Meteoritics & Planetary Science 49, Nr 6, 1119–1138 (2014)
doi: 10.1111/maps.12298

Report

Oral histories in meteoritics and planetary science—XXIV: William K. Hartmann

Derek W. G. SEARS

Space Science and Astrobiology Division, NASA Ames Research Center/BAER Institute, MS245-3,
Moffett Field, Mountain View, California 94035, USA
E-mail: derek.sears@nasa.gov

(Received 06 January 2014; revision accepted 10 March 2014)

Abstract–In this interview, William Hartmann (Bill, Fig. 1) describes how he was inspired as a teenager by a map of the Moon in an encyclopedia and by the paintings by Chesley Bonestell. Through the amateur journal "Strolling Astronomer," he shared his interests with other teenagers who became lifelong colleagues. At college, he participated in Project Moonwatch, observing early artificial satellites. In graduate school, under Gerard Kuiper, Bill discovered Mare Orientale and other large concentric lunar basin structures. In the 1960s and 1970s, he used crater densities to study surface ages and erosive/depositional effects, predicted the approximately 3.6 Gyr ages of the lunar maria before the Apollo samples, discovered the intense pre-mare lunar bombardment, deduced the youthful Martian volcanism as part of the Mariner 9 team, and proposed (with Don Davis) the giant impact model for lunar origin. In 1972, he helped found (what is now) the Planetary Science Institute. From the late 1970s to early 1990s, Bill worked mostly with Dale Cruikshank and Dave Tholen at Mauna Kea Observatory, helping to break down the Victorian paradigm that separated comets and asteroids, and determining the approximately 4% albedo of comet nuclei. Most recently, Bill has worked with the imaging teams for several additional Mars missions. He has written three college textbooks and, since the 1970s, after painting illustrations for his textbooks, has devoted part of his time to painting, having had several exhibitions. He has also published two novels. Bill Hartmann won the 2010 Barringer Award for impact studies and the first Carl Sagan Award for outreach in 1997.

DS: Bill thank you very much for doing this. I would like to start with a very general question. What is the one incident in your life above all others that has determined the nature of your career?

WKH: I would say that what initially stirred my excitement for this topic were the books I stumbled across as a teenager. One event I recall was that my brother, who was 8 years older than I was, had a young person's encyclopedia called the *Book of Knowledge*. One day I was looking at that book and there was this map of the Moon. Craters, mountains, plains, all sorts of features. That blew me away. The concept that there was this other land, not just a shining thing in the sky, but a geological body, a new geographical *place*. There was also a book by Willy Ley and Chesley Bonestell, *Conquest of Space*, which had all these marvelous paintings by Bonestell, visualizing what it was like on other planets. It came out in 1949. I am fond of my copy of that book because my father somehow managed to get Willy Ley, a German expatriate colleague of von Braun's, a writer and popularizer for space, to come to our town and give a talk and autograph my book. Many years later I met Chesley Bonestell and got him to autograph the book. There are not very many copies of that book with the signatures of both authors! The paintings gave me a real desire to want to know what it would be like on other worlds.

What the map of the Moon and the Bonestell paintings showed were real geographical places, a real contrast with the situation today where children get excited by, say, the Harry Potter and Tolkein books,

Fig. 1. William K. Hartmann taken 2010 Aug 2 (Photo: Gayle Hartmann).

now known mostly as movies. These are pure fantasies. The images I was looking at, were dealing with real places! So there was a group of us in the 1950s who got hooked on the "fantasy" of real places and ended up with careers in planetary science and exploration.

In terms of what set me off on a career in planetary science, I would have to say my graduate assistantship with Gerard Kuiper. In 1961, I arrived in Tucson from near Pittsburgh, where I grew up. Kuiper put me to work on a project that was called the Rectified Lunar Atlas where we projected photographs of the Moon—taken from ground-based telescopes—onto a globe. This way we could take a camera and move it round the globe and see the surface everywhere as if we were looking down from right above. During that first year, I came in one day and saw this projection of the Moon taken at just the right libration, and with the right shadows and the right lighting, so that when you walked around to the side of the globe, you could see this enormous bull's eye pattern. We now know it as the Orientale Basin. It had never been seen before. It was one thing to see the rings, but it was something else to realize that it led us to a *pattern* of basin structures. I had read Ralph Baldwin's book on the lunar features and realized that the bull's eye pattern meant that all giant lunar impact scars create multiring fracture systems. I took the pictures into Kuiper and told him that we have a very exciting feature here. He graciously let me be the first author on the discovery paper. We coined the term "multi-ring basin," and showed that nearly all the big impact features over a few hundred kilometers across had at least vestigial, concentric, multiring systems. Chuck (Charles A.) Wood and I did a 1971 paper summarizing the transitions from simple craters to central peak craters and multiring features, and coined the term "peak ring crater" for the rare transitional form where the central peak has spread out into a ring of peaks, as in Schrödinger on the full-Moon, leading to full-fledged multiring basins. Of course, in later decades, they began to be seen on other planets as well, and transitions occurred at different sizes on each world. I wrote a paper suggesting a relation between the transition sizes and gravity.

Then, in the summer of 1964, Kuiper sent me to Mauna Kea on the Big Island of Hawaii to do site testing for what he envisioned would be a new observatory. There was a gentleman on the lunar lab staff named Alika Herring who had been hired by Kuiper. He did the first site tests and then I relieved him for 6 weeks. It was my first big travel adventure. I was on my own, living on the mountain top, going down Hilo every few days. I had a two-way radio, so if I started up the mountain at 10 P.M., I would let a ham radio operator in Hilo know where I was. That led to a long love affair with the "Big Island" of Hawaii.

While I was on Mauna Kea, that summer, Ranger 7 became the first probe to take close-up photographs of the surface as it fell into the Moon. It struck me that while everybody was racing to see smaller and smaller details, in fact in Kuiper's lab we were still discovering "new" 1000 km scale features! In retrospect, I see this as a subconscious pattern in my career. While many scientists are trained to look for ever-finer detail, I like to back away from the detail and look at a much larger scale.

DS: So what stimulated your interest in planetary science were the books you read as a teenager and what established you in this career were your experiences in graduate school, discovering the structure of Orientale, being on Mauna Kea, and thinking about the new Ranger images of the Moon. You also seem to have been very interested in Baldwin's book.

WKH: Yes, and an interest in craters in general, and their size distributions, from the smallest to the largest. The crater size distributions were interesting to me because there was still an argument about whether they were the result of asteroid impact or volcanism. One of the points we could show was that the size distribution of craters on the Moon was the same as the size distribution of craters that would be caused by impacts of asteroids in the asteroid belt. Those were two totally different bodies of literature until you saw the connection.

DS: So that is what got you started. What I want to do now is a biography with an emphasis on your scientific work. So let's go back to your earliest days. Where you are born, your parents, tell me something about your childhood.

GROWING UP IN PENNSYLVANIA, BONESTELL, MOORE, AND THE STROLLING ASTRONOMER

WKH: I was born in New Kensington, Pennsylvania, which is a little town up the river from Pittsburgh. My father was a civil engineer with an aluminum company, Alcoa. He was in research and testing for Alcoa. His work was about utilizing aluminum with its various micro properties for everything they could think of. He became the head of research for the last few years of his career. My mother's father was a mathematician and something of a philosopher, fairly well-known, and my father's father was a painter. I suppose subliminally they may have affected me. I've always wondered about it. I was never very mathematical or analytical but I was very good at putting ideas together. And my parents were always interested in nature and in explaining things. So I grew up in a household where everything was interesting. Nothing was very dogmatic. I got interested in archaeology. I had this tiny museum, with all sorts of Indian artifacts, partly from my grandfather's farm. Ancient things always intrigued me.

Somewhere, about age 10 or 12, I began to realize that my interest was shifting to planets and astronomy. There was a period of lost love; I loved archaeology but suddenly there was this new interest. I have tried to recall how I became interested in astronomy. Earlier I mentioned the map of the Moon and the Bonestell book. I loved Bonestell's paintings—the whole idea of them. As a teenager I tried to make my own paintings of the planets. Bonestell usually gave the angular width of his paintings. He thought of everything in terms of the angles, the way a typical camera lens would render the scene. A typical snapshot has an angular width of approximately 35 or 40°. So, he would figure out the angular size of Saturn as seen from one of its Moons. If he had 20 inch wide "typical snapshot" painting, and Saturn was 10° wide from that particular moon, it had to cover about a quarter of the width of the picture.

DS: Well, he was trained as an architect.

WKH: Well, as an architectural draftsman. He was the leading Hollywood special effects artist during the 1940s. He worked on *Citizen Kane* and *The Hunchback of Notre Dame*. They built the bottom 10 feet of the Cathedral and he painted the rest, knowing where the camera was.

DS: What were your other childhood influences?

WKH: In 1954, I think it was, my father got a 4 week vacation because he had worked for Alcoa for 25 yr. The family had never been to Europe, so we went. I went into this bookstore in London on the first day we were there and there was Patrick Moore's *Guide to the Moon*. That, together with the map, really made me want to look at the Moon and planets with a telescope. Well, we came back from the trip and I had to have a telescope. For Christmas, I got a little 2.4 inch refractor. Around my freshman year in the high school, I joined a local astronomy club, which was connected to the Pittsburgh amateur astronomy club and was very active. It had a mirror grinding laboratory and I actually ground an 8 inch mirror. My dad helped me with the telescope.

DS: In about 1962, I wrote to Patrick Moore for instructions on making a telescope and he sent me back a two-page handwritten, tightly written, letter packed full of information. He started out by saying that he knew nothing about this, but then went on to write two pages, ending by giving me another name to contact. It's amazing that at the time when he was such a big TV personality that he had time and inclination to write a two-page personal letter to a 13 or 14 yr old. You were about the same age when you vacationed in Europe?

WKH: I was 14, so I did not really appreciate all that there was to see, all the European history for example. My father's father, the painter, came from Switzerland. So, we visited Bern, and saw the large, old clock tower. At noon, a crowd would collect and watch while automated doors opened and little carved characters would come out and march around. Nearly fifty years later, I had an invitation from the International Space Science Institute in Bern and I walked by that tower and felt the ghost of 14 yr old Billy standing outside, looking up at this clock.

DS: It was probably 14th century!

WKH: Well, the cornerstone of the nearby apartment building I lived in, in old town Bern, dates that building at 1695!

DS: So astronomy became a big hobby?

WKH: All of us in Kuiper's group were about the same age and had all joined the Association of Lunar and Planetary Observers (ALPO), which was the American equivalent of the British Astronomical Association (BAA). The BAA in England, of course, was almost professional in observing planets, clouds on Jupiter, dust storms on Mars, all the things you could see with a modest backyard telescope. (It always seemed strange to me, that the country with the worst cloud cover would have the most active amateur astronomers.) The ALPO attracted most of us in that generation of students. I was a member, so were Dale Cruikshank, Al (Alan B.) Binder, and Clark Chapman, from high school to college and graduate school. When we met each other in Kuiper's lab in graduate school, most of us had been sending visual observations to the ALPO. In fact, most of us had already known each other through our observations since high school.

DS: So you were a teenager, and you were in this club, how did you make contact with the others?

WKH: The ALPO had a journal, called the "Strolling Astronomer," edited by Walter Haas, in New Mexico, and it had a "section" for each planet, like the British Astronomical Association. We sent in our observations and were seeing each other's names. At the end of each apparition of Mars, for example, the Mars recorder would combine our drawings, so they could track changes, dust storms, recession of the polar cap, and so on. We would send the observations and get these very nice professional looking letters back. They were not necessarily from someone as elevated as Patrick Moore, but they certainly encouraged us. It was so exciting to see our drawings of Mars in this publication.

Let me add something before I forget. We had all gone through teenage years observing with our backyard telescopes knowing the names of all the features. So, it was a kind of disillusionment to start going to professional university seminars with the famous names of the day, and realize that this handful of important names in planetary science knew hardly any of the names of the features on the planets. Their view was more detached and theoretical.

DS: Did you say whether your mother had a job?

WKH: No she didn't. She taught math until she was married. She was a typical well-educated woman of the 1950s. She was a member of a book club, a piano group, a sewing club; she had a number of social functions. They were all-women activities; very gender separated. The exception was the bridge club, when they would play as couples.

DS: So you grew up in a very educated environment?

WKH: Yes. There was nothing particularly unusual among our family friends. I suppose we were upper middle class, but I was not very conscious of any class or income level, because there was not the huge income gap that we have now. We lived in a two-story brick house within walking distance of my dad's lab and we had one car, a Chevrolet. All the kids in town went to the same high school. Black, white, richer, poorer. No charter schools. No schools teaching creationism. That was what it was like in the 1950s. It is not like that now.

DS: When you were a teenager, was the European vacation the only one you took?

WKH: No. Each summer, we would take some vacation in the family car, but it was just 2 weeks. We always went to Illinois where both sets of grandparents were, and I had an aunt and uncle in Houston, Texas. One year we went to the Rocky Mountains. The trip to Europe was unique.

DS: So at some point you thought about college. How did that happen?

COLLEGE AND MOONWATCH

WKH: I had an aunt who was Dean of Students at the University of Indiana and she arranged to have Frank Edmonson, who was an astronomer there, show us the observatory. That was a big thing. I was then about 16 or 17. I was trying to figure out where to go to college and Edmonson's advice was that if you were interested in astronomy do not go straight into astronomy but take physics. So I ended up at Penn State studying physics. There was no question I would go to college. It was always assumed I would go and I really think I had a marvelously peaceful, simple adolescence. Somehow my upbringing gave me enough momentum that I was just sailing along without any great angst.

DS: Can you say something about your time at Penn State?

WKH: Carl Bauer was the astronomy professor at Penn State. A very interesting guy, with a sense of humor. One day, our class decided to play a joke on him and we all set our watches 6 or 8 min slow to arrive late and convince him his watch was wrong. He one-upped us. He had started lecturing on time to the one student who wasn't in on the joke, filled the board with equations, and tested us on it during the next class period. I had an assistantship with him. He was one of the pioneers in measuring the helium-3/helium-4 ratios in meteorites. Those were beginnings of cosmic ray exposure age techniques. Somehow I got the message from Bauer, to cut back on the amateur observing and put down the paints, because to be a serious student I had to focus on my degree in physics. All during that college period, however, I was sending occasional drawings to the "Strolling Astronomer" and communicating with Cruikshank and the others by occasional letters.

In 1956, President Eisenhower announced that America would launch the first artificial satellite, during the International Geophysical Year. All the amateur astronomers were organized into what was called Project Moonwatch. That was a program in which amateurs would spot the satellites and record their position in the sky. Amazingly enough, when they launched those things, they were not exactly sure where they would go. What orbit they would have. Every launch had its own quirks. Of course, the Russians were the first to launch a satellite, on October 4, 1957. The Moonwatch program started in 1956, when I graduated from high school, and in 1957, they were building a Moonwatch facility on the roof of Allegheny Observatory where I had a wonderful summer job working on stellar parallaxes (Fig. 2). The Moonwatch facility was a row of chairs in a north–south line and there was a pole in the middle. Each observer on the team sat with a small telescope, which was aimed at the top of the pole so, as a team, they were covering the sky along the Meridian. Penn State, where I was during the winter, had its own Moonwatch team and we frequently went out at dusk or dawn, when satellites were visible. If you ever want to be cold, sit in a Moonwatch chair for an hour or so in December in Pennsylvania watching for satellites.

DS: What was the mood of the group, looking at the first Earth satellites?

WKH: It was so exciting. Would our target show up on time? How good was the orbit? How bright would it be? Would it be tumbling, end-over-end?

DS: So how long did you keep this up?

WKH: For most of the time, I was in college; I was active in Moonwatch, either at Penn State in the winter or at Allegheny Observatory in the summer. They were less necessary after that.

DS: How did you select The University of Arizona for graduate school?

THE UNIVERSITY OF ARIZONA AND GERARD KUPIER

WKH: In 1960 and 1961, there were really were only a handful of graduate schools where you could study planets. Harvard had a strong astronomy program with Fred Whipple, but I was looking for somewhere where I could study planets. Kuiper had just moved to The University of Arizona, in Tucson, from Yerkes Observatory, and NASA or NSF had a program of creating centers of excellence. They wanted to take 8 or 10 grade B universities and promote them to grade A research universities. So, out of that situation, Kuiper got a whole building, which became Kuiper's Lunar and Planetary Laboratory. So I got into the Arizona program and got assistantship from Kuiper. I drove out there with my parents; they had their car and I had mine, a used Ford Falcon. They installed me in a room of a boarding house owned by a lady who I discovered, from the literature lying around the house, was not in great health and was sending her money to a miracle cure evangelical group that was located in southern

Fig. 2. Young Bill, seen here with his home-made 8 inch reflector telescope, joins the local Moonwatch team, as reported in the New Kensington, PA, local paper for March 8, 1957.

Arizona. She had books with pictures of demons, with forked tails and horns, with labels to show which demons caused which diseases. A sad and medieval environment. Illuminating about our country! That was my first year at Tucson, 1961.

Kuiper's initial office was in the physics building, but his setup for the Rectified Lunar Atlas photography and crater cataloging was in a Quonset hut-like building called Temporary Six, T-6. Dale and I used to joke about Kuiper's sporadic needs for a grad student to help with something. Dale, in particular, would get calls at 11 o'clock, "Are you doing anything, Dale?" "Well, I'm trying to sleep!" And whenever there was a crisis, Kuiper would be in the hall crying, "Call T-6. Call T-6." Naturally, we think there should be a giant plaque over there on the site where T6 used to be. Instead, there is the science library.

Kuiper came from the European tradition where major observatories and labs would produce their own set of publications, for example, catalogs of, say, 500 star positions, which would take up too much space in a journal. So, Kuiper started his own inhouse journal, *Communications of the Lunar and Planetary Laboratory*, which enabled him to publish anything he wanted. However, it did not go over terribly well with some of his critics, since it also let him do an end-run around the peer-review process.

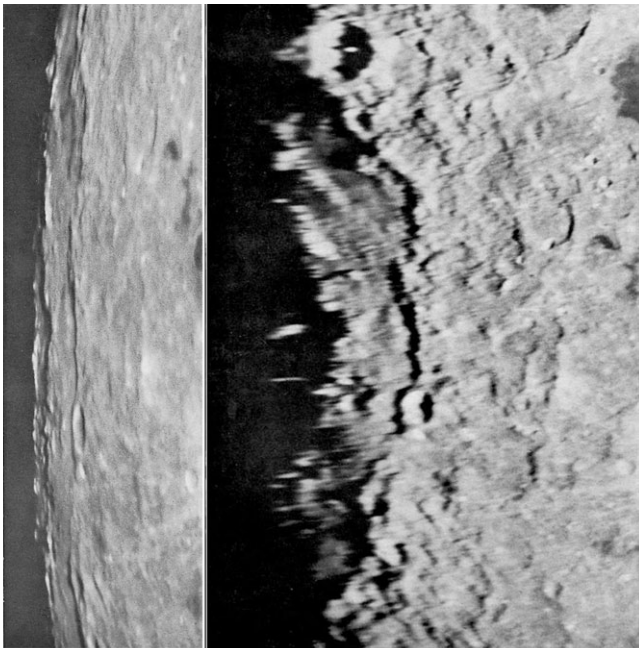

Fig. 3. Discovery of the Orientale Impact Basin. Earth-based photographs showing multiring basin on the limb of the Moon, and the discovery photograph of the Orientale multiring impact basin on the Moon. The left-hand photograph shows the normal, foreshortened view from the Earth. The right-hand photograph was made by projecting an Earth-based photograph onto a white globe, and rephotographing the globe, in a pre-Apollo mapping program designed by G. P. Kuiper (Hartmann and Kuiper 1962).

Harold Urey was one of the critics; he and Kuiper had a famous and long-term feud. It was interesting to begin to perceive that from our lowly position as grad students. When Kuiper and I published the Orientale discovery paper in the LPL communications, we included a whole series of the multiringed basins we had observed (Fig. 3). Once we had seen Orientale, it was shockingly easy to go back and find dozens of concentric ring structures. Baldwin was really on to it too, but somehow if you read his book, you do not get the feeling that this was a common universal pattern. Curiously enough, there is a √2 ratio from one ring to the next ring and Kuiper and I talked about all that in our paper. A few weeks later, I found in my mailbox a letter from Harold Urey. "Oh my gosh, I'm a graduate student getting a letter from a Nobel Prize winner." I opened it and the first line was—I feel like I have this quote fairly accurate—"As an older man to a younger man, I want to tell you that this is not a good way to start your career." He launched into a big attack on the paper we had published. He could not see the rings. In particular, he objected to our comments on Imbrium. In his famous 1952 book, *The Planets*, he had published an interpretation of the Imbrium impact feature where he thought that the actual impact was in the Sinus Iridum area, the trajectory was tangential, and that the symmetry was bilateral, not concentric. Urey could not abide a concentric pattern. Meanwhile, I published a couple of follow-up papers in the LPL Communications on the strong radial patterns of grooves and other lineaments around basins. It seems to me we still don't understand the mechanics of formation of the concentric rings, the √2 spacings, or the relative importance of fractures, flying ejecta, and base surge striations in the radial systems—although recent researchers such as Jim Head and his colleagues have done some interesting work on the subsurface structures of the rings, using GRAIL gravity data.

Kuiper had some disagreements with Shoemaker, too, when Kuiper, Shoemaker, and Urey were all on the Ranger 7 team. Ranger 7 revealed that craters at <100–200 m in size were crowded and often had very soft rims, making barely visible depressions. Shoemaker showed that many or most of the soft-rimmed craters seen on the Moon were probably secondary ejecta impacts. His research kept me interested in the possible utility of the impact craters for dating. Kuiper, however, thought the "soft craters" were sag pits formed as lavas flowed over pre-existing craters and cavities. I think he was naturally frustrated as Shoemaker's view prevailed.

Listening to some of those argumentative discussions as a graduate student finally made me realize that if you find yourself thinking that other people are too dumb to understand you, it may mean that you yourself are not presenting your ideas clearly.

DS: What other memories do you have of graduate school? You have talked about your research.

WKH: There was no planetary science department in those days, so I enrolled in a Ph.D. in astronomy. The astronomy department was just across the street from the lunar lab. A professor in the Geology Department, Spence Titley, was active in the new planetary mapping programs of the U.S. Geological Survey, and volunteered a lot of his own time to take Dale and Al and me under his wing and give us some crash courses in basic mineralogy and petrology. As a result, I got my MS in geology in 1965 and then my Ph.D. in 1966 in astronomy. When it was time for me to defend my thesis, I got a strong grilling, particularly from a geochemist who said that if my work on impact cratering rates was true, why weren't there craters all over the Earth. I think he knew full well, but was being a devil's advocate. I answered something about how "The Earth is so geologically active, and the craters get obliterated." At that time, we were talking merely about "mountain building," fluvial erosion, etc. This was before it was understood how profoundly Earth has been resurfaced by plate tectonics. But all those studies of craters and cratering rates laid a lot of groundwork for my later work in helping to develop basic concepts of crater chronometry.

DS: You were in astronomy. Were the others in astronomy?

WKH: No. Al Binder had always been interested in the geology of Mars, and Dale was working for Kuiper building spectrometers to study planetary surface compositions, and so they ended up in Geology. Still, we attended a lot of lectures together.

DS: Were you still in graduate school when the building opened? You remember all the construction work?

WKH: Yes, that is what I remember. Certainly by the time I was a fresh minted Ph.D. and an assistant professor, we were in the new building and I taught a class on the solar system over in the astronomy department. Kuiper assigned Pat (Elizabeth J.) Roemer, a comet expert, and me to rough out a curriculum for a planetary science Ph.D. degree. Pat was chair and I was a member of the committee. We did the initial work on that. Kuiper was not terribly gracious toward the geology department and, in particular, Spencer Titley. Kuiper believed that planetary science came from the astronomy done at big observatories, and not from geology.

Kuiper had a passion for big observatories and building spectrometers. He loved taking spectrometers to McDonald Observatory in Texas, for instance. Dale Cruikshank's assistantship work was in the spectroscopy laboratory. In a lot of ways, he had a tougher time than I did, because he had to build something that had to work next Tuesday, whereas my job was to go in the dark room and print pictures for the atlas, and measure the diameters of craters.

BEGINNINGS OF A POSTDOCTORAL CAREER AT TUCSON

DS: You completed your Ph.D. in 1966. What do you remember from your early post-Ph.D. years?

WKH: The lunar lab under Kuiper and David Arthur published the catalog of the craters on the Moon and I thought it would be fun to look at the size distribution of those craters. The only literature data I could find on size distributions of lunar craters were a couple of obscure articles in the journal of the BAA where one of the English observers had tabulated the bigger craters and plotted size distributions. It was an important subject because there was still this dispute about whether the craters were formed by impact or volcanism. There was beginning to be data on the size distribution of the asteroids, based on statistics for ground-based observations of the larger asteroids. The crater size distribution seemed to match what would be produced if asteroids hit the Moon, which helped cement the idea that craters were impact features.

So, here was young Bill about to complete his Ph.D. beginning to plot size distributions of craters; I already mentioned that Ranger 7 had given us crater sizes down to very small sizes. So, now Mariner 4 flies by Mars in 1965. A character that I heard much about but never met was Ernst J. Öpik. He was in Ireland and published out of the Armagh Observatory and to my mind he was always about 20 yr ahead in his thinking. He published an article in the Armagh Observatory journal in 1965, and then I think in *Science*, in 1966, only a few months after Mariner 4 flew by Mars—he could write something and get it into Armagh Observatory journal in a month or two—and pointed out that the size distribution was flatter than the one on the Moon. In other words, if you go to half the crater size for the Moon, the frequency goes up by a factor of four, but on Mars, it would be only a factor of two. Then, he stated that if there was a continuous process like deposition of dust filling up the craters that would reduce the slope by unity—as observed on Mars. A number of us wondered where he got that. Clark Chapman, working with Jim Pollack and Carl Sagan, was first to publish a derivation of what I call now the Őpik effect. They put out an SAO paper in 1968 and then an *Astronomical Journal* paper in 1969.

I had many conversations with Clark—he came out to Arizona for a semester or two of study in Kuiper's lab. I wrote a paper for *Icarus* in 1971 revisiting the derivation and the consequences. The slope on the size distribution is giving us a clue as to the erosional climate on that planet. Think how lucky we are! Nature is going around with a cookie cutter making these circular holes in the ground at a specific rate. The number of craters per km^2 gives us a chance to estimate how long that surface was exposed. On the Moon, that's the end of the story. But on Mars, where there are dust storms and lava flows and ice-rich mantling that covers craters, and water-flow or ice effects erode them, the older, smaller craters will be missing and the slope will change, and we can measure this evidence for an active surface. It's still not fully appreciated that the Őpik effect is a very powerful tool.

I got interested in all this at about the time I got my degree. Kuiper had edited a volume from the University of Chicago Press not long after he got to Arizona. One of the articles concerned the newly discovered impact craters in Canada. So, the year before I got my degree, I published a 1965 paper in *Icarus,* taking the new results from Canada. I took the lunar lava plains' crater size distribution and divided by the rate at which big craters were formed in Canada and got a number of 3.6 Gyr for the age of the lava plains on the Moon. Which turned out about as good as you could get with only two significant figures.

DS: You got the 3.6 Gyr age for the Maria before the return of the Apollo samples?

WKH: Yes, to do any better you needed samples. That was a very interesting period because Kuiper thought that the lunar plains' age was of that order of magnitude. Now, we were on the run-up to the Moon landings. Shoemaker was very influential and he was privy to some defense department data concerning shock waves in the atmosphere. Everybody was worried about nuclear tests, which was why they had these detectors. If there is an explosion over the Pacific, nobody would see the explosion but you can easily detect the shock waves. Well, it turns out that the observed shock waves were mostly caused by meteoroids. From the shock wave energy, you can get the size of the meteoroid. This was pioneering stuff, and—uh oh!—the calibration from shock wave strength to meteorite size in those days was way off. Shoemaker used those figures and was going round giving talks—hardly anyone remembers this now—and he said the new information indicated that the lava plains were only a few hundred million years old. This seemed very revolutionary to everybody, but he carried so much weight that that number started to circulate. My number of 3.6 Gyr was put on the back shelf.

That was the environment in which the first rock sample ages were reported. Wasserburg's lab was one of the best known labs, and Gerry is a wonderful flamboyant personality with a great sense of humor. So, he came to campus one day, shortly after Apollo 11 and 12. He'd just finished dating the rocks, and gave a big public talk. He looked down his nose at the crater counts, because ages from crater counts might be a factor of two off, whereas those guys could measure ages to several significant figures. So, here's young Dr. Bill sitting in the audience when Gerry proclaimed, as I remember, "Now you can flush crater count ages down the toilet." I was mortified. They got 3.0–3.5 Gyr for the samples so, thinking of Shoemaker's ages, he thought the guys doing crater counts did not know what they were doing. Of course, 40 years later, we do not have rocks from every planetary surface, so if we can just calibrate the crater count dating with samples from the Moon or, better yet, Mars, we could really get quite precise calibration for the crater densities for surfaces of various ages. Then, you could really date all the formations on a given planet without any more samples. Of course, you need the samples for chemical and petrologic reasons. But, the idea of craters as a profoundly new geological tool is something I've been interested in, ever since that period, 1966–1972.

Then, in 1971–1972, I found myself on the Mariner 9 imaging team.

DS: Mars?

WKH: Right. If we can just calibrate a given crater density on Mars with an age, we can add a third dimension—time—to all the two-dimensional mapping of Martian geologic features. The calibration at the moment is based on taking the cratering rate for the Moon and applying it to Mars. But, the greatest uncertainty—at least in my mind—is the ratio of the cratering rate on the Moon to the cratering rate on Mars. If we knew that ratio, we could do so much better, although there is still uncertainty of about a factor two in that impact ratio, and maybe even a factor four when all the gravity and velocity scaling laws are applied to get the Martian crater diameters. That sounds awful until you realize the crater densities on Mars range over four orders of magnitude. It has enormous geophysical value to say whether volcanic, fluvial, or glacial activity on Mars is 10^6 yr or 4×10^9 yr old.

What would be really beautiful would be to land three missions on Mars, at a young, intermediate, and old site, that were well preserved, and get the age of those. Even with remote instrumentation, we could put radiometric dating instruments on Mars, and even though the uncertainties are quite large, that would mean a lot of progress. Then, we would have the curve that would allow the dating of any surface on Mars. However, I would like to stress that if there is one legacy I want to leave behind, it is crater counting is not just a method for dating, but also for looking at erosion on the surfaces of planets.

DS: Throughout this period, you are on the faculty at Arizona?

A NEW CAREER

WKH: Yes and no. I was an assistant professor for a few years after my 1966 Ph.D. Then, one day Kuiper disappointed me. Everyone said that young Ph.Ds should move on and get experience somewhere else. So, Kuiper had this fatherly chat with me. At the time I thought he was trying to get rid of me, but, looking back, I had such a good relationship with him that I don't think that was the case. He just thought I needed to be independent. Which was true! I had no idea where the money was coming from, or how to generate funding.

Just at that time, my phone rings one day and it is Bruce Murray, later head of the Jet Propulsion Laboratory. They were about to launch, or had just launched, these two probes to Mars, Mariner 8 and 9. Would I like to be on the camera team? Well, gosh yes, I guess I would! I feel so lucky about the timing of my career because there was our group of fresh-minted planetary scientists—Dale, Al, Clark, and me—with careers in planetary science and there were just not very many others—probably no more than a dozen names in the whole country.

DS: This is one of the things that interests me, how did we evolve from the telescope age into the space age? How did planetary astronomers with telescopes become planetary scientists with spacecraft? There cannot be very many subjects that changed their character so profoundly overnight.

WKH: Yes, I often say that planetary science morphed from astronomy to geology in my lifetime.

DS: Mariner 4 returned 11 images, yet look at the effect it had. Suddenly, Mars looked like the Moon; quite an eye opener when all you had previously was telescope images.

WHK: Mariner 4 was the nadir of our perception of Mars. I have a book on this, *The Traveler's Guide to Mars*, which has just come out in a Spanish edition! To go back to Lowell's day, there were civilizations, then the estimates of air on Mars got thinner and thinner, and Kuiper came in with his infrared spectrometer in the 1950s and 1960s, and the Martian air pressure was down to 100 mb. Toby Owen was a year or two ahead of us in Kuiper's laboratory—he was the graduate student we all looked up to—and he was running experiments where they had pipes tens of meters long running the length of the physics building. They filled the pipes with CO_2 and other gases, at various pressures, which produced spectra they could compare with Mars, and soon the Martian air pressure was down to 10 mb. After Mariner 4, Mars seemed much like the Moon, with just a little bit of air to blow the dust around. Then, suddenly Mariner 6 and 7 sent back a Mars with strange chaotic terrain, and then Mariner 9, which I was involved with, discovered dry river beds all over the place, giant volcanoes, and the great rift canyon (Fig. 4).

DS: You were talking about Kuiper encouraging you to go elsewhere.

WHK: Yes, right. The linkage in that chain is that Murray called; I am now on the Mariner 9 team. I have a budget. It is worth reflecting that all the money in Kuiper's lab came from Kuiper himself. It was the European model; the great professor ran his lab. He talked about "big science" and he made frequent trips to Washington, D.C. There were never any great Champaigne events; new grants came in quietly and we just continued our work. We just knew the money was always going to be there. It was the 1960s, the golden age of planetary science; money flowed to science like water flowed downhill. We were going to the Moon and the nation would finance everything.

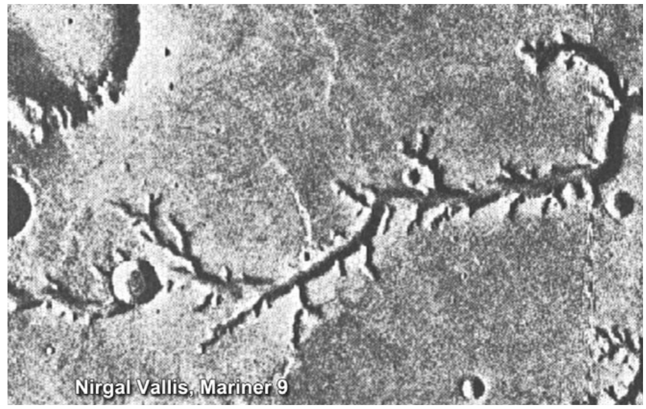

Fig. 4. Mariner 9 photograph of a Martian channel, Nirgal Vallis. In 1971–1972, Bill was a co-I on the Mariner 9 mission, which first mapped Mars in detail. With Bruce Murray, Carl Sagan, and others on the imaging team, he discovered Mars' dry river channels, volcanoes, and other features.

But now, I had a small Mariner 9 grant of my own and that made me attractive to other groups. Immediately, I got a call from IITRI. (The acronym stood for Illinois Institute of Technology Research Institute, although by then they had broken away from the Illinois Institute of Technology). Toby Owen had gone there, and Al Binder had gone there, and Al had dreamed of coming back to Tucson, and talked them into opening an IITRI office in Tucson. So, I was hired into that office. That was the DNA of what is now the Planetary Science Institute. That was 1969–1970. We were active as IITRI for several years, but we realized the overhead was getting bigger and bigger. I now recognize that as a common pattern. As grants come in, the administration gets bigger, then as grants are lost, scientists disappear, but administration goes on and on, and needs bigger overheads. There is a tendency for the ratio of administrators to worker-bees to go up.

We decided this was getting out of hand, and thought of creating a new institute, so we pulled out, a little at a time. I was the first and I pulled out in 1971, just as Mariner 9 was approaching Mars. My wife, Gayle, and I went over to Pasadena because no one had a way to distribute digital pictures; all the scientists had to gather in Pasadena where the images were. When we got back, we opened up the "Planetary Science Institute" in our living room. The rest of the group pulled out of IITRI and we created the institute on Groundhog Day in 1972. That's the story of my professional emergence from Kuiper's nourishing cocoon. The Planetary Science Department at The University of Arizona came into existence about a year after I ended my faculty position there. When our new institute started, there were exciting times ahead. There were the missions to Mars and we had a critical mass of people in our group. Our philosophy was to bring in people who overlapped with existing people in interests but also brought something new. This is a little different from most university departments who have to cover discrete teaching areas. Don (Donald R.) Davis, came in, Clark Chapman, Stu (Stuart J.) Weidenschilling, and others.

DS: When did Binder leave?

WKH: He was in for just a few years. It was his vision to have such an office, but he was less happy as it evolved and more people came in. He was interested in Germany and worked there many years.

DS: Chuck Wood?

WKH: Chuck was a couple of years later. He was an undergraduate when I was a graduate student. He and I wrote a big 1971 paper on lunar basins. We did crater counts for about 25 basins, and ranked them by age. I'm astonished that some people think Serenitatis is younger than Nectaris, since we found lower crater densities and younger morphology on the Necataris multiring structure. I still think the paper is important. Chuck has had a very interesting career. He was in the Peace Corps working in Kenya, then did additional work in Ethiopa, with a geophysical observatory there, and then went on to pioneer web-based education techniques. He still has a connection with PSI.

DS: You had five or six scientists?

WKH: In the first years, a group of five, six, seven scientists.

DS: You have an administrator?

WKH: Our original administrator was a wonderful guy called Dave Roberts. He had been the division head in IITRI and I learned a lot from him. He made it his job to take us fledgling Ph.Ds to Washington, D.C. and talk to the managers of the programs there, which, of course, were not as formally organized as it is now. We had lots of meetings. We served on lots of committees. Dave helped us learn how to write proposals.

DS: He got his salary from the grant overheads.

WKH: Yes. The first iteration of the PSI was actually a division of a new company, which at that time was Science Applications Incorporated, which was formed by an interesting physicist named Bob Beyster. My employee number was 424, but now the numbers are in hundreds of thousands! Beyster told us he had been in some parent company, but the overheads got too big, so he pulled his division out. Then, he started getting phone calls from others that wanted to join his outfit, so he had cancer groups, space groups, and so on. Science Applications Incorporated became very big in Washington, D.C. and does lots of government work. A problem for our country is that the government announces a big budget cut, so the agencies cut their staff but hire a private outfit across the street, and many of their people just move across the street, but they are technically no longer government employees. So, the government gets its cut in size, but they farm out the work, and the company owners get rich. OK, that's a sarcastic way to look at it, but not without truth.

Meanwhile, PSI and SAI went through the usual history, in terms of the growth of management. We became a smaller and smaller part of the action and we got less and less access to Beyster, so we finally pulled out. We joined an interesting third group for a while, which was the San Juan Research Institute, started by Doug Nash, from JPL. We became the Tucson branch. It worked well until Doug retired and we were becoming the dominant office. So, we became the independent, nonprofit Planetary Science Institute (PSI) that we have today.

DS: How does a relatively small private research institute compare with, say, a university or a government laboratory?

WKH: Well, with our current director, Mark Sykes, we embarked on a rapid expansion, with around 100 people now, many in Tucson, but many scattered around the country and in other countries.

DS: What is the difference in a career with PSI and a career with a university or as a NASA civil servant?

WKH: That's a really good question. We were always determined to be an institute organized by scientists, run by scientists, for the benefit of scientists—in the words of Don Davis. Historically, we avoided corporate formality. Being a small group, initially we all knew each other. There were no rules about coming in at nine and going home at five. As long as the science gets done, that's all that matters. As I used to say, the only thing that counts is what goes out the door. If we are doing good research and getting it published, we don't try to constrain how our researchers do it.

Another thing we did in the 1970s emerged from the partial overlap in our expertise. We had Don Davis, Rick Greenberg; they are both celestial mechanics people; Clark was known particularly for asteroids; I was known for cratering and impact experiments—I was doing impact experiments in the 1970s and 1980s—and Stu Weidenschilling who was interested in the formation of planets, so, as a team, we put together some of the first numerical modeling programs to work on the growth of planets. You put into the computer 1000 bodies, 1 km across, start them moving randomly, let them collide and merge.

The initial collisions happened at low velocity. I went to the NASA Ames Vertical Gun to learn what happened when particles collide at 100 m s^{-1}. This is another example of what I talked about at the beginning, working in a direction that was not the direction everyone else was going. Everybody else was trying to study high velocity collisions. People like Don Gault were trying to simulate impacts at 5 km s^{-1}, nominal for the asteroid belt. But, we were talking about the beginning of the solar system and objects on circular orbits, so we were talking about low relative velocities. Low speed impacts. So, at the vertical gun, the issue was how to reduce the velocity to 100 m s^{-1} or less. In the end, we also rigged up a device simply to drop the projectiles in the vacuum chamber, onto the target at a few meters per second. That information was plugged into our new numerical model.

Here's another curiosity for you. Don Gault had commissioned people to make these beautiful basalt spheres for his impactors or targets. Why spheres? So you can get repeatable results! But, nature doesn't deal with spherical planetesimals or spherical cows! So, I went outside the lab and picked up natural rocks. What you find is that if you use perfect polished spheres, they bounce almost up to the height you drop them. But, if you take a natural rock, it may strike on a corner and a lot of the energy is lost to rotation, so they bounce off more slowly. I came away feeling that the quest for repeatability in science—spherical cows—can actually reduce understanding. One of my first results was that natural samples have such low rebound velocities; it is much easier for mutual gravity to produce accretion. Another thing I found was that if you have a thin layer of dust, with a thickness of, say, 10% of the projectile, it kills almost any rebound. The act of moving around all that dust absorbs a lot of energy. So, a fragmental layer of thickness T promotes capture of bodies of size <T. As soon as you attract any granular material on the surface, impactors fall back. My line was that "regolith begets regolith." Regolith helps capture more impactors, which may have been a key to the earliest planetesimal accretion (Fig. 5). So, I claim that "natural experiments" may be more informative than idealized experiments.

DS: One major difference in the PSI work is that in universities, you have lots of students and you get a teaching load.

WKH: Yes, I should have said that first. You have no teaching or faculty committees, but on the other hand, you have to keep yourself funded. It's all soft money. Our funding has always been mostly from NASA, but at times, we get astronomy grants from NSF. There's also the private sector. Raytheon in Tucson has a large space exploration division and we have occasional collaborations with them. They often go after big grants, the buzz word is "architecture," they develop mission "architecture," and we've tried to get a small part of that work.

DS: But, what I have heard you say is that you do not want those kinds of project.

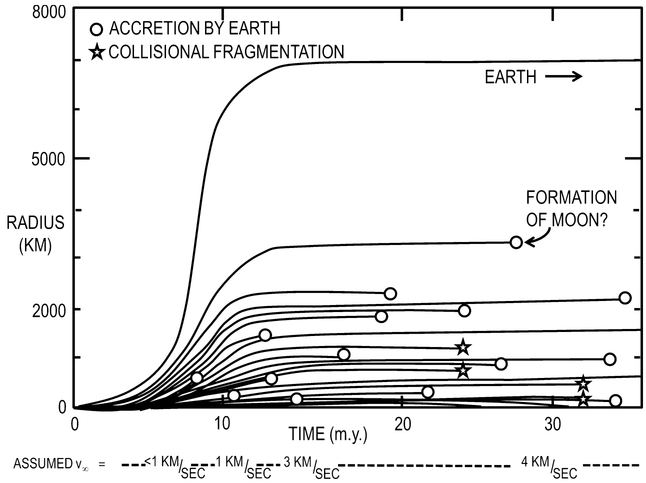

Fig. 5. Diagram from Hartmann and Davis (1975) showing schematic history of growth of planetesimals near Earth's orbit. The authors suggested that the second-largest body grew to large size before hitting Earth, ejecting mantle material that formed the Moon.

WKH: I think that an advantage of a nonprofit is that we can pick any topic that we think contributes something to society, as opposed to something that generates profits for short-term investors. So, it's a question of whether the project is something we really like to work on.

DS: Funding drives universities too. It really homogenizes the system.

WKH: National peer review tends to homogenize everyone's research too. But, I do think that the ordinary day-to-day working environment in the PSI is very positive, and I hope there will be a growing role for nonprofits in our society. It's something I've written about in what I hoped would be my third published novel. One editor accepted it, but then the deal fell through.

DS: You mentioned Don Davis, how did you and he come up with the idea that the Moon was made by a giant impact on Earth? What is the history of that topic?

WKH: I was much influenced by the discovery of the Orientale basin on the Moon and also by reading Safronov's ideas about competitive accretion of planets, in which not only a single planet grows but also a second-largest and third-largest body, and so on. So by the late 1960s, I wondered: what are the largest bodies that ever hit in the Earth–Moon system? If a large enough impactor hit an Earth where most of the iron was in a core, wouldn't it eject only iron-poor upper mantle material, and couldn't that explain the iron-poor lower density of the Moon? As PSI formed, Don Davis was a dynamicist who had had just come from helping to save Apollo 13. He was starting to do models of planet growth, so we agreed to look at the problem of how big could the second-largest body grow in Earth's zone. If it hit Earth, could it blow off enough material to form the Moon? Don ran our then-emerging PSI models of planet growth, to see if a second largest body could grow to big enough sizes. We found second largest bodies might grow as large as Mars before hitting Earth. But, it was a stochastic matter. The second-largest body might not survive for that long or grow that big, so not all planets would have an Earth-like moon, of appreciable size relative to its planet.

In 1974, I presented our results at a Cornell meeting about satellites, and in 1975, our paper came out in *Icarus*. At Cornell, after my talk, Al Cameron commented that he and Bill Ward were working on a similar idea, but basing it on angular momentum constraints, and they concluded a Mars-sized body was needed. They published their results in an abstract in 1976.

These ideas about the origin of the Moon have held. Looking back, I realize that in 1975, after we published the first paper on the modern giant impact idea, my inclination was to move on to something new. But now, I sense that I should probably have gone to 10 more meetings and promoted the idea, because the idea faded, since making the Moon by a violent collision was too catastrophist for the 1970s. Earth-scientists had been properly taught Lyell's dictum that geologic evolution was mostly uniformitarian, and Earth-scientists had been burned by the Velikovsky affair in the 1950s, so catastrophes were frowned upon. It wasn't until 1984, at the famous Kona, Hawaii, conference on lunar origin that John Wood ranked our idea as the leading hypothesis of lunar origin. It became the basis for new work by Canup, Cameron, Ward—new numerical models of giant impact.

SHOT-GUN-WEDDINGS AND CRATER CHRONOMETRY

DS: Returning to our chronology, what else was going on in the 1980s?

WKH: Even before that, Tom Gehrels was beginning to put together conferences that would result in a book that began the Arizona Space Science Series. Brilliantly, he favored shot-gun-wedding papers. He would organize the meeting and then identify areas with a big problem and he'd find researchers who had been disagreeing for 5 or 10 yr, and force them to come to the meeting—it was their subject, so they had to come—and then they would have to write a paper together. It was actually very successful. I had this experience later, with Gerhard Neukum. He is a German crater count expert who had been a critic of my crater chronometry efforts. At meetings, there was always a bit of tension. But, at this International Space Science Institute (ISSI) in Bern in the late 1990s, we were on a Mars project together and the wonderful director of ISSI, Johannes Geiss, forced us to write a joint paper. We were actually later jointly given a medal by the European Geophysical Union for our work. I sensed it was because of that paper we wrote together—but I hope it was also because the paper was good! I'm happy to say that I feel like we're friends and colleagues, even if we differed on some interpretations. I've served on Gerhard's camera team on Mars Express. Scientists need to be role models in that regard!

DS: But the differences were in the details, weren't they, you were both basically doing the same thing?

WKH: Yes, that's right. I don't think we were ever that far apart. It was really a collision of philosophies. Gerhard is a wonderfully typical German—I can make fun of Germans because I have two Ns on my name and my father's mother was German. German in that Gerhard has been very systematic, very orderly, step N followed step M, step M followed step L—all the way back to step A. Therefore, the answer must be right. But, that assumes every step is precisely right. It doesn't allow for what was overlooked. What was outside the box? My scientific personality is at the other pole. I think every reported "scientific fact" comes with an uncertainty. Crater densities and possible ages range over 3 or 4 orders of magnitude, especially on Mars, so I'm comfortable to cite uncertainties as big as a factor 2 or 4, but Gerhard is inclined to think the uncertainty is much less.

DS: Was it just personalities? There was this long-term competitiveness, but it was based on differences in style and not substance? Is that what you are saying?

WKH: I think that's the right way to think about it, and it's scientifically interesting. Gerhard and his group might report an age—I would call it a model age—of 135 Myr, with three significant figures. I might get an answer of 2×10^8 yr within a factor of two, and I might regard the two data sets as within some sort of agreement, given an initial unknown range from perhaps 10^6 to some 10^9 yr. But, Gerhard was uncomfortable with that. I felt that 2×10^8 yr an advance in our knowledge, but Gerhard kept saying the best answer was 135 Myr.

DS: There is also a difference in application. As I recall, some of his papers deal with very small areas on Mars, say, and small differences between regions. He really needed relative ages more than absolute ages.

WKH: We have both worked on small areas, especially with new images that can resolve craters down to 1 or 2 m. We both wanted absolute ages, but the smaller the area, the less reliable they are, because you have few craters to count. Nonetheless, Gerhard had a much bigger, more sophisticated lab than I ever had. He had a big team and stereographic facilities, and so on, so I do think he had greater internal precision in his methods than mine. But, I felt that such a level of effort, or claims of absolute precision, were not warranted, given the factor 2–4 uncertainties in converting the crater chronometry system to Mars.

DS: How did you resolve your differences?

WKH: That was an interesting process. Johannes Geiss at ISSI assigned us to write those papers in the ISSI book. The first would start with the basic background: how did we do the basic lunar counts. Gerhard chose to be the first author on that. That made me first author on the Mars paper. Gerhard very much wanted to have a single calibration curve that would be what he called a "final answer," but I kept saying that I thought we should present both of our two calibration curves because we had spent 20 yr working on those curves independently and the rough agreement of our two curves gives the rest of the community a sense of how robust the work is. It was very interesting and enlightening to work on that together.

Mariner 9 was where I did the first serious Martian cratering studies. I started with the Moon, where we had rocks and could compare our ages with isotope ages. But on Mars, we did not know the cratering rate, so we ended up taking the lunar curve and then estimating the relative rate of cratering on Mars compared with the Moon. So, in the early 1970s, on Mariner 9, I did a very crude calibration for Mars. I got few-billion-year ages for the heavily cratered regions, but I kept coming up with ages of a few hundred million years for the sparsely cratered Martian formations, like the big volcanoes. That's when Gerhard was starting. He and Don (Donald U.) Wise did a 1976 paper where they said those last ages were too young, and that volcanism had ended during Mars's early history (Neukum and Wise 1976). But, by the time we did our 2001 joint paper for ISSI, we agreed on few-hundred-million-year ages for the young volcanic and other features on Mars, even for some of the fluvial features. We think there is a limit of about 3.9 Gyr we cannot see beyond, but that brings up the issue of the terminal cataclysm—which, according to me, is a terrible can of worms.

I should mention that at this point, in our narrative, I am reminded of a comment—I think it is attributed to Rutherford—that scientists over 50 are a danger to science because they spend the rest of their lives defending their youthful work. And I'm way over 50!

DS: I have four more topics I would like to bring up at this point. Your perception of the history of research on the late heavy bombardment, the asteroid and comet work you have done, then I want to talk about your work as an artist, and then finally I have a surprise question.

WKH: Then I'll be so worried about the surprise question that I can't focus on the other questions!

DS: Let's start with getting you to think out loud about the history of—and the reality of—the late heavy bombardment.

THE LATE HEAVY BOMBARDMENT THAT WASN'T

WKH: Well, that's certainly an important, interesting subject for all of planetary science! I feel I'm out on a limb, relative to our community, but here goes. What I remember, from living through it, is that Harold Urey and others of his caliber were selling the lunar landings partly on the grounds that the whole history of planetary evolution would be lying there on the lunar surface. There was supposedly no weathering. So we'd get the history all the way back to 4.5 Gyr ago. There would be "genesis rocks," and the astronauts were trained to find them.

Fast forward to 1969–1970. The samples come back from the Moon. Gerry (Gerald J.) Wasserburg's lab and other labs get dates. Big surprise! There were very few rocks older than 4 Gyr. But, radiometric dates were good to three or four significant figures and that's when Gerry said we should flush crater counts down the toilet.

DS: He probably just meant they got better ages.

WKH: Yes, they certainly did! Three significant figures versus an order of magnitude! But they made an assumption that if you can't find ages >4.0 Gyr, then some global cataclysm happened at that time. The more I thought about it the more I realized there was a flaw in the logic. Suppose there's some process from 4.5 to 4.0 Gyr that tended to reset older ages or destroy surface rocks. Then, it's conceivable that we begin to see rock ages only after the end of that process—without any "terminal cataclysm." That's a uniformitarian model. The process could be early intense cratering.

As an analogy, suppose aliens come to Earth and they find lots of humans who date back to about 1920, so they conclude a huge cataclysm occurred at 1920–1925, which wiped out all earlier humans. Is that reasonable? No.

Nonetheless, the idea of a cataclysmic episode of cratering at 4.0 Gyr got a big boost in 1990 when Graham Ryder wrote an EOS paper specifically about impact melt rocks in the Apollo sample collections. Graham published an article showing an enormous spike in impact melt ages at 3.9 Gyr ago, with virtually nothing older. His conclusion was that there were no big impacts before 3.8 Gyr. This is what I call the "strong form" of the Graham Ryder argument. It fitted well with Wasserburg's hypothesis, but it's the logical equivalent of concluding there were no humans before 1920.

The next phase of this was the "Nice model." Our colleagues at the Observatoire de Nice came up with the suggestion that the giant planets were moving around and creating resonances that scattered outer solar system asteroids all over the solar system. They were very up-front and said that they did not have a way of attaching ages to their process, but if they picked 3.9 Gyr for onset of their process, then it would be consistent with the Wasserburg-Ryder hypotheses.

There is another parallel thread in the story. The interpreters of lunar rock radiometric ages interpreted their results within the emerging Wasserburg-Ryder-Nice paradigm, and concluded that not only the Imbrium basin had been well dated (around 3.85 Gyr ago) from the Apollo samples, but that many other basins such as Nectaris, Serenitatis, and Crisium, could be dated from scattered rock fragments. Dieter Stöffler and Graham Ryder summarized the topic in the 2001 book we did for ISSI—also published in *Space Science Reviews*. They stated quite strongly that we know the ages of several impact basins on the Moon—Imbrium, Necatris, Serenitatis, Crisium—and they were all about 3.85–4.0 Gyr old. Thus, the cataclysm was confirmed! Today, the dominant paradigm is that science has proven that virtually all multiring basins formed within a 150 Myr interval!

This makes me very nervous. We land at site A and pick up a few rocks and then attribute any anomalous ages to another basin in location B. There were geochemical arguments but I found the whole idea questionable. In a 1973 *Icarus* paper, I think I coined the term "megaregolith." If you look at the cratering in the highlands, it is close to saturation. If you plot crater size distributions, you can calculate the area covered by craters in each size bin, and identify a critical crater diameter D_{critical}, and depth d_{crtical}, where 100% of the area is covered by craters larger and deeper than that size. We can continue that calculation and go down to 200% covered, to take into account crater overlap. You can use those numbers to give an order of magnitude estimate of the size and depth at which 100% and 200% of the area has been covered. Then, you can use the depth of those craters to estimate the depth of the surface that has been ground up, assuming all of the ejecta is falling back onto the surface. If you go to the lunar highlands, the highlands reach 100% and 200% coverage on the 10–20 km scales, you end up concluding that the megaregolith should be a few kilometers deep. My 1973 paper proposed a depth of at least approximately 2 km; the GRAIL mission team recently published that they found a layer of high porosity and low density "a few kilometers" deep.

A provocative curiosity is that as the crater density increases throughout the history for the Moon, the size distribution of craters at multi-kilometer sizes hits the saturation line at all sizes simultaneously. The bottom line there is before 3.9–4.0 Gyr ago, there should have been an explosive production of regolith. At the time I was working on this, the Watergate incident occurred and the buzzword *du jour* became "stonewalling," so in a 1975 paper, I used the term "stonewall effect" to express that rocks ejected onto the surface by cratering before 4 Gyr ago were rapidly destroyed by impact erosion until about 3.9 Gyr ago, but rock ejected onto the surface after 3.8 Gyr ago have mostly survived. That's my critique of the terminal cataclysm idea—people are not taking into account the effects of megaregolith development.

I made some mistakes in presenting this. I'm not a radiometric dating person, so I used inappropriate language about "resetting dates" by impact. Gerry and his coworkers properly pointed that out, and the argument went on. Every 10 years or so, I remind people of my views. I did it after the Ryder paper. Graham and I had a scheduled debate at the Perth Meteoritical Society meeting, and I thought it was my chance to convince people about why the cataclysm paradigm was wrong. I completely lost that debate! I went skulking home with my tail between my legs.

But!... I think I can say today that the evidence claimed to support a terminal cataclysm is wildly inconsistent. For example, Barbara Cohen, Tim Swindle, and Dave Kring started a brilliant project about 15 years ago to look for little impact melt clasts in lunar meteorites. They got great radiometric age data on KREEP-poor lunar meteorites, selected to represent remote parts of the Moon, far from Imbrium and Apollo landing sites. Their 2000 paper in *Science* used a title "Support for the Lunar Cataclysm Hypothesis from Lunar Meteorite Impact Melt Ages" and they repeated that general tone in two later papers. But, their dozens of samples show no anomalous spike at 3.9 Gyr! I think their data are great, and I once ended a presentation with a Conclusion slide saying "Give Barbara Cohen more money," but we disagree on interpretations. We got into a wonderful game where every time they gave a talk I would stand up and ask, "Where was the 3.9 Gyr spike?" And Barb answers that the terminal cataclysm model is favored because they have virtually no pre-4.0 samples, invoking the Ryder idea the idea that "no impact melts = no impacts." I think the field was too influenced by Wasserburg–Ryder paradigm. In my view, all we know is that few samples *survive* from before 3.9 Gyr ago—but that doesn't prove that they never existed.

As I emphasized in my 2003 *MAPS* paper on the cataclysm, the asteroidal meteorites, as well as the lunar meteorites, show no sign of a Ryder-like spike at 3.85–4.0 Gyr ago. Instead, they show a very broad maximum in impact-related ages, from around 4.2 to around 3.4 Gyr ago.

DS: Let me ask you this, then. Counting craters on a photographic image versus obtaining ages with a mass spectrometer are two fundamentally different processes, so how do the practitioners talk to other? Jargon is an issue, like "resetting" means something very specific to the isotopers, but in general use, it can mean something different. You have an idea that is a minority idea, and you have carried it for several decades. What are your thoughts about carrying an idea you believe to be true in an environment where you are not convincing anybody and there is a danger that people have stopped listening?

WKH: That is well put. It is a dangerous thing to do. If you reach a certain stature, that I have not reached, you might get away with it. And planetary scientists have created a mutually supportive community, I think in part due to Carl Sagan's early influence. You still get invited to meetings even if your ideas are on the edge. I try not to get into it at every meeting, but if I sense the evidence for my idea is still accumulating, then I dare to bring it up every few years.

DS: There is an Anders quote to that effect, that with skill and imagination an idea you had in graduate school can take you through a career.

WKH: Particularly if it is right! There is a wonderful irony here. I suggested a stochastic, catastrophic origin for the origin of the Moon—but when it comes to the terminal cataclysm, everyone else is catastrophist and I'm the uniformitarian. But, hey, another part of the answer is that the elephant in the room: evidence favoring a terminal cataclysm is a complete mess. If there were no impacts between 4.4 and 4.0 Gyr ago, followed by a solar-system-wide cataclysm happened at 3.9 Gyr ago, why do lunar upland breccia impact melts indicate large impacts before 4.0, and why do data from lunar meteorites, Vesta, and other asteroids show no spike at 3.9?

DS: So your answer to my question is, keep a sense of humor, keep a low profile, and watch the data.

WKH: That's probably good advice for life in general! Yes, yes, yes. In my mind we are all in this together, there is no need for pounding on the table. I feel quite comfortable. I don't feel isolated. We need to keep our great community!

ASTEROIDS AND COMETS

DS: One of the papers you included in your most important 12 publications was with Tholen and Cruikshank on asteroids. Tell me about that.

WKH: Dale Cruikshank, Dave Tholen, Johann Degewij, and I did a lot of observations on Mauna Kea for a decade or so in the 1980s. I was between spacecraft missions, and I'd always been interested in asteroids and the relationship between asteroids and comets. This relates to something I said earlier, that if you spend a decade on a topic, and then move onto something different, you don't make the same impact as if you spend your whole career on a topic. So, it's a toss-up. Is it more productive to stay put or more interesting to move on to new ideas that excite you? Are you a dabbler or a dilettante if you move about too much?

What happened with the asteroid work was that my best graduate student friend, Dale, was at the University of Hawaii, so he had plenty of telescope time and we formed a pleasant and useful alliance. I could think up projects and handle the conceptual end, and do first drafts of papers, and he was the instrumentation expert and observer who could interpret our data, so we worked together on lots of issues. One example is the whole relationship between asteroids and comets.

At this time, I was writing textbooks—it's a wonderful way of getting a good overview of the field. What struck me in the 1970s was that the distinction between asteroids and comets was Victorian; a point of light means asteroid and a fuzzy thing means comet. Whole meetings, and divisions between sessions, were based on that archaic distinction. At the LPSC, you had a comet session and all the gas spectroscopists went to that session and you had an asteroids session where you find all the mineralogists. But, such a division breaks down as you think more about it (Fig. 6).

DS: I can't resist interjecting that meanwhile the meteorite people are at neither session; they might even be at another meeting.

WKH: Yes, you are right. Because of our training, experience, or instrumentation, we go to different sessions or even different conferences. Why aren't these people all together in one room?

DS: How did you and Dale and the others address these issues?

WKH: We started some of the first efforts to use large telescopes to observe very faint objects well beyond the asteroid belt: Trojans, Centaurs, receding comets as they lost their coma. Tom Gehrels had shown that the largest Trojan, 624 Hektor, had a huge amplitude light curve, greater than 2:1. In 1978, we published in *Icarus* the first simultaneous thermal infrared and visible light data to prove that the amplitude was due to elongated shape, not albedo differences as on Iapetus. We proceeded with light curve observations of Trojans and found that the Trojans

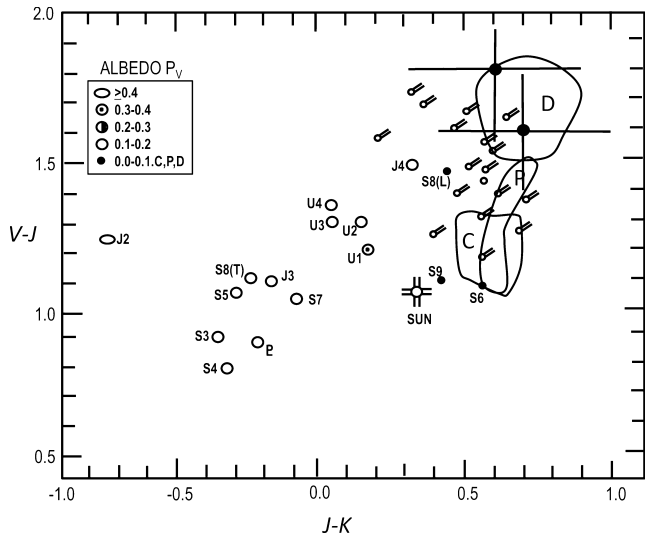

Fig. 6. Two-color diagram relating comets to the sequence of colors of icy bodies and asteroid taxonomy of outer solar system. From this diagram, the authors correctly predicted, in 1985, prior to Halley's comet's arrival, that its nucleus has an albedo of 0.04 and found that most of the outer solar system bodies are similarly dark (Cruikshank et al. 1985).

have more elongated objects than the main belt. Reason? Unknown. In one of my favorite of our papers (Hartmann et al. 1982), we used broad-band colors, so that we could observe crude spectra, from "V" visible light to the three "JHK" near-infrared bands, to study the faintest and most remote asteroids, as well as distant comets with little coma activity. Starting with brighter objects, we established a correlation between VJHK colors and spectra, hence composition. Ice-rich surfaces like Europa or Enceladus were bluer than the Sun, while dark, carbonaceous objects were neutral, and the dark, reddish outer solar system objects were redder than the Sun. A nice result was that as comet Halley was falling toward the inner solar system, when a review article in *Science* predicted a 28% albedo, we used our VJHK work to measure an albedo of 4% (Cruikshank et al. 1985). Dale reported it at a conference in Flagstaff and was roundly criticized. Everyone knew that you could not use colors to determine albedo. A few months later, Giotto measured the albedo. It was 4%.

PAINTING AS A FORM OF SCIENCE

DS: Let's talk about your artwork (Fig. 7). You have told me once that you did not like to be called an "artist."

WKH: My id likes to be called an artist, but my superego insists I am a painter who is interested in the interface between painting images and science. Actually, I think there is a close relation between the goals and dreams of artists and scientists. They start not with some motivation to get rich, but rather because they are interested in something, and hope to make some kind of contribution to civilization. I think the art–science division has something to do with left brain and right brain, the whole idea of where knowledge is coming from. I'm impressed by historic painters and modern painters that I know, because I think they have an intrinsic knowledge about nature that scientists don't think about—light reflecting off rocks onto other rocks; colors of the undersides of clouds; ambient light effects coming from the atmosphere; related effects of light on foliage, grasses, and rock powders, etc.

When I was in high school, I grew up thinking that scientists deal with 90% of natural phenomena, but now I feel that scientists deal with only 10 or 20% of phenomena, and that there are a lot of other things going on. My proof that natural phenomena can be understood in a visceral way by painters is demonstrated by the fact that Rayleigh scattering could (or should?) really be called Da Vinci Scattering. Da Vinci wrote an unfinished book about painting, in which he says there's a brick wall, and the tops of four church steeples show in the distance, but one is much farther away than the others. He says that a good painter should be able to show which one is farthest away. He explained that as the steeple gets farther away, there is not only less contrast but also the atmosphere adds blue light. The more atmosphere, the more blue light. So, he was aware in an *empirical* way of Rayleigh scattering. Now, we give credit to Rayleigh because he worked out the mechanism and the equations that can make quantitative predictions. But, the *bodily, visual awareness* of the phenomenon is what fascinates me. I see an increasing tendency at scientific meetings to assume that we don't have useful human knowledge until we have a "model," no matter how crude. Hence, the assumption in our society and in academic/journalistic circles that when discussing the planetary eco-economic system, traditional economic "models" trump other sources of understanding. From planetary economics to planetary regolith evolution, I'm convinced that there are so many unknown parameters that such models are sometimes premature, and perhaps dangerous to our understanding.

There is something that I call "body knowledge." My example involves tennis. You may have all sorts of knowledge about momentum, coefficients of restitution, etc., but that does not make you a good tennis player. There is some other experiential form of knowledge about tennis than the physical description of it. When I spend 3 days camping with friends in the desert, and

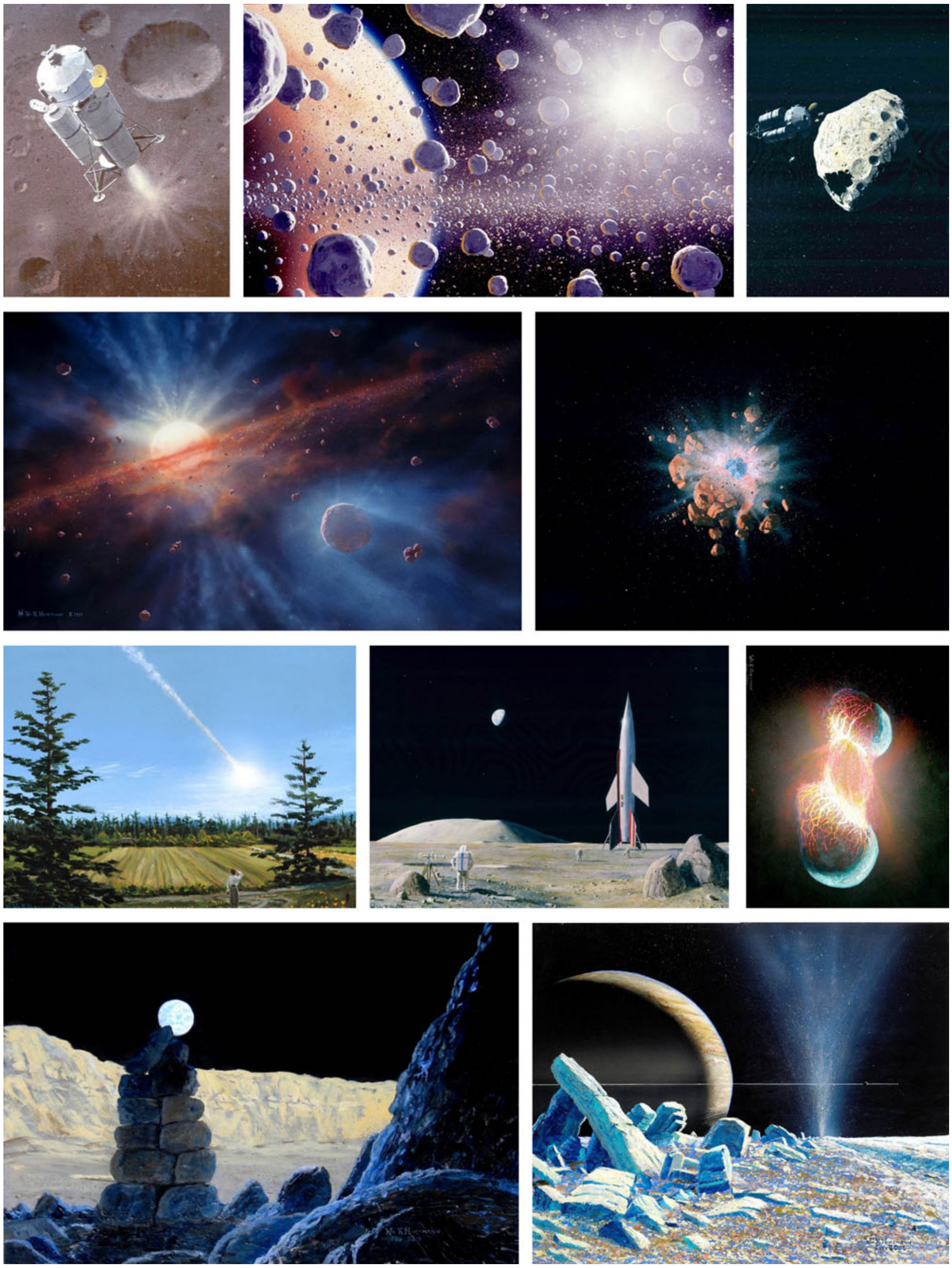

Fig. 7. A selection of Bill Hartmann's artwork in a chronological sequence. Left to right, top to bottom: Human exploration: leaving an asteroid (1981). In Saturn's rings (1982). First human visit to an asteroid, Earth–Moon in distance (1982). Formation of a terrestrial planet (1999). Collisions of a C-type asteroid with a D-type asteroid (1995). Tunguska fireball 1 s before explosion, from 400 km to SE (1995). Homage to Chesley Bonestell (his 1949 rocket on modern Moon) (1995). One hour after the giant impact, showing exchange of material between the bodies (2005). Someone was here (cairn on the Moon) (2014). Eruption in Enceladus (2006).

painting what I see, I get knowledge about deserts that you cannot get any other way. I think that the time I spent in the desert is informing my approach to Mars. I've been at Mars mission team meetings when people examine photographs and describe computer models of what is seen in the photos; meanwhile, I'm thinking, "I camped in a place that looked just like that 2 weeks ago."

DS: A lot of insights in science have been purely descriptive.

WKH: Einstein talked about this, too. A quantitative approach without experience can distort the true understanding of the relationships between things. It's better to have a descriptive understanding than to believe in a premature quantitative model. If the model does not predict anything, it is worthless. You have got to have those perceptive steps in there, during the development of the model.

DS: These sorts of things are talked about a lot in the philosophy of science literature.

WKH: Working scientists need to think about this, too. I'm not sure it is getting any better. There are folks who teach students to come up with a hypothesis and then go out and test it. That is not really the way it works. Read the 1925 book by Wolfgang Köhler, called *The Mentality of Apes*, with convincing descriptions of chimpanzees solving problems not by step-by-step experimentation, but by seeing relationships between the parts in a flash of insight. We see things, we sense a connection, and only after that do we make a hypothesis and then go out to chase it down.

DS: That's true, but science is a lot messier than this. The bottom line is that no known understanding is beyond amendment in the light of subsequent information.

WKH: I think our country suffers from a problem in the reporting of science, probably the whole world. The cliché is that each new finding "re-writes the textbook." This implies that each discovery undoes the earlier work, which fuels the talk-radio idea that scientists don't know what they are talking about. What's missing is that our discoveries add significant figures, but the bigger picture stays the same. So, it was that I went to the Grand Canyon National Park bookstore, to investigate a book they carry, which explains that Earth was formed 6000 years ago, and the canyon was carved during Noah's flood. Responding to my questions, a park bookstore attendant explained to me that she was happy that they carry a book explaining because it "presented the alternatives" to the scientific view.

DS: Mike Brown said that demoting Pluto is a good example to the public of how science works. We believed for years that it was a planet, but new facts made us think this is untrue.

WKH: Well, that's just a semantics issue. I am not very interested in that. Changing the descriptor word did not change Pluto's physical nature.

DS: For 80 years, scientists had one idea, but overturned it when new data came in. My last art question, and then we'll move on to something else. Why do you not put more figures in your paintings?

WKH: The short answer is that I'm not very good at it. I spent more time learning to paint landscapes than learning to paint people! I can't do portraits, but I like to put in very small figures sometimes. It reminds me of the early pictures of the frontier where painters used small figures to emphasize how big nature is. Moran and other 19th century American frontier painters put in tiny figures for the same reason.

DS: There is a Rockwell picture of the Gemini IV astronauts suiting up. It is typical Rockwell, but it is about the space age! That picture has always fascinated me. The space age has become part of the American self-image, like the patriarchs and farmsteads.

WKH: Rockwell had closets of costumes he would use with his models: pilgrims, revolutionary war soldiers, and so on. I have a story about Rockwell and Chesley Bonestell, my boyhood hero-artist, whom I befriended many years later, when he was in his eighties. During one visit, he pulled out the then-recent U.S. postage stamp with a Rockwell painting, showing Armstrong stepping out on the Moon, issued just after Apollo 11. Earth was illuminated from one direction, while Armstrong was lit from a different direction. Bonestell loved to point out logical problems in other artists' paintings, and he chortled that of course sunlight shining on Earth, as seen from the Moon, comes from the same direction as that light on the lunar foreground, and that Rockwell, of all people, should know this! Some people still do not get that the Sun is lighting up everything from a large distance, although the ancient Greek naturalist, Aristarchus, realized this and used the simple geometry of lunar and terrestrial illumination to deduce, correctly, that the Sun is much farther from Earth than the Moon is.

SOME FINAL QUESTIONS

DS: Okay, my final question. Put your own work aside and tell me what you think the five major advances in planetary science have been since the early 1950s.

WKH: Well, there is radiometric dating. That is so important, because it adds the time dimension to our human understanding. And, we can get so many kinds of chronologic information from different systems.

I think the concept of collisional accretion is important, I mean the sort of thing Safronov and Wetherill wrote about. Their work opened the door to occasional, stochastic catastrophic events. Impact cratering, craters on the Moon, the discovery of the Earth's craters and why they are rare, the Giant Impact hypothesis for the Moon—they all grow out of the Safronov–Wetherill work.

Something that has really impressed me—although it is too soon to be sure of its final importance—is the fact that the obliquity of Mars wanders all over the place and the related observation that Mars we see today is not the Mars of the past. Planetary environments are not necessarily stable! All those days spent at the telescope looking at the current Mars, or taking pictures from space probes, were never going to tell us that story.

The astronauts' "little blue dot" observation of the Earth, made possible by flights to the Moon, was profound—the perception that our planet is such a tiny, finite globe. I'm convinced that that image caused a pivotal change in the environmental movement. "Earth day" was started in the 1970s just after Apollo. Many American writers were writing environmental books well before that, but the planetary, ecospheric view did not gain any traction until the Apollo program. I still do not understand why our generation of economic theorists, at least those we hear on public radio, still assume that consumption is a measure of planet's economic health. They assume all jobs are equal; I would argue that jobs building solar panels are better than jobs burning coal.

Our "environment" today is not just nearby meadows, but the whole inner solar system. The idea that there are resources in space, and that humans have the capability to operate in space, will allow us to change things on our planet. Solar energy is available in space for 24 h a day. We can build giant collectors in space and beam pollution-free solar energy back to Earth's power grid. Asteroid resources of metals and other materials may allow Earth to relax back toward a less polluted state, and expand our space-based infrastructure. The sleeper issue is that we have to think about how space resources are used on Earth in a way so as to decrease the gulf between the rich and the poor. We cannot have this 16th century notion that whoever gets there first gets the resources. That model led to five centuries of continental-scale warfare. It terrifies me that the existing entrepreneurial paradigm promotes exactly that same model.

DS: What about the space treaty?

WKH: The space treaty is being picked apart! I have a book from Springer publishing, called *Who Owns the Moon?* It's fascinating. As early as page 48, it says that not all asteroids are celestial bodies! An interesting idea! The proposed, legalistic reason involves movable objects. Furthermore, the legalists and privateers (I use the word deliberately) point out that the space treaty talks about how celestial bodies cannot be owned by terrestrial government; they say this means they can be owned by private companies. A second treaty was the "Moon treaty" which was not signed by America and many other major space-faring nations, because it referred to space resources as a "common heritage of mankind." That phrase made some countries and most entrepreneurs see red. It limits their abilities to grasp and sell the resources.

And shouldn't we count the discovery of planetary systems around other stars as another crucial advance in human understanding of our cosmic environment?

DS: You hear it said a lot, that NASA spends a fortune on missions and puts a pittance into the Research and Analysis programs that mine the mission data.

WKH: I'm lucky and happy that for most of my career I've been funded out of those R and A programs. Spacecraft missions are of course crucial for our expansion into our space environment. But during years of financial crisis, there are plenty of cheaper discoveries to be made from the accumulated data!

DS: Well, Bill. We have reached the end of my questions. Do you have anything you want to add?

WKH: No, except that I hope I've not been too boring.

Acknowledgments and Notes—This interview was recorded on May the 14th and 17th 2013, in Tucson, Arizona, and edited by the author and WKH. I am grateful to NASA for financial support and to Dale Cruikshank and Hazel Sears for reviews and Hazel also for proofing. WKH acknowledges the freedom of expression that is allowed in the USA and by nonprofit research institutes.

Editorial Handling—Dr. Carle Pieters

SELECTED BIBLIOGRAPHY

Cruikshank D. P., Hartmann W. K., and Tholen D. J. 1985. Colour, albedo, and nucleus size of Halley's comet. *Nature* 315:122–124.

Hartmann W. K. 1965. Terrestrial and lunar flux of large meteorites in the last two billion years. *Icarus* 4:157–165.

Hartmann W. K. 1966a. Martian cratering. *Icarus* 5:565–576.

Hartmann W. K. 1966b. Early lunar cratering. *Icarus* 5:406–418.

Hartmann W. K. 1973a. Ancient lunar mega-regolith and subsurface structure. *Icarus* 18:634–636.

Hartmann W. K. 1973b. Martian cratering 4: Mariner 9 initial analysis of cratering chronology. *Journal of Geophysical Research* 78:4096–4116.

Hartmann W. K. 1975. Lunar "cataclysm": A misconception? *Icarus* 24:181–187.

Hartmann W. K. 1980. Dropping stones in magma oceans: effects of early lunar cratering. In *Proceedings of the Conference on the Lunar Highlands Crust*, Houston, Texas, November 14–16, 1979, edited by Papike J. and Merrill R. New York: Pergamon Press. pp. 155–171.

Hartmann W. K. 1997. *Cities of gold: A novel of the discovery of the Southwest*. New York: Forge.

Hartmann W. K. 2003. Megaregolith evolution and cratering cataclysm models—Lunar cataclysm as a misconception (28 years later). *Meteoritics & Planetary Science* 38:579–593.

Hartmann W. K. and Davis D. R. 1975. Satellite-sized planetesimals and lunar origin. *Icarus* 24:504–515.

Hartmann W. K. and Kuiper G. P. 1962. Concentric structures surrounding lunar basins. *Communications of the Lunar and Planetary Laboratory* 1:51–66.

Hartmann W. K. and Wood C. A. 1971. Origin and evolution of multi–ring basins. *The Moon* 3:3–78.

Hartmann W. K., Cruikshank D. P., and Degewij J. 1982. Remote comets and related bodies: VJHK colorimetry and surface materials. *Icarus* 52:377–407.

REFERENCES

Köhler W. 1925. *The mentality of apes*. 1917 Berlin: publishes as "Intelligenzprüfungen an Anthropoiden" Berlin. 1925 transl. from the 2nd German edition by Ella Winter. London: Kegan, Trench and New York: Harcourt, Brace and World. WKH's edition, Vintage Books (1959). Liveright 1976 reprint: ISBN 978-0871401083.

Neukum G. and Wise D. U. 1976. Mars—A standard crater curve and possible new time scale. *Science* 194:1381–1387.