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The Earthbound View
1961–1963

THE CHALLENGED (MAY – DECEMBER 1961)

The pace picked up dramatically after Kennedy's May 1961 challenge,[1] but the imbalance in the space race continued for several more years. The United States had managed to keep a few Explorers, Vanguards, and Pioneers from burning up or falling back to Earth, and in July 1961 Gus Grissom escaped from his sinking Mercury capsule after a short ballistic flight. But in August the first U.S. Ranger fell ignominiously back to Earth and the Russian cosmonaut Gherman Titov orbited the Earth 17 times in Vostok 2 — a risky mission apparently designed to distract attention from the debut of the Berlin Wall on 13 August.[2]

In the same month, the International Astronomical Union (IAU) turned the Moon upside up from the viewpoint of geologists and astronauts in anticipation of the new era. Previously, published illustrations of the Moon traditionally showed south at the top and north at the bottom, as it looks in an astronomical telescope, and the limb of the Moon's disk that is seen nearest to Earth's western horizon was considered the west. This is the *astronomical* convention. At their general assembly in Berkeley that August, IAU's Commission 16, The Moon, with Audouin Dollfus as president, accepted member Kuiper's recommendation that maps and charts destined for use in exploration employ an *astronautical* convention with north at the top and east at the right.[3] An astronaut walking on the Moon's surface would now see the Sun rise over the eastern horizon just as he would on Earth. This is why Mare Orientale (Eastern Sea) is now on the Moon's western limb.[4] Old-timers like Ewen Whitaker and myself have to stop and think every time we state a direction.

Also in August 1961 the year-old USGS Astrogeologic Studies Group in Menlo Park was augmented by Daniel Jeremy Milton (b. 1934), whom Shoemaker had hired in June to study shocked rocks. Dan, usually called Danny by those who had known him as a child (his father, Charles, was a USGS mineralogist), had worked for Shoemaker on the Colorado Plateau way back in 1952. Later in 1961, Milton, Shoemaker, and Eggleton toured many of the craters and astroblemes in and near the Mississippi Valley and thereby began what developed into Dan's and Astrogeology's study of craters on all continents.

September 1961 was another month for new beginnings. On 18 September the Astrogeologic Studies Group was upgraded into the Branch of Astrogeology within the USGS. The next day greater Houston was announced as the site for the new NASA Manned Spacecraft Center (MSC), after a successful campaign by Texas congressman Albert Thomas and Vice President Johnson. In the same September, ACIC acted decisively on another bit of advice from Kuiper that would change the way they made lunar charts. He had pointed out that the best telescopic photographs can usefully resolve objects on the lunar surface no smaller than about half a kilometer across, whereas visual observations with a big telescope can fix detail down to about 200 m during moments of sharp "seeing" when Earth's atmosphere briefly stops shimmering. A photograph almost always integrates such prime instants with the previous and subsequent ripples. ACIC was attracted by the availability of the 24-inch refractor at Lowell Observatory with which Percival Lowell had investigated (and proliferated) the "canals" of Mars.[5] Pat Bridges used the telescope once in October 1960; then, in September 1961, she moved permanently to Flagstaff along with observers William D. Cannell and James A. Greenacre. Kuiper's wisdom soon became apparent. The group, led by Cannell, eventually grew to 22 people, including a dozen illustrator-cartographers, before it was disbanded in early 1968. They achieved results with the telescope that were considerably more reproducible than Lowell's and, amazingly to me, were able to integrate the visual observations with studies of photographs. The result was superb airbrush charts that have never been superseded by spacecraft data except in a few places such as the narrow strips overflown by Apollo spacecraft.

RINGS AND THE BASIN-MARE DIVORCE

The 36-inch reprojection globe that initially had been one of Dai Arthur's projects at LPL also went into operation in 1961. Lunar photographs projected on its surface re-created the Moon spectacularly. If you wanted to look straight down on a limb region just as you would on the central near side, all you had to do was walk around the side of the globe while a photo that included the limb was being projected. Something apparently new appeared immediately: systems of concentric rings fairly leapt off the globe at observers. The star of the show was the ring system that surrounds Mare Orientale, which can be viewed from Earth only during favorable librations. The discovery of the rings had a strong effect on lunar research from this time forward. The privilege of reporting and interpreting it fell to William Kenneth Hartmann (b. 1939), a young crew-cut astronomy major who arrived at LPL in the summer of 1961. Hartmann does not remember whether he was the one who first recognized the Orientale rings as such;[6] Charles Wood has told me that it was Kuiper's son Paul. In any case, Hartmann recognized the ringed structures as sufficiently different both from craters and from maria that they required a new name: *basins.*[7]

Baldwin disputed Hartmann's priority for the discovery, and it is true that Baldwin's 1949 *The Face of the Moon* contains several references to rings.[8] However, as Hartmann put it, the book's "impact on the recognition of multi-ring basins as a repeated type of structure was diluted by [its] sheer scope." Awareness of rings came to me and my USGS colleagues, at least, through Hartmann and Kuiper's paper "Concentric Structures Surrounding Lunar Basins," dated 20 June 1962. Much of the scientific community, however, might have missed the message because this paper was published "in house" by LPL — one more example of burial of important studies of the Moon in obscure publications.[9]

As Hartmann pointed out,[10] the ring discovery illustrates how images are used to study complex phenomena like planetary surfaces. The same pattern of rings appeared on LPL's globe around 12 maria. Even if they had been seen before, the ring systems had not been grasped as belonging to what psychologists call a *gestalt,* a pattern that one perceives differently when seeing it as a whole than when isolating its component parts. Recognition by Gilbert and Baldwin (and others such as Delmotte and Darney) that the Imbrium radials had something to do with circular Mare Imbrium is an earlier example of gestalt. The radials and the rings belong to a unified class of features that reappear in a roughly similar way in every occurrence. In Baldwin's words, "too many people had been enamored with learning more and more about less and less and hence did not see the big picture."[11] Now that the big picture, the unifying pattern, had been pointed out and dramatically illustrated, everybody could see the rings, even where they are poorly developed. Nonpsychologists call this peculiarity of human perception the educated eyeball. You see best that which you are prepared to see, or, "I wouldn't have seen it if I hadn't believed it." More formally we call it pattern recognition. The false association of circular maria and arcuate mountains shows that it can be misleading. But when properly used, visual pattern recognition is a powerful analytical tool that has enormous value in reconstructing how planetary surfaces were shaped.

The geologist's favorite type of pattern recognition is one that reveals age sequences. Consider an often-cited example, the stratigraphic relations of the crater Archimedes (30° N, 4° W). Even Urey realized that Archimedes is younger than the Imbrium basin ring and older than Mare Imbrium, but he explained the relation by an unlikely splash in the still-molten mare. The impossibility of such ad hoc explanations is demonstrated by a light-toned plains deposit, probably first recognized by Robert Hackman, which intervenes stratigraphically between the Imbrium basin and Archimedes.[12] Ejecta and secondary-impact craters of Archimedes rest on the plains, which in turn fill nooks and crannies in the Apennine Mountains, a relation referred to by planetary stratigraphers as *embayment.* The mare material not only fills the center of Archimedes but also cleanly chops off (*transects*) its secondaries that lie on the plains. So there was a sequence: (1) Imbrium basin, (2) light plains, (3) Archimedes, and (4) mare lavas. In geologic terms, the mare materials are a three-dimensional stack of bedded rocks that partly cover other beds composing Archimedes, the plains, and the basin. The time gap between basin and mare is supported by distinct differences in the densities of craters superposed on the two, an important observation also probably first made by Hackman.[13]

By early or mid-1962, therefore, the studies by Baldwin, Hackman, Mason, Shoemaker, Hartmann, and Kuiper should have dispelled all doubt that the soup filled the bowl long after the bowl was sculpted. The time delay shows, further, that the "soup" originated in the Moon's interior: it is volcanic. Endogenists might also stake a reasonable claim on smaller craters or parts of craters, but the "bowl," its rings, and its radials form such an immense unified ensemble that they can only have been created by an "irresistible force meeting an unmovable object"[14] — a cosmic impact. I am not sure exactly who should get credit for the first clear enunciation of the basin-mare divorce. Correspondence between Dietz and Baldwin in February 1962 shows that Dietz still thought that the maria were created by the basin impacts, but he agreed to "rethink" the matter at Baldwin's urging. Very likely the idea had been groaning for recognition and finally penetrated the skins of all the investigators at about the same time. This is the way most scientific ideas take hold. Person A says or writes something that makes person B think a bit more than previously about some subject, and then person B gains a new insight that goes beyond what person A said. Person B forgets where he got the new idea. Their colleagues are bruiting the same idea about in different ways. Nobody remembers accurately who said what when. It is as if the world of science is a giant Brain in which each scientist is one cell. Scientific advances are conceived by this Brain as a whole.

Of course, not everyone was convinced, and some amateurs and science writers still continue to confuse the basin and its mare. But in 1971 the Apollo 15 astronauts landed near one end of Palus Putredinis and collected mare basalt and Apennine Mountain impact breccia that differ in age by more than 500 million years — a time longer than life has occupied the lands of Earth.

THE MYSTERIOUS MOON

At the beginning of 1962, the far-side flyby by the Soviet Luna 3 in October 1959 still remained the only spaceflight to obtain any significant data about the Moon. Kuiper, Shoemaker, and Urey were named as the Ranger science experimenters in October 1961, but on 28 January 1962 the string of American failures continued as Ranger 3 missed the Moon by 37,000 km. In February 1962 the space race heated up a bit as John Glenn made his famous three revolutions about the Earth. The public, if not all scientists, knew that progress was being made. Propagandists claimed more progress when Ranger 4 crashed on the Moon's far side (15.5° S, 130.5° W) on 26 April 1962, but the spacecraft was useless. On 24 May 1962 Scott Carpenter flew the second manned Mercury orbital mission, but he let his enjoyment of the flight distract him from his piloting duties and he was lucky to land safely, 420 km off-target.

What I call mainstream lunar science was only beginning to move from the minds of its investigators onto the printed page at the beginning of 1962. Ralph Baldwin's 1949 book had already unleashed the forces of reason against selenological dilettantism, but what he had been doing since was not yet known to the lunar community. The Soviets had pioneered the Space Age, and a collection of their lunar papers published in Russian in 1960 and in English in 1962 was available to show us Westerners what the competition was doing.[15] Mostly the Soviets were doing "hard science"—traditional astronomical observations of whole-globe and surficial properties. This may have been Russian tradition, but the same emphasis pervades Fielder's 1961 *Structure of the Moon's Surface* and all sections devoted to the Moon in the third volume of Kuiper's series *The Solar System.*[16] The basin rings had sprung from the reprojection globe before the eyes of Hartmann and the Kuipers but had not yet been described in print. The *Engineer Special Study* by Hackman and Mason had been published formally, but not the cratering and stratigraphic papers by Shoemaker that really got lunar geology under way.

Nor was it clear in early 1962 exactly how space science in general and lunar scientific exploration in particular should be conducted. Recognizing the need and overcoming what he called a love-hate relationship between himself and the National Academy of Sciences, Homer Newell, since November 1961 the chief of a new NASA Headquarters office called Office of Space Science (OSS),[17] cooperated with members of the academy's Space Science Board in instituting the first of a series of joint "summer studies" that would punctuate the rest of the Apollo era and beyond.[18] More than 100 scientists convened between 17 June and 10 August 1962 in Iowa City, the home turf of the conference chairman and Space Science Board member James Van Allen. Although the board was now chaired by the foresighted Princeton professor of geology Harry Hess, the conference report reflects the scant attention paid to lunar geology at the time.[19] Van Allen's discoveries had been the first triumph of NASA space science, and space science was still commonly regarded as essentially equivalent to space physics. The closest the report came to the topic of the present book was in the first of many Space Science Board pitches for the inclusion of scientist-astronauts in the Apollo program, and in a contribution by stratigrapher Hollis Hedberg (1903–1988), who did no subsequent lunar work. This innovative and influential philosopher of stratigraphy recognized the great benefit-to-cost ratio of the new ACIC topographic and USGS photogeologic mapping programs.

One geologist at Iowa City who did reappear later in the lunar business was a Pennsylvania Dutchman with the mixed-nationality name Donald Underkofler Wise (b. 1931). Don got interested in the Moon in the late 1950s through a tectonics course and realized that formation of the Earth's core could have caused a spin-off of material to form the Moon. His paper was rejected by all the journals, but one day he walked into Harold Urey's office in La Jolla and presented his idea to the great man. Urey's first reaction was "another damn geologist," but a week later he told Don, "Next to my own ideas, I like yours best."

Reading the Iowa City report reminds today's reader how mysterious and exotic the Moon was perceived to be. Just to get near this place you had to build great fleets of spacecraft and worry that you might have ignored some cosmic mystery that would do you in. Later we shall examine how the elaborate schedule of flights suggested at Iowa City shrank as schedules slipped and telescopic studies began to deflate the mystery level. Events occurring elsewhere at the time of the conference would also make the progress of American manned spaceflight smoother than had been feared. On 11 July 1962 the crucial announcement was made in Washington that the vigorous debate about how to get Americans to the Moon had been resolved in favor of rendezvous in lunar orbit, the mode insistently advocated to a resisting though not closed-minded NASA by then-obscure but now-famous Langley engineer John C. Houbolt.[20] On 11–12 August 1962 Vostoks 3 and 4 were launched together but could not rendezvous in orbit. The implications of these seemingly unrelated developments were profound but did not become obvious for another seven years.

Eugene Shoemaker was at the Iowa City conference but limited his written contribution to a discussion of the possibility of collecting lunar samples from two points in space (libration or Lagrangian points) where they were thought likely to accumulate.[21] In the spring of 1961 Polish astronomer Kazimierz Kordylewski had reported brightenings in the direction of the points that might indicate particle concentrations. While the conferees were watching fireworks (literal ones) on Van Allen's lawn, USGS geologist Elliot Morris and photographer

Hal Stephens were pointing a light-gathering camera through the thin air above Mount Chacaltaya, Bolivia, in an attempt to photograph these "Kordylewski clouds."[22] The results were negative, as they were when others tried to photograph them from Earth or through the windows of Gemini capsules. It has proved easier to get lunar samples from the Moon than from the Lagrangian points.

THE GEOLOGISTS' MOON

Lunar geologists increasingly wooed the Moon away from the astronomers and physicists in the early 1960s. We were confident that our science could make the most of the grand opportunity being presented by NASA's lunar program, and eventually we sold NASA on the notion.

The wooing and selling was, at first, mostly the work of Eugene Shoemaker. Gene is enormously persuasive. When he talks, everybody listens. I am told he was feisty and fiery before Addison's disease hit him in 1963, and I did hear a fine shouting match between him and Henry Moore that year. Although he now has a calm, deliberate delivery, he is not at all boring even when he is talking about boring subjects. He has a hands-off management style and a way of making his listener feel that he or she is sharing in some grand project on an equal footing. Most of all he is passionately devoted to whatever project he currently has in view. The result was a generation of scientists convinced of the value of lunar geology and geologically based lunar exploration. Not that the selling was without obstacles. His Survey colleagues used to call him Super Gene (a play on the term for a type of ore deposit), partly respectfully and partly in spoof. Survey geologists have a long tradition of chopping each other down to size in annual satirical shows called Pick and Hammer, which are based on kidding-on-the-square that can border on the cruel. They are proud of their debunking of pretentiousness and nonsense but do not always recognize these attributes in themselves. The show of 27 March 1962 in Menlo Park, titled "Circum-Galactic Geological Excoriation," featured one Dream Moonshaker. The sarcastic dialogue went on at a personal level for what must have seemed an eternity, snidely commenting on such things as the hot lava beneath impact-advocate Moonshaker's feet.

During a trip to Flagstaff earlier in March 1962, Shoemaker (who did not attend the show, thankfully) heard Dan Milton casually let slip the thought, "Why not move the branch here?" Everything the budding Astrogeology Branch needed was nearby: Meteor Crater, volcanic craters of every type, young volcanic flows and cones, the Lowell and U.S. Naval observatories, a college then called Arizona State College (Northern Arizona University after May 1966), and last but not least, a reasonable geographic separation from Tucson and the bearlike embrace of Kuiper. Shoemaker, who loved the Colorado Plateau, small towns in general, and Flagstaff in particular, jumped at Dan's suggestion. A partial move from Menlo Park began in December 1962, with Chuck Marshall as the point man. Robert Gilruth and some of the other old Langley hands of the Space Task Group had moved reluctantly from gentle eastern Virginia to the Manned Spacecraft Center site in the smelly wasteland south of Houston proper.[23] Shoemaker felt no similar reluctance, but some of his astrogeologists did.

I don't know whether the crusty USGS geologists or the bright, shiny NASA engineers were harder to convince, but a large portion of both eventually came around to Shoemaker's viewpoint. In September 1962 he suddenly left Menlo Park for a one-year assignment at NASA Headquarters. It was not foreordained that Apollo would be influenced by geology or any other science. Two NASA managers who thought it should be were Homer Newell and his deputy, Oran Nicks, director of the Lunar and Planetary Programs Office that Newell had created in OSS in January 1960. Newell formed an OSS-OMSF Joint Working Group to develop a plan for scientific manned lunar exploration and asked Shoemaker to chair it.[24] Shoemaker had been dismayed by the antiscience attitude displayed at Iowa City by personnel of NASA's Manned Spacecraft Center and was not about to refuse this golden opportunity to influence the subsequent course of geologic exploration by Project Apollo—and more than incidentally, to get himself a trip to the Moon. His plan appeared in a report by the Lunar and Planetary Programs Office, known by the name of its chief scientist, physicist and magnetics specialist Charles Sonett. On 30 July 1963 Newell reorganized the working group as the Manned Space Science Division.

All this was over the strenuous objections of Newell's counterpart at NASA Headquarters, Dyer Brainerd Holmes (b. 1921), an electrical engineer who had been recruited from RCA in October 1961 to head the new Office of Manned Space Flight (OMSF). Holmes's attitude to scientists was, essentially, "buzz off."[25] The president had directed us to get Americans to the Moon and return them safely to Earth in this decade, but nowhere did he mention picking up stones or taking pictures. And then there was the devilish problem of the influential space physicists, who scorned rocks and pictures. If Shoemaker had not gone to NASA Headquarters to lobby for geology, and if Holmes had stayed there (he left in September 1963), it is entirely possible that we would have no samples or photographs from the lunar surface.

Two apparently permanent fixtures of Astrogeology arrived in Menlo Park in September 1962 to find that the man who had hired them was heading east. Michael Harold Carr (b. 1935) had emigrated from his native Leeds in 1956 to escape the English class system. Originally a metamorphic petrologist, Carr switched to geochemistry for his doctorate at Yale because of his understandable distaste for fieldwork during hot Connecticut summers. Faced with the lack of good job opportunities in geochemistry, he took a job working with the physics of shock at the University of Western Ontario. Thus he had already applied himself to three totally different subjects, as remains typical of him today. At a Harvard-Yale reunion at the Geological Society of America's annual meeting in Cincinnati in November 1961 he had met Harvard Ph.D. Dan Milton, who had been assigned by Shoemaker to an experimental investigation of shock processes. Dan did not particularly want to do the project, and Mike Carr was his escape hatch. Mike wrote to Shoemaker, Shoemaker visited him at Western Ontario in the spring of 1962, and the deal was on. In December 1962 Shoemaker extended the long arm of authority from Washington and put Mike on an additional project of investigating cosmic dust that had both meteoritic and military ramifications. The dust project would have kept anyone else totally occupied, and it did dilute Mike's efforts for several years.

The second September 1962 arrival was geologist Harold Masursky (1922–1990). Hal was more a facilitator of others' work than a contributor of original science. Such was already his reputation in his previous positions in the USGS, and it suited Shoemaker just fine because he needed someone to manage the lunar geologic mapping effort that was beginning. Hal had a good understanding of what geologic maps were all about and how to manufacture them even though he seldom worked on them himself. He soon began as well to promote the acquisition of cameras for lunar use at Lick Observatory and to establish ties with nonastrogeologic but pro-Moon USGS geologists at Menlo Park who did not perform in the Pick and Hammer show. Hal's subsequent career was characterized by a similar promotion of cameras for spacecraft and establishment of ties with NASA movers and shakers and the news media. This perceptive and witty geologist played a major role in keeping disparate scientific and flight organizations aware of one another's activities. Readers may know him from his many television appearances commenting on each new lunar or planetary mission. Hal did not take over leadership of the Menlo Park office yet; the acting branch chief in Shoemaker's absence was his old acquaintance Don Elston.

Data were sparse and the effort to scrape up more was formidable. Earth rocks had to serve as best they could as imitations of the rocks of the Moon. Theory and laboratory simulations had to substitute for witnessing lunar processes in action. Nor did Ranger seem likely to provide more direct data soon. On 21 October 1962 (18 days after Wally Schirra's Mercury flight, the fifth manned Mercury) Ranger 5 missed the Moon by 720 km, having already lost the power from its solar panels — not that the world cared, for this was the time of the Cuban missile crisis. Anyway, the areas that would be viewed by the crash-landing Rangers were very small, so for some time to come the Moon would have to be probed by Earth-based telescopes and all the instruments that could be hung on them.

I was glad this was true. On innumerable weekends during my childhood and youth, while my peers were at the beach or wherever, I had haunted the Griffith Observatory above Los Angeles. I discovered both stars and rocks in this beguiling place but was more inclined to astronomy. Among the things I remember learning there was that what you see on the Moon depends on how high the Sun is in its sky—the lower the better, down to a point. I even made a (nongeologic) map of the Moon when I was 15 or 16. Because my first love was astronomy I chose Pomona College in southern California for my undergraduate studies (on the basis of personal advice from astronomer Seth Nicholson, who is often credited as a codeterminer of the Moon's surface temperature and who added that I would become a social misfit if I went to Caltech as I had been considering).[26] But real-world astronomy was not for me, and I majored in geology at Pomona and the University of California at Berkeley and Los Angeles. Between 1957 and 1962 (with a year's interruption for a rewarding though not very geologic Fulbright Scholarship at the University of Munich), I was at UCLA preparing to deal as geologists usually do with messy oil fields or mines or heat, cold, rattlesnakes, cow pies, and poison oak. During the UCLA grind I visited JPL and saw Ranger spacecraft being built. Knowing of my interest in such matters and my lack of interest in the oil companies that hired most geologists, another student[27] told me about some guy who was at Caltech interviewing people who might want to work on the Moon. Shoemaker presented an unsurpassable opportunity to combine my childhood interest in astronomy with my adult profession of geology. After later reminding him who I was by means of a letter that included words to the effect, "Obviously I'm your man," I arrived at Menlo Park on Monday morning, 3 December 1962, a month and a half after the missile crisis and three days after finishing my Ph.D. dissertation.

Fantasy became reality within a week as I took my first turn observing visually with the magnificent 36-inch refracting telescope of Lick Observatory. All astrogeologists were assigned a LAC quadrangle to map geologically, as well as to one or more other projects that more or less matched their interests or talents. Dick Eggleton became our teacher in the methods and facts of lunar geology once he returned to work after a serious automobile accident that had occurred on the weekend I was driving north to Menlo Park. Until the first spaceflights provided better data, all of us were required to observe the Moon on good nights whenever the terminator (boundary between illuminated and dark zones) was in or near our assigned quadrangle. We hoped to capture moments of superior seeing and favorable shadows that would reveal some critical detail for learning a feature's origin or relative age. For example, the craters Reinhold and Lansberg look so similar even on a superior photograph that Shoemaker assigned them to the same map unit, but Eggleton discovered an age difference when he observed secondary craters of Reinhold superposed on the mare unit that buries Lansberg's ejecta. I relished this telescopic observing more than anything else I did during my career. This taste was not shared by everybody—Mike Carr, for example. But picture the dome's interior rimmed by soft red lights, the gentle onshore breeze from the nearby Pacific, and the night quiet except for the humming telescope drive, classical music from the radio, and only an occasional creaking noise from somewhere in the dome to remind one that the earthly remains of James Lick are entombed in the telescope's pier.

CRATERS

A much larger published trove of relevant information was available at the end of 1962 than at the beginning. We owe a particular debt to the civilized and erudite Czech astronomer Zdeněk Kopal, who promoted two review volumes whose rich assemblages of up-to-date reviews include the landmark papers by Shoemaker from which lunar geology leapt into the modern era.[28] Then, early in 1963, there appeared what other kinds of publishers would describe as "Sensational! Reveals all!"—Ralph Baldwin's richly documented magnum opus, *The Measure of the Moon.*[29] Among much else, the book contains more than 180 pages on craters and cratering that could form a modern textbook on the subject. Baldwin and his predecessors had marshaled definitive evidence for the impact origin of that vast majority of circular, rough-rimmed, depressed-floor, central-peak craters that Jack McCauley and I, in nostalgic reference to our youthful interest in the stars, later called the "main sequence." In a letter dated 23 January 1962 to the University of Chicago Press, referee Robert Dietz wrote that "a book like this is worth a half-dozen trips to the moon by any astronaut of the future." Just as I am reassured by reading the work of Kuiper or Urey that we did indeed learn much about the Moon during the Space Age, so I wonder if we did when I reread Baldwin's two great books and find on every page some fact or interpretation that men and machines would labor to rediscover. In 1964 Ernst Öpik suggested that Baldwin be recommended for a Nobel Prize. Unfortunately, Alfred Nobel had something against astronomers and geologists, so theirs are not Nobel categories.[30]

Field geologists were also adding richly to the store of cratering knowledge. With eyes newly calibrated to see circles, they were spotting terrestrial craters and astroblemes on aerial photographs, then visiting them in the field, often finding shatter cones or shock-created minerals such as coesite or diagnostically structured quartz.[31] A love of fieldwork and remote locales led many geologists to join the Astrogeology program during the affluent golden age of the early 1960s. One such was Dan Milton, who began his official world traveling in 1962 when he went to examine the cluster of meteorite craters at Campo del Cielo in Argentina. Starting in July 1963, Dan ventured to the Henbury meteorite craters in the outback of Northern Territory, Australia. Both he and his companion in the first Henbury work, Frank Curtis Michel (b. 1934), were hoping to become astronauts and to perform fieldwork on the Moon. Physicist Michel, who did survive the rigorous selection process to become a scientist-astronaut in June 1965, was eager to learn what geology was all about. Their careful fieldwork enabled them to map loops of rays containing ejecta fragments from one Henbury crater that are like those of Copernicus, and to identify their source in the crater, thereby substantiating Shoemaker's interpretation of the Copernicus secondaries.[32]

Although the terrestrial work on craters is central to the story of lunar geology in the Space Age, I have to brush over it here, and also over the very significant craters made artificially by chemical and nuclear explosions and in the laboratory.[33] An especially rich crater-hunting ground has been the Canadian Shield, whose aeon-long record of large impacts was preserved by a sedimentary blanket and the shield's relative inactivity, and then re-exposed by the Pleistocene glaciers. These field and experimental investigations combined to show how lunar features that no one could imagine as the products of impact were in fact not only compatible with but diagnostic of impact. At the beginning of the decade of the 1960s about 32 meteorite craters were known; at the end of the decade about 47 had been identified.[34] Spurr and his followers could no longer claim that the absence of terrestrial impact craters disproves that origin for their lunar counterparts.

THE IMBRIUM BASIN

In 1963 no one quite knew what basin ejecta looks like, where it is on the Moon, and in what sequence the basins formed relative to each other and to the rest of the surface features. These are the questions geologists always ask about everything: What? Where? When? The *hows* and *whys* usually come last. The basins were unusual in that their general origin was evident from their size and symmetry. However, there were plenty of detailed *hows* for the future to work out. Some are still unclear today.

Like crater ejecta, basin ejecta has done two things: pile up on the surface, and gouge depressions in the surface. Again like craters, the piling up occurs near the basin rims and the gouging occurs farther out where the ejecta strikes with more destructive energy. All this was realized in the early 1960s; but still unknown for basins was where the transition between "near" and "farther out" was located.

Historically, the gouging was the first to be understood. Gilbert, Dietz, and Baldwin knew that objects flying out of the Imbrium crater at very low angles created the sculpture, clipping the tops of craters but missing many low points. That is to say, secondary impacts of the basin ejecta formed the sculpture. But this interpretation was temporarily subordinated in the 1960s to a hybrid interpretation. An origin of the sculpture as faults that had been triggered by the impact was favored by Shoemaker, most of his followers (including myself), and Bill Hartmann, who strayed from his astronomy studies long enough to take geology courses and write a master's thesis in geology espousing the idea.[35]

I believe the prevalence of faulting interpretations is rooted in geologists' adherence to the concept of *uniformitarianism,* usually summarized as "the present is the key to the past." Uniformitarianism originated from eighteenth- and nineteenth-century theological debates about the creation of the Earth in seven days, its supposed origin in 4004 B.C., the significance of Noah's flood to fossils, and so forth. *Catastrophists* attributed everything to divine acts. *Uniformitarians* attributed everything to processes that are still observable today acting at approximately today's rates. Today these origins of uniformitarianism are often forgotten and the concept taken too literally to mean that catastrophes did not help create the geologic record.[36] I think that is how we felt about the sculpture; flying fragments were too catastrophic, too ad hoc for our tastes. However, meteorite impact is uniformitarian because it is still occurring according to the same laws as always, although not with the great frequency or profound effects of the past. Great storms and floods are other examples of catastrophes that shape the terrestrial geologic record more acutely than do everyday erosion and sedimentation. Another decade would have to pass before the pervasive effects of impacts on the Moon, Earth, and other planets were fully accepted.

The piled-up ejecta, whose history was the opposite of the sculpture's, was either not noticed or not stressed by Gilbert, Dietz, and Baldwin but was well understood by Shoemaker and Hackman. Gilbert mentioned "solid, pasty, and liquid" ejecta. He used the solid to make the sculpture, misunderstood the liquid by thinking it was the mare material, and did not dwell on the pasty. He observed a softening of the sculpture and attributed it to the ejecta, but in the absence of geologic maps or other illustrations we cannot know exactly what he meant. In his 1946 paper Dietz correctly interpreted the rough textures of the flank ("dip slope") of the circum-Imbrium mountains as indicating a relatively young mass of rubble but did not explicitly state how he thought it got there. Kuiper in his 1959 paper came even closer by referring to "rough ejectamenta" on the circum-Imbrium mountains, but then punctured any hope that he understood what had happened when he referred to "viscous-appearing" ejectamenta, by which he meant the lava. Hackman and Mason came still closer when they stated, "The Pre-Maria rocks appear in places to be overlain by material ejected from some of the maria" (that is, the basins).[37]

Finally, the classic 1962 paper by Shoemaker and Hackman cleared the air. They pointed out that the other writers had "interpreted, each in a somewhat different way, part of the material . . . as ejecta from some place in the region occupied by Mare Imbrium." Then they described the blanket in correct geologic terms: "Essentially the materials of the Imbrian system form an immense sheet partly surrounding Mare Imbrium." None of the others seem to have thought of it as a three-dimensional blanket—a stratigraphic unit. Dick Eggleton later measured its thickness by the depth to which it buries craters, to prove it did indeed have a finite depth.[38] The discovery of the blanket was a major finding and led to the choice of a landing site for an Apollo lunar module a decade later.

For the ejecta blanket, the operative word was *hummocky.* Shoemaker and Hackman had used the term to describe irregular, closely packed, low hills of the Imbrium ejecta. To a geologist the term calls to mind landslides, debris dumps, and other such messy deposits, and that is what was meant by it. The term may sometimes have been taken as equivalent to a description of the Imbrium blanket. For example, the hummocky half-exposed rim of the crater Letronne (11° S, 42° W), 1,600 km from the center of Mare Imbrium, was mapped in 1963 as "regional material of the Apenninian Series," the unit equated with Imbrium ejecta, even though it now clearly appears to be a separate, later deposit.[39] Another tale about the early astrogeologic eagerness to map distant hummocky lands as "Apenninian" begins in a short contribution to a progress report in our "gray literature" dated March 1962 and authored by Eggleton and Marshall, and culminates in chapter 16 of this book.[40] Eggleton and Marshall searched the telescopic photographs for hummocky material and produced a quite modern-appearing map of the Imbrium ejecta. In addition they found an isolated patch of the hummocks near the crater Descartes, some 1,750 km from the center of Mare Imbrium. To those of us who entered on duty after this work was done, this mapping seemed to be an example of excessive Imbriophilia.

MARIA

The maria were the best understood, least mysterious lunar features at the beginning of the telescopic scrutiny of the 1960s. Of course, there were Urey's projectiles and Gold's dust. Kindred to Gold's notion was physicist John J. Gilvarry's long-held belief that the maria owed their dark color to organic matter in sediment deposited in a deep global ocean.[41] But volcanism was winning the interpretive battle. The maria are flat and relatively smooth, contact their containers sharply along level contours, and fill embayments. This leveling out means that the rock which constitutes them was fluid when it was emplaced. Experience with Earth showed that the most common kind of fluid magmas are basaltic. Basalts are dark and denser than light-colored rocks containing more feldspar and quartz, so should sink below the terrae, as observed. By 1962 even Urey was weakening a little and at least considered the possibility that the maria were volcanic basalts; he said that sampling was needed to settle the matter.[42]

There was, however, one fairly robust competitor to the basalt hypothesis during the 1960s: *ashflow tuff,* consolidated rock originally emplaced as hot, fluidlike flows of volcanic ash or other volcanic fragments. At this time more and more terrestrial geologists were realizing that rocks previously assumed to be lavas were in fact welded ashflow tuffs.[43] For example, silicic ashflow tuffs cover vast areas in the Basin and Range Province of the western United States. John O'Keefe and his colleagues therefore liked it because it was a possible lunar source for the silicic tektites.[44] More to the point I am making now, it is highly fluid when emplaced, as any analogue of the maria must be. It is lighter in color than basalt, but that did not seem a serious objection, for solar radiation or some other mysterious cosmic emanations could turn it dark as does desert varnishing or other weathering on Earth. I had mapped ashflow tuff in my dissertation field area and was among those who temporarily liked it as an alternative explanation for the maria, and even more for the terra plains, because it differentially compacts after it flows over obstacles so that the obstacles remain visible in a subdued, ghostlike form that is common on the Moon. At least one distinguished geologist, University of Texas professor Joseph Hoover Mackin (1905–1968), still favored ashflow tuff as the mare rock in the late 1960s.[45]

A hypothesis for the mare composition that combined basalt and ashflow tuff had substantial currency for a while. Robert Dietz, Paul Lowman, and geologist-astronaut Jack Schmitt were among the fans of an origin of the maria as *lopoliths* like those which fill large terrestrial astroblemes such as Vredefort in South Africa and Sudbury in Canada.[46] Lopoliths (a term that even many geologists will have to look up in their glossaries) are lens-shaped intrusions of basalt with some rock that is more silicic than basalt and some that is less silicic (ultramafic). The silicic magma commonly rises to the top and forms a caprock. If it did this on the Moon, the maria could be silicic ashflow tuff. Lopoliths helped O'Keefe's group at Goddard explain the silicic compositions of tektites while obeying the laws of isostasy and avoiding the need for the Moon to generate unreasonably great amounts of silicic rock.

SPECIAL FEATURES

I have been describing interpretations of the important landforms and geologic units of the Moon — craters, basins, maria, and the stratified beds of materials that compose them. Now we come to what *really* interested most people before and during the 1960s: the oddities, the "special features." Leaf through any book or article about the Moon from that era and even later and you will see mostly craters and special features. I remember my excitement when I thought I had discovered the Hyginus Rille and its aligned craters during my first look through the 36-inch Lick refractor in December 1962.

Special features played a central role in the debate about the origin of the maria. Telescopic observations made under very low Sun illumination showed a type of feature that fascinated amateurs and interested professionals: a kind of low, shieldlike dome with a summit crater. The composition of the dome-forming magmas would imply a similar composition for the flat parts of the maria. On Earth, similar domes are formed by basaltic magmas if the magmas reach the surface, but may be formed by more silicic magmas if the magmas spread out just beneath the surface and deform it like a skin blister to create mushroom-shaped intrusions called *laccoliths.*[47] In support of their belief that the maria were silicic ashflows, O'Keefe and his colleague Winifred Sawtelle Cameron thought the domes were laccoliths and that lunar ridges, "spines," and steep "domes" were formed by a silicic volcanism of a type that also produces ashflows on Earth.

Sinuous rilles are particularly eye-catching. These squiggles meander through the maria in many places, especially the northwestern quadrant of the near side. Look at a picture of the Apennine Mountains and you probably see Hadley Rille. Sinuous rilles became associated early with the ashflow tuff idea because Jack Green and Winifred Cameron, among others, compared them with channels cut by "glowing avalanches" (*nuées ardentes*) of flowing silicic ash.[48] As late as 1969 Gilvarry claimed they were riverbeds in which water had flowed.[49] Any book or review article from that period can supply additional interpretations. For some reason, the origin that turned out to be right — channels and collapsed tubes in basaltic lavas — did not step strongly to the forefront in the 1960s despite the numerous good though smaller terrestrial analogues in Hawaii and elsewhere.[50]

Almost anything on a planet can be interpreted as volcanic or tectonic based on imagined terrestrial analogues. Irregular craters can be calderas or volcano-tectonic depressions. Circular ridges can be exhumed ring dikes. Clustered craters of similar sizes might be parasitic vents or maar fields. Some special features disappear when well photographed and served only to misdirect telescopic observers into the camp of the volcanologists. For example, the otherwise discerning Mike Carr fell into the telescopic-resolution trap and perpetuated the myth that the peaks of some craters are surmounted by craterlets. As had other observers, he thought he saw many of these in one of his assigned quadrangles and went so far as to explain this concentration by an unusually high thermal gradient in Mare Imbrium caused by heat from the original Imbrium impact—a generally reasonable idea based on an incorrect observation.[51]

In the absence of samples there was no sure way to know in the 1960s which special features were real and which were imaginary. The persistence of their devotees at least kept some minds open and much ink flowing. And they are there to remind us that the human mind focuses on the unusual at the expense of the commonplace.

THE SURFICIAL MATERIAL

The uppermost few meters were an especially mysterious aspect of the mysterious Moon.[52] Astronomers gathered data in wavelengths from x-ray to radio that they, geologists, and engineers could translate into clues about grain size, intergrain structure, compaction, and slopes at various scales. There was almost unanimous agreement that a surge in brightness and uniform limb-to-limb illumination near full moon indicated a loose, porous structure. On an exotic Moon this might be spongy pumice, lacy concretelike structures, or loosely stacked fibers like toothpicks or tiddlywinks. Culinary comparisons were made with cotton candy, honeycomb, and Cracker Jacks. Probably the favorite analogy was "fairy castles" like those of home aquariums but consisting of gently deposited loose dust barely adhering in the much-discussed lunar vacuum. The safety of a lunar landing depended on the nature of the surface.

Meditations about the surface were tied to the impact-volcanic controversy. If the craters were volcanic, as the hot-mooners thought, then the surface material might be entirely volcanic as well—lava, ash, or tuff (consolidated ash). If impacts created the craters, the bedrock could be of any origin, but the impacts would create from it a surface layer of rubble and dust. This was the view of Baldwin, who had concluded by 1949 that the surface is covered by "large quantities of dust or fine particles spread over the ground."[53] In 1959, after a thorough discussion of the problem at a lunar colloquium in Dallas, the general opinion was that the surface dust was produced mostly in place by micrometeorite impact and solar "emanations."[54] The majority further thought that the layer was on the order of a few meters thick. But voting does not establish the truth of a scientific question. The impact debris might be ejected from the Moon as fast as it formed, leaving bare volcanic lava or slag. At the opposite extreme was

Gold—he was always at an extreme—who believed that the dust layer was dangerously weak and hundreds or thousands of meters thick. Neither he nor Gilvarry seemed to understand that lava could lie beneath surface dust; to them the maria "were" dust.[55]

Some other possibilities for the nature of the surficial material provided wonderful diversions. One was expressed in what Baldwin in 1963 referred to as a "now famous" comment by biophysicist John R. Platt in 1958 that "the first man who plants a rubber boot on a lunar surface may be in for an unpleasant surprise" because the surface may be covered by interstellar dust that might react violently to the intrusion.[56] Another was that if impacts eject much fragmental debris and if many impacts are still occurring, then the astronauts could be endangered by the flying particles. One of the early cooperative efforts between the USGS and the NASA Ames Research Center addressed this danger.[57] Don Gault, Gene Shoemaker, and Henry Moore concluded that considerable debris *would* be sprayed around on today's Moon and might present a hazard if an astronaut stayed long enough. They bowed to Gold by remarking that this cloud of particles was a good way of producing his dust. But they correctly concluded that the amount of debris generated greatly exceeded the incoming mass, and that enough would stay on the Moon to accumulate on the entire surface as a deposit of poorly sorted debris.

As with our other topics, we can look back and find the wheat amidst the chaff. Baldwin and the USGS-Ames studies had the surface layer about right, as did geologist and remote-sensing specialist John Salisbury, who was at the Air Force Cambridge Research Laboratory between 1959 and 1976. A perception that is close to today's appeared in a 1964 book edited by Salisbury and his colleague Peter Glaser, with a preface by Baldwin, that constituted the proceedings of a 1963 conference on the surface layer.[58] The voting had decided that the entire lunar surface is covered by a deposit with variable thickness consisting of mixed, unsorted impact debris ranging from microscopic particles to large blocks. So it is; but the effort to prove it continued for several more years.

Although I acknowledge that the astronomical remote-sensing work on the surficial material was necessary, it always bored me. So it was with trepidation that in the spring of 1963 I volunteered to accept Shoemaker's assignment of an investigation of lunar polarization. As this overlapped with another nongeologic project described later, slope studies, I began to feel that I was reverting to the student-era grunt work that I had hoped to escape in the USGS. But these feelings were soothed by the location of the assignment: the Observatory of Paris (Meudon), where I would enjoy the hospitality of the world's expert, Audouin Dollfus. Dan Milton was in the outback of Australia where he wanted to be and I was in *la douce France.* In May Gordon Cooper had closed out the Mercury series of one-man flights and opened the way to begin the Gemini series of two-man flights a year later. Exhilaration was emanating from the Kennedy Camelot, and the space program was bursting with vigor and opportunity. I visited Germany and was kindly shown around the Ries and its lovely surrounding country and towns by the late Walter Weiskirchner of the University of Tübingen. I would worry about polarization later.

PICKING THE LANDING SITES, ROUND 1

The sites of the American as well as the Soviet landings were determined primarily by where the rockets and spacecraft could go. The Soviets do not seem to have employed geologic advice in their early lunar mission planning, possibly because their traditional inclination to the "hard" mathematical and theoretical sciences made them mistrust geologists as much as the American space physicists did,[59] or possibly because their landing sites could not be specified any more closely than the western near-equatorial zone.[60] The American missions had "windows" too. But within these, sites could be chosen for their value to science.[61]

Shoemaker had long realized that geologic maps would be needed for selection of scientifically productive and safe exploration sites. In August 1961 he had performed a three-day helicopter-supported reconnaissance of New Quebec (Chubb, Ungava) Crater in the Canadian Arctic as an example of how fieldwork might be done during similarly short stays on the Moon. This exploration plan was included in a 1962 article in *American Scientist,* which he wrote at the request of associate editor A. F. Buddington, as a prototype plan for the next decade.[62] In this generally prescient article Shoemaker predicted that perhaps a thousand scientists and technicians would be required to attack the mysteries of the Moon before the manned landings. Both reconnaissance maps at small scales and many detailed maps at large scales would be needed.[63] The 1:1,000,000 scale was selected for the reconnaissance because that was the scale ACIC had chosen for its LAC base charts. Which areas would be geologically mapped was also determined by which LACs were available, and this, in turn, was dictated by the target zone of the first spacecraft. The first four LACs and geologic maps covered a 960-by-1,200-km rectangle centered on the western equator called the Lansberg region (16° N–16° S, 10°–50° W).[64] It had been known at least as early as 1959 that rockets and spacecraft launched from Cape Canaveral would expend less energy in approaching the Lansberg region than any other part of the Moon. Early plans called for Ranger to head for the Lansberg region and for manned landings to follow there or still farther west.[65]

These four geologic maps included estimates of roughness and other terrain characteristics for each geologic unit. Terrain estimates depended partly on geologic interpretations. Impact-crater ejecta would be rough and lava would be smooth, or at least flat. A favorite phrase in the explanations of the maps, repeated to the point of amusement, was "probably chiefly crushed rocks with large blocks." The map texts were simple statements of mapping principles, stressing the then-novel idea that the surface of the Moon is heterogeneous, another phrase that eventually wore thin despite its verity.

Shoemaker and those who worked for him also labored to estimate terrain characteristics from quantitative measurements of one sort or another. In the early 1960s this required extrapolating from telescopic data to the scale of interest for spacecraft landings. Mostly it was assumed that if the terrain looked rough at the telescopic scale, it was probably even worse at the human scale; relief was additive. How were we to keep track of what was known and what was guessed? The first published geologic map, by Hackman of the Kepler quadrangle, introduced a major innovation into lunar geologic mapping. The units on Shoemaker's original Copernicus map and also on early versions of Hackman's Kepler map had such names as "ejecta" and "breccia." Shoemaker knew from the beginning that this would never do, and the forces of scientific purity indeed rose in a protest that I suspect was partly motivated by the then-common skepticism that the Moon could be mapped geologically at all. He therefore devoted much effort to editing the explanation for the published version of the Kepler map. Henceforth the unit descriptions of lunar and planetary geologic maps of the USGS had two parts: *characteristics*, the objectively observable properties, including coarse topography; and *interpretations*, the speculations on origin and inferred terrain properties. When honored, this split has served planetary geology well ever since.

The dual need to stay objective and to estimate terrain for exploration planning launched Shoemaker and the rest of us on an extended search for measurable properties — measurable, not necessarily significant. Albedo was both easily measurable and significant, standing almost alone in both respects in the early 1960s. The first semiannual progress report of the Astrogeologic Studies Group featured an albedo study, the early geologic maps stressed it heavily, and it was pursued hammer and tong for the rest of the decade.[66]

Most astrogeologists took their turns in the quantitative barrel. I went in twice, once for the polarization study — an excellent example of an easily measurable but unimportant lunar property — and the other time to find a way of determining the slope characteristics of geologic units photometrically. Although I regarded the slope study as a distraction from geologic mapping, I engaged in it with some interest. Starting from a suggestion by Dick Eggleton, I sat around coffee houses in San Francisco in early 1963 figuring out how to do it quantitatively and mechanically (two words not usually in my vocabulary), and yet simply and correctly. The result was that I reinvented and extended a technique invented by Dutch astronomer Jan van Diggelen, who used it to determine the slopes of individual mare ridges.[67] He had showed that within a unit with uniform albedo, the brightness varies only with the Sun's elevation (which is known) and slope. My contribution was to compensate for brightness variations due to albedos, which vary among units, by photometrically scanning and comparing low-sun and high-sun photos of the same region.

To describe what became of the slope study I must introduce my friend and close co-worker John Francis McCauley (b. 1932). Jack and I both had been interested in astronomy in our youths and had owned telescopes. Also, we knew how to make geologic maps and felt stratigraphy in our bones. Already a veteran of two state geological surveys and private consulting, Jack had become interested in the new lunar program early in 1962. He had recently achieved tenure as associate professor of geology at the University of South Carolina and invited Ed Chao to give a lecture. At a social affair following Chao's fascinating talk, Jack mentioned his interest in working in the Moon business. A few months later, in November 1962, he received a phone call from the Columbia airport. It was Gene Shoemaker, on his way to the annual meeting of the Geological Society of America in New Orleans. The upshot of the story is that Jack became the second geologist after Chuck Marshall to set up shop in Flagstaff and the last of the early group of branch geologists. He entered on duty in February 1963, and soon afterward we discussed our similar philosophies of lunar geology and the significance of lunar exploration in the bars at the top of the Fairmont and Mark Hopkins hotels in San Francisco. I was enamored with lunar geologic mapping, so in November 1963 I managed to slide the slope study off onto Jack, who, skilled wordsmith that he is, coined the now-accepted term *photoclinometry* to describe the technique.

Another attack on terrain studies was initiated by that inveterate promoter Hal Masursky. In late 1963 and early 1964 Hal recruited a dozen Menlo Park geologists not in Astrogeology[68] to observe terrain roughness with the 36-inch Lick refractor when the terminator was in their areas. Masursky's motive was mostly ulterior: to get expert geologic talent from outside the Branch of Astrogeology involved in the lunar program. These good field geologists also employed the mostly rather poor telescopic photographs then available. I was jealous of the time they consumed at "my" telescope and disapproved of Masursky's manipulations. However, many of them did contribute good ideas, reviews, and counsel to our lunar effort.

Descendants of these two types of terrain study played a major role in locating landing sites for Surveyor and Apollo. The work of integrating our and others' site-selection efforts with other aspects of Apollo missions fell to a private consulting organization that will appear often in the rest of this history: Bellcomm, a subsidiary of AT&T established in March 1962 at NASA's request. Bellcomm advised OMSF not only with analyses of the Apollo communication networks that are a natural for AT&T but also about the flight hardware, including the magnificent Saturn 5 moon rocket. As time went on, Bellcomm took over an increasingly large role in preparing and coordinating the site-selection strategy for the Surveyor, Lunar Orbiter, and Apollo missions, partly by default and partly because of the people they happened to hire. In the first of the many Bellcomm memoranda I have seen, dated March 1963, they worried much about the landability (as they called it) of the lunar surface. They thought that its strange photometric properties would degrade Ranger's attempts to photograph it, were cool to Surveyor because it was only a point probe, assumed that an unmanned rover would be required, preferred manned to unmanned orbital surveys, and recommended that the lunar landing vehicle, then called the *lunar excursion module* (LEM), be overdesigned just in case.[69] We will see that the memo was partly right, partly wrong.

NOVEMBER 1963

The mention of November 1963 still sends a chill down the spines of those of us who experienced the breaking news of the assassination of President Kennedy in Dallas on the twenty-second. It hit me while I was in my office at Ellington Air Force Base in nearby Houston under circumstances the following chapter describes. But our story concerns more mundane though decisive events clustered at the end of October and early November 1963.

In September Brainerd Holmes had resigned from NASA in protest of Administrator Webb's refusal to approve a supplemental appropriation for OMSF that the politically astute Webb, backed by Kennedy, knew would not set well with Congress and would rob the unmanned programs.[70] Holmes's replacement, another electrical engineer, was George Edwin Mueller (b. 1918) from the Space Technology Laboratories. Mueller was generally more reasonable and worked better with Homer Newell, though the two did not agree fully about whose office should manage the manned science program.[71] Almost immediately, on 29 October 1963, the able Mueller announced what proved to be a critical decision in meeting Kennedy's deadline: all components of rockets and spacecraft would be tested "all up" instead of separately and sequentially. A major reorganization of NASA by Webb effective 1 November included the absorption of the Office of Applications (OSA) by Newell's Office of Space Science

(OSS) to create the Office of Space Science and Applications (OSSA), and the promotion of Newell to associate administrator for OSSA. Shoemaker's NASA tour of duty also ended officially at the same time, and he was succeeded as head of the OSSA-OMSF Manned Space Sciences Division by Willis B. Foster.

Exotica intruded into most geologic thought in the 1960s, and on the day Mueller was announcing "all up," a nonphysical type of special feature intruded itself into our thinking. Throughout the decade much fuss was made over *transient phenomena;* that is, telescopic sightings of flashes, clouds, and so forth on the Moon's surface. On 29 October experienced ACIC observer Jim Greenacre at Lowell Observatory reported red spots at Aristarchus, the Cobra Head, and Schröter's Valley. The fact that Greenacre's favorite drink was boilermakers aroused some skepticism, but the observations were confirmed the same night by new observer Edward Barr and later by three others, including John Hall, the director of Lowell.[72] I held up the newspaper headline, "Moon 'Eruptions' Seen Here," for the viewing of my Menlo Park colleagues arriving at the Flagstaff airport. The Moon seemed to be volcanically active!

We were in Flagstaff for a three-day meeting of the dozen astrogeologists who then made up most of the branch's scientific talent pool. The meeting would change the way the Moon was interpreted and manipulated geologically. The old guard of Gene Shoemaker, Bob Hackman, and Dick Eggleton were called on to defend their concepts by the more recent mapping recruits Mike Carr, Don Elston, Hal Masursky, Jack McCauley, Dan Milton, Henry Moore, Spencer Titley of the University of Arizona, and myself.[73] The fourth early mapper, Chuck Marshall, a well-dressed but bohemian dropout from the working world, was probably also present but during the month ended his three-year association with Astrogeology to pursue art full time.

The conference was much concerned with a scheme for conveying age relations of lunar geologic units on geologic maps. In the early 1960s, after two-thirds of a century, the USGS followed Gilbert's lead in establishing the stratigraphic framework of the Moon. Gilbert had divided lunar stratigraphy into pre-Imbrium ("antediluvial"), Imbrium, and post-Imbrium ("postdiluvial") classes. Hackman and Mason, knowing that the Imbrium basin and Mare Imbrium differed in age, showed premare, mare, and postmare units. Most observers — even Urey — had realized that rayed craters are the Moon's youngest; and Shoemaker, while mapping the Copernicus region, had seen the rays of Copernicus crossing the (seemingly) nonrayed but also postmare crater Eratosthenes. Add Archimedes and the plains sandwiched between its deposits and the Imbrium basin and you have a quite complete sequence based just on these few observations.[74] Shoemaker, first on the Copernicus prototype map and then formally in the Shoemaker-Hackman paper, had attached *system* names to the divisions suggested by these relations: pre-Imbrian, Imbrian, Procellarian (the mare material), Eratosthenian, and the youngest, Copernican.

Until 1962 or 1963, most people thought that the maria were formed essentially simultaneously. Mare synchrony became embedded in the Shoemaker-Hackman scheme, and all maria were assigned to the Procellarian System. At the stratigraphic shootout several geologists pointed out that some mare flows called Procellarian are younger than some craters called Eratosthenian. Since systems cannot overlap, something had to go. The conferees decided that what went would be the Procellarian System. Henceforth mare units were assigned to whatever system (Copernican, Eratosthenian, Imbrian) their stratigraphic relations indicated they belonged to.[75] By the end of 1963, therefore, the basic lunar stratigraphic scheme was open for business. Two changes in nomenclature but none in concept have been made since.[76]

Some people love nomenclature and others don't bother with it.[77] I think it has been a necessary tool in lunar geology, and the sharper the tool, the better. Consider the "hummocky material that surrounds the Imbrium basin." You can't call it by that mouthful every time you mention it. "Regional material" is not much better because you always have to explain *what* regional material. Interpretive names are no good for planetary geologic units because of the many uncertainties involved, so "Imbrium ejecta" was out. Nor should planetary geologic units be identified one to one with a time-stratigraphic unit such as "Apenninian Series," because they may turn out to have a wider age range. All this was thrashed out at the meeting. Under the stratigraphic code, physical, material units are given *formational* names or descriptions that match their objectively observed properties. To find a formational name for the "Apenninian" we looked at a lunar map and found the crater Fra Mauro near the most typical hummocks. Dick Eggleton carefully and objectively described the unit in his assigned quadrangle, Riphaeus Mountains, and documented the basis for the name Fra Mauro Formation.[78]

Another unit destined for fame was named at the conference: the light plains deposit sandwiched stratigraphically between Archimedes and the Imbrium basin and forming most of what Hackman informally called the Apennine Bench. Hackman had concentrated his work on the Apennine region with a view toward publishing another 1:1,000,000-scale geologic map.[79] He deferred to Shoemaker so completely as to write his reports in exactly the same words as Shoemaker's except for feature names that fit his own study area. Nevertheless, as I recall, he agreed with Shoemaker that the plains were composed of impact material of Imbrium, and not the volcanic rock most of the rest of us preferred. Hypotheses for the origin of the Apennine Bench Formation continued to waver between impact (specifically, impact melt) and volcanic even after pieces of it turned up in the Apollo 15 sample suite.

I remember another conversation about light plains from the conference. Shoemaker and Eggleton were arguing that the light plains in the crater Ptolemaeus consisted of Imbrium ejecta or marelike material covered by same. A rebellious faction, including myself, was arguing for volcanism of the whole Ptolemaeus fill, stressing the clear transection by the plains of the Imbrium sculpture that cuts the rim of Ptolemaeus. We maintained that geologists should always pay more heed to stratigraphic relations as obvious as this one than to some model of feature origin, and that the obsession with Imbrium and impact was getting ridiculous; consider the great role of volcanism in shaping Earth. Later chapters show that comparisons with Earth's geologic style, though inevitable, have proved to be treacherous guides to the Moon.