn?

RBB: I stayed there until early June of 1942 when I suddenly moved to Washington. I say "suddenly" because in March my wife, Lois, and I had moved into a new house we had built near Dearborn; in June, we moved out of it never to return. I went to Washington to take a job in classified military research—so classified that the nature of it was a secret even from me, until after I got there.

UBM: But, now we know you went to Silver Spring, Maryland to work at Johns Hopkins University's Applied Physics Laboratory.

RBB: Yes, I joined the effort to develop the radio proximity fuze. That device, mounted in the nose of an artillery shell, would emit radio waves and could receive these waves reflected from an enemy plane and cause its attached shell to burst at the point where the fragments would destroy the plane. The first such fuzes were for anti-aircraft use. Subsequently, other, similar fuzes were used as anti-personnel weapons in howitzer shells. During World War II, it proved to be a fantastically effective anti-aircraft, anti-buzz-bomb and anti-personnel weapon. In the Pacific, it was instrumental in driving the Japanese Navy back toward its homeland and it reduced the threat of Kamikaze attacks to the point where our fleet could penetrate Japanese waters in reasonable safety. In Europe, it stopped German buzz-bombs aimed at London and later at Antwerp. The advancing German troops at the Battle of the Bulge were severely treated by howitzer shells bursting over them at an optimum height. The prisoners we took said they couldn't believe the scale of our bombardment. Some military experts have estimated that the fuze shortened the war against Japan by about a year and the European war by months.

UBM: You have published two books on it—and you do spell it "fuze," which my spell-checker never fails to tell me is wrong.

RBB: But "fuze" is the proper military spelling. I wrote:

The Deadly Fuze: Secret Weapon of World War II, which appeared in 1980, and *They Never Knew what Hit Them: the Story of World War II's Best Kept Secret*, which appeared in 1999.

UBM: And, for your work at APL, you and others in the group received multiple honors, including the Presidential Certificate of Merit. But, while helping with the development of the fuze and training personnel to use it, you did not entirely forget about lunar craters.

RBB: No, I didn't. I took advantage of being in Washington by asking the librarians at the U. S. Naval Observatory and the Geological Survey to search for references to the Moon. They provided me with thick files of them. In addition, I got crucial data from my friends at the Pentagon on crater morphologies produced by shells, bombs, and high explosives. I had concluded that I couldn't make a case for explosive impact craters without a book-length argument.

UBM: And that book would become *The Face of the Moon*. When did you begin writing it?

RBB: Really, not until after the war. When the war ended, I was asked to stay on at the Applied Physics Laboratory. At about the same time, my father and his brother asked me to come to Grand Rapids, Michigan and work with them in the family concern, the Oliver Machinery Company, which made woodworking machinery, packaging machinery, and other items. I agreed to stay at APL for one year, but a few months later, Lois remarked that she was having daily headaches, which actually had begun when we first arrived in Silver Spring. We did some soul searching and decided that I would change my profession from applied physics to private industry and we would move to Michigan—provided that my family would allow me to continue my astronomical work there. They assured me I could, as long as my research did not affect the company adversely.

When I told the APL people that I had to leave, they asked if I would set up a secure room at my new place and continue my classified research until my projects were completed. I agreed. We bought a house in East Grand Rapids and the Navy set up a secure room in the basement, outfitted with a 1400-pound safe for classified materials. Once there, I began to spend evenings and weekends working on *The Face of the Moon*.

UBM: That book launched research on the Moon as a modern branch of science—although some time passed before it really took effect.

RBB: You once wrote me saying: "You had that rarest of things, a book that made a difference." Thanks for your compliments.

UBM: Which of your arguments do you think made the most persuasive case for impact craters?

RBB: I think my best argument was presented in my log-log diagram on which I had plotted the depth versus diameter for well-preserved craters of all sizes—from small bomb and shell craters to the immense craters of the Moon—and showed that they all clustered along the same smooth curve. Then, I added the four freshest terrestrial craters that had meteorites associated with them, and they, too, fell on the curve. Henbury 13 in Australia, and Odessa 1 & 2 in Texas, plotted among the military explosion pits, and the Meteor Crater in Arizona plotted at the lower end of the lunar craters. No such regularity applies to volcanic craters and calderas.

UBM: Many scientists must have looked twice at your diagram and begun to reconsider their opposition to impact craters. But, perhaps it would be worth recalling how strong the opposition was to the very idea. In 1924, A. C. Gifford in New Zealand, who had calculated that the energies released by impacts would excavate craters much larger than the projectiles, summarized the three main objections of geologists to the idea of impact. First, to produce the formations we see on the Moon would require a terrific bombardment by meteorites, including many of vast dimensions.

RBB: That's exactly what it did take.

UBM: But geologists, schooled in uniformitarianism, found it just too catastrophic to contemplate. The second popular objection Gifford listed was that if the lunar craters were due to meteoritic bombardment, the Earth should show similar scars, but (as everyone knew) it didn't.

RBB: The Earth began to show them as soon as people began seriously looking for them. By now, we have identified more than 150 of them just on the 30% of Earth's surface occupied by lands instead of seas. But, until the late 1920s, the Meteor Crater was the only candidate, and many refused to accept that one.

UBM: The third argument Gifford listed was that the circularity of the lunar craters implies that all the impacts were vertical, whereas most meteorites would strike the surface obliquely.

RBB: It took people a long while to comprehend that a high velocity impact acts as a point-source explosion.

UBM: Some people thought they were being asked to imagine the meteorites, themselves, as explosive objects—like bombs or shells. In the mid-1960s, an elderly professor, who shall remain nameless, asked me what would make the Canyon Diablo irons explode.

RBB: And he wasn't joking?

UBM: Unfortunately, he wasn't. He did get the point, however, when I explained that pieces of iron began to come off the incoming body during its flight in the atmosphere; it was the main mass that exploded, horrifically, when it plunged into the ground at cosmic velocity and excavated the crater. This explanation also helped him to grasp the irrelevance of the objection to impact craters put forward by the geologist Nelson Horatio Darton: "When you find meteorites on a drumlin, you don't say the meteorites made the drumlin."

RBB: I evidently was up against a more stubborn type of resistance than I realized.

UBM: You surely were. As we mentioned earlier, much of the resistance was based on the uniformitarian dictum that present processes are the keys to past ones, and nobody ever had seen a meteorite blasting open a crater instead of just making a hole in the ground. Furthermore, geologists were trained to believe that all causes of change are intrinsic to the Earth. As late as 1965, someone lamented that to call upon astronomy to solve our problems would minimize the usefulness of geology.

RBB: You wrote a review of this problem a few years ago, didn't you?

UBM: Yes, it appeared as a chapter in a book published in 1999 by the Geological Society, London.

RBB: I remember that. I enjoyed reading the reprint you sent me. As you pointed out, we owed a lot of the resistance to Grove Karl Gilbert, the chief geologist of the U. S. Geological Survey, who declared in 1896 that the Arizona crater was volcanic and the iron meteorites had fallen around it by coincidence.

UBM: Yes. And the sad part is that Gilbert went to Arizona to study Coon Butte (Meteor Crater) in hopes of finding evidence that it was formed by a late-falling planetesimal. Gilbert wrote to a colleague that he was going to hunt a star. I believe, don't you, that if he had found any good evidence of impact he would have been perfectly willing to flout the rules of strict uniformitarianism and geological norms.

RBB: Yes, but he didn't know what to look for, and all his tests failed. He thought the main mass of the iron should be lying beneath the floor, and that an impact crater should be oval because most meteorites fall at oblique angles. He also thought that the volume of the ejecta on the rim should be greater than that of the bowl because of the extra volume of the buried iron.

UBM: But, his dip-needle showed no evidence of a big magnetic iron under the floor; his plane-table map showed the crater to be essentially circular; and he measured equal volumes of 82 million cubic yards for the rim and the bowl. Gilbert could not have known that the crater formed about 50,000 years ago, and since then, the rim has eroded to about half its height and the bowl holds about 70 feet of Pleistocene lake beds.

RBB: And he had no idea that high velocity impacts would destroy the projectile and blast a circular crater from almost any angle, although I do think he should have realized that before he died in 1918.

UBM: He had a real problem, though, when he didn't find any volcanic rocks at the crater. We'll never know how he would have explained their absence if the crater had been in a non-volcanic area like Massachusetts or Virginia, but in fact it lay within sight of the fresh-looking San Francisco volcanic field of northern Arizona, so he hypothesized that magma migrating at depth had encountered water and generated a gigantic steam explosion that created a maar without releasing any eruptive ash or rocks.

RBB: His volcanic hypothesis wasn't so very uniformitarian either.

UBM: No, it wasn't, as was noted by my students in a seminar on the resolution of scientific problems that I taught for some years in the Geology Department at Harvard. At the opening meeting of each new class, I assigned Gilbert's 1896 paper in *Science*: "The origin of hypotheses: Illustrated by the discussion of a topographic problem." I told them that Gilbert was the first scientist in history to investigate a crater for possible evidence of an impact origin, and asked them to critique how he solved his problem.

RBB: How did the students respond to that?

UBM: Many of them were astonished that anybody could write so well back in 1896. Gilbert sounded "modern" to them. But they learned that even a leading scientist can use a properly scientific approach and still get the wrong answer—by starting out with the wrong basic assumptions. I told them I rather admired Gilbert for yielding up his pet hypothesis with good grace, when many scientists cling to theirs long after their cases become hopeless.

RBB: Actually, Gilbert should have clung to his hypothesis a while longer. I assume you told them that soon after Gilbert made his study, Daniel M. Barringer, an entrepreneur, staked a mining claim on the crater and he and his partner published valid evidence for an impact origin as early as 1905.

UBM: I did tell them that, and pointed out that an impact origin seemed so obvious to Barringer that he staked his claim (which was signed by President Theodore Roosevelt, himself) before he visited the site—lest his presence there tip off his rivals.

RBB: Barringer found the flap of overturned sedimentary rocks on the rim and the huge tonnage of quartz flour in the crater floor, which he (rightly) attributed to the shock of the impact. Gilbert had failed to notice either of those features. Barringer also found specimens of Ni-Fe oxide shale buried within the rim, showing that meteorite fall was contemporary with the formation of the crater.

UBM: Both men started with the assumption that an impact would puncture the ground and come to rest beneath the crater floor. When neither could find it, Gilbert opted for the wrong mode of origin and Barringer clung to the right one; it just didn't produce the ore body he was looking for. Few scientists would listen to Barringer's evidence when one of their own had decreed otherwise.

RBB: Gilbert bears a heavy responsibility for holding up research on terrestrial impact craters, particularly within the U. S. Geological Survey, for about five decades.

UBM: But now the Geological Society of America presents the G. K. Gilbert Award to scientists who make significant contributions to the planetary sciences. In fact, they presented it to you in 1986.

RBB: Yes, and I greatly appreciated that. The award is named for Gilbert because of his remarkable paper "The

Moon's Face," published in 1893. It is interesting to note that he did his field work at Coon Butte in 1891 and published it in 1896. "The Moon's Face," came midway between the two.

UBM: What are your thoughts about "The Moon's Face"?

RBB: That was an extraordinary piece of work, based on just 18 nights of observing the Moon at the U. S. Naval Observatory in Washington. Gilbert argued that the Moon's surface is covered by impact craters of all sizes. His main problem, of course, was that the craters are predominantly circular, so he assumed they had to be formed by vertical impacts. To explain this, he pictured the Moon as having coalesced from a ring of moonlets that was in orbit about the Earth. Their bombardment caused the growing body to tilt this way and that until the entire surface was pockmarked by circular craters.

UBM: He also noted the radial grooves around Imbrium.

RBB: Gilbert observed them and argued that an immensely powerful impact at Imbrium had sent rock fragments scouring through the surrounding highlands, just as I envisioned 50 years later. He saw that Imbrium was the youngest of the large lunar impact craters, and he concluded that the heat of impact had extruded a vast flood of dark, liquid and plastic debris that covered 1/3 of the visible face of the Moon. Since the dark lunar plains were more lightly cratered and, hence, younger than the highlands, he set up a stratigraphic time scale of ante- and post-diluvial events using the standard geological principles of overlapping formations, cross-cutting relationships, and states of preservation.

UBM: Had you read Gilbert's paper when you wrote *The Face of the Moon*?

RBB: No, I hadn't. My book was actually in press when I got a letter from Professor Reginald A. Daly at Harvard with whom I had been in correspondence earlier. He asked if I still could send him reprints of my first two papers in *Popular Astronomy*; he would treasure them. He added that my work resembled that of G. K. Gilbert and he gave me the reference.

UBM: Your ideas were similar in some ways but very different in others. You didn't need the ring of moonlets because you knew vertical hits were not required to make circular craters. You distinguished between the different maria and made the key discovery that the large craters (they were not yet called basins) are substantially older than the mare flows that partially fill them.

RBB: Yes, and eventually that discovery would lead me into heated debates with Harold Urey. On the whole, though, I felt fortunate that Gilbert had given his talk—actually it was his presidential address—to the Philosophical Society of Washington and had published it in the Society's Bulletin. That was such an obscure journal, at least for astronomers, that his conclusions vanished for half a century.

UBM: His paper was available to many geologists, though. Shortened versions of it were presented to the National Academy of Science and to the New York Academy of Science, and abstracts were printed in their journals and several others. In addition, I assume that Gilbert's audience would have been packed with distinguished geologists from the Washington area, and beyond. Still, they ignored it and left the field to you. I would suggest that it was his subject matter, the Moon, which failed to attract anyone's interest. The Moon was just too exotic, and ideas about it were too untestable, for their tastes. Like Gilbert, himself, most of them were field geologists, and they knew there wasn't a prayer of confirming anything he proposed.

RBB: So that left the Moon to me, and 50 years later, I found that the attitude of geologists hadn't changed much. The rare exception was Robert Dietz, who published an article in 1946 in *The Journal of Geology* titled "The meteorite impact origin of the Moon's surface features." I knew nothing of that paper when I wrote my book, but then, Dietz knew nothing of my articles in *Popular Astronomy* when he wrote his paper. We became good friends when my book appeared.

UBM: Perhaps we should mention that, powerful as it was, the opposition to impact cratering was not unanimous. In the early years of the 20th century, several eminent scientists accepted the impact origin of the Meteor Crater. These included George Merrill of the Smithsonian, William Pickering of the Harvard College Observatory, Elihu Thompson, the acting president of MIT, and Herman Fairchild, a Professor of Geology at the University of Rochester and a former President of the Geological Society of America. In 1929, Fairchild published an article in *Science* chiding the U. S. Geological Survey for its stubborn refusal to concede an impact origin. He said that a great bureau of the people's government, supported by public money, had no ethical or legal right to suppress any geologic truth for any reason—such as a dislike of Barringer or a wish to suppress the fact that Gilbert had made a mistake. He said it added no luster to Gilbert's fame for the Survey to neglect to admit the evident truth.

RBB: His article made no difference, however, either in the Survey or among geologists in general. I suppose that members of the Survey were wholly involved in their own research projects, and realized how counterproductive it could be for their careers if they took up an unpromising topic like impact cratering.

UBM: So it fell to you, although you, too, had a full-time job. Did you volunteer your book to the University of Chicago Press?

RBB: Yes, I wrote them a letter describing it in detail and they wrote back and asked to see the manuscript. I had only two copies of it and wouldn't have submitted it uninvited. After a while, I got a letter of acceptance, mainly, I think, on the strength of a strong recommendation by Fred Whipple of the Harvard College Observatory. Much later, I learned that Gerard Kuiper, at the University of Chicago, had refused to referee the manuscript on grounds that he was not familiar enough with the subject.

UBM: That sounds odd today, when we think of Kuiper mainly as a lunar scientist.

RBB: Yes. But at that time, he had not yet begun his detailed studies of the Moon. The book could have been issued in November of 1948, but the Press waited until 1949 to make it seem more recent. Even so, the book's sales were poor.

UBM: But, it fell into the right hands. A pre-publication copy fell into Harold C. Urey's hands, for example. Have you heard that story?

RBB: I have heard that Urey borrowed a copy he saw at the office of the U. of Chicago Press, and read it on a trip.

UBM: Let me tell you my version, which I got from my colleague, John Wood, who, in turn, got it from Cyril Stanley Smith, a professor of metallurgy and of history of science at MIT. In the latter 1940s, Cyril had been a professor at the University of Chicago where he had a connection with the U. of C. Press. One evening, the Smiths held a cocktail party for faculty members, and Harold and Frieda Urey were among the first to arrive. Harold seated himself comfortably at the end of the couch and Cyril handed him a book saying, "Harold, you had better look at this." Hours later, someone asked Frieda where Harold was: "Isn't he coming?" "Why, yes," she said, "we came together, he has been here all evening." A search located Harold still sitting at the end of the couch totally engrossed in *The Face of the Moon*. He often remarked that reading your book changed his career.

RBB: He told me that, too, when we finally met.

UBM: I, personally, can testify that in 1949, Urey discussed your book at one of the lectures he gave to the geology department at the University of Chicago. I was there as a research assistant and took every opportunity to hear the guest speakers. Urey held up *The Face of the Moon* and told us what a remarkably insightful book it was. He said he had just finished reading it on a trip, so I assume he got a copy of his own after the leaving Smith's party. He visited you in Michigan, didn't he?

RBB: Yes, he waited until his own book, *The Planets*, came out in 1952. I had ordered a copy and it had just arrived when he telephoned and said he would like to come over and visit me. I invited him to our home for a weekend and we had two days of lively conversations. He fully concurred with me about the impact origin of lunar craters, but we didn't agree on much else. Urey urgently wanted the Moon to be a primitive body that had remained cold, or at least cool, ever since it originally aggregated from cold, nebular particles. He believed that the huge circular features like Imbrium were formed by impacts of the final large bodies to accrete into the Moon.

UBM: And, since he wanted the Moon to be a surviving relic of the birth of the solar system, he had convinced himself that that is what it is. In the fall of 1968, Urey gave an invited talk at the meeting of the Meteoritical Society we hosted in Cambridge. He started out by saying that if the Moon isn't primitive, it isn't interesting, so he would assume it to be old and primitive. At least he was honest.

RBB: He always was honest about his opinions. He argued that the Moon's tidal bulge demonstrated that it had formed elsewhere and been captured by the Earth. I argued that its tidal bulge shows that it originally formed close to a large body, most likely the Earth. He argued that the Moon is cool and rigid because it maintains its bulge and various topographic features out of isostatic equilibrium. I argued that the lunar interior has been hot enough for at least partial isostatic adjustment of its major features. He argued that Mare Imbrium was flooded with dark melt rock formed by the impact that excavated the crater. I argued that Imbrium was flooded by dark lava that flowed into it after the crater had stood open quite a while and been pockmarked by younger craters. The buried rims of the younger craters are clearly visible through the lavas in several large craters, and it is possible to identify a whole succession of older and younger lava flows. We had other disagreements, but after our heated arguments, Urey and I would sit peaceably over coffee, as friends should.

UBM: Do you remember when Urey was speculating that the maria were composed of dark carbonaceous sediments?

RBB: No, I never heard of that.

UBM: It was in the late 1960s. He telephoned me about it at least twice after we had met at various conferences. Urey was wrestling with the problem you mentioned earlier: the Moon was just too small to heat up internally and fill the maria with volcanic lavas. He said he could show mathematically that neither the heat of compaction, nor internal pressure, nor radioactivity could accomplish that. So, he developed a hypothesis that, during its capture, great volumes of Earth's ocean waters had splashed onto the Moon and ridden away on it. The waters filled the large craters and then evaporated, leaving behind bedded sediments, conceivably containing terrestrial marine organisms.

RBB: What did you say when he called you about that?

UBM: I said it was an interesting idea—which it certainly was—and I would give it some thought, but I admitted to having serious doubts. He listed additional evidence of water—the rilles that looked to him like dried up river systems and beach ridges on the slopes bordering the maria. Later on, when the preliminary examination team at Houston began to find basaltic rocks in the Apollo 11 samples, he proposed the alternative hypothesis that the impact of a comet like Halley on the Earth would vaporize enough water to cover the globe to a depth of 60 feet and the splash would send earth rocks as well as water to the Moon. He never mentioned that version to me, but both of his ideas about water on the Moon are reported by Henry Cooper in his book *Moon Rocks*. Cooper joined Urey to watch the Apollo 11 landing on television at Houston.

RBB: Urey was ingenious in thinking up explanations for lunar features when no source of heat was imaginable to him.

UBM: Yes. But when the lunar samples, including igneous rocks such as basalts and anorthositic gabbros proved to be totally lacking in water—even water of crystallization—Urey accepted the facts but still puzzled over where the heat came from. Urey died in 1981. Had he lived for three more years, he would have seen the widespread excitement over the giant impact hypothesis of the origin of the Moon. What do you suppose he would have made of that?

RBB: I think he would have become reconciled to the facts and probably would have supported it. The hypothesis doomed his primitive Moon, but it bore similarities to his idea of lunar capture, and at least, it would have provided him with a heat source. That scenario yielded enough heat to mostly or wholly cover the Moon with an ocean of roiling magma.

You remember, don't you, that Reginald Daly wrote a paper in 1946 in which he proposed that the Moon resulted from a glancing collision of the Earth with another planet-sized body?

UBM: Yes, I have a copy of Daly's paper and of the review of it that you and Don Wilhelms published in 1992. Daly referred to "craters" in quotation marks because, to him, that word implied volcanism and he favored lunar impact craters. That was a remarkably prescient paper, but it was written at a time when you were learning for yourself that there was no support for the idea of lunar impact cratering.

Back to Urey for a moment. We owe him an enormous debt of gratitude because, with his immense national and international prestige, he helped mightily to persuade NASA of the importance of going to the Moon and conducting scientific research there.

RBB: Yes, indeed, otherwise the space program might have focused strictly on the physics of particles and fields, with little or no attention to the Moon or planets. And I might still be trying to convince people there are impact craters on the Moon.

UBM: You would have had better luck with respect to terrestrial craters, though. When *The Face of the Moon* reached Peter M. Millman, of the Dominion Observatory in Ottawa, Peter passed it on to Carlyle S. Beals, the Dominion Astronomer, and they discussed the possibility of finding impact craters on Canada's ancient Precambrian Shield. Beals instituted a search by air and on the ground and we know the results: today there are 29, and counting, proved impact sites in Canada. Were you ever consulted about this program?

RBB: Yes, I was. Beals invited me up there early in their program. They had a large picture on the wall and he took me over to it without saying a word. The feature was so eroded that only somebody who believed in the meteoritic impact theory could see a crater in it. I saw one; it was Manicouagan in Quebec.

UBM: That picture was on display when the Meteoritical Society met in Ottawa in 1963. It was extremely impressive.

RBB: It surely was. But, I think the crater I enjoyed most was Brent. They drilled it in the winter while the lake in it was frozen. One day, Beals wrote and asked me how far below the surface of the ice they should find the contact between the base of the Paleozoic lake beds and the brecciated materials of the crater floor—at the point where they were drilling. I went back to my data and answered that the contact should be something like 1,061 feet deep; they found it at ~1,049 feet. That reinforced Beals' belief that he was investigating a genuine impact crater.

UBM: In 1981, the Royal Astronomical Society of Canada elected you to honorary membership with a citation describing *The Face of the Moon* as the generating force behind modern research on terrestrial and lunar impact craters. It stated that seldom has a single book had such far-reaching consequences in the progress of science.

RBB: Ian Halliday read the citation and I was delighted with it.

UBM: We've not yet mentioned your book's influence on Eugene Shoemaker, who had resolved to go to the Moon while he was a student at Caltech. In 1949, Gene combed the literature and found what he described as "nothing but nonsense" about the Moon with the conspicuous exceptions of a paper published in 1893 by Grove Karl Gilbert and the newly published book by Ralph B. Baldwin.

RBB: Gene, with his youthful enthusiasm and drive, was tremendously important in planning for the lunar landings, training the astronauts, and so on. He had managed to get permission from the Barringers to make a quantitative study of the mechanism that produced Meteor Crater at a time when USGS people, of which he was one, were persona non grata there.

UBM: He was given permission by D. M. Barringer, Jr. when Gene was introduced to him by "Major" Lionel Brady, an all-round naturalist who ran a school for boys in Tempe, Arizona, of which Barringer and one of his brothers were alumni. Such happenstances can yield major consequences. Did you begin to follow Gene's career when he made his crater study?

RBB: Yes. And I was pleased to learn that after he had repeatedly petitioned the USGS to create a special branch for lunar and planetary research, they finally established one in 1960 with Gene as the director.

UBM: He called it the Astrogeology Branch. That caused some grumbling because "astro" means star, and stars are not rocky so they have no geology. However, by then we had astronauts, who don't fly to stars. What do you remember as Gene's most interesting projects?

RBB: Offhand, I would say his use of shock effects to identify impact craters, his USGS-style quadrangle maps of the Moon, and his initiation of the training of the astronauts to interpret lunar geology. In the summer of 1960, Gene and Ed Chao reported the first two natural occurrences of coesite, the highly shocked form of silica, at Meteor Crater and the Ries Kessel. That confirmed them both as impact craters.

UBM: Just as you had said they were back in 1949. From

1960 on, shock metamorphism, along with Bob Dietz' shatter cones, became the key factor in identifying terrestrial impact craters.

RBB: I would add to my list that although Gene was terribly disappointed when he learned that his health would disqualify him from astronaut training, he led the Branch all through the Apollo missions. Later on, he and his wife, Carolyn, did systematic searches of the night skies for asteroids in orbits threatening collisions with the Earth, and they undertook a crater search in Australia and identified nearly a dozen new ones out there.

UBM: In 1990, they led 48 extraordinarily fortunate meteoriticists on a 19-day, 8000-km camping expedition to visit craters in the western outback of Australia. We drove from Perth to Hall's Creek, to Alice Springs, to Adelaide and then flew back to Perth to attend the Meteoritical Society meeting. I have compared distances on maps and found our route would be about equivalent to driving from San Diego to Butte, Montana, to St. Louis, to New Orleans; or from Barcelona, to Stockholm, to Belgrade, to Istanbul.

RBB: But on different kinds of roads.

UBM: Very different roads; some of them just tracks in the desert. We field-trippers changed our seats every day so that each of us got to ride in the big, 6-wheeled desert truck and in all six of the small vans rented from Mr. Budget. Our tents and other gear were carried in a four-wheeled drive Mercedes truck. All vehicles were air-conditioned, especially the van from which the windshield blew out one day.

RBB: Which craters did you see?

UBM: Dalgaranga, Teague Ring, Connoly Basin, Veevers, Wolf Creek, Gosses Bluff, Henbury, and Lake Acraman. One day, several participants took over-flights in a bouncy little plane to see three more craters, Piccanniny, Spider, and Goat Paddock in the far-northern Kimberley Range. We also visited the astonishing stratigraphic section in the Flinders Range, 350 km east of Lake Acraman. Up there, we saw a thick layer of breccia, with clasts ranging from sand to football sizes, of 1,600 million year-old volcanic rocks that match the bedrock at Lake Acraman. The breccia is exposed in a 600 million year-old horizon within bedded meta-sediments. How to explain the one billion-year difference between zircon dating of the clasts and the stratigraphic dating? Answer: impact. A reinvestigation of the Lake Acraman structure had revealed shatter cones, melt rock, and the dimensions of Australia's largest crater. That trip proved to be literally a once-in-a-lifetime opportunity.

RBB: It surely did—unfortunately. I was terribly shocked when Don Wilhelms called with the news of Gene's death. The man was a giant, and the loss to our science is tremendous.

UBM: There is no question about that. In a chapter I wrote for a book published in 2002 by the Geological Society, London, I described you, Harold Urey, and Eugene Shoemaker as three scientists who played crucially important leadership roles in bringing about the transformation of geology from a strictly earth-centered to a planetary science in the 20th century.

RBB: You promised me a reprint, and I'm still waiting for it.

UBM: It is on its way. But fortunately, Gene lived for seven more years after the crater trip, and in that time, he had the ultimate experience of witnessing more than 20 fragments of Comet Shoemaker-Levy 9 plunge into the thick atmosphere of Jupiter! Gene's elation was contagious, even on TV.

RBB: I was elated too, but not on TV. Although it didn't happen on the Earth, doesn't that qualify impact as a geologic process?

UBM: It surely does. Right after it happened, Bob Dietz wrote to me saying: "Impact is an actualistic process now."

To go back to our earlier discussion, when did you begin work on your next book: *The Measure of the Moon*?

RBM: I knew as soon as I finished *The Face of the Moon* that I wanted to do a quantitative study of the relationship between the scaled depth of bursts and crater dimensions. I realized that much of my data on bomb and shell craters wouldn't apply directly to the lunar craters because the fuzes often would hit the ground and explode the shell before the shell had penetrated the ground. This actually would have produced an air burst. I believed that the big lunar craters were essentially from surface bursts. By that, I meant, for example, that a giant impacting projectile might have burst at a depth of 30 km and produced the Imbrium Basin, 1200 km across. I needed to do a lot of experimentation and make some detailed measurements of the lunar topography, but I didn't get a chance to do much until well into the 1950s.

UBM: You detonated a lot of explosives yourself, didn't you?

RBB: Yes. A favorite place for that was near our cottage on Lake Michigan. I knew a fireworks expert who made charges for me of weighted amounts of various explosives—black powder, blasting powder, TNT, and so on. I knew the energies of their explosions, so I found places on the beach where the sand was slightly damp so my craters wouldn't collapse right off. Sometimes I arranged sands of different colors in layers and set off explosions in them. For each crater, I measured how much was overturned, how much was pushed downward, and how much was gone altogether. I had an interesting time, but the neighbors didn't like the noise very much.

UBM: Was it this experimentation that led you to conclude in *The Measure of the Moon* that the Tunguska explosion was an air burst?

RBB: No. I came to that conclusion from the reports of pressure waves in the atmosphere measured from the Kew Observatory in England shortly after the explosion occurred and from ground waves reported by many seismographs in Europe and Siberia. The evidence indicated that about 5000 times as much energy went into making air waves as into ground waves. I was the first person to write that the body never hit the ground.

UBM: I recall that you argued that the Tunguska body was a stony meteorite rather than an iron. Do you now have a preference between a stony meteorite and a comet?

RBB: I lean toward a stony body. Its path through the atmosphere was a long one, and yet, the body did not explode until it was approaching the ground. It seems more probable that a comet would have been composed of more frangible material.

UBM: As part of your own experimentation, didn't you also fire rifle bullets into the sand?

RBB: I did. One day a friend came to our cottage with a high-powered rifle which we fired at different angles against the damp sand—always aiming at places where nobody could possibly be in front of the gun. It wasn't until we got down to about a two-degree angle of fall that the craters would stop being round. Below two degrees we would get a long gouge.

UBM: Like those radial valleys you described on the Moon.

RBB: Yes. The radial valleys around Imbrium are explicitly that kind. I don't know how close the lunar fragments came to two degrees, but they went flying out of Imbrium almost tangentially because some of the valleys are quite long.

UBM: By the time you were writing *The Measure of the Moon*, other people were beginning to find impact craters and to measure their depths and diameters, weren't they?

RBB: Yes, they were, and I kept a file of all the data they published. I also went out to look at some of them myself. I identified one of the so-called cryptovolcanic structures as a probable impact feature.

UBM: Which one was that?

RBB: The one at Decaturville, Missouri, which I visited in 1961. When I arrived there, I found a nice mobile home right in the middle of the structure with a pleasant couple, Mr. and Mrs. H. B. Hart, living in it. As soon as I told them the purpose of my visit, they invited me to have lunch with them. That was most enjoyable because Mrs. Hart was an excellent cook. Mr. Hart told me he owned that whole area and he wanted to mine the structure because there were some peculiar chemicals in the rocks. He had drill cores of the central region stored in a shed. This was about a year after Robert Dietz had declared shatter cones to be diagnostic of impact structures, so I looked for them in a few of the cores without finding any.

UBM: Today, we would expect to find the shatter cones around the perimeter instead of in the middle of the structure.

RBB: That's right. I wasn't looking in the best place. But, I did determine that the rocks of the central region had been uplifted above their normal level, so I told Hart I was reasonably certain that he was living in the center of an eroded four mile-wide impact crater with a slightly raised rim. I admitted I wasn't positive of this, but I said I would tell other crater specialists about it. When I got back to Grand Rapids, I contacted Ed Chao and Gene Shoemaker and they both visited Decaturville and found good shatter cones there.

UBM: So, one more impact structure was added to the world's growing list. And you described it in. . . .

RBB: *The Measure of the Moon.*

UBM: You included a new and improved contour map of the Moon's Earth-facing side in that book. Tell me about that.

RBB: I wanted to construct a new contour map to determine as accurately as possible the shape of the Moon's equatorial bulge that is aligned with the Earth. I believed that this could contribute essential information on the processes that had been at work on the Moon. I felt that earlier attempts at making contour maps were unsatisfactory, so I tried the stereopticon method. I got a list of all the lunar photographs at the Lick Observatory and selected five with widely differing librations. I had copies of these five made for me on 8 × 10-inch glass plates. Then, I needed a measuring machine. I found that most of those at observatories were in use, but I remembered one at the Dearborn Observatory that had been used for measuring stellar parallax. I checked with the Dean at Northwestern who found it still was there, in storage, and was large enough for my needs. I could use it if I insured it for its replacement value. That was easy; I bought the insurance and drove the machine from the Dearborn Observatory to my home.

UBM: Where did you find room for it in your house?

RBB: In my basement office, which previously had served for storage of jams and jellies. It still had some of them on its shelves. I mounted enlargements of my five plates on paper-like material 0.5" thick. On them, the Moon's diameter was about 30 inches. I studied each plate in detail and identified numerous sharp features that I could easily locate on matching plates. I marked and numbered them on the enlargements. To measure heights of lunar features required multiple measurements of their positions on at least two plates. I hoped to measure at least 1000 points, but I actually measured only 733 of them.

UBM: Did you draw your contours from those?

RBB: I used only the 696 best ones.

UBM: Obviously, this involved a lot of very meticulous work.

RBB: It did. After I had started on Plate I, I came downstairs one night to continue my measurements and found the machine was a tiny bit out of line. I thought the machine, itself, might have moved slightly due to a change in temperature or a vibration, or that the plate had slipped. Any problem like that could have aborted my mission. I went upstairs and asked if anyone had touched the machine. To my great relief, my son, Dana, said his elbow had touched it when his mother sent him down to get a jar of something.

UBM: Had his slight touch done any damage?

RBB: Nothing serious. I had to start over, but this promised to be such a long job that a few hours wouldn't make any difference.

UBM: What was your contour interval?

RBB: Once I had all my data reduced, I plotted lines of equal altitude on a circle on graph paper in units of 0.00050 times the lunar radius, or 0.870 km. I believe my contour map still is the best one available except in areas covered by the altimeters carried by space capsules orbiting the Moon.

UBM: What were some of your main findings?

RBB: The map showed that the maria were lower than the highlands by an average of about 2 kilometers. The upland areas sloped generally downward toward the shores of the maria for about 150 km. The deepest portions, approaching almost 6 km, were in Mare Imbrium. I was very pleased when the first Ranger to reach the Moon landed on Mare Tranquillitatis and found that it was depressed by more than a kilometer. The earth-facing bulge appears to be about 2 km in height, but its meaning is uncertain because it now appears that the center of mass of the Moon is displaced toward the front face by perhaps 2 or 3 kilometers.

UBM: Did you publish your map as soon as you finished it?

RBB: Yes. It first appeared in *Sky & Telescope* in February 1961, and then in a Japanese publication in July. I soon received a letter from Peter Hédervári in Hungary, who enclosed hypsometric charts he had made comparing the Earth with the Moon, using my map. He also informed me, very diplomatically, that I had made an error in almost the center of the Moon. I checked and found he was right, so I corrected the error and gave him credit. The corrected version was the one I used in my book.

UBM: Now I can see why you named your book *The Measure of the Moon*.

RBB: That was a title that Lois proposed during a meeting we had in the editorial office of the University of Chicago Press. We had batted around several ideas until she came up with that one, which fitted the book like a glove.

UBM: You presented all the best data then available on the Moon and also on terrestrial craters. So the book was a great boon to those scientists who were becoming aware of the research possibilities of the newly opened Space Age. It got resoundingly favorable reviews from Robert Dietz and others.

RBB: Dietz had read the manuscript for the University of Chicago Press, and in his letter of recommendation, he wrote that if I had done nothing for the last thirteen years except prepare this new book, I had wasted little time.

UBM: Where did you go to watch the Apollo 11 mission?

RBB: I went to Cape Kennedy with my son, Dana, to see the blast-off and then stayed glued to a television screen. It was wonderful to see the lunar surface close up and to watch men doing field work there.

UBM: You must have been pleased when the maria proved to be basaltic lavas of different ages and the oxygen isotopic compositions of the lunar samples showed that the Moon and Earth originated in the same neighborhood of the solar system.

RBB: Yes. I felt fully vindicated on both counts.

UBM: And when the Apollo 16 rocks proved to be impact breccias instead of youthful volcanics, as had been predicted by the astrogeologists, you were more than vindicated on the importance of impact cratering.

RBB: Yes. And the later orbital missions such as Clementine provided us with good views of immense basins, mostly with no mare filling, on the far side of the Moon.

UBM: What are your thoughts on the hypothesis of a terminal cataclysm on the Moon proposed in 1974 by Tera, Papanastassiou, and Wasserburg?

RBB: I don't agree that there ever was a lunar terminal cataclysm. My work in counting small craters within larger ones showed that there had been a smooth decline in numbers of infalls from the last time when the Moon's surface was saturated with craters. The terminal lunar cataclysm, if any, would have occurred when the production of new craters had reached a minimum rate of about 0.004 relative to the maximum early in the Moon's history. The "evidence" for a terminal cataclysm is that the returned samples show a clustering of radiometric ages between 3.95 and 3.85 billion years, and they contain no impact glasses older than that. To me, this clustering reflects the fact that the Apollo samples were collected mainly from the ejecta blankets surrounding the Imbrium and Orientale basins, which formed about 3.9 and 3.85 years ago, respectively. Other than those two large basins, there was no giant, or even a small increase in the rate of impacts of any size. The other large basins on both the near and far side, were distributed evenly in time back to the ending of the saturation time.

UBM: So, you view the cataclysm as an artifact of sampling?

RBB: Yes. And I am not alone in holding this view. Some highly distinguished lunar investigators share it with me. In his book, *To a Rocky Moon*, Don Wilhelms names Bill Hartmann, Gene Shoemaker, Ross Taylor, and George Wetherill, and I could add Bevan French, who recently helped me try to persuade a young student that the terminal cataclysm didn't happen. To no avail—she accepts the word of the "authorities."

UBM: Have you read the paper by Cohen, Swindle, and Kring in *Science* in 2000? They thought lunar meteorites might contain impact glasses older than those in the Apollo samples, but they didn't find any; the glasses in the meteorites ranged from a maximum of 3.92 down to 2.76 billion years. This does not confirm the terminal cataclysm but neither does it provide older ages of impact glasses that would support your view.

RBB: Yes, I have read it. But to explain my argument in more detail, I first selected a group of 81 basins and craters larger than 161 km across. I designated the large craters that were bright and clear with high rims and distinct features as Class 1. I then assigned older craters to classes 2–10, with the older ones having progressively subdued features and lower rim heights. These estimates seem to have been generally correct within plus-or-minus one Class.

It is well recognized that very ancient, Class 10, craters appear to have been formed on a surface that was saturated with craters, that is, where craters were being formed at the same rate as they were being destroyed by newcomers. I arbitrarily assigned an age of 4.3 billion years to the oldest, Class 10, craters. This date is not exact, but a modest change in it, either way, would not affect my conclusions.

Then, I counted new craters that had been formed on each Class 10 crater and its rim. For each large crater or basin, I recorded the results on a semi-log chart, with the diameters plotted horizontally against the log of the number of craters of each size. The large craters and basins differed in area, yet my charts all showed a slope of about minus 2, so I assigned an index for each crater. By my classification, the giant basins ranged in age from Class 10, for the unnamed basin between the Altai edge of Nectaris and Werner, through Class 8 for Humorum, to Class 1 for Imbrium and Orientale. My results showed that the rate of crater-forming infalls declined steadily by a factor of 250 from the date of saturation to the date of Imbrium. Within that time span, I found no sign of a slowing down of cratering rates followed by any terminal lunar cataclysm.

The ejecta from Imbrium and Orientale was spread over the entire lunar surface and, as I said earlier, it must have made up the bulk of the samples picked up by the astronauts. So, it would not be surprising if these samples showed a limited range of dates from about 3.9 to 3.85 billion years. We know that impacts continued at a low rate after the time suggested for the terminal cataclysm, and they are still continuing.

UBM: This helps to clarify many things for me, and may do so for other readers.

RBB: Why hasn't anyone suggested a cotemporal terminal cataclysm for the Earth?

UBM: I think the most likely reason is that on the bigger, hotter Earth, the initial Hadean Eon of wild volcanism and impacts destroyed most patches of the crust that formed before about 3.8 billion years ago. By then, Orientale had been excavated on the Moon, and the heavy action in our neighborhood was over.

But, it is not quite over. You have written about the dangers we face from asteroidal bodies in orbits that may some day bring them into collision with the Earth. Did you ever name the K/T as a possible example?

RBB: No. I didn't list the K/T specifically. In *The Face of the Moon*, I referred to tiny asteroids which could, in some future year, entirely devastate an American state or European Country, or wipe out local species of flora and fauna. I mentioned that sudden disappearances of long-established groups of contemporary life have been recorded in geologic history and asked if it were not possible that the causes of these occurrences were meteorite impacts. That was a straightforward conjecture that failed to attract attention until 1980 when the Alvarez group at Berkeley published clear, geochemical evidence of an impact at the K/T boundary.

UBM: By then, there was an active international community of scientists interested in impact processes, so within a few years, we had a plethora of confirmatory evidence. I have been fascinated, though, with the duplications of effort I found while reviewing the history of impact studies. The impact origin of Imbrium and its radial grooves formed by flying ejecta were proposed by G. K. Gilbert in 1893 and again by you in 1949; multi-ring craters were described by you in 1949 and again by Hartmann and Kuiper in 1962; the Chicxulub crater was proposed as the site of the K/T impact by Penfield and Camargo in 1981 and again by Hildebrand and Boynton in 1991. New observations evidently have to be advertised, and the scientific community has to be ready to assimilate them.

On another topic, you have corresponded with many lunar and planetary scientists, but didn't you once exchange letters with Stephen Jay Gould?

RBB: Yes, I did. In 1993, in one of his essays in *Natural History*, Gould wrote that theory must guide observation; otherwise, we will not see what lies before our eyes, for the conceptual tools will not be available to us. I had reached the same conclusion independently during my early experience in trying to teach people to look at the solar system in a new way. I wrote to Gould to congratulate him on his article and tell him how he had helped my understanding of people's reactions to new ideas. Gould thanked me for my letter and said that he knew, for a very personal reason, the story of my campaign to prove the impact origin of lunar craters. In 1958, for the first paper he wrote as an undergraduate at Antioch College, he had chosen the topic of lunar topography, just as my views were finally beginning to receive wider acceptance. He said that he loved my book and he strongly supported it in this, his first effort at writing about science.

UBM: I had no idea that Steve had started his writing career with an essay on lunar topography.

As my final question, what, to you, is the most impressive aspect of meteoritics today?

RBB: The thing which strikes me more than anything else is the increase of accuracy and precision of radiometric dating. Dating meteorites, dating the Earth, dating the Moon. Everything happened almost at the same time way back there.

UBM: In 1950, we didn't know how old the Earth was.

RBB: In *The Face of the Moon*, I quoted Arthur Holmes' age of 2 billion years.

UBM: But, the age of the Earth more than doubled in 1956 when Clair Patterson at Caltech declared it to be 4.55 ± 0.07 years old.

RBB: Now they're arguing over the third decimal. Most of them are coming in at 4.56 ± 0.001. To me, the amazing part is that a human being can take a whole series of individual observations and draw conclusions that are meaningful about what happened nearly five billion years ago. It's mind boggling.

UBM: Ralph, I am most impressed that, although you have been pursuing your scientific research alone and in your spare time, with no corps of students or of colleagues with whom to maintain a daily exchange of ideas, you are the only scientist besides Eugene Shoemaker to be presented by the Meteoritical Society with both its Leonard Medal and its Barringer Medal, and also by the Planetary Science Division of the Geological Society of America with its G. K. Gilbert Award. I cannot resist quoting Don Wilhelms who dedicated his book *To a Rocky Moon* to: "The amazing Ralph Baldwin, who got so much so right so early."

Thank you very much for giving me this interview.

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