The Quickening Pace
1957–1961

FIRST REACTIONS (OCTOBER 1957 – DECEMBER 1958)

The famous beep-beep telemetry of Sputnik immediately immersed the United States in a yearlong debate about how to respond. The U.S. space program was under way when Sputnik went up, but there was no sense of urgency until 4 October 1957.1

The now-famous JPL in Pasadena, California, was well positioned to respond to Sputnik.2 JPL was born in 1936 as the Guggenheim Aeronautical Laboratory of Caltech, and in 1940 began contract work for the U.S. Army Air Corps to develop jet-assisted takeoff (JATO) for propeller-driven airplanes, at which time it moved its rocket-shooting activities to its present site next to the Arroyo Seco (Dry Wash) and the then-unobscured San Gabriel Mountains. It acquired its present name when it began work on tactical ballistic missiles for the army in 1944, and was still an army establishment in 1957, though administered by Caltech then and now. Only three weeks after the Sputnik launch, JPL’s director, William Pickering, proposed to leap beyond the Sputniks with a project called Red Socks which could send a spacecraft to the Moon as early as 1958. Red Socks would be based on the Explorer spacecraft that JPL was already building. Red Socks fizzled, however, for lack of interest on the part of the Department of Defense, which by default was overseeing the space program in 1957.

So it was up to a Navy project called Vanguard to respond first to the Soviets. This it did on 6 December 1957 by blowing up spectacularly while trying to lift off the launching pad. An impatient Army-JPL team led by Wernher von Braun (1912–1977) was called off the bench, and on 31 January 1958 the United States finally achieved Earth orbit when a modified army Redstone called Jupiter or Juno launched Explorer 1 to an apogee (farthest point from Earth) of 2,500
Another reaction to Sputnik came in the form of conferences. On the early date of 13 May 1958 the Missile Division of North American Aviation in Downey, California, hosted what the organizers believed was the first colloquium devoted to lunar exploration in America and possibly in the world. Young folks planning for the twenty-first century might be surprised to learn that this Lunar and Planetary Exploration Colloquium grew from earlier discussions of a lunar base. Its aspiration was to bring scientists and engineers together for a cross-fertilization of views—something both sides knew would not be readily achieved. Among the 38 of 68 colloquium members who attended the first colloquium were three speakers who will appear later in this history. Astronomer and colloquium co-organizer Dinsmore Alter (1888–1968), the courtly Kansan who had retired as director of the Griffith Observatory six weeks earlier, led off with a lunar tutorial that included mention of possible gas eruptions from Alphonsus, a bit of exotica that reappeared often in later colloquia and in the planning of the Ranger 9 mission six years later. The argument about crater origins began immediately in the discussion that followed Alter's talk. He favored impacts followed by volcanic modifications, but he was challenged by another character who would pepper the debate for many more years to come: geologist Jack Green (b. 1925). Green was a former student of anti-impactists Walter Bucher and Arie Poldervaart at Columbia University, and in 1958 was at the Chevron California Research Corporation in La Habra. He believed—as he repeatedly told later colloquia and many other forums as well—that the Moon experienced violent and global “degassing” that led to the production of calderas by both explosion and subsidence. The Moon might have a few impact craters, he said, but most were calderas. Another speaker was geophysicist Frank Press (b. 1924), then at Caltech, who displayed the combination of scientific imagination and political sense that later led him to become science adviser to President Carter and then president of the National Academy of Sciences. Press brushed off the origin of craters as a relatively minor problem, highlighted the few geophysical facts that were known about the Moon and the Earth, and suggested how lunar exploration could help remove some of the mystery about both bodies.

At the time of the second colloquium, 15 July 1958, Congress was passing the legislation that established the administrative framework for carrying out that exploration, the National Aeronautics and Space Act, which President Dwight D. Eisenhower signed two weeks later on 29 July. On 3 August 1958 came the
official announcement that the National Academy of Sciences had established a Space Science Board to recommend science projects for the new space agency to conduct. In that same month the army authorized work on a giant Saturn rocket with 1.5 million pounds of thrust that von Braun had proposed in December 1957. Events were unfolding quickly in mid 1958—30 years ago as this is being written in a less active epoch. Thirteen additional colloquia were held over the next 5 years at JPL, the Rand Corporation and Miramar Hotel in Santa Monica, the Griffith Observatory above Hollywood, the Space Technology Laboratories in Los Angeles, and the North American, Northrup, Douglas, and Lockheed aircraft companies.3

The Rand (Research and Development) Corporation, founded in 1947, was an early institutional entry into the space business. Even before Sputnik, Rand scientists had studied techniques of Moon exploration for their air force patrons, the party who then had both the money and the interest. A 1956 Rand study (classified secret) considered lunar soft landers. Geodesist Merton Edward Davies (b. 1917) participated in this study and designed a panoramic camera, employing a side-to-side scanner and recovered film, that could have been flown with already existing technology on an unmanned orbiter.4 Rand and Davies were also active in the “black” space program that parallels NASA’s “white,” open program; but that is none of this book’s business.5

Rand’s early start illustrates the common assumption that the military would conduct the effort to meet the Soviet challenge. President (and five-star general) Eisenhower, however, was disinclined to place the exploration of space in the hands of the “military-industrial complex.” The Space Act therefore decreed strictly civilian status for the new agency. Although its astronauts would be test pilots who were currently or formerly in the military, the agency would conduct its operations openly and for peaceful purposes, in contrast to the secretive military-operated Soviet effort. The conservative 43-year-old National Advisory Committee for Aeronautics (NACA) would blossom into a bold new form. Thus, on 1 October 1958, three days before the first anniversary of Sputnik, was born the United States National Aeronautics and Space Administration.6

NASA took over NACA’s aeronautical research functions and its research centers Ames and Edwards in California, Lewis in Ohio, and Langley in Virginia. It also took over NACA’s much-respected director, Hugh Dryden, but as deputy to NASA Administrator Thomas Keith Glennan, from the Case Institute of Technology, because NACA’s stodgy reputation did not fit NASA’s desired image. Three weeks after its founding, the new agency obtained from the Naval Research Laboratory a mathematician who plays a major role in the story told in this book, Homer Edward Newell (1915–1984).9 At first, Newell was deputy director
to former Lewis director Abe Silverstein in the Office of Space Flight Development, which controlled all NASA flight projects. On 5 November 1958 NASA established the Space Task Group at Langley, and giants like Robert Gilruth and Maxime Faget began planning how to put Americans into space.\textsuperscript{10}

NASA's acquisitions included the Vanguard and Pioneer projects. Four Pioneers were launched by air force and army rockets between 17 August and 6 December 1958, the anniversary of the Vanguard humiliation (appendix I). The air force wanted to achieve lunar orbit with its Pioneers “O,” 1, and 2, which carried infrared scanners that could have made crude pictures of the Moon had they gotten that far. They did not; they fell back to Earth from altitudes of 16, 113,830, and 1,550 km, respectively. The army’s Pioneer 3 would only have sensed the Moon with a photoelectric device as it flew past on the way into solar orbit. It fell back from 102,320 km. At the same time the first U.S. manned spaceflight program received the name Project Mercury suggested by Abe Silverstein. Pioneer had been the world’s first reach toward the Moon.\textsuperscript{11} Mercury would turn out to be the first step of humans along the same path.

Effective 1 January 1959, NASA also gobbled up a less easily digestible delicacy as JPL’s physical plant and contract were transferred from the army. JPL’s scientists had always felt beholden to no one. Newell, Administrators Glennan and (later) Webb, and other high NASA officials did not feel that JPL and Caltech were earning the large sums they received for administrative support and were thoroughly annoyed by their presumption of academic superiority.\textsuperscript{12} JPL’s independent attitude harassed them for years, and whether JPL is a NASA center or not has never been settled to both sides’ satisfaction. Today, NASA’s name is above those of JPL and Caltech on the sign outside JPL’s gate, but JPL employees still get their paychecks from Caltech. Caltech and JPL appear in this history almost as often as any of the human characters, for these first-class institutions touched a large fraction of the people, science, and missions that supported lunar exploration.

\section*{KUIPER AND THE DEPARTMENT OF DEFENSE (JULY – NOVEMBER 1958)}

The Moon was already in the plans of both Gerard Kuiper and the Department of Defense, who knew the value of maps and photographs in exploring unknown territory. The collection of photographs often called the Kuiper Atlas and later published as the \textit{Photographic Lunar Atlas} had received its first air force funding in the fall of 1957.

Kuiper occasionally found time amidst his many other duties to observe the Moon visually and to meditate about its history, especially after he tangled with Urey in 1954 and 1955. In fact, their feud may have heightened the interest of
each in examining the Moon to prove his point: Kuiper by means of ground-based studies, Urey by means of spacecraft. In July 1958 Kuiper presented an updated summary of his thinking in an illustrated lecture at a symposium on astronautics, whose best feature may be a description of the maria and their landforms.

However, Kuiper’s promotion of lunar and planetary science and his assembly of its basic data proved much more valuable to the space program than his own lunar studies. The atlas project led to the permanent immigration to the United States in September and October 1958, respectively, of Dai Arthur and Ewen Whitaker. Whitaker contributed his great skills in photography to the project, and Arthur contributed the selenodesy and anything else requiring quantitative treatment. Kuiper also wanted Arthur to construct a spherical surface on which photographs of the Moon could be projected and rectified. These two and others on the Yerkes staff helped in the enormous labor of assembling the negatives for the atlas.

RAND’s air force think tank got into both the black and the white space programs early, the Air Force Cambridge Research Laboratories in Massachusetts (AFCRL) launched the atlas, and another air force agency eventually became the prime producer of the best lunar charts and maps. But the first direct action toward actual mapmaking was taken, in November 1958, by the U.S. Army Map Service (AMS). AMS tried to use stereophotogrammetric techniques, but these were doomed to failure by inadequate data and inadequate plotters. Nor did they portray the appearance of the Moon as faithfully as Kuiper’s group would have liked. Dai Arthur, who seldom feared to speak his mind, made these points in print, souring relations between the Kuiper group and AMS for many years. Nevertheless, AMS did publish some handsome telescope-based maps between 1962 and 1965 and continued to contribute during the era of spaceflight.


Most space scientists in 1958 blanched at the thought of the Moon as an object of exploration. They were what JPL historian Cargill Hall has called “sky scientists” — physicists and astrophysicists interested in properties of the upper atmosphere and interplanetary space. They dominated the IGY and the Vanguard, Explorer, and Pioneer experiments. With some exceptions, they regarded quantitative measurements like those that led to the discovery of the Van Allen belts as the only justifiable type of space science. Another never-settled problem constantly argued among NASA Headquarters in Washington, the former NACA centers, JPL, the Space Science Board, and the universities, was who should do the science and what it should be. We have Newell’s testimony that the claims of
many of the scientists made them seem like petulant brats.\textsuperscript{20} If the sky scientists had remained in the saddle, the Moon would probably have remained only one of many sources of data for passing spacecraft.

But in 1958 a new breed of space scientist was being heard from: the geologists, geochemists, geophysicists, and some astronomers interested in the Moon and planets in their own right. One geochemist (cosmochemist) whose interest was definitely awake was Harold Urey, who believed that, like a comet or a simple asteroid, the Moon has been untroubled by all the geologic indignities that have been inflicted on Earth and other large planets by internal heating and corrosive atmospheres. All one had to do was reach out 400,000 km and scoop up a sample of this primitive Rosetta Stone.

On 29 October 1958 Urey, who earlier in the year had retired at age 65 from the University of Chicago and moved to the brand-new University of California at San Diego (UCSD), made his views known to people who mattered at the Third Lunar and Planetary Exploration Colloquium. He based much of his presentation on Gilbert's work and noted that when he had read Gilbert's "immensely impressive" 1893 paper he realized at once that he was "reading the paper of an extremely competent scientist." Urey's presentation included some of the charming remarks that characterized him. He was "immensely pleased to learn of the existence of a group of this kind," and he expected "to have a very red face in the course of a few years" (after chemical and geological data about the Moon have been accumulated). And: "Some wonderful photographs of the moon have been taken in this century, but I believe very few of the physical scientists have paid much attention to them. Yet many wish to get photographs of the side we have not seen. Well, if it is not important to look at the front of the moon, why is it important to look at the back?"

Presumably without knowing it, Urey was foreseeing the entire subsequent history of the American space program. Launching new projects has always taken precedence over digesting the results.

At the same time, Newell hired theoretical physicist Robert Jastrow (b. 1925), formerly under him at the Naval Research Laboratory, to head a small theoretical division consisting of physicists and mathematicians who would pick scientific plums from NASA's spaceflights.\textsuperscript{21} They worked in Washington at first but soon moved to a new non-NACA center of NASA's own in Maryland, named the Goddard Space Flight Center in May 1959 and dedicated in March 1961. Jastrow was searching the literature to come up on the curve in his new job and came across Urey's epochal pioneering book \textit{The Planets}. At the end of November 1958 he had hired, from the U.S. Army Corps of Engineers, astronomer-geodesist John Aloysius O'Keefe (b. 1916). O'Keefe, who knew Urey, introduced the two.
Jastrow, a sky scientist, was impressed by the type of deduction from basic laws of physics that characterized Urey's thinking. Urey's science looked much better than the inductive science (Jastrow thought) of those who collect butterflies, beetles, or rocks and then draw conclusions from all the assembled data—meaning geologists. Jastrow was sold on Urey and the Moon and was now going to sell NASA.

He quickly got help from the Soviets. On 2 January 1959 the USSR launched a probe with the self-explanatory name Luna 1. The U.S. Pioneers had been the first to try to reach the Moon but had fallen way short. Now, Luna 1 escaped from Earth and missed the Moon by "only" 5,000 km, less than two lunar diameters.

Two weeks later, Urey went to Washington, gave a two-day series of lectures (15–16 January 1959), made a favorable impression on NASA, and wrote a memo (edited by Jastrow) extolling the virtues of the Moon. Newell quickly formed an ad hoc Working Group on Lunar Exploration chaired by Jastrow. On 5 February 1959 there appeared at JPL a contingent of this group, including Jastrow, O'Keefe, Urey, chemist James Arnold from Urey's old department at Chicago and his new one at UCSD, Frank Press, chemist-meteoriticist Harrison Brown, also from Caltech, and Ernst Stuhlinger from von Braun's shop at the Redstone Arsenal in Huntsville, Alabama. The group was at JPL to deliver the word from NASA Headquarters: NASA has adopted lunar exploration as part of its program; there shall be instrumented "hard" landings (crashes), "rough" landings (with retrorockets), lunar satellites, and soft landings. The hard landers could go within 12 to 18 months if initiated immediately. The last phase, the soft landings, probably to include sample return, could be achieved within three to four years of initiation. This plan was to be the basis for JPL's next projects. The Moon had been a secondary objective to Venus and Mars in the view of Director Pickering and many of his managers and scientists. Now it was becoming second only to the Earth-orbiting Project Mercury on NASA's list of priorities.

The pace was definitely quickening. On 3 March 1959 the United States finally achieved escape from Earth with Pioneer 4, though it was deliberately flown past the Moon at a substantial 60,500-km distance. A memo dated 23 March from Newell to Silverstein officially proposed a major lunar program. On 9 April 1959 the seven Mercury astronauts were introduced to the public. Then, in July 1959, NASA Administrator Glennan formally recommended that the Moon be emphasized. The USA would beat the Russians to the Moon and determine the origin of the Solar System à la Urey.

Jastrow expressed the history of America's concentration on the Moon as follows: "Urey was the trigger, I was the bullet, and Newell fired the gun." He might have added that the Russians had furnished them the arms. NASA changed
course in reaction to a Soviet initiative, as it would do many times during the Space Age. In the summer of 1959 it was preparing for the Moon and waiting for the USSR to drop the other shoe.

FIRST CONTACT (SEPTEMBER – DECEMBER 1959)
The other shoe dropped at two minutes and 24 seconds after midnight Moscow time on 14 September 1959. The end of the long era when knowledge about the Moon came from quiet nights at the telescope was heralded by the crash of the Soviet spacecraft Luna 2 onto the rim of the crater Autolycus (1° W, 30° N). Scientifically, Luna 2 ("Lunik" 2) did little more than reach its target and show that the Moon possessed little or no magnetic field or radiation. However, it initiated the era of direct contact that would be necessary for learning the composition and age of the lunar surface rocks. In the same month, the United States lost another Pioneer on the test pad.

The following month, on 7 October 1959, the Soviets obtained humankind's first view of the lunar far side. The Automatic Interplanetary Station Luna 3 returned a full-face image that was good enough to show major contrasts in brightness (albedo). There were clearly far fewer maria than on the near side, as had been predicted by Nathaniel Shaler from his observation that the Moon's edges (limbs) have relatively few maria. However, Mare Moscoviense was there, and a large mare-filled crater that stood out like a sore thumb amidst a crowd of ordinary craters was given the worthy name Tsiolkovskiy. Luna 3 also revealed long bright streaks that the Soviets called the Soviet Mountains and that Russian geologist A. V. Khabakov, a believer in the importance of lineaments, claimed are parallel to major faults on the near side. Ewen Whitaker, however, pointed out the embarrassing fact that the "mountains" are coalescing rays of two young craters and therefore are quite flat. Incidentally, Patrick Moore, the British lunar enthusiast and popularizer of astronomy, has stated that the charts the Soviets used for the Luna 3 flight were the detailed but very unrealistic ("unrealistic" is my observation, not Moore's) line drawings laboriously prepared over decades by British amateur Percy Wilkins. New charts were obviously needed.

NASA took a moonward step of its own within a month of Luna 3's flight as it announced on 21 October that it would acquire von Braun's Army Ballistic Missile Agency in Huntsville after the Department of Defense decided it did not need the Saturn rockets. Von Braun's group would become the nucleus of a new NASA center at Huntsville called the George C. Marshall Space Flight Center (MSFC) and devoted mainly to the rockets that would launch men toward the Moon. Another unnumbered Pioneer blew up in November 1959. In Decem-
ber the plans that had been incubating during the year finally hatched as headquarters and JPL initiated the first earnest U.S. lunar project, Ranger.

**GeoLogic Mapping (Early 1959 – July 1960)**

The months of the Luna flights were also when the mainstream mapping program for lunar exploration began at the U.S. Air Force Chart and Information Center (ACIC) in St. Louis, under the direction of Robert W. Carder. Someone at ACIC suggested that the best way of portraying the lunar surface with both qualitative fidelity and topographic accuracy was the artistic technique of airbrushing. Keeping her efforts secret from AMS, Patricia Marie Mitchell Bridges (b. 1933) then quickly prepared the prototype of the chart series that would become basic to the lunar program, the 1:1,000,000-scale *lunar astronautical charts* (LAC). After some help from Kuiper's group at Yerkes, the publication of this chart in February 1960 launched ACIC's systematic production of LACs.

Geologists had plenty of uses for the ACIC and AMS charts. No solid planet is either a homogeneous blob or a disorganized jumble; each is made of discrete pieces—the geologic units. Each geologic unit was formed in a certain way and in a finite time. Each has depth as well as length and breadth, and geologists are always trying to look beneath the surface to reconstruct this three-dimensional structure that is hidden from direct view. The geometric relations and distribution of the units show their age relations to other units, something about the processes that formed them, and something about how far below the surface they extend.

There is a lot of information here. Collecting it is a big job, and telling others what you have seen and learned can become equally complicated. The sparse graphs of the physicists could never do it. The medium that geologists have evolved to record and convey their observations and interpretations in a relatively simple and economical way is the *geologic map*. We take a base map like a LAC and draw boundaries (contacts) between geologic units, scribbling notations all the while. The base shows the positions and the geologist adds the third dimension by interpreting the surface appearance in terms of geologic units. The final geologic maps (usually finished after innumerable revisions) show what and where an area's rocks are and when they were emplaced.32

Readers undoubtedly see the names of the U.S. Geological Survey and Eugene Shoemaker coming again. It was indeed the Survey that introduced and nurtured the modern form of lunar geologic mapping, and Shoemaker who, eventually, sold the technique to NASA and other lunar scientists. Chapter 1 tells of his tentative approach to USGS Director Nolan in 1956. In mid-1958 the USGS uranium project was closed down abruptly by the discovery of an overwhelming
TO A ROCKY MOON

abundance of the stuff at Grants, New Mexico, creating one of the Survey's recurring shortages of money and surpluses of geologists and occasioning Shoemaker's move to the USGS Pacific Coast Regional Center at Menlo Park, California. The lunar project might be one small way to help alleviate the money and personnel problems. Assistant Chief Geologist Montis Klepper inquired in late 1958 at the Survey headquarters in Washington about who might be interested in lunar work, and shortly afterward pursued the matter during a visit with Shoemaker in Menlo Park. Shoemaker drew up a research plan, but it was consigned to the back burner for a year.

And so it happened that the impetus for the first U.S. Geological Survey lunar-geologic mapping effort came from an entirely different direction. Arnold Caverly Mason (1906-1961) seems to have had an up-and-down life and career, never settling on a completely satisfactory project he could call his own. The lunar Space Age provided one. The meticulous Mason plunged into a study of the Moon both on his own time and in his official position as a geologist with the Military Geology Branch of the Survey, whose chief, Frank C. Whitmore, Jr., also caught the Moon bug. Whitmore brought in Gerard Kuiper as consultant and obtained a commercial package of lunar photographs and maps costing a few dollars as initial raw material. It was Mason who conceived of conducting a terrain analysis of the Moon. Kuiper had told of the possibilities of viewing the Moon stereoscopically, and (probably) in early 1959, Mason sought help from the chief of the Photogeology Branch of the Survey, William A. Fischer. Fischer made available his branch's modern stereoplotters and assigned Robert Joseph Hackman (1923-1980) and Annabel Brown Olson (1922-1992) to the project. Hackman, who had no academic degree when he joined the Survey and was mostly self-taught, later devised a simpler and more suitable stereoscope than those used by AMS and Photogeology for viewing Kuiper's large lunar photographs. The Survey obtained funds from the U.S. Army Corps of Engineers, who had a long-standing working relation with Military Geology and a mutual interest in such matters as terrain analysis and trafficability. Mason and Hackman put the project on the front burner and worked with AMS in preparing the base map. The resulting Engineer Special Study of the Surface of the Moon was first printed in July 1960, although it bears the publication date 1961. It contains four sheets: one detailed text by Mason and three maps at a scale of 1:3,800,000 by Hackman, assisted by Olson.

One map shows crater rays. Another is a physiographic classification of the surface. The third map is called a "generalized photogeologic map" and shows only three units—"pre-maria rocks," "maria rocks," and "post-maria rocks." Nevertheless, it deserves credit as the first modern lunar geologic map based on stratigraphic principles. Despite its apparent simplicity it was an enormous
advance over portrayals of the lunar crust merely as a series of structural lines. It shows that the lunar uplands formed first, then the maria, then a few more craters; something obvious to today’s lunar geologists but not to those who followed Spurr and thought of each “lineament” or hill as an independent entity that might have formed at any time in lunar history by any imaginable internal process. At first, Hackman in fact toyed with the Spurr concept, and the map does feature swarms of straight lines interpreted as faults, which very few of them are. Olson remembered suggesting to him that the Moon could be better understood in impact terms, though she did not remember whether or not she got the idea from advisers Kuiper, Dietz, or Shoemaker—impacters all.

One can speculate that the impact model took hold on the map’s authors during a trip in October 1959. Kuiper had invited Shoemaker, Mason, Hackman, Olson, and Dietz to observe the Moon at McDonald Observatory. All except Kuiper and Olson made a side trip to the nearby geologically complex feature known as Sierra Madera. The trip was the idea of Dietz, who, building on the work of Boon and Albritton, had taken an early lead in demonstrating the impact origin of complexly deformed and broken-up rock structures that had been called cryptovolcanic, naming them first cryptoexplosions (to satisfy the skeptics) and later astroblemes (star wounds, which is what he knew they were). Dietz suggested that Sierra Madera would be a good place to look for shatter cones, conical fracture surfaces 1 cm to more than 10 m in size with striations that radiate from the centers of great explosive forces. Shoemaker had been skeptical that “cryptovolcanic” Sierra Madera was an impact structure. But while ascending the structure’s flank, Hackman picked up a striated object and asked Dietz, “Is this what you’re looking for?” Sierra Madera is an astrobleme.

Hackman and Mason ultimately accepted the impact origin of most craters and went so far as to state that “formation by meteoric impact is [more] commonly accepted” than volcanism. They also correctly interpreted the maria as volcanic lavas; but the old mistake persists: they thought the lavas were released by the impacts that formed the surrounding ring mountains. They thought that the maria all formed in a short time despite the correct observations that (1) the Imbrium impact came between the Serenitatis impact and the lavas of Mare Serenitatis, and (2) the lavas of Mare Imbrium delayed filling the Imbrium basin long enough for flooded craters to form.

Gilbert, Dietz, Baldwin, Kuiper, and others who have been named had arrived at correct interpretations without making geologic maps. Spurr, Khabakov, and German geologist Kurd von Bülow had made geologic maps of sorts without arriving at correct interpretations. From now on, mapping and genetic interpretations ping-ponged, each testing the other. The concept of geologic units and the impact hypothesis for crater origin enabled the Hackman-Mason map
to be so simple because they match how the Moon is built. All the lineaments only modify the material geologic units. The mistake about the maria shows, on the other hand, that incorrect interpretations do not necessarily affect the mapping; Hackman and Mason mapped the relations correctly while getting the cause of mare-lava extrusion wrong. In chapter 16 we will encounter a case where interpretations did affect geologic mapping.

An all-too-human footnote ends the story of the first modern geologic mapping. The outwardly self-controlled Arnold Mason committed suicide on Halloween 1961 for reasons that are not entirely clear and are undoubtedly complex, but which seem to have included nonrecognition for his original and ardent pioneering of lunar studies for the U.S. Geological Survey. He, Hackman, and Olson deserve much credit, unfortunately posthumous in all three cases, for their truly innovative contributions.

**SHOEMAKER’S CREATIVE BURST (1959–1960)**

Shoemaker was far from idle while Hackman and Mason were stealing the march in geologic mapping. Few individual scientists have contributed so much of fundamental importance as Eugene Shoemaker did in 1959 and 1960.

He had been unlocking the secrets of Meteor Crater since 1957, and in 1959 was ready to report his results. He established in detail how the meteorite interacted with its target rock, how it piled the target beds of sandstone and limestone upside down on the crater’s rim, how it was altered and dispersed, and how its energy is related to the crater’s dimensions. The term “explosive” reflected great strides in understanding the cratering process since Gilbert and others of his day had pictured impacts as denting and splashing their targets mechanically. Semantically, however, the term implies that the ultimate cause of cratering is the vaporization of the meteorite. Shoemaker emphasized that the ultimate cause is actually the creation at the collision interface of two shock waves, one that engulfs the projectile and another that races into the ground away from the impact zone. The first shock wave explains why Daniel Moreau Barringer almost went broke; nothing could have withstood it. The second explains the properties of craters in detail: it compresses the target rock to such an impossible degree that the target rock “tries” to react with equal violence, so that it utterly disintegrates and is expelled from the growing cavity. The effect is explosive, but the basic workings of the process are unique to its shock origins. While working on the nuclear craters at the Nevada Test Site, Shoemaker came across the shock concept in an unpublished 1956 paper by David Griggs and Edward (“H-bomb”) Teller of the University of California bomb factory, then called the Livermore site of the Lawrence Radiation Laboratory and today, less
threateningly, the Lawrence Livermore National Laboratory. The lessons of Meteor Crater have been extended from Shoemaker’s study to craters in general, and this relatively small crater has become the model for others in its size range on Earth and Moon.

Professional scientists in general and USGS geologists in particular are supposed to have Ph.D.s, so in the summer of 1959 Shoemaker (who already had one master’s degree from Caltech and another from Princeton) sent a long version of the Meteor Crater study to Princeton geology department chairman Harry Hammond Hess (1906–1969) as a dissertation. He also needed a manuscript for the quadrennial meeting of the International Geologic Congress that was coming up in the summer of 1960 in Copenhagen, and sent off a short version of the study for that purpose.  

His first major entry into the lunar science limelight, however, came at the Eighth Lunar and Planetary Exploration Colloquium, held on 17 March 1960 at the North American Aviation Recreation Center in Downey, California. He had been immersed in a study of the crater Copernicus that concentrated on the ballistics of crater ejecta as revealed by the patterns of the smaller craters that surround all young and many large old lunar craters—the satellitic craters. The rays of the youngest craters extend far beyond the crater rims. Careful telescopic observers had seen the concentrations of small craters along the rays and elsewhere around young and large craters. These satellitic craters were, of course, claimed by both the volcanic and the impact camps. Baldwin compared valleylike grooves radial to such fresh craters as Aristillus to the Imbrium sculpture and inferred “that these grooves were actually gouged out of the solid crust by some process associated with Aristillus and do not represent graben or downfaulted blocks of the crust.”  

Kuiper similarly noted that the many small cuts and grooves he observed with the McDonald telescope around Tycho were formed by ejected “boulders.” Shoemaker showed that the distinctive patterns of loops and stringers in the retinue of the Copernicus satellitic craters were what would be expected if the ejecta that formed them came from the shock engulfment of precratering structures expectable in the region. During a cosmic collision, enormous amounts of ejecta are hurled from a crater as it is being excavated. Some of this ejecta lands near the crater and builds up a thick, rugged deposit on the crater’s outer flank. The ejecta launched farthest (and first), however, hits harder when it lands and digs a hole instead of building up a deposit. These satellitic holes are secondary-impact craters, called simply secondaries by their many admirers. Thus it was established that secondaries can create an enormous range of lunar features, including all sorts of the chains and clusters that the volcanologists cited in defense of their theories.

The Copernicus study soon led Shoemaker in yet another direction. In early
1960 the USGS proposal was still on the back burner and Shoemaker was entertain­ing two job offers in case the USGS program did not materialize. One offer was from RAND, whose personnel had seen him in action at the colloquia. The other was from JPL, which he visited partly to check on the job offer and also at O'Keefe's suggestion. He was astonished to see a copy of the ACIC prototype LAC of the Copernicus region by Pat Bridges lying on a table in the trailer office of his former Caltech classmate Manfred Eimer, assistant chief to Albert Hibbs of JPL's Space Science Division. Robert Carder at ACIC had also turned to JPL in the effort to get a mapping program started. Shoemaker was already studying the Copernicus region intensively with a superb photograph (purchased at the Caltech bookstore) that Francis Pease had taken with the 100-inch Mount Wilson reflector on 15 September 1919. Thus he had the makings of a geologic map; he also had already thought of what he would show on such a map if he were to make one.

Now was the time. He went back to Menlo Park, had a copy of the LAC base made, set to work, and a week later had completed the second modern lunar geologic map. There were map units for parts of craters, the maria, the mare domes, and a regional terra-blanketing unit, all of which were arranged in order of age into seven classes packaged into five named age units: the Copernican, Eratosthenian, Procellarian, Imbrian, and pre-Imbrian systems. Shoemaker sent a hand-colored copy to Eimer and then traveled to St. Louis, where Carder enthusiastically cooperated in printing a trial run of the geologic map in color on the LAC base. Hackman later added some lineaments and the map was ready to show at the International Geologic Congress. Though not the last word, the map marked the birth of the systematic lunar-geologic mapping program that was carried out by the USGS for the next two decades and that continues today in the more general form of planetary mapping.

LUNAR AND PLANETARY LABORATORY (1960)

Kuiper had long wanted to establish an institute devoted to that neglected and scorned subject planetary science, and he realized that the start of the space race would favor his goal. Yerkes Observatory and the University of Chicago were intellectually brilliant but atmospherically murky, cramped for space, and not entirely pleased by Kuiper's aggressive promotion of his lunar projects. Modern observatories need the clear, dark, dry skies offered by areas like the southwestern United States. In 1955 he had sent out astronomer Aden Meinel as a scout, and the search for a good site culminated in March 1958 when the Tohono O'dham (formerly Papago) Indians approved the lease of their most sacred mountain, Kitt Peak, near Tucson, for an observatory. Kuiper wanted to
be associated with a university, where his institute could teach planetary science and where diverse specialists, including geologists, were accessible. He also wanted to be near geologically interesting terrain. He visited the University of Arizona in Tucson in January 1960 and planted the seeds that in February would sprout as the Lunar and Planetary Laboratory (LPL).43

The prodigious efforts that Kuiper, Whitaker, Arthur, and the others expended on the lunar atlas came to fruition when it was printed in April 1960.44 After Arthur and Whitaker joined the westward migration in the summer of 1960, they quickly turned to the task of completing supplement 1 of the atlas, a version that, among other uses, would provide the basic selenodetic control for ACIC's charts.45 The nascent Space Age had obtained its first widely available and utilizable collection of lunar photographs. Arthur additionally launched into the major effort of preparing a four-part catalog and a four-quadrant chart of measured, positioned, and named lunar features that also involved the labor of two youngsters still in planetary science today, Clark R. Chapman and Charles A. Wood.46

**USGS ASTROGEOLOGY (1960)**

In 1960 the Survey still had too little money and too many geologists, whereas the reverse seemed to be true in NASA. In late 1959 or early 1960 Shoemaker had suggested to the Survey's new chief geologist, Charles Anderson, that the proposal for a small USGS lunar program be dusted off. In early 1960 Anderson turned the matter over to Survey geologist-geochemist Lorin Rollins Stieff (b. 1920), who predated even Shoemaker in the USGS uranium project on the Colorado Plateau and who became his close friend and antiestablishment scientific ally. (Stieff's wife, Harriet, remembers asking Shoemaker in those early days where he wanted to be in 20 years, and receiving the reply “up there!” as Gene jabbed a finger at the Moon.) Anderson hoped to get NASA funding for a geochemical study that would benefit the Survey's well-equipped, well-staffed, but underfunded analytical laboratories.

Reenter tektites and John O'Keefe. Harvey Nininger had suggested in 1936 that tektites come from the Moon, and O'Keefe believed deeply that it was so. He wanted to go to the Moon himself to find them. In January 1958 *Nature* had published a paper of his along with others by Thomas Gold and Carlos Varsavsky favoring a lunar origin, and papers by Urey, astronomer Zdeněk Kopal, and tektite pioneer Virgil Barnes opposing it.47 O'Keefe and Urey subsequently argued the matter with considerable ardor, as Urey continued to do with other "tektites from the moon people."48 On 24 February 1960 O'Keefe was among the standing-room-only crowd of more than 300 attending a talk by Shoemaker
at the venerable Cosmos Club of Washington, once frequented by Gilbert (and thoughtfully provided with a special entrance where geologists can enter without jackets or ties). O'Keefe was fascinated by Shoemaker's impact interpretations of Meteor Crater and lunar craters because impacts were the means (he thought) of throwing tektites from the Moon to the Earth. O'Keefe and Stieff visited William Henderson at the Smithsonian and the three agreed that the Survey should make a study of lunar geology that would include tektites.

This proposal went forward along with a separate one for geologic mapping and crater investigations prepared by Shoemaker. NASA would put up the money the first year, including some with which the Smithsonian could buy the tektites for distribution. After that, the money would come directly from Congress via the USGS because Stieff feared that NASA would let down the Survey as the Atomic Energy Commission had done when it suddenly cut off funds for the uranium project. Let me jump ahead a quarter of a century and tell a long story in one phrase: all but about $100,000 of the funding for the USGS lunar program came from NASA.

Stieff, on the scene in Washington while Shoemaker remained in Menlo Park, walked the proposals through both the USGS and NASA. O'Keefe did his best to promote the proposals at NASA; however, they encountered a stubborn obstacle there. Urey was upset because he wanted the study to go to his institution, the University of California at San Diego.

At this juncture came one of those confluences of events and people that reroute history. In 1953 Loring Coes had squeezed quartz in a hydraulic press and created a new mineral, coesite, with a higher density than quartz. The key to its existence was extreme pressure. In 1956 Nininger had suggested that a search for coesite in the quartz-rich Coconino sandstone at Meteor Crater "might have significant results." Shock waves were not only a good way but probably the only way to produce natural coesite at the Earth's surface. Though Stieff did not know of this prediction and Shoemaker had forgotten it, Stieff and O'Keefe obtained some Meteor Crater samples from the Smithsonian just to get studies of moonlike materials started.

Now there appeared on the scene the third founding father of the USGS lunar program, along with Shoemaker and Stieff, Edward Ching-Te Chao, then of the USGS Geochemistry and Petrology Branch. Chao was born in 1919 in Suzhou (west of Shanghai), emigrated to the United States in 1945, got his Ph.D. at the University of Chicago in 1948, and became a U.S. citizen in 1955. In May 1960 he was assigned his first Survey project of his own: tektites. A week later O'Keefe gave him one of the pieces of Coconino sandstone from the Smithsonian and asked him to find out whether glassy material in it had any connection with tektite glass. Chao crushed part of this already small (2 by 2 by
1 cm) sample, immersed it in a special oil, and saw some grains with an unusually high index of refraction. He immediately x-rayed a powdered sample and obtained patterns that matched those of artificial coesite. He still had not seen a tektite or heard of Gene Shoemaker. His branch chief, William Pecora, later a USGS chief geologist (1964–1965) and director (1965–1971), did not trust the finding and asked others to verify it. Stieff told O'Keefe about Chao's discovery, and O'Keefe told Urey in a letter dated 14 June 1960 (pointing out that the shock overpressures were also a way of creating the diamonds found in the nearby Canyon Diablo meteorites, which had always interested Urey). When Pecora was convinced that Chao was right, they held a press conference (20 June) to announce the momentous discovery that would prove the meteorite-impact origin of many terrestrial craters, and Chao authored a paper for *Science* reporting it. Chao then visited Shoemaker in Menlo Park and showed x-ray technician Beth Madsden how to identify coesite. Because the Survey's proposal to NASA was still stalled, Pecora got Shoemaker and Madsden added as authors of the *Science* paper to show that the Survey had a complete team for performing lunar investigations. Although Shoemaker deciphered the geology of Meteor Crater, I think it is fair to say that it was Chao who set the modern study of impact-shock mineralogy into motion.

But the Survey proposal remained stalled. The next act in the drama came in the month the *Science* article appeared, July 1960. Shoemaker was on his way to Copenhagen for the congress and stopped en route for some geologic sightseeing. This is when he saw the type locality of the explosive volcanic maar craters, whose German or Rhinelander name is derived from some small craters in the Eifel district west of the Rhine. Even before the coesite discovery he had figured out from the literature that the Rieskessel surrounding Nördlingen, Bavaria, was in no way a caldera or cryptovolcanic feature, as was the general assumption, but an impact crater, as had been proposed by German investigators as early as 1904 and reproposed by Baldwin in 1949 and Dietz in 1959. He had done his homework as usual, and on arriving at the Ries one evening in July, made a beeline for a quarry (Otting) where he knew he could find the bomblike, partly glassy material called suevite. He had prepared himself to seize still another opportunity and was the first person to realize what the suevite probably was — target rock that had been highly shocked and partly melted by the impact that dug the Ries. Suevite contains silica, so it should also contain coesite. The next day he viewed what are really the best exposures of suevite — the walls of the main church in Nördlingen (St. George's), which are made of rock quarried from the Ries — and mailed seven samples from the quarry to Chao. As the cliché goes, the rest is history. One of the samples contained enough coesite to be identified. Shoemaker added the result to his Meteor Crater presentation at
the congress and Chao wrote the reporting journal paper. The Ries moved decisively from the cryptovolcanic to the impact camp, some local German geologists were chagrined, O'Keefe was vindicated, the NASA obstacle was overcome, and the USGS got its first NASA funding of $200,000.

Shoemaker carried along on his European trip a manuscript destined for Zdeněk Kopal's forthcoming book on the Moon. Kopal knew of his Copernicus ballistics study through the colloquia and had asked him to contribute a chapter. Shoemaker wrote much of this great synthesis in Copenhagen while at the congress and after leaving the Ries while Chao's telephoned reports and his own observations were fresh in his mind. It was Chao, however, who continued to investigate the Ries intensively in later years.

NASA's money funded Chao and five other geochemists in Washington and an equally small Astrogeologic Studies Group at the USGS center in Menlo Park. The group began officially on 25 August 1960 (five days after Ewen Whitaker arrived in Tucson, incidentally). Although the technique of photogeology led off the new effort, Shoemaker rejected the term as a name for the group because he wanted to focus on the basic methods of geology. He knew that photogeology as practiced then would have little value without support from terrestrial studies, nonvisual remote sensing, and, ultimately, fieldwork and sample collection on the Moon itself. Anyway photogeology conjured an image of lazy people sitting around offices guessing about rocks they will never see in the field. Hence astrogeology was chosen, even though it agitated speakers of English because stars have no geology.

One of Shoemaker's hopes was dashed soon after the lunar proposal was funded. He had considered Lorin Stieff a likely future chief of the Astrogeology Branch when he (Shoemaker) went off to become an astronaut. Instead, Stieff left the Survey to embark on a career that he hoped would contribute to arms control.

The Astrogeologic Studies Group embarked on three main tasks that built on the history of its founding. One, of course, was the study of tektites. The tektite study occupied half of the group's first semiannual report and continued vigorously for another decade, but eventually faded away when Surveyors and Apollos found out what the Moon is made of.

The second project was geologic mapping, based on the now-confluent efforts of Hackman and Shoemaker. Hackman incurred Shoemaker's displeasure by refusing to move from Washington to Menlo Park, or, later, to Flagstaff. Nevertheless, he added to his "firsts" by completing the first published (1962) geologic map at the LAC scale of 1:1,000,000, that of the Kepler region (LAC 57). The first and second Astrogeology semiannual reports contained prepublication versions of the Kepler map, in which form I studied it with fascination.
during my last year at UCLA, 1962. Inquiries in Washington starting at the end of 1958 had smoked out several other mappers. One was Charles Harding Marshall (b. 1916), who was now assigned to Astrogeology by the Photogeology Branch and who prepared a study of mare-material thickness in his assigned LAC quadrangle (LAC 75, Letronne).66 Another recruit was Richard Elton Eggleton (b. 1932), who came from the Engineering Geology Branch (he had mapped the site of the present Dulles Airport) and was among the most enthusiastic and perceptive of the early mappers. He had already written down his ideas about lunar exploration at the time of Sputnik. Dick was a precise person; for example, he is the only one I have ever heard pronounce the letters in NASA separately to distinguish it from its predecessor, naca. He arrived in Menlo Park in October 1960, shortly after Marshall and Henry John Moore II (b. 1928), Shoemaker’s former field assistant in the uranium project whom Shoemaker had rehired in September.

Another landmark paper, written by Shoemaker and edited by Eggleton as his first task in the studies group, was a spin-off of the Copernicus study. This short but important paper appeared in the first semiannual progress report under the auspicious and fully justified title “Stratigraphic Basis for a Lunar Time Scale.” In December of that creative and intense year Shoemaker presented the paper at Symposium 14 of the International Astronomical Union at the Pulkovo Observatory near Leningrad (5–11 December 1960), and both it and the Ries coesite discovery were rereported in New York (27–30 December).67 Twelve pages long in its final published form in the symposium volume,68 this paper laid the foundation for all subsequent studies of the crusts of the Moon and planets based on historical concepts.69 It shows how the geologic units of the Moon’s crust can be recognized, ranked in stratigraphic sequence, and pigeonholed in systems by methods that are the same, in principle, as those applied to Earth’s rocks. The concept of systems and time-stratigraphic units of other ranks (series, stages) is a convenient and powerful way of organizing the many observations about relative age that are made during a geologic study. Once you have decided what system a rock unit belongs to, you have also shown which ones it does not belong to, and you are well on the way to placing it in its historical context.

To create the time scale one must attach absolute ages in years to the framework—the matrix, the “stratigraphic basis”—furnished by the systems. Absolute ages were the subject of still another major paper written in 1960, “Interplanetary Correlation of Geologic Time.”70 One way to determine the time when lunar units formed is to determine the crater density on Earth, where ages in years are known, and compare them with those of such widespread key units on the Moon as the Imbrium ejecta or “the” maria. The number and ages of Earth craters were very poorly known then and are still uncertain. Nevertheless,
the paper correctly concluded that the heavily cratered uplands, which the Copernicus map and "Stratigraphic Basis" had called pre-Imbrian, were formed in a much shorter time than were the relatively few postmare craters that constitute the Eratosthenian and Copernican systems. Another way to date rocks is to go to the Moon and collect datable samples. The paper correctly predicted that the primitive crust would be covered or broken up and that the main kind of rock would be breccias (complex assemblages of angular fragments broken from earlier rocks and cemented together). A prediction to which later geochronologists would say "amen" is that the breccias would be hard to date radiometrically because their isotopic "clocks" would be reset to a greater or lesser extent by the impacts that created them. Less clairvoyantly, the lunar origin of tektites was favored and the maria were assumed to be 4.5 aeons old and essentially synchronous. Strangely, the latter conclusion contradicted data gathered by Hackman and given in the paper itself, which showed that the crater density of mare surfaces differed by a factor of 2.

The third major component of the first year's Astrogeology program was cratering studies in the laboratory and field. The lab studies began through another of those ripe opportunities that Shoemaker exploited so well. The NASA Ames Research Center, devoted primarily to aeronautical research (and adjacent to huge dirigible hangars visible for miles around), lies within a short drive of the USGS center in Menlo Park. Shoemaker met Ames Assistant Director Harvey Julian Allen, who was interested in meteorites because of his pioneering work developing the blunt shape for heat shields of reentering spacecraft, and learned about a new gun promoted by Allen and developed by Alex Charters. Tiny models of experimental airplanes (full-scale versions would not fit into the Ames wind tunnels) were propelled by gas. The models reached velocities greater than the sound waves they set up (hypervelocities) and so, of course, were destroyed on impact. Recognizing an opportunity, Shoemaker offered to supply a target that would show how craters form during such impacts. Charters suggested that a young aeronautical engineer named Donald Eiker Gault (b. 1923) perform the experiments. One day in 1960 Gault fired an aluminum sphere into a piece of rock like that at Meteor Crater (Kaibab Dolomite) and got structures that looked very much like shatter cones. Thus began the enormously productive series of impact experiments with this gun and a successor developed by Gault. The USGS end of the cooperation with Ames was held up by Henry Moore, who was interested in breccias and therefore presumably in the rocks that the impacts would create. The experiments, which continue today (without Survey input) whenever money can be found, have shown like no others what kinds of craters are created by various impact velocities and angles in various kinds of target materials.
In January 1961 Shoemaker, Dick Eggleton, and another branch pioneer, Carl Roach of the Denver office, returned to Sierra Madera to begin serious fieldwork and concluded that it is the breccia lens of a crater 3 km in diameter. Later, Shoemaker, Eggleton, and Donald Parker Elston (b. 1926), who was drawn along with several other astrogeologists from the pool of Shoemaker’s associates in the Colorado Plateau uranium studies, returned to begin the geologic mapping. As has often been the case, the overworked Shoemaker later ceded the Sierra Madera study to an entirely new crew, who established that it is the rebounded central peak of an otherwise almost completely eroded 13-km crater.

**The Challenge (January – May 1961)**

On 28 July 1960, while Shoemaker was in Europe and LPL was taking shape, NASA announced a new plan to orbit a three-man spacecraft around the Earth and possibly the Moon and called it Project Apollo. The inauguration of President John F. Kennedy and Vice President Lyndon B. Johnson on 20 January 1961 ushered on stage the two political leaders who not only defined Project Apollo’s purpose but also established the nation’s role in space during the next 12 years. Eisenhower and his science adviser, James Killian, had not been worried by the Soviet lead in space, but Kennedy and Johnson were. After a classic LBJ arm-twisting there entered the man who led the counterattack and built NASA into a major force, James Edwin Webb (b. 1906), director of the Bureau of the Budget between 1946 and 1949, under secretary of state between 1949 and 1952, and NASA’s second administrator as of 14 February 1961. In March, Webb, Hugh Dryden, Kennedy, Johnson, and other stellar officials decided to push ahead with development of von Braun’s Saturn rockets.

On 12 April 1961 Russian cosmonaut Yuri Alexseyevich Gagarin (1934–1968) became the first man to venture beyond Earth’s atmosphere as he made one orbit in Vostok 1 of what Tsiolkovskiy had called the cradle planet of humankind. Something would finally have to be done. Within a week of Gagarin’s flight, the Bay of Pigs invasion of Cuba was bungled. On 20 April President Kennedy asked for a memo from Vice President Johnson recommending how the United States could restore its prestige. On Saturday, 29 April, he received the reply: land a man on the Moon.

In October 1960 a Vienna-born NASA engineer named George Michael Low (1926–1984) had sent a memo to Abe Silverstein, the director of the Office of Space Flight Development, recommending a start in planning such a landing, and now the study was ready with the data that showed it could be done. In a repeat of the Eisenhower administration’s attitude, Kennedy’s science adviser, Jerome Wiesner, and the President’s Science Advisory Council chaired by
Wiesner spoke against the manned landing as unscientific. Kennedy himself asked, "Can't you fellows invent a race here on Earth that would do some good?" But he had almost decided that the answer was probably no. On Friday, 5 May, a little of America's pride was restored by the first manned flight of Project Mercury, a 15-minute up-and-down suborbital ride by Alan Shepard. While Shepard was being honored at the White House the following weekend, Webb, NASA Associate Administrator Robert Seamans, and one representative each from the Department of Defense and the Bureau of the Budget burned the midnight oil preparing a strong statement recommending the manned lunar landing. The memo was in President Kennedy's hands on Monday, 8 May, and (in another harbinger of things to come) Lyndon Johnson prepared to leave on a trip to Southeast Asia knowing that he had won.

Then, on 25 May 1961, came the dramatic speech that determined which way and with what spirit the United States would make its move into the cosmos. President Kennedy voiced his ringing challenge for Project Apollo to land Americans on the Moon and return them safely to Earth before the end of the decade. In July Congress approved the initiative after relatively little debate. A great program was put in motion. The sky scientists complained when the unmanned program was reconfigured to support Apollo and have never stopped complaining. Other critics pointed at the waste of money, which they felt should be spent on more noble causes of their own choosing rather than on a welfare program for white male engineers. But Kennedy gave us a goal and purpose such as a nation rarely offers its citizens in peacetime. We were going to the Moon.