I

A Quiet Prelude

1892–1957

GILBERT

This history could begin any time after the first human beings discerned a man, a maiden, or a rabbit on the Moon's face, but let us skip all the early studies that had only a peripheral influence on lunar geology in the Space Age.[1] Our modern line of inquiry began in August 1892.

People seem to need heroes, so let us consider Grove Karl Gilbert (1843–1918) of the U.S. Geological Survey. Gilbert was surely one of the greatest geologists who ever lived, and his genius touched almost all aspects of the science: geomorphology, glaciology, sedimentation, structure, hydrology, and geophysics.[2] He was in Berkeley in April 1906 when he awoke early one morning "with unalloyed pleasure" at realizing that a vigorous earthquake was in progress, and he caught the first available ferry to San Francisco. He carefully recorded how long the subsequent fire took to consume the wooden buildings on Russian Hill (where I now live) and contributed major parts of the subsequent official report. His personality seems to have been mild and subdued, even "saintlike," in an era of rough-hewn and feisty pioneers of western geology. His recent biographer Stephen Pyne has applied to him the same term Gilbert applied to the Geological Survey: a great engine of research.[3]

In 1891, while chief geologist of the Survey (the insider's term for the USGS), Gilbert was attracted by reports of large amounts of meteoritic iron, the Canyon Diablo meteorites, around a crater in Arizona then called Coon Mountain or Coon Butte.[4] Apparently he had already been thinking about the possible impact origin of lunar craters, and he alone realized that the Coon crater might itself be a "scar produced on the earth by the collision of a star." If so, a large iron meteorite might lie buried beneath the crater. He reasoned that such a body should (1) show up magnetically at the surface and (2) displace such a large volume that the ejecta of the crater should be more voluminous than its interior.

He tested both ideas and got negative results. In October 1891 he and his assistants carefully surveyed the volumes of the ejecta and of the crater and found them to be identical at 82 million cubic yards (63 million m^3). Their magnetic instruments showed no deflections whatsoever between the rim and the interior. Gilbert reluctantly concluded that the crater was formed by a steam explosion; that is, it was a *maar.* There the matter appeared to rest for a while.

But he was not ready to give up on impacts. Calling himself temporarily a selenologist, he observed the Moon visually for 18 nights in August, September, and October 1892 with the 67-cm refracting telescope of the Naval Observatory in Washington. A member of Congress assessed this activity and Gilbert's parent organization as follows: "So useless has the Survey become that one of its most distinguished members has no better way to employ his time than to sit up all night gaping at the Moon."[5] But those 18 nights left a tremendous legacy. The use to which Gilbert put them shows that the quality of scientific research depends first and foremost on the quality of the scientist's mind. It was not lack of data that led others of the time to so many erroneous conclusions.

Gilbert presented his conclusions in a paper titled "The Moon's Face," the first in the history of lunar geoscience with a modern ring.[6] He knew he was not the first to suggest an impact origin for lunar craters; he mentioned Proctor, A. Meydenbauer, and "Asterios," the pseudonym for two Germans.[7] Apparently, however, he was the first to adduce solid scientific arguments favoring impact for almost all lunar craters from the smallest to the largest—"phases of a single type" as he put it. Most earlier observers had seen the trees but not the forest: the subtypes but not the overall unity of form. Gilbert's contemporary, Nathaniel Southgate Shaler (1841–1906) recognized the unity of origin but got the origin wrong.[8] Now, almost everything fit. Gilbert's sketches, descriptions, and interpretations could be used in a modern textbook. He knew that the inner terraces of craters formed by landslip. He wrote that the depression of lunar crater floors below the level of the surrounding "outer plain" made them totally unlike most terrestrial volcanoes. He noted that central peaks are common in craters of medium size but not in those smaller than about 20 km across and rarely in those larger than 150 km; but this is a regular relation and does not destroy the basic unity of form. The peaks lie below the crater rim and even mostly below the outer plain, unlike cones of terrestrial volcanoes of the Vesuvius type. The volcanic-collapse craters (*calderas*) of Hawaii were a somewhat better match, as others had said, but Gilbert listed enough dissimilarities to damn this comparison as well. He pointed out that the largest lunar craters (including those we call basins) far exceed the largest terrestrial craters in size. In his words, "volcanoes appear to have a definite size limit, while lunar craters do not. Form differences effectually bar from consideration all volcanic action involving the extensive eruption of lavas."

What Gilbert called "meteoric" theories fit the craters' sizes and forms much better. Impacts could have created the raised, complexly structured rim-flank deposits that he called "wreaths," the low floors, and even the central peaks, which he surmised were formed when material responded to the impact by flowing toward the center from all sides. He realized that impacts would weaken lunar materials to the point of plasticity (hence the peaks) and could melt them (hence the flat floors). His conclusions were based partly on simple experiments with projectiles and targets composed of everyday materials. He so completely accepted the origin of the "white" *rays* which radiate from many craters as splashes from impacts that "it is difficult to understand why the idea that they really are splashes has not sooner found its way into the moon's literature."

One property of lunar craters stopped him: their circularity. If objects coming from all directions in space had created the craters, why were there not more elliptical craters? His experiments showed that many of them should be. Therefore he launched upon a long quantitative argument, which led to the idea that "moonlets" must have rained down from a Saturn-like ring. Even heroes have their Achilles' heels. As so often in science, he adjusted his calculations to fit his concepts. Later we will see that he was missing a critical fact that misled him into thinking that low-angle impacts would dig elongated scars.

Gilbert apparently was the first to be impressed by an extensive system of "grooves or furrows" and parallel ridges that he called *sculpture.* He certainly was the first to interpret the sculpture correctly. When he plotted the trends he found that they "converge toward a point near the middle of the plain called Mare Imbrium, although none of them enter that plain." His conclusion ushered in the investigation of lunar impact basins. "These and allied facts, taken together, indicate that a collision of exceptional importance occurred in the Mare Imbrium, and that one of its results was the violent dispersion in all directions of a deluge of material—solid, pasty, and liquid." Solid fragments thrown out of Imbrium gouged the furrows (visible in frontispiece, center).

By "liquid ejecta" Gilbert meant that the Imbrium "catastrophe" formed the maria peripheral to Imbrium, such as Sinus Roris, Mare Frigoris, Mare Tranquillitatis, and even distant Mare Nubium (see frontispiece); Oceanus Procellarum "may have been created at the same time or may have been merely modified by this flood." This error was universal before the late 1950s, but it is surprising coming from Gilbert because the distinction between the maria and the basin yields so readily to stratigraphic analysis, as I will show (see chapter 3). Although he subordinated stratigraphy to physical processes in his terrestrial research,[9] Gilbert was the first lunar stratigrapher. He classed lunar features around Mare Imbrium into the sharply distinct categories "antediluvial" or "postdiluvial" according to their relation to the sculpture and these maria. He recognized a more gradational series of ages among the "honeycomb" of densely packed craters in the southern highlands beyond the reach of the sculpture. This "untouched" area, he thought, "probably represents the general condition of the surface previous to the Imbrian event." And so it does.

He was not an impact fanatic, however. He gave credence to the volcanic camp by calling attention to the similarity of small lunar craters to maars, which also have depressed floors. In his words, "limited use may be found for the maar phase of volcanic action in case no other theory proves broad enough for all the phenomena." Possibly Coon Mountain was still on his mind, but, strangely, he did not mention it in his lunar paper. Gilbert started with two working hypotheses, impact and volcanic, but was drawn inexorably to the former as more and more observations fit the impact theory and fewer and fewer the volcanic.

Which brings us back all too briefly to the fascinating story of Gilbert at Arizona's Coon Mountain.[10] His report of his investigation of the crater, published five years after he performed it, is a model of scientific inquiry that is more concerned with methods and the reasoning process than with results.[11] The report's title, "The Origin of Hypotheses, Illustrated by the Discussion of a Topographic Problem," does not even mention the crater, and, also strangely, the text does not mention "The Moon's Face." At Coon Mountain Gilbert quantitatively tested two working hypotheses according to the theory available to him and felt forced to accept the volcanic one against his deepest instincts. He had to conclude that the Canyon Diablo meteorites fell near the crater by coincidence. In retrospect we might say he should have trusted his intuition more than the facts as he knew them. As one who disparages blind reliance on quantitative modeling in science, I feel a certain satisfaction that he was much more successful when "gaping" than when calculating or measuring. But more commonly, scientists become so emotionally involved with their brainchildren that they defend them to the death. We will encounter others whose names begin with *G* who relied so fanatically on their intuition that they became blind to the facts. On balance, Gilbert's calm and careful objectivity is better.

INTERLUDE

For the next half century, only a few geologists or astronomers thought about the Moon at all, and most of those still favored origins of the lunar craters as calderas, "bubbles" formed by bursts of steam or volcanic gas, or ramparts built up when Earth tides kneaded the Moon's crust. In the United States, the Carnegie Institution of Washington, D.C., formed a high-level committee of astronomers and geoscientists to ponder the Moon between 1925 and the outbreak of the Second World War. Although this "Moon committee" dealt with lunar polarization and other surficial properties, made good photographic globes of the Moon, and generally kept track of lunar research, it worked only intermittently and does not seem to have broken through any scientific barriers.[12]

Two intertwined developments during the interlude began to whittle away at the majority endogenic view of crater origin.[13] One came from the intense scrutiny to which the Coon Mountain crater was subjected in the course of mining entrepreneur Daniel Moreau Barringer's (1860–1929) single-minded search for the large meteorite that he was certain had formed the 1.2-km-wide crater and which would yield a fortune in iron, nickel, platinum, and iridium. Barringer heard about the crater and the small nearby iron meteorites in 1902, began the search in 1903, and continued steadily at first, and intermittently later, to drive shafts and drill holes at ruinous cost until his death in 1929. He was an able and observant man, but he was obsessed by the crater. He refused to listen to any evidence against the impact origin or his belief that the impactor was still sufficiently intact to be minable. His obsession pressured others to examine carefully the nature of the impact process and eventually to find proof that Meteor Crater — Coon Mountain's name since 1907 or 1908 — was indeed formed by the impact of a meteorite. When they did, their findings proclaimed that large meteorites (1) do exist, (2) can create large craters on Earth, and (3) should be reexamined as the cause of lunar craters.

The emerging truth was less kind to Barringer's hopes for the condition of the meteorite. The other development in cratering was a new understanding of how violent cosmic impacts are. Interestingly, the often-wrong Shaler realized that a cosmic projectile would release enormous energy on impact and would itself be vaporized, although he did not realize that lunar craters manifested the results. Gilbert groped for an explanation for the circularity of lunar craters and rejected an impact origin for Meteor Crater because he did not know about the energies. Now, some of Barringer's associates and prestigious consultants were closing in on the truth that would damn the mining project.

Apparently, however, the first person who grasped the full implications of cosmic impact and performed the relevant calculations, in 1916, was the Estonian astronomer Ernst Julius Öpik (1893–1985), whose life and career were as rich as they were long.[14] This versatile scientist pioneered the study of the masses and orbits of the Solar System's meteoroids, asteroids, and comets, and was the first to show that their mass distribution is reflected in the size distribution of the lunar and planetary craters they have created. He predicted the craters on Mars and the existence of the Oort cloud of comets, and made original contributions to many nonplanetary astronomical subjects as well. He worked mostly in isolation, and his early papers, written in Estonian or Russian and published in obscure journals, were not rediscovered by the world at large until after Ralph Baldwin's first book was published in 1949.[15]

Obscurity was also the fate of similar insights reached in 1919 by American physicist Herbert Eugene Ives, who realized that a meteorite striking the Moon would be "a very efficient bomb."[16] To Ives, the similarities to lunar craters of experimental bomb craters at Langley Field, Virginia, "largely speak for themselves." In particular, the bomb craters' central peaks, which "formed apparently by a species of rebound," resembled not only the larger lunar peaks but also smaller ones that appeared in pellet and bullet experiments. Another impact advocate was German meteorologist and geophysicist Alfred Lothar Wegener (1880–1930), who knew and admired Gilbert's work, performed similar impact experiments, and added the crazy impact idea to his then even crazier one that the continents had drifted.[17]

Historically, however, the honor of bringing the discovery to the world has belonged mainly to New Zealander Algernon Charles Gifford (1861–1948). "Uncle Charley" Gifford had picked up the idea from someone else — the history of science makes one wonder if *any* ideas are truly original — but he developed it in essentially its modern form and wrote it up clearly and explicitly, starting in 1924.[18] Öpik, Ives, Gifford, and then other astronomers and physicists all pointed out that because of their enormous energies,[19] cosmic objects are much smaller than the craters they create on impact; they blast out circular craters almost regardless of their impact angle; and they are themselves almost completely dispersed or vaporized in the target rock and crater ejecta. Barringer was right in his belief that a meteorite had made the crater but very wrong in his hope that it had survived partly intact.[20]

As far as I know, the word *stratigraphy* was not applied to lunar studies during the interlude. In a paper written in 1917 and published posthumously in 1924, however, noted geologist Joseph Barrell (1869–1919) used a favorite word of stratigraphers, *superposition*, to describe relations among craters (which he assumed to be volcanic) that indicate their relative ages.[21] Barrell, who knew Gilbert, also recognized the age significance of the progressive reduction in slopes of the old craters and the rays around the young. Barrell tossed off these concepts and the relative youth of the maria (lavas) compared with the "chaotic upland surface" as if they were self-evident. He used the relation between the melted rock of the maria and the unmelted, heavily impacted older uplands to support his contention that Earth's ocean basins also formed at the expense of the continents when the continents foundered.

Before the Second World War, Meteor Crater was joined in the ranks of definite meteorite craters—all with associated meteorites—by one crater group on each continent.[22] In 1936 meteoriticist Harvey Harlow Nininger (1887–1986 [!]) made another connection between Earth and Moon that was to significantly influence lunar geology in the Space Age. He suggested that *tektites*, small glassy objects that evidently were shaped by high-speed flight through the atmosphere, were ejected by impacts on the Moon and hurled through Earth's atmosphere.[23]

At the same time, American geologists John Boon and Claude Albritton broke entirely new ground. Again, earthshaking discoveries were published in obscure journals, this time the geologic journal of Southern Methodist University, *Field and Laboratory.*[24] Their contribution concerned not the familiar cup or rim but the underpinnings of craters. They knew that rock would not only be deformed by the shock of an impact but would react violently when the shock had passed. Rebounded central peaks were one result, and another was chaotically broken-up rock beneath the peak and the crater floor. Such chaos characterized peculiar features that the influential geologist Walter Herman Bucher (1888–1965) had called "cryptovolcanic" on the assumption that they were created by subsurface volcanic explosions.

In 1937 a Mount Wilson photograph of the Moon was inspiring another geologist, Josiah Edward Spurr (1870–1950). Spurr was a mining geologist with vast experience in many corners of the world and a strong streak of independence, presumably stemming from his New England *Mayflower* origins. His biographer and ardent admirer Jack Green stated that his background gave Spurr "common sense" and "geological foresight" and resulted in a view of the Moon that was "mostly right" and "refreshing."[25] But I can state dispassionately, with all due scientific objectivity, that Spurr and Green were mostly wrong. Spurr's systematic, minute, and independent examination of the Moon's features generated four privately published volumes under the overall title *Geology Applied to Selenology* and dated between 1944 and 1949 that unfortunately gained considerable influence in the small world of lunar observers. A ruling prejudice underlay Spurr's work (and not only his): that the Moon was a little Earth and could be described in terrestrial terms. He dismissed the impact theory in short addenda to two of his volumes and concluded, or assumed at the outset, that all lunar features were created by endogenic melting and fracturing triggered when the Moon was captured by the Earth. He is usually given credit (or blame) for originating the concept of the *lunar grid,* a threefold set of lineaments (N-S, NE-SW, NW-SE) conforming to simple models of how solids deform under stress.

This book will have much to say about the grid, little of it favorable. Let us give G. K. Gilbert a posthumous last word about Spurr: courteous in public, in private Gilbert considered Spurr a virtual crackpot.[26]

Not everyone blundered so badly in mid-century. Two papers dated 1946, between the publication of Spurr's second and third volumes, provide relief from his tedious ramblings. The first to appear was by Harvard professor emeritus of geology Reginald Aldworth Daly (1871–1957).[27] Refuting geologists with endogenic views, this great geologist cited Gilbert in support of his own advocacy of impact — which he believed to be consistent with a fascinating impact mechanism for the origin of the Moon itself (described in chapter 18 of this volume).

The second prescient paper by a ground-breaking geologist was by Robert Sinclair Dietz (b. 1914), who also cited Gilbert's work but added more of his own observations than did Daly. Dietz listed eight properties of lunar craters that distinguish them from terrestrial volcanic craters and drew the obvious conclusion, which somehow escaped so many others, that these differences indicate nonvolcanic origins. To drive home this point he picked on two longtime favorites of the endogenists. The first was circularity and radial symmetry: Dietz was aware that it is *volcanic* craters that are elliptical or asymmetrical. The second item was the central peaks. Even Lick Observatory Director and University of California President William Wallace Campbell (1862–1938), who agreed with the impact origin of Meteor Crater, thought that the craterlets that appeared to be centered on lunar central peaks were fatal to the impact theory. Dietz anticipated later findings from Lunar Orbiter photographs: the "craterlets" are merely the effects of shadows cast by parts of the peaks, which, in a large crater, cluster around a depression as do the points of a molar tooth.

Dietz's 1946 paper includes other modern interpretations too numerous to mention here; however, he repeated the standard error of equating a circular structure that contains a lunar mare with the mare itself, thinking they were created by the same impact. Dietz's interest in terrestrial craters and the Moon continued, but he did not contribute further papers directly pertaining to the Moon.

Now let us meet a man who for half a century has looked up, down, and all around him for clues to the origin of the Moon's features, the man who introduced lunar science to the twentieth century.

BALDWIN

Ralph Belknap Baldwin (b. 1912), astrophysicist by education, industrialist by profession, and versatile lunar scientist by avocation, constructed in almost complete solitude what hindsight clearly shows was the most nearly correct early model of the Moon.[28] Baldwin, a big man physically as well as mentally, represents the can-do midwestern work ethic that has contributed an oversize share of America's inventiveness. He has always been able to focus totally on one of his many interests in science, the family business (Oliver Machinery Company of Grand Rapids, Michigan), education, history, woodworking, athletics, or raising two sons and a daughter, until switching with equal intensity to another interest or coming deliberately out of focus to relax.[29] This Newton-like ability to concentrate on diverse subjects is abundantly evident in his lunar publications. Baldwin proved himself able to function as a geologist, geophysicist, and geochemist as well as astronomer and physicist.

He first appeared on the lunar scene in two papers written in 1942 for *Popular Astronomy.*[30] Well, they weren't exactly written *for Popular Astronomy.* He actually wrote them for the major astronomical or astrophysical journals but they were rejected. Baldwin became interested in the Moon one day when he was killing time in the halls of the Adler Planetarium in Chicago while waiting to lecture.[31] Viewing the photographic transparencies on public display, he noticed the linear grooves that Gilbert had called sculpture and wondered what they might be. He found no explanation in the literature that made any sense, for he did not encounter Gilbert's paper. He therefore worked out the sculpture's origin on his own and arrived at a conclusion similar to Gilbert's: "Mare Imbrium," too big to be volcanic, was formed by an explosion, and these grooves and ridges "were caused by material ejected radially from the point of explosion." In the second paper Baldwin added that the impactor was flattened by shock and thus excavated the cavity laterally—a very sophisticated conclusion that explains, among other phenomena, why sculpture close to an impact point consists of grooves and not crater chains. But when he originally submitted these findings he met rejection. The journals' editors did not consider the Moon a serious subject for astronomers. Their attitude infuriated Baldwin and made him resolve to bear down on the Moon. He found that he had the luck, almost unprecedented in the twentieth century, to have a major subject of scientific inquiry all to himself.

During and after wartime service helping to devise the proximity fuze,[32] he prepared a book-length synthesis of his lunar observations, experiments, and literature search. The result is one of the landmarks of lunar literature and probably the most influential book ever written in lunar science, *The Face of the Moon.* The book opened the modern era of lunar studies when it was published in 1949. The Moon, like any planet, is the sum of diverse parts. Before the Space Age only Baldwin considered and integrated them all, extracting one secret after another from each by means of his unrelenting logic.

Only shortly before press time did he become aware of Gilbert's work, which Reginald Daly called to his attention in the course of asking Baldwin for reprints of his 1942 and 1943 papers.[33] Nevertheless, many of Baldwin's conclusions were the same as Gilbert's. A prime example is the sculpture, "a series of formations which has been noticeably avoided by early selenographers."[34] The astronomer, like the geologist, also realized that almost all craters are fundamentally similar despite differences in morphology related to size and age. Baldwin knew that if craters were formed by impacts, the Moon should possess big ones as well as the obvious ones smaller than 450 km across, because large as well as small potential impactors are abundant in the Solar System. The big craters were not obvious — unless they were what we call basins and he and everybody else then called maria or seas. He actually found more differences than similarities with craters, but he was saved by the sculpture. These valleys with raised borders "clearly identify the great, round seas as being the centers of explosions so mighty as to dwarf the crater-forming blasts into insignificance." Although most of the valleys "point accusingly toward Mare Imbrium," he added a number of other basins to Gilbert's Imbrium. Both Gilbert and he also knew that the rays were created by crater ejecta rather than some endogenic agency like "gas emanations" from "cracks."

It was Baldwin who championed the concept that craters were formed by great explosions caused by impacts — a fundamental, course-altering contribution that William Hoyt rightly called Baldwin's "manifesto."[35] He had not encountered the work of Öpik when he wrote *The Face of the Moon,* and he credited Gifford with discovering the explosive effects and realizing that they would create circular craters. He himself was well on the way to working out the physics of the impact process. His observations of military ordnance showed that the higher the velocity of impact, the quicker will the projectile be decelerated and the energy released. The result is a near-surface burst. He suggested that the volcanic hypothesis became popular because sharp, dark shadows make craters look much deeper than they really are. He was aware of the same terrestrial meteorite craters as Dietz (whose 1946 *Journal of Geology* paper he had overlooked while researching *The Face of the Moon*) and added some additional ones. He also reviewed the properties of some older supposedly cryptovolcanic structures that he, like Dietz, knew had the right properties to have been formed by impacts.

Some endogenists had worried about the great size of lunar craters but rationalized it because the Moon's surface gravity is one-sixth that of Earth. Baldwin showed that although the lower gravity would allow explosive ejecta to fly farther, it would have only a minor influence on the size of the pit. The nearly random distribution of craters within a given terrain, which Shaler missed but Baldwin carefully tested and demonstrated, was more consistent with impact than volcanism.

But the single item in *The Face of the Moon* that most convincingly demonstrated the impact origin of lunar craters to others was a logarithmic plot showing a regular relation between diameters and depths of terrestrial explosion craters, terrestrial meteorite craters, and fresh, nonshallowed (referred to as class 1) lunar craters.[36] The plot represents a great cache of research and insight. He compiled 300 measurements of lunar-crater dimensions and made 29 more himself. Like Ives after an earlier war, he made use of bomb and shell craters to add the properties of this intermediate size range. Only four terrestrial impact craters were applicable, but they nicely filled the gap between the military and lunar craters. Most lunar craters were formed by the "impact and sudden halting of large meteorites," period.[37]

Most but not all. About some small, low-rimmed craters he stated: "There does not seem to be any question but that they are volcanic blowholes of some kind."[38] Five dark spots in Alphonsus and a crater chain between Davy and Alphonsus were on his list of volcanic craters—as they were later on the lists of people who were looking for landing sites for Apollo astronauts. Thus he was (unknowingly) agreeing with Gilbert that all craters need not have the same origin. The trouble is, both men were thinking mainly of the same conspicuous chain that meanders north-south between Copernicus and Eratosthenes. This book will have more to say about the Davy Rille and this other chain, now known as Rima Stadius I.

Another, especially fortunate, parallel between astronomer Baldwin and geologist Gilbert is that both thought in terms of relative age.[39] An excellent example of this happy leaning clearly demonstrates the power of lunar stratigraphic analysis: "The lava flow which has covered so much of the floor [of Mare Serenitatis] is of later vintage than the [Imbrium radial] grooves and valleys in the Haemus Mountains."[40] These relations establish a threefold sequence: (1) Serenitatis basin, (2) Imbrium basin, (3) Mare Serenitatis; therefore Mare Serenitatis did not form when its basin did. Also, Mare Nectaris is known to be younger than the Nectaris basin because the crater "Fracastorius was a later occurrence than the primary cavity of Mare Nectaris as is shown by its superposition, and yet the crater is filled with the once molten rock" (21° S, 33° E, frontispiece). Baldwin's perception of age relations also led him to state that the great "chains" of supposedly related large craters so beloved by the endogenists are "composed of craters of widely different ages."[41]

Like Gilbert, Baldwin thought in 1949 that the lava of Mare Nectaris and the mare patches that cover parts of the sculpture were liquid ejecta from the Imbrium explosion. Although he recognized historical sequences and a delayed filling of Mare Imbrium, he still envisioned a unified origin of the Imbrium basin and the maria. He pictured the first response to the Imbrium impact as a massive dome 800 miles across that stayed elevated long enough for meteorites to form Archimedes and the crater that encloses Sinus Iridum.[42] The dome then settled, creating ring faults along the front of the Apennines, the mountains that border the mare. Then the "superheated magma welled and bubbled up" burying "the moon's greatest crater . . . then burst its bonds . . . and spread out rapidly to produce" the other maria. He was wrong about this origin of the maria but right that the mare lavas were very fluid.

Baldwin also carefully considered the astronomical subject of the Moon's global shape (*figure*). Astronomers had carefully measured the Moon's *librations* — the real (physical) and apparent (optical) wobbles that enable earthbound observers to peer a little around the edges (*limbs*) and see at different times a total of 59% instead of only half of the Moon's surface. They found what seemed to be a bulge facing Earth, although they were never sure whether this was a real bulge in the Moon's figure or some internal distribution of densities that had the same effect on the librations.[43] Baldwin tried to find out. According to Gilbert Fielder, Baldwin was only the second (after William Pickering) separately to measure the departure from sphericity of the "continents" and "seas," and the first to do it well.[44] He concluded that the present lunar figure "bulged" toward Earth much more than it would if the Moon pliably adapted only to its present centrifugal forces and the present gravitational pull of Earth. Thus he agreed with the authoritative Cambridge geophysicist Sir Harold Jeffreys (1891–1989) that the bulge was a *fossil tidal bulge* acquired when the Moon was closer to the Earth than it is now and the Moon's outer materials were weaker than they are now. His measurements showed that the maria and the uplands have the same overall curvature and bulges. Since the uplands are heavily cratered and obviously ancient, the maria must also be ancient. Since the maria are *relatively* young, ergo, the Moon's entire surface is ancient. Beginning in late 1959, Baldwin spent a year in his basement measuring points on glass photographic plates to refine his measurements of the figure and create a new contour map of the Moon.[45]

Baldwin's interest in the strength of the bulge also led him into a lifelong interest in the dimensions of lunar craters. Originally deep craters become shallow with time because they "attempt" to restore the condition of mass balance that existed before the impact, a condition known as *isostasy* ("equal standing"). The cavity created by the impact disturbs the isostatic balance by abruptly taking away mass, and planetary crusts do not like such imbalances; they like each vertical column to exert the same pressure on some depth chosen by geophysicists. The ability of a mass of rock to achieve isostasy is dependent on its viscosity and the time available. Baldwin's results showed that craters, and therefore the bulge, could and did adjust, but only very slowly.

The Face of the Moon did not sell well despite the publisher's ploy of tacking one year onto its actual completion date of 1948 to make it seem more up-to-date.

But it had some important readers. It had an instantaneous effect on a Nobel chemist and on an equally brilliant geologist, both of whom would shape the course followed by the exploration of the rocky Moon.

UREY

Chemist Harold Clayton Urey (1893–1981) devoured Baldwin's book during a train trip to Canada or in the midst of a scientific gathering, according to different versions of the story.[46] Chemist Sam Epstein, Urey's colleague at the University of Chicago between 1947 and 1952, says that the Moon totally consumed Urey's interest for years.[47] His reading of *The Face of the Moon* started a chain of events that eventually led to the choice of the Moon as America's main goal in space.

Urey's interest in the Moon was based less on any interest in explaining this or that surface feature than on his belief that it is a piece of the primordial Solar System, probably older than Earth and captured by Earth. He enthusiastically accepted Baldwin's impact interpretation of the craters, and furthermore thought that only the rayed craters were much younger than the Moon itself. Urey's own original interpretations used basic scientific principles to make deductions from a few hard facts, such as the existence of the bulge and other irregularities in the Moon's shape. Because these are incompatible with the forces presently acting on the Moon, they must be old, as Baldwin also thought. If they are old, the Moon's material cannot be pliable (contrary to what Baldwin thought). Therefore it is cold; the Moon formed by accretion of cold objects and stayed cold. High mountains like the Apennines could not be supported by a weak, warm crust. Therefore the mare lavas (and he called them that despite his antivolcanic stance) must have been produced by impacts, not by internal melting.

Urey published his meditations in two long works with similar content within 3 years of reading Baldwin's book, and he repeated the same ideas several more times over the next 15 years.[48] His interpretations were very influential because of his status and his enthusiasm for the Moon. Some of his interpretations were right for the right reasons; for instance, the impact origin of lunar craters. Others were nonsensical; for example, that Sinus Iridum (45° N, 32° W, frontispiece) marks the entry hole of the body that created Mare Imbrium, or that nickel-iron projectiles were required to gouge some of the Imbrium radial grooves. Others that seemed reasonable in the 1950s, such as the incompatibility of the strong lunar crust with volcanism, turned out to be wrong. He explained away Baldwin's recognition of the age series Serenitatis rim–Imbrium radials–Mare Serenitatis with the "obvious explanation" that Mare Serenitatis "was still molten when the Imbrium collision occurred."[49]

During most of the 1960s Urey clashed head-on with geologists and other "second-rate scientists" (his phrase) because most of us were not "selenologists," knew little basic science, and had published little about the Moon. However, he admired Eugene Shoemaker and the long-ignored Gilbert (whose 18 nights of observing, he realized, came between his own conception and birth), and admonished others henceforth to pay more attention to prior work "as is [the practice] in other fields of science."[50] And when lunar exploration finally proved wrong his theories about lunar volcanism, history, and composition, he accepted reality and became friendly with some of us "interlopers." This book will have occasion to contrast Urey's latter-day flexibility and graciousness with the hardheadedness of some of his contemporaries.

SHOEMAKER

Enter the central character in our drama, geologist Eugene Merle Shoemaker (b. 1928).[51] Thirty-five years younger than Urey and 16 younger than Baldwin, he nevertheless seems to have become fascinated with the Moon at least a year sooner than Urey did and only 6 years after Baldwin did. He hurried under wartime pressure through high school (Fairfax in Hollywood, which I attended less hurriedly) and then Caltech, where he graduated in 1947, got his master's degree in 1948, served as cheerleader (unsurprising to those who know this effervescent man), and met his roommate's sister Carolyn, who later became his wife. After a pause to catch his breath he joined the USGS at the tender age of 20 to work with the uranium-vanadium deposits of the Colorado Plateau. Among his sources for news of the outside world was the Caltech newspaper, which carried items about its affiliate, the Jet Propulsion Laboratory (JPL). Therein he learned of the experiments being conducted at White Sands, New Mexico, with the V-2 rockets salvaged from Germany. In search of a postwar reason for its existence, JPL had stuck a second stage on the V-2s. Shoemaker tells us that on his way to breakfast one fine summer day in 1948, he thought, "Why, we're going to explore space, and I want to be part of it! The Moon is made of rock, so geologists are the logical ones to go there — me, for example!" Of course, he had to keep this crazy idea to himself. But he never afterward deviated from his ambition to personally perform geologic fieldwork on the Moon, until he was disqualified by Addison's disease in 1963. Shoemaker's 1948 vision led directly to the lunar fieldwork carried out two decades later by another geologist and a group of 11 geologically trained astronauts.

The following year Shoemaker intensively combed the existing lunar literature. In 1949 most of it was nonsense, with the conspicuous exceptions of "The Moon's Face," by Gilbert, and the newly published *Face of the Moon,* by Baldwin.

Thus, right at the beginning of his lunar studies, he was exposed to two leading advocates of the impact theory. I have no doubt that eventually he would have arrived at similar conclusions on his own, but even geniuses see farthest if, as Newton said, they stand on the shoulders of giants.

There was no stopping him now. This book follows his career in considerable detail to show how geology became an integral part of the American lunar program and to illustrate how a shrewd and motivated person can seize opportunities. The first of these was a chance to study the *diatreme* volcanoes of the Hopi Buttes on the Colorado Plateau, which erupt at the surface through maars.[52] Because maars have low rims and depressed floors and are commonly aligned in chains or rows, Gilbert and Baldwin both thought that they resemble some of the smaller lunar craters. Shoemaker knew of these analogies, but how was he to study the Hopi Buttes maars without interfering with his Survey commitments? Well, the diatremes penetrate to great depth, and the material they eject through the maars is a mix of volcanic rock and all the rocks they traverse — which happened to include uranium-bearing lake beds relevant to his USGS work duties during those uranium boom days.

He next turned to nuclear bomb craters at the Nevada Test Site (NTS); specifically, to the craters Jangle U and Teapot Ess that were formed by shallow 1.2-kiloton explosions in late 1951 and March 1955, respectively. These too looked lunar, and the analogy was not coincidental: Baldwin had shown that impacts cause shallow bursts. In 1955 Shoemaker got the opportunity to map the NTS craters because a then-secret project to create plutonium by wrapping uranium around a buried nuclear bomb was being planned. He could predict where the plutonium would end up by tracing the rock that had been shocked and dispersed by the Jangle U and Teapot Ess explosions.

There was another, 10-times-larger, crater near the Hopi Buttes that looked like Jangle U and Teapot Ess and could not fail to attract Shoemaker's attention: Barringer's Meteor Crater. The Barringers still did not take kindly to people who did not believe that the crater was formed by a large meteorite. Unfortunately for USGS geologist Shoemaker, the worst of their enemies included USGS geologists G. K. Gilbert and N. H. Darton, both of whom were on record — Darton recently and insistently — as considering it a maar. Shoemaker himself once thought this might be its origin. Again, he found a way around an obstacle to his designs. He made the acquaintance of one "Major" L. F. Brady, who was at the Museum of Northern Arizona in Flagstaff after retiring as the headmaster of a school in Tempe attended by D. M. Barringer's sons. Shoemaker visited the crater with Brady, who then vouched for Shoemaker's acceptability to Moreau Barringer, Jr. ("Reau"). Thus began, in 1957, Shoemaker's close relation with the Barringers and his enormously productive investigation of Meteor Crater.

He did not intend to be the only lunar geologist. In 1956 he broached the possibility of involving the USGS in a program of lunar investigations—or one might say "reinvolving," considering Gilbert. Thoughts of the Moon and space travel were still considered a little weird, and he went with some trepidation to USGS Director Thomas B. Nolan to suggest a modest four-man effort. Nolan did not laugh, however, and sent Shoemaker to the visionary geologist William Rubey.[53] Rubey did not laugh either, and he checked whether anyone else in the Survey was doing lunar work. No one was. The way was clear in principle; but another series of fortuitous and well-exploited events had to occur before the first figurative spade could be turned in the new ground. Chapter 2 resumes the story of this initially one-man show that blossomed into a major program conducted by hundreds of scientists inside and outside the USGS.

KUIPER

Astronomer Gerard Peter (Gerrit Pieter) Kuiper (1905–1973) belongs on any list of principal figures of planetary science active before the Space Age.[54] We know from his fellow Dutch astronomer Bart Bok that Kuiper was already inclined to the planets in 1924, when the two men entered the University of Leiden together. On their first day, Kuiper told Bok that he would study the nature and origin of the Solar System, and so he did for most of his career.[55] He started with the "relatively simple" problem of gravitationally bound pairs of stars (binary stars), which were the subject of his Ph.D. dissertation at Leiden and of visual observations at Lick Observatory in the two years following his immigration to the United States in 1933. The list of his other major contributions to planetary astronomy is very long. He started to observe Solar System objects toward the end of 1943—that is, shortly after the start of Baldwin's lunar interest—and soon discovered the first example of a satellite with an atmosphere, Saturn's Titan. A nonastronomical achievement, inspired by the German invasion of his homeland, was to follow the American lines into Germany to find out what the Germans had done in rocketry (plenty) and atomic energy (nipped in the bud, fortunately). After the war he discovered carbon dioxide in the atmosphere of Mars, the Uranian satellite Miranda which proved so fascinating during the Voyager 2 flyby in January 1986, and the Neptunian satellite Nereid.

Kuiper almost single-handedly provided the thread of continuity in planetary astronomy during the long dry period in the 1940s and 1950s. He was the only respected astronomer in North America and one of the few in the world to pursue the subject full time. Planetary studies were generally frowned upon as uninteresting compared with the great astrophysical issues, stars and galaxies. Kuiper's positions as chairman of the University of Chicago astronomy department and director of the combined Yerkes and McDonald observatories (1947–1949 and again in 1957–1960) lent respect to planetology, as did, no doubt, his strong and authoritative (some would say authoritarian) personality. The list of his students who went on to careers in planetology includes Alan Binder, Dale Cruikshank, Bill Hartmann, Tobias Owen, Carl Sagan, and Charles Wood.

Baldwin tells us that Kuiper felt he knew too little about the Moon to referee *The Face of the Moon* when asked to do so in 1948 (Solar System astronomer Fred Whipple of the Harvard College Observatory did it instead). In 1953, however, he turned in earnest to the Moon. The Moon interested him because so little was agreed about the origin of its features and because it contains, he supposed, a record of the early Solar System. So far so good; this use of the Moon was and still is widely appreciated by astronomers. But he violated one of modern astronomy's strongest unspoken taboos by observing the Moon visually. Bad enough that he had looked with his own eyes through major telescopes at binary stars and planets — but the *Moon?* The source of this heresy seems to have been his pride in his great visual acuity. As director of McDonald Observatory he could get away with mounting a binocular eyepiece on the 82-inch reflector, the world's third-largest telescope at the time, and he made a number of observations that led to later papers.

Kuiper's first paper with what we would call a geologic content was published in 1954, shortly after his first observations at McDonald. He led off with a startling summary of his conclusions: "the moon was nearly completely melted by its own radioactivity, some 0.5 to 1 billion years after its formation, and . . . the maria were formed during this epoch and . . . not, as has been supposed, primarily the result of melting caused by the impacts themselves." These conclusions were novel in their day. Kuiper allowed for both impact and internal generation of surface features, and his classification of them into "*premelting, maximum-melting,* and *postmelting* stages" is a fair though overly interpretive description of a stratigraphic classification relative to the maria.

But let us examine how he arrived at these prescient conclusions. Like a good quantitative-minded scientist, he based them on properties of the bulk Moon such as its irregular shape. However, he held the minority opinion that the irregularities were not inherited from tidal attractions by Earth but were created by large impacts on a molten Moon. Astronomers had measured the Moon's size (3,476 km diameter) and mass (1.2% of Earth's), and from this got its density, which I round off in my geologist's way to 3.3 g/cm^3. Kuiper argued that this was merely the average density and that the Moon need not be homogeneous, a correct conclusion supported by "the writer finds it difficult to see . . . ," a phrase that should raise a red warning flag in any scientific paper. Given a nonhomogeneous Moon, Kuiper assumed a core and a compensatory low-density crustal material that might be the silicic source of the tektites. Caltech investigators had recently found ages of 4.6 aeons for some meteorites and inferred that the Earth was almost as old.[56] Kuiper arbitrarily upped the age of the Solar System to 5 aeons and declared that the undecayed radioactivity at that time would have melted the Moon after it formed and sent the low-density rock to the surface "unless the composition . . . is very abnormal — for which there is no apparent reason." (Red flag, though true.) He pointed out that even some meteorites showed signs of differentiation caused by radiogenic heat.

Kuiper's careful telescopic observations led him to conclude that central peaks are volcanic. He was aware of the rebound model, "but, while one can visualize a rebound in a liquid or plastic, there seems to be no reason to suppose that a solid can act in this manner." By this remark he revealed his ignorance about the behavior of rock that Gilbert had understood 60 years earlier. The model-dependency of his conclusions made him think that the peaks formed only around the time the maria formed, which is not at all true.

Kuiper agreed with Gilbert and Baldwin about the origin of the sculpture, but he made the familiar error of confusing Mare Imbrium with the Imbrium basin. So, sculptured craters are "premelting"; true enough, but the "melting" had nothing to do with the sculpture. Some of his other statements about surface features also contain correct conclusions based on erroneous deductions. He concluded, correctly, that the maria differ in age, but based this conclusion, erroneously, on their different elevations. He believed that parts of the terrae (he called them continents) were primitive, a conclusion fraught with later consequences for landing-site selection; but he chose as primitive the least-cratered upland tracts, which are sparsely cratered because they are young. He correctly concluded that crater floors are isostatically compensated — that is, have become shallow à la Baldwin in the "attempt" to restore an even balance of mass — but based the conclusion on examples of floors that (in my opinion) are not uplifted.

No doubt the reader has noticed that this paper annoys me. Earlier I referred to its conclusions as startling. Neither "annoyed" nor "startled" adequately describes Urey's reaction to it. He unleashed a lengthy tirade against Kuiper[57] in which he exclaimed that "he has not observed anything markedly different from what has been previously observed." "In the fall of 1953 I remarked to Professor Kuiper . . . that the moon would not have melted, [showing that] I had already made Dr. Kuiper's calculations in regard to the melting of the moon." In regard to Kuiper's arguments that the equatorial bulges are not fossil tidal bulges, Urey made the good point that this is *exactly* what they would be if the Moon had been molten, for it would have adjusted perfectly to the Earth's gravity. "Kuiper's very brief discussion of this subject is at least internally inconsistent," he fumed. Furthermore, "it seems most improbable that any surface features of the moon acquired previous to complete melting would remain after this melting process, as he assumes" (red flag, but Urey is right). Urey also nailed Kuiper on the behavior of solids under high pressure; his suggestions about central peaks "are similar to Shaler's, which I studied and rejected five years ago." And so on to, "It would be a thankless task to review adequately this paper in all details."

Urey and Kuiper remained on hostile terms for many years. When I first read their arguments I was nauseated by the egoism and reliance on pseudoquantitative arguments by both parties, but I was a little more favorably inclined to Kuiper because he disliked only certain geologists, not the whole profession. But time and the facts have not been kind to Kuiper's first entry into lunar science.

His telescopic observing taught him that existing photographs and maps of the Moon were inadequate even as a base for recording observations. Thereby lies an important tale. He attended the Ninth Congress of the International Astronomical Union (IAU) in Dublin in August and September 1955 in his capacity as president of IAU Commission 16, Physical Observations of Planets and Satellites.[58] Urey's attack, published just before this, strengthened Kuiper's resolve to do something about the Moon. At the congress he asked for suggestions on how a new lunar atlas might be constructed. Here was sown the seed of the unique series of atlases that he and his colleagues eventually constructed with U.S. Air Force funding. The sole suggestion came from Ewen Adair Whitaker (b. 1922), who had been an astronomer at the Royal Observatory, Greenwich, for six years. This civilized, self-taught Englishman had an early interest in the Moon, starting in 1951 with the British Astronomical Association of amateur astronomers. He was and is skilled in all matters photographic and observational (except that he is partly color-blind). He and Kuiper were introduced at the IAU meeting, and their association led to Whitaker's work on the atlas, starting with a visit to Yerkes in October 1957.

At the meeting Whitaker also mentioned to Kuiper the interest in the Moon of another Briton who would contribute greatly to "Kuiper's" atlas and subsequent lunar cartography, the irascible Welshman David William Glyn ("Dai") Arthur (b. 1917). Arthur had served with the British army in North Africa in the Second World War. At the time of the Dublin meeting he was working as a photogrammetrist with the British Ordnance Survey (the British government's mapping agency). On the strength of this mention, before meeting Arthur, Kuiper asked him to write the selenography chapter for the fourth volume of his series *The Solar System.*[59] Like Whitaker, Arthur was largely self-taught, was a member of the British Astronomical Association, and became skilled in visual telescopic observations of the Moon — more so than his future boss judging by the written record.[60] Whitaker's and Arthur's lack of academic degrees neither bothered Kuiper nor kept them from one sophisticated achievement after the other.

Kuiper obtained start-up funding for the atlas from the National Science Foundation in April 1957 and a more substantial contract from the U.S. Air Force Cambridge Research Laboratory in Massachusetts in the fall of 1957. The air force contract enabled work to begin in earnest. Kuiper considered the atlas the first task in a long-term project: the establishment of an institute devoted to lunar and planetary studies.

So it was to be. We shall meet Kuiper, Whitaker, and Arthur again in chapter 2 as the atlas work continues and Kuiper establishes the Lunar and Planetary Laboratory in Tucson. All of these men were among the great doers in lunar and planetary science, although in entirely different ways. Kuiper combined a prodigious energy and strong will with political skills and a knowledge of basic physical science. He could wear out several shifts of night assistants and seemed to get by with only a few hours' sleep. In the 1960s he hinted to me and others that his institute just might be a good place to "coordinate" activities of lunar stratigraphy and other strictly geologic aspects of lunar science. Many people who dealt with him considered him arrogant, but colleagues attest to his loyalty and concern for their personal welfare. Arthur and Whitaker contributed more quietly to a long list of projects that chapters 2, 5, 8, and 9 describe. That IAU meeting in Dublin worked out well.

OUTSIDE THE MAINSTREAM

By concentrating on the train of thought that began with Gilbert and Baldwin and came to govern the course of American scientific exploration of the Moon, I have had to ignore the competing, mostly endogenic, views developed in Europe and America before the Space Age and still held during its early years. Suffice it to say that most of them were knowing or unknowing adherents to Spurr and his lunar grid.[61] But no account of the preparations for lunar landing can omit the name of astronomer Thomas Gold (b. 1920). Gold, born in Vienna, spent part of the Second World War in a peculiar Canadian camp for educated German-speaking Jewish refugees where the main recreation was intellectual exercise.[62] He never obtained the Ph.D. "ticket" that buys professional status. His standing was enhanced, however, in 1948 when he enjoyed success as coformulator (with Fred Hoyle and Hermann Bondi) of the (now-unpopular) steady-state theory of the universe. In a paper published in 1955 the scientific world learned of another interesting idea of Gold's that it would not soon forget.[63] He favored the impact hypothesis for crater origin and realized that the differences in sharpness of upland craters were the result of erosion. From this impeccable starting point he concluded that the eroded material was just about right to constitute the maria. Small impacts and "electrostatic forces" arising from such otherworldly phenomena as solar radiation would loosen the dust and keep it moving until it settled down into the mare basins. The dust is darkened by radiation damage. His mathematics fit his ideas perfectly, of course, as mathematics can always be made to do.

Gold clung tenaciously to his idea of oceans of lunar dust even after the Apollo missions had returned many kilograms of solid rock from the lunar maria. When Robert Hackman once mentioned to him that lunar lineaments were probably faults, Gold's eyes grew wide as he said, "Ah, but wouldn't it be wonderful if they were something more interesting!"[64] His creative imagination was sometimes vindicated, as in 1968 when the astronomical establishment scornfully rejected his interpretation that the just-discovered pulsars are fast-spinning neutron stars, only to have the idea proved correct a few months later and gain a Nobel Prize for its discoverers.[65] But the Gold-dust straw man cost the community of lunar scientists and engineers considerable time and money before it was finally disposed of.

SPUTNIK

The end of one era and the beginning of another was signaled on 4 October 1957 when the Union of Soviet Socialist Republics launched the first man-made object to orbit the Earth, the 84-kg satellite Sputnik 1. The Space Age had begun.

Most people who are old enough remember what they were doing when they heard about Sputnik, though Ralph Baldwin remembers only that it was a Friday and he was going about his usual routine. Gene Shoemaker was told about it when he arrived back at his field camp at the Hopi Buttes from a trip to Oak Ridge, Tennessee, in connection with the uranium-plutonium experiment. His reaction was, "But I'm not ready yet!" Ewen Whitaker had seen the headlines as he was leaving the London airport to begin his work with Kuiper at Yerkes Observatory, and he told Kuiper and French planetary astronomer Audouin Dollfus the news when they met him at the then-primitive O'Hare Airport in Chicago. Lorin Stieff, a friend of Shoemaker's from the Colorado Plateau who will be introduced in the next chapter, was at the annual meeting of the Geological Society of America in Atlantic City and remembers that people were talking about Sputnik, and with some bitterness because they knew the United States could have been first. I was at UCLA slogging through my geological education with an interest in astronomy but little hope of studying the Moon or planets professionally.

On 3 November 1957 the Soviets followed up their success with the launch of a still-heavier satellite, Sputnik 2 (508 kg), carrying the famous doomed dog

Laika. If Sputnik 1 could be dismissed as a stunt, no one could now doubt that the Soviets were serious about space and that their plans included manned flights. They were fulfilling the legacy of Konstantin Eduardovich Tsiolkovskiy (1857–1935), the deaf Polish-Russian schoolmaster who, working alone by the force of his genius, devised in detail the theory of spaceflight, including the use of staged rockets and environmental support systems. Tsiolkovskiy (Ziolkowski in Polish) had regarded Earth as the cradle from which humankind would eventually leave for the stars. Now his countrymen had begun the journey. Although the Soviets had publicly announced their intention to launch satellites during the International Geophysical Year (IGY) (1957–1958), the rest of the world was surprised and the Americans were stunned. The rocket that launched Sputnik could obviously carry an H-bomb across an ocean. Ever since the United States had built the greatest military machine in history almost from scratch during five years of the Second World War and then dominated the postwar world economy, most Americans seemed to assume that theirs was the only nation capable of great technological and industrial feats. Apparently that was not true.

THE JOB AHEAD

Now, a great technological challenge awaited the United States and the world. Foresighted scientists felt a glove touch their shoulders, too.

The pioneers we have been following had set the stage for understanding the Moon, but the stage was still bare in 1957. As Baldwin put it, "There must be something about the Moon which causes astronomers and others to suffer severe attacks of imagination."[66] He had begun to synthesize a complete model of the Moon, but only begun. Urey and Kuiper were just speculating. Shoemaker had not begun to integrate his ideas. There were no professional organizations devoted to the Moon. The impact theory of crater origin was far from being generally accepted. The maria and the basins were equated, and the maria were not understood beyond the agreement that they consisted of lava—which to some meant impact-melted rock. Relative chronologies of lunar events were known locally but not globally, and absolute ages of the main lunar features were guesswork. Not even the 59% of the Moon that can be seen from Earth had been completely photographed or mapped except at crude scales, and the other 41% of the Moon had never been seen at all. Facts were what was needed. A major scientific effort would be needed to unlock the Moon's secrets.