4

Preparing to Explore
1963–1965

TRAINING THE EXPLORERS

Fieldwork is beloved by most geologists, or at least it was before the computer age, and is accorded a vital role even by people like Mike Carr and myself who do not like to do it personally. Many geologists who joined Gene Shoemaker's Branch of Astrogeology hoped to ply their trade on the Moon itself, a hope that had triggered Gene's own interest in the Moon and the space program. At first, he wanted us to start learning the flying part of the job by taking lessons with light airplanes. Some geologists obtained licenses and eased the burden of travel between isolated Flagstaff and Menlo Park or JPL by piloting jointly owned or leased airplanes. But the lunar orbital rendezvous mode adopted in July 1962 for lunar missions demanded piloting finesse beyond the reach of most incidental pilots. Astronauts who were already pilots, preferably test pilots, would perform at least the first phases of lunar fieldwork. It was the job of earthbound geologists to train them in geology.

The training was the result of one of Shoemaker's initiatives at NASA Headquarters. A trial run quickly got under way in January 1963 with the nine newly selected (September 1962) test-pilot astronauts of the second, so-called Gemini group as guinea pigs. Gene subjected them to an intensive two-day field trip in and around Flagstaff that included Meteor Crater, nearby volcanic features, classroom lectures, telescopic observing of the Moon at Lowell Observatory, and little sleep. The astronauts were favorably impressed and seemed eager for more.

At about the time of this field trip, Shoemaker and NASA concocted a plan to establish a resident staff of USGS geologists at the Manned Spacecraft Center in Houston to train the astronauts intensively and to provide other geologic support such as instrument development and mission simulation. In April 1963 letters and memos to that effect were exchanged over the signatures of USGS

Director Thomas Nolan and MSC Director Robert Gilruth. The agreement was worked out on the Survey side by Shoemaker and on the NASA-MSC side by the cooperative Maxime Faget, chief of MSC's Engineering and Development Directorate, and the less cooperative John Eggleston, assistant chief of the Space Environment Division. Six geoscientists (geologists and geophysicists) were to be assigned to the Space Environment Division and would take up quarters at Ellington Air Force Base, the interim site of MSC (now Johnson Space Center, JSC) during the construction of the spacious new "campus" at Clear Lake that is its present site. Shoemaker chose USGS geologist-petrologist Everett Dale Jackson (1925–1978), a Marine veteran of the Iwo Jima landing, to lead this presumably less arduous effort.

Over lunch in the NASA Headquarters cafeteria one day Homer Newell and Nolan had agreed that the Survey should take on the major role in supporting NASA geologically.[1] NASA would not build up a little USGS of its own. Specifically, NASA would not build up a laboratory capable of analyzing the forthcoming Moon rocks; the USGS was available for that. The duties of the USGS Houston office originally even included the establishment of what eventually became the Lunar Receiving Laboratory.[2] These informal agreements, which were never sent up to NASA Administrator Webb, would affect the rest of our careers and the course of lunar geologic fieldwork. But in the near future their effects would be zero, or rather, negative. Dale's surprise was profound when, on arriving at MSC in July 1963, he found a group of NASA Space Environment geoscientists all set up to do his job. The parallel with Iwo Jima might be closer than imagined.

There was another potential problem. Dale had heard a story about a certain geoscientist of the Goddard Space Flight Center who had got himself either into the ready room or actually to the port of a Mercury capsule ready for launch, holding a piece of basalt. He is alleged to have attempted to inflict a crash course in geology on the irritated astronaut. So, despite the success of the January field trip, the astronauts' reaction to geology training was uncertain. Another cafeteria was the scene of the next milestone. Dale recognized Wally Schirra, well known for his Mercury flight in October 1962 and now the astronaut in charge of training, sitting in the Ellington cafeteria and thought, well, it's now or never. Schirra said they would gladly learn geology, and the program could begin.

Dale was accompanied in July 1963 by Dick Eggleton as a temporary assignee. In that same month the telephone rang while I was sitting at Dollfus's desk in Meudon (a minor miracle for French telephones in those days) with a call from Shoemaker asking me if I would move to Houston as the resident lunar expert. I had been studying the Moon for six months. I did not hesitate to accept, despite my desire to remain in the San Francisco Bay area, and I arrived at Ellington

Field in October 1963 along with geophysicist Marty Kane. One old-fashioned field geologist, Alfred Herman Chidester (1914–1985), had arrived in August, and another, Gordon Alfred Swann (b. 1931), would arrive in March 1964. Impact expert Dan Milton also arrived in March 1964 to complete the USGS crew for what we all thought would be a two-year stint. An intensive course of 58 hours of classroom lectures and numerous field trips was planned. The USGS people would take the lead in the geologic aspects of the courses, our NASA counterparts would concentrate on mineralogy and petrology, and both groups would conduct the field trips. Dale was to be the overall boss but would plan the program jointly with the leader of the NASA group, Ted Foss.

One of the first casualties of MSC's newfound interest in geoscience was the Survey's control over sample analyses. Already in 1964, meteoriticist Elbert Aubrey King, Jr. (b. 1935), who had joined the NASA group in August 1963, began to plan what eventually became the Lunar Receiving Laboratory (LRL). In this laboratory the sample boxes would first be opened, time-critical examination could be conducted, both the astronauts and the alien Moon rocks would be carefully isolated from Earth's atmosphere, and, once given a clean bill of health, samples would be distributed to laboratories around the world. LRL would also provide permanent controlled storage for the lunar samples. Somebody evidently had read H. G. Wells's *War of the Worlds,* for the quarantine requirement greatly increased the size and cost of what was at first planned as a modest facility. This over the objection of King, who argued that if you wanted to design a sterilizer, you would design something very much like the Moon's surface.[3]

A new, third group of 14 astronauts was announced in October 1963 and reported for duty at MSC in January 1964, including Buzz Aldrin, who was already working at MSC. At the time of its selection, the group boasted one Ph.D. (Aldrin) and eight master's degrees, but all were trained as military pilots. A Houston newspaper, pitifully grasping for a prestige restorer, headlined the announcement of the selection with: "New Astronauts Outshine Russ with Education." This was the so-called Apollo group, as opposed to the Mercury (first) and Gemini (second) groups. A month later all the active astronauts of all three groups were sitting in the first geology class.[4] That gave us a total of 29 students, John Glenn having resigned in January to direct his attention to politics. The geology training would gobble up large chunks of the astronauts' valuable time. But they were bright-eyed and bushy-tailed and wanted to miss nothing. Moreover, the supposedly ailing former Mercury astronaut whom they had chosen as their chief, Donald Kent ("Deke") Slayton (b. 1924),[5] required them all, including himself, to attend all the lectures and field trips unless excused by flight preparations or some other unavoidable commitment. Our point of view

was that the Moon is made of rock, and a large block of relatively inexpensive shirt-sleeve time on Earth might be the key to choosing the most important samples during those precious hours on the Moon.[6]

On 5–6 and 12–13 March 1964, after four general lectures by Dale and myself and an orientation by Chidester, two groups of astronauts and geologists climbed down the Grand Canyon—not a moonlike place, but Dale thought it would impress on the astronauts the fundamental geologic concept that young rocks lie on top of old rocks. We had often encountered the amazing inability of nongeologists to grasp this notion or to apply it to the Moon, but we hoped two days on shoe leather and muleback might do the trick. I was among the 31 people on the first section of the trip, along with Dale Jackson, Dan Milton, and Al Chidester of the USGS; Ted Foss, Uel Clanton, and Elbert King of NASA; and astronauts Scott Carpenter, Al Shepard, Neil Armstrong, Elliot See, and the entire group of 14 Apollo astronauts. USGS geologist and Grand Canyon expert Ed McKee gave the orientation at Yavapai Point before we descended. Groups of four, each consisting of one geologist and three astronauts, then hit the trail. Our students proved to be much quicker at getting the point than many scientists, and they also seemed to understand what their teachers were saying about the origin of the rocks and the faults that cut them. My only other relevant memory of the trip is our amazement that while everybody else was accumulating a layer of field dirt, the athletic Scott Carpenter did not even soil his white tennis shoes. A week later Jack McCauley helped instruct a second group whose stars included Mercury astronauts Gordon Cooper, Gus Grissom, and Wally Schirra and second-group future stars Frank Borman, Pete Conrad, Jim Lovell, Jim McDivitt, Tom Stafford, and John Young.

Another basic geology lesson came on 2–3 and 15–16 April 1964, when we interpreted and mapped some of the assorted structural and stratigraphic relations that are nicely exposed in the Big Bend–Marathon Basin region of west Texas. We were accompanied for the first time in the field by Gordon Swann, who had joined our group the month before, and by University of Texas geology professor Bill Muehlberger, who served as an expert local guide. Gordon and Bill were getting their first but far from their last taste of fieldwork with the astronauts; six years later they would lead the geology teams that guided the geologic exploration of the Moon.

The purposes of the trips were both to teach principles and to walk moonlike terrains. Between 29 April and 2 May 1964, and again between 20 and 22 May, we went back to Arizona, this time on a double-feature trip more directly lunar in content. Half of the show was volcanic and was presented near Flagstaff. We went on the ground to the Sunset Crater cinder cone and the nearby lava flows. Pilot Jack McCauley and nonpilot Gene Shoemaker conducted fly-arounds in

light planes to view cinder cones, maars, a small caldera, and Meteor Crater from the photogeologist's or orbiting astronaut's perspective. The other half of the trip was to the new Kitt Peak National Observatory near Tucson. Astronomers are jealous of their time on large telescopes, but they could not use the McMath solar telescope at night, so we projected a beautiful "live" 85-cm image of the Moon on its viewing table and spoke thereto. Hal Masursky, geologist Spencer Titley from the nearby University of Arizona, Jack McCauley, and I were the Moon experts. Special viewers had been fabricated by Elliot Morris to enlarge selected parts of the image. This was my trip to organize, and I made sure no time was wasted. I remember in a fit of scientific purity chasing off NASA public relations man Paul Haney and the photographers who were always hanging around, thereby proving that I did not know who was running the manned spaceflight program.

It was on this trip that Dale Jackson acquired a nickname that stuck. *Arizona Republic* (Phoenix) reporter Harold Williams wrote that Dale was a "burly man who resembles a lumberjack more than a doctor of geology," so ever after Dale was the "burly lumberjack." Whatever he looked like, Dale was a fine geologist, and he understood profoundly what the exploration of the Moon would ask of an astronaut holding a rock hammer. Dale may have been a little too sure that he knew, though, and he did not gladly suffer fools or people he thought were fools.

A less spectacular fourth and last trip of this general phase 1, or first term, of the training was on 3–6 June 1964 to the Philmont Boy Scout Ranch in New Mexico. There were no obvious lunar analogues at Philmont, and the geology was hard to follow on the existing geologic map. But Gordon Swann said, this was "facts of life" geology—messy, hard to work out, and thus in a sense probably quite lunar. Our students this time were a single group of 20 astronauts, including Ed White, whom Deke Slayton had yanked from the celebration of his brother's graduation from the Air Force Academy to fulfill his duty to the geology training.

The Philmont trip closed out both the field season and, after a total of 13 lectures, the residence of the USGS personnel in Houston. The conflict with the NASA geoscience group had proved intolerable. The animosity between Dale and his NASA counterpart, Ted Foss, was particularly severe. Dale could not forgive NASA for going back on the agreement to let the USGS run the entire training program. I cannot recall what Foss's problem was. At any rate, Dale, Dan Milton, and I went back to Menlo Park, Gordon Swann temporarily went back to Denver, and Al Chidester transferred to Flagstaff. The jovial Chidester took over management of the USGS end of the training program from the less jovial but better focused Dale. Lectures in Houston would be given by visiting experts. A formal agreement (drawn up without Dale) spelled out that Chidester

would *recommend* training areas and the outside expert in the area's geology, Foss would handle all interactions with the astronauts, and the thorny problem of press relations would be neutralized by telling the reporters that the visiting outside expert was leading the trip.

In July 1964, as the first phase of the training ended and the Ranger project finally had a success on its seventh try, the Astrogeology office at Flagstaff welcomed the entry on duty of another geologist who would literally leave his mark on lunar geology in a way none of the rest of us could — Harrison Hagan ("Jack") Schmitt (b. 1935). Jack had inherited geology from his father, Harrison Ashley Schmitt, who had been a mining geologist in New Mexico. Jack had his bachelor's degree from Caltech, his brand-new Ph.D. from Harvard, and bachelorhood combined with what are often described as swarthy good looks. His opportunities seemed boundless.

THE GROUND SUPPORT

The Flagstaff Astrogeology office began in 1964 to gear up its program of planning and simulating the surface missions, efforts that were part of the original charter of the Houston office but never materialized there beyond the writing of a few reports.[7] When the Houston office dissolved, the branch was organized formally into three divisions: one each for support of unmanned and manned missions and a third for "pure science." Shoemaker turned each of the three disciplines over to a coordinator. Jack McCauley coordinated support of the Ranger, Surveyor, and Lunar Orbiter programs under the heading Unmanned Lunar Exploration Studies. Don Elston coordinated Manned Lunar Exploration Studies (or Investigations). Hal Masursky, still in Menlo Park, led Astrogeologic Studies, which included outgrowths of the branch's original threefold investigations of lunar, cratering, and tektite-meteorite subjects.[8] In August 1964, when about 14 professionals and many helpers were in Flagstaff, ground was broken for a building that the branch could occupy permanently, thus promising to end the time-wasting game of musical office buildings that characterized its first years on the Colorado Plateau.

In January 1964 Surveyor investigations, a long-standing passion of Shoemaker, became an official USGS project and the largest item in the unmanned-studies docket. Shoemaker had been the principal scientific investigator of the television experiment since January 1963, and now an extensive program of testing and calibration of the Surveyor cameras would demand much effort from him, geologist Elliot Morris, photogrammetrist Ray Batson, and a growing staff of able specialists in electronics, optics, and instrument making. In the summer of 1964 test cameras built by Hughes Aircraft Company were set up on

the young Bonito lava flow in Sunset Craters National Monument 25 km northeast of Flagstaff because astronomical data suggested that the rough lava-flow surface would reflect light as the lunar surface does. The tests were conducted in close collaboration with JPL, and the Santa Fe trains and branch planes shuttled personnel and equipment from both organizations back and forth between Flagstaff and Pasadena.

McCauley was closely involved with testing a rover (the *Surveyor lunar roving vehicle* [SLRV]) that was proposed for the Surveyor program in late 1963. The SLRV was conceived as a lightweight (about 45 kg) machine that could range at least 1.6 km from a landed Surveyor and test the roughness and bearing strength of the surface for Apollo by means of a penetrometer. The rover would traverse back and forth along a grid, and an accurate topographic map would be made from stereoscopic imagery transmitted from a small facsimile (scanning and digitizing) camera manufactured by the Aeronutronic Division of Ford Motor Company.[9] The complex proposal died when Surveyor was scaled back in mid-1965.

The mission-support studies blossomed during 1964 and began to dominate Flagstaff's geologic efforts. People and equipment began to arrive in quantities which in today's penny-pinched world would make grown scientists weep. A group of dedicated and competent geologists, most of them with fresh Ph.D.'s, was assembled with the aim of eventually supporting the geologic exploration of the Moon by the astronauts. In October 1964 the manned-studies group got one of its main stalwarts, Gordon Swann, from the Denver office of the Survey along with Joseph O'Connor. The supportive USGS assistant chief geologist for engineering geology to whom the Branch of Astrogeology reported, Verl R. ("Dick") Wilmarth, went around to universities recruiting students. Pennsylvania State University was the richest source, furnishing John M'Gonigle, David Schleicher, Tim Hait, Ivo Lucchitta, and Baerbel Lucchitta. Baerbel accompanied Ivo in the role of housewife at first but soon tired of that. At the University of Cincinnati Wilmarth recruited Lawrence Rowan, who in turn interested Gerald Schaber. Gerry entered on duty in Flagstaff in July 1965 and worked on field exercises and analytical instruments that were to be used on Surveyor and later manned missions.

A mock-up of the LEM was supplied through the Space Environment Division of MSC despite their recent clash with the USGS. In early 1965 there arrived a big, fancy, well-equipped truck called the Mobile Geological Laboratory. A flying machine that readers of Buck Rogers comics would recognize as a flying belt actually got people off the ground and safely back down again. "Manned" personnel, especially Swann, Hait, O'Connor, Schleicher, and George Ulrich, would become thoroughly familiar with the wearing of space suits.

George Erwin Ulrich (b. 1934) and Mortimer Hall Hait (b. 1931) were two stalwarts of the geology team who glued the program together throughout its later history. George entered the USGS on the Kentucky Project, as did many other geologists in the 1960s, and joined Astrogeology in September 1965. He is a straightforward chap with a dry wit who modestly downplays his role in the lunar program, but he was a major player in the mission training and simulations starting with Apollo 14 and a crucial organizing force in mission operations starting with Apollo 15. At the suggestion of fellow Penn Stater John M'Gonigle, Tim Hait came to Flagstaff from Texaco in January 1966 and was in the back room for every Apollo mission. Besides the field exercises, his jobs within the whirlpool of mission preparation included studies of the hand tools and the means of communicating geological observations verbally. The tool work included tests, with Gordon Swann, in the Vomit Comet KC-135 that served NASA throughout the manned program as an inducer of weightlessness as well as airsickness.

The manned studies became a beehive of activity, and few of its personnel remember just who did what for which activity and under which branch subdivision. They blasted craters in a volcanic cinder field near Flagstaff to simulate the lunar surface, and looked farther afield for more terrains to conquer. They tirelessly devised mission profiles, time and information studies, all sorts of time-saving surveying and data-collecting gadgets, communication devices, cameras, and anything else they or MSC could think up. Their task was totally new, and they were not sure what would matter and what would not. The astronaut training effort under Chidester also occupied a box on the organizational chart, and in late 1965 the amiable Chidester became chief of the manned studies. An "in situ" geophysics project to develop methods for determining the near-surface properties of rock units, a neglected subject in traditional geophysics, became very active and visible under its ambitious chief, Joel Watkins. Other projects were devoted to surveying, electronics, and documentation. A project called Lunar Field Geological Methods was led by Jack Schmitt. Geologic mapping was supposed to be a tool for learning about the Moon and a unifier of all the otherwise diverse activities of the branch, so like almost everybody else in Astrogeology, Schmitt was assigned a lunar quadrangle for mapping in addition to his mission-support jobs. His task was to pick up the mapping of the Copernicus quadrangle where Shoemaker had left off four years earlier. This was the era of special features, and the sharp-eyed Schmitt was adept at finding them in and near Copernicus.[10] Somewhere in this book—it might as well be here—I have to report that his nickname at both Harvard and Flagstaff was Bull Schmitt.

Actually, I'm not entirely sure of what the manned-studies group did do. The mission-oriented efforts signaled a split in the ranks of the USGS astrogeologists

based on personal predilections. Some people were attracted to this nuts-and-bolts activity that would directly influence what happened on the Moon. Their efforts seemed scattershot in 1964 and 1965 but eventually funneled into well-honed and clear-headed preparation, back-room support, and reporting of the astronauts' geologic work on the Moon, as we shall see. Other geologists, including myself, preferred more academic activities like lunar geologic mapping. Each faction was bored by the work of the other. To each his own, and it is good that both types of geologists came into the program.

The split engendered a mild rivalry that coincided in part with another rivalry, also based on personal predilection, between Flagstaff and Menlo Park. This split was basically between country boys and city boys. After March 1962 Shoemaker made clear to those he interviewed that their job was in Flagstaff. The USGS still had no telescope of its own, though, and because visual lunar observations were considered vital, most of us Menlo Parkers were allowed to stay put temporarily to use the Lick Observatory telescopes. My excuse for remaining in Menlo Park was that I had the polarization project to perform, and it was then thought to require a refractor; the Lick 12-inch refractor was ideal and was not being used much. A full-time effort by Elliot Morris beginning in late 1962 to obtain and install a new 30-inch reflecting telescope culminated when this excellent instrument became operational on Anderson Mesa near Flagstaff in May 1964—and proved suitable for the polarization project. By then, though, Shoemaker had relented in his requirement that we all move to Flagstaff. Mike Carr, Henry Moore, and Dan Milton, the original impetus for the Flagstaff move, had dug in their heels in Menlo Park. Hal Masursky also still preferred the diversity of urban life at that time and argued for the value of our contact with Menlo Park's hundreds of experienced terrestrial geologists in other branches of the USGS. Hal ran the more nearly pure science effort from Menlo Park via the frequent telephoning and traveling that always characterized his work week.

Astrogeology's new building in Flagstaff was dedicated in October 1965 in an all-out two-day affair that brought all of us from Menlo Park. Also there were Oran Nicks, director of Lunar and Planetary Programs in OSSA; Willis Foster, also of OSSA, who since November 1963 had been director of the Manned Space Science Division that Shoemaker had started unofficially in 1962 and 1963 and who reported to both OSSA and OMSF;[11] recently appointed USGS director Bill Pecora; and many other dignitaries. To the annoyance of us Menlo Parkers, the new building was called the Center of Astrogeology, and to the annoyance of a later Survey office chief, the sign out front included no mention of the U.S. Geological Survey. Pecora was, I believe, the only Survey director to rise higher in the political hierarchy; he served as an under secretary in the Department of the Interior between May 1971 and his death in July 1972. But Shoemaker was

his political match. During the dedication Pecora said in public, "The Survey is proud of its daughter organizations," implying he would like Astrogeology to be the next daughter. In private he said, "I would sell this outfit to NASA if I could get a good price." And, "This would never have happened if I had been director then." I said, "Shoemaker is hard to stop." Pecora said, "Wanna bet?" I should have made the bet; Pecora lived to realize that he had underestimated Shoemaker. But a substantial group of astrogeologists stayed in Menlo Park and carried on a friendly competition with the Flagstaffers throughout the Space Age.

MORE BASINS

For the first half of the 1960s, Imbrium remained the most intensively studied basin for the unexceptionable reason that it is the biggest conspicuous basin on the near side of the Moon. The sculpture studies by Gilbert and Baldwin, the ring studies by Baldwin and by Hartmann and Kuiper, and the stratigraphic studies by Shoemaker and Hackman had already established by the start of 1962 that Imbrium has (1) concentric rings, (2) radial grooves and ridges, and (3) hummocky deposits. But how typical is Imbrium of other basins?

The rings that shone forth from the Lunar and Planetary Laboratory's rectifying globe showed that Imbrium is just one basin among at least 12. Rings seemed to be spaced at distances that increased from one to the next by a factor of 2, or, more likely, the square root of 2. There seemed to be some underlying physical law that rings are created with these separations when large objects strike planets. Whether there is such a law has been debated ever since, and that pesky square root of 2 keeps popping up. But at least we realized that basins form a related class of objects. The most spectacular of all, in fact, is not Imbrium but Orientale. Before the end of the 1960s the Orientale basin would take its place beside Imbrium as the other classic "type" lunar basin.

To talk of Orientale is to talk again of Jack McCauley. February 1963, when McCauley joined the branch, was just ever so slightly later than September 1962, when Mike Carr and Hal Masursky did, and December 1962, when I did. McCauley therefore got the best map assignments remaining after the rest of us got ours, and he ended up with two quadrangles—Hevelius (LAC 56) and Grimaldi (LAC 74)—way around on the west (formerly east) limb of the Moon. He was determined to make the best of those seemingly leftover quads and studied them carefully on photographs and at the telescope, which I think he enjoyed using as much as I did. He also conferred with Bill Hartmann in Tucson, whose rectified views had resurrected Orientale from limb limbo. From his telescopic observations, McCauley now identified and mapped the hummocky ejecta blanket and even took a crack at measuring its thickness from its burial

of craters.[12] At first he was a little unsure about the relative ages of Orientale and Imbrium, but that later became clear as Orientale was revealed from crater counts, topographic freshness, and superposition relations as the Moon's youngest large basin. He also found Orientale structures cutting across the Humorum basin and an indistinct basin south of Orientale that Hartmann and Kuiper had called the Southeast basin.[13]

Only one other basin besides Imbrium and Orientale, Humorum (centered at 24° S, 39° W), was studied really carefully before the Apollo landings. It had been assigned to Chuck Marshall before he quit. Dick Eggleton also worked on it. Then it was passed on to two geologists whose specialty was finishing the work of others. First came Spence Titley, one of the few non-Survey geologists who participated in the mapping program in the 1960s.

I digress to pursue this point a little. The USGS was sometimes criticized for being the only lunar geologic game in town, but Kuiper never mounted a concerted effort to supplant us, and nobody else tried at all. We tried to bring in outsiders but had only limited success. The Moon frightens people for some reason. They think its study is something exotic, when really it is just a different form of geology. In particular there was a peculiar silence about lunar geology from the hallowed halls of academia. Titley was one exception, and chapter 10 will tell of the brilliant entry into the field by Tim Mutch of Brown University. A few other university geology professors tried their hand at lunar geologic mapping, some after taking two-week courses run by Jack McCauley and Northern Arizona University in 1967 and 1968 under the sponsorship of the National Science Foundation. But nothing of much value came of these professorial efforts in the 1960s. As a group they caused me, in my role as coordinator of the geologic mapping program since 1964, more trouble and annoyance than any group of "in-house" mappers except one or two who will remain unmentioned. Most of the professors seemed to be good geologists, but maybe their university commitments kept them from devoting the time that was required for a credible job of lunar mapping. Other geoscience contributions were made by Professors Aaron C. Waters (University of California at Santa Barbara), J. Hoover Mackin (Texas), and Edward N. Goddard (Michigan), recruited by Wilmarth to serve on an Apollo Field Geology Planning Committee headed by Shoemaker, which grew into the Apollo Lunar Geology Field Teams. The universities were active in space physics, complaining all the while about NASA while NASA complained about them.[14] Nevertheless, I think it is fair to say that with these and a few other exceptions the universities mostly held back from involvement with lunar geoscience until the time to study the Apollo samples drew near. Spence Titley might give a different interpretation about their noninvolvement in lunar geology. USGS astrogeologists pretty much ended up dictating to Spence how and what to map.

The mapping of Humorum was finished by still another relief pitcher, the exceptionally able Newell Jefferson Trask (b. 1930, nine days after me). Newell entered on duty in September 1964 and, like so many others not hired and inspired directly by Shoemaker himself, never really warmed to the lunar work. Still, he mastered and advanced it. Being more quantitative minded than I, he was better suited to the polarization project, which he took off my hands.[15] He also essentially took over the Humorum and adjacent Pitatus quadrangles. I really do not know at this point who did what; probably this was another case of collective consciousness. At any rate, it was concluded that Humorum has a hummocky ejecta blanket and a rugged rim like those of Imbrium, and the hummocks are not from Imbrium; Humorum has a pre-Imbrian planar bench that is not covered by Imbrium; and Orientale deposits overlap the western Humorum terrain.[16]

So in the mid-1960s we had started the divorce from Imbrium and had begun to build a moonwide stratigraphy. The Imbrium deposits remain a major stratigraphic marker—the base of the Imbrian System—but geologic units exist on the rest of the Moon, too, and could be fit into a stratigraphic framework whether Imbrium existed or not. A decade after the Humorum work the framework was completed. Imbrium is far from the only basin on the Moon; when I last counted it was only one of 45 larger than 300 km across.[17]

MARIA AND DARK MANTLES

Mike Carr was deeply involved with the shock and dust studies and was not considered primarily a geologic mapper, so his two quadrangles were occupied mostly by the simplest type of lunar geologic unit, the maria. Despite having only one usable eye after January 1964—because he picked up an explosive charge being used for the shock study to see why it failed to explode—Carr made a major discovery in one of the quadrangles, Mare Serenitatis. Shoemaker and Hackman had interpreted dark, hummocky terrain around the Imbrium basin as a dark facies of the Imbrium ejecta. Carr found some critical geologic relations that told a different tale. He discovered terrain adjacent to the dark hummocks that was equally dark but smooth and level. Unlike myself, he did not like to map the Moon geologically (I finally tired of it too). Yet when he applied one of his typical flurries of energy to lunar geology, he usually came up with original and sensible observations. The dark mantle was one of these. Carr saw that the dark materials are not what the party line said but rather a type of rock related to the maria. Since the dark materials mantled hummocks and flat terrain alike, they are younger than the maria and probably *pyroclastic;* that is, ash or other volcanic fragments that rained down from above. Carr also suggested

that (1) the pyroclastics and related rocks were erupted from the Sulpicius Gallus, Menelaus, and Littrow rilles along the border of Mare Serenitatis, and (2) some dark mantling deposits are older than the adjacent mare units.[18] Like many other USGS telescope-based observations, these affected where astronauts landed a few years later.

Carr's study had another strong though indirect effect on landing-site selection as well as on our geologic mapping. He thought that the central light-colored part of Mare Serenitatis has more craters than the bordering dark mare and dark mantling material. Moreover, in most though not all cases, dark mantling units and mare units overlie brighter mare units, meaning that the dark units are younger than the bright ones. This made sense; brightness was presumably due to the slopes of the many unseen craters that had accumulated on the old mare units. Thus arose the rule of thumb: dark = young. In contradiction, R. T. Dodd, Jack Salisbury, and Vern Smalley at the Air Force Cambridge Research Laboratories detected more craters on the dark border than on the light center of Mare Serenitatis though they did not claim certainty for the result.[19] Another dissenter in the mid-1960s was the astute Newell Trask, who realized that mare albedo may well be related to composition, not age.[20] The rule of thumb would not be challenged definitively until Apollos landed on the Moon.

VOLCANOPHILIA LIVES ON

As part of the follow-up studies of the Fra Mauro Formation left over from Eggleton's work I tried to pin down a description of the formation with all the trimmings of complete and objective terms demanded by the stratigraphic code by dividing it into *facies* (laterally gradational textural variants). In so doing I felt compelled to separate the light plains from the hummocky deposits.[21] Ah, the plains. Most of us thought they were volcanic. When Dan Milton correctly pointed out that they contain no marelike "wrinkle ridges," and so probably are less consolidated than the mare basalts, he was thinking more of tuff than impact breccia.[22] I was much impressed by the seemingly clean transections by the plains of Imbrium sculpture and also by their nonhummocky textures. Such different units as the hummocky Fra Mauro and the plains should be mapped separately for objectivity no matter what one thinks about origins, especially in the early data-gathering stages of an investigation. Origins are especially hard to determine for units with so few distinguishing characteristics as plains. Our discrimination of the circum-Imbrium plains from the Fra Mauro Formation led me to establish another formational unit about which the world would hear much, the Cayley Formation.[23]

The hummocky and pitted terrain near Descartes, first identified by Eggleton and Marshall as Imbrium ejecta,[24] also got caught up in the special-feature volcanophilia, with major consequences. Most of us rebels argued for volcanic origin of these special features in the terrae. The first to put this thought on paper was Dan Milton, to whom the quadrangle that contains them, Theophilus (LAC 78), was assigned for geologic mapping.[25] Dan is enlivened by vast knowledge, total recall of the many subjects that interest him, and a very high IQ, but also by a substantial negative streak. I think this contrariness, along with a probably related dislike for lunar geologic mapping, is what led him to so firmly reject the Shoemaker-Hackman-Eggleton emphasis on the Imbrium impact. His advocacy of the volcanic origin of the Descartes hummocks supported my own inclination to volcanic origins (based largely on my love for stratigraphic purity in lunar geology). Dan pointed out the superposition of the Descartes hummocks (which he called Material of Kant Plateau) on what he identified as Imbrium ejecta. I accepted his belief that the hummocks are distinct from the Imbrium material.

Sometimes the volcanophilia of the 1960s was justified. In his mapping of the Kepler region, which in 1962 became the first of 44 maps published at the 1:1,000,000 scale, Bob Hackman had identified some small hills near 50° west longitude as our familiar Imbrium basin "hummocks." Similar hills west of 50° were in one of Jack McCauley's limb quadrangles (Hevelius), and he studied them carefully. First, he noted that they are dark and suggested that they are "hummocky Apenninian" covered by volcanic ash.[26] Jack also worried about this "Apenninian" age. As a good geologist, he studied their age relations with the telescope and observed that they, or something, seemed to obscure the secondary craters of the nearby crater Marius (12° N, 51° W). Since Marius is too fresh to predate the Imbrium basin, the hills must also be younger than the Imbrium basin. After further detailed study he concluded even more boldly that the hills have no ejecta component at all but are volcanic cones not only younger than Imbrium but possibly younger than the great expanses of the "Procellarian" mare material that surrounds them. Jack also suggested that their steepness indicates composition by rocks more highly differentiated than the mare-forming basalts. Thus came on the scene a region that would remain in the forefront of spaceflight mission planning as late as 1971, the Marius Hills.[27]

The rest of us were also trying to find more interesting things than the monotonous Imbrium basin geology that the old guard thought covered everything. Hal Masursky, in one of his rare writing efforts,[28] described the isostatic rebound of crater floors that partly explains the brim-full appearance of Ptolemaeus and also explains many special features, as later chapters show. During

his sessions with the 36-inch telescope studying the Aristarchus Plateau and Harbinger Mountains, Henry Moore "discovered" a large variety of volcanic features in addition to dark mantling material (he mentioned the pyroclastic idea but was afraid to state it boldly). There were numerous "domes"; rimless, probable maar craters; sinuous rilles including the granddaddy of the class, Schröter's Valley; and the large cone containing its source, known from its relation to the snakelike valley as the Cobra Head.[29] Some of these volcanic features are indeed present, though not quite so many as Henry thought then. Hal's floors and Henry's special features resurfaced a few years later when the time came to pick targets for Lunar Orbiter photography and manned landing missions.

I have a special dislike for one class of lunar features: lineaments. The Moon does, of course, contain linear and gently arcuate features. Negative ones include straight and arcuate rilles, which are *graben* (strips that sank between parallel faults). Positive ones include the wrinkle ridges that characterize the maria. Graben and wrinkle ridges exist and tell us something about the Moon's tectonics. By "lineaments" I mean all the vague alignments of features that are well seen on poor photographs and poorly seen on good photographs. Like the canals of Mars, they go away when seen more clearly. Most notorious is the lunar grid, beloved by Josiah Spurr, A. V. Khabakov, Kurd von Bülow, Val Firsoff, and Gilbert Fielder, but firmly put down as nonsense by the sensible Baldwin.[30] Fielder was not a grid fanatic at first, but in a 1965 book with the promising title *Lunar Geology* he explained the grid and almost everything else by endogenic mechanisms that now seem naïve. My colleague Dick Pike has heard that Fielder came too much under the influence of still another English endogenist astronomer, Brian Warner, and the many references to Warner in the 1965 book support this idea. Fielder also cited as a grid-generating mechanism the lunar convection being advocated, then and now, by English geophysicist Stanley Keith Runcorn.[31] I am sure the reason lineaments are so attractive is that they are quantifiable. If you make rose diagrams of trends — and anybody can do it — you are doing science. If you make geologic maps — which seems to be a rarer skill — you are guessing. However, the Moon and planets are made of bedded rocks, not networks of lines.

AGES

For the rest of the decade and beyond, USGS astrogeologists and the few others who then constituted the lunar geologic community were busy checking and fitting observed geologic units into the stratigraphic scheme devised at the November 1963 conference. Spence Titley observed that Gassendi and a number of other craters have the Archimedes-type relation of superposition on

the Humorum basin and flooding by Mare Humorum. Jack McCauley thought that the crater Crüger is overlapped by the Orientale deposit and filled by the mare — that is, that Crüger is the Archimedes of Orientale — but neither he nor I have ever been sure that it is. All mappers of all basins were finding light plains deposits superposed on the basin but covered by the mare material, as is the Apennine Bench Formation at Imbrium. Several of us found evidence for more than one mare emplacement episode, as did Dodd, Salisbury, and Smalley. Henry Moore found evidence that some young-appearing craters have been flooded by one mare unit but are younger than other mare units, indicating a spread in mare ages in relatively recent times.[32] The span of mare emplacement was therefore being extended beyond the single pulse or short period that almost every early investigator had hypothesized or assumed.

Thus lunar stratigraphy was getting complex in detail though not in principle. At each basin there seemed to be a sequence: (1) prebasin rocks, (2) basin, (3) light plains and craters interfingering, (4) mare units, and (5) more craters and other units thought to be young because of their albedo extremes.[33]

We thought those "other young units" were of two kinds: unusually dark and unusually bright. Mike Carr's study, supported by most of the rest of us, had suggested that the darkest mare units are the youngest. But rays are bright, and so are steep lunar slopes; a full-moon photograph (which shows albedo and not shadows) can be used to a first approximation as a ray and slope map. The steepest and brightest slopes, such as the upper walls of Copernican craters, are usually young, not having been worn down. On such slopes, downslope movement was presumably exposing the fresh rock and soil that we called Copernican slope material faster than it could be darkened by solar or cosmic radiation. So *bright* seemed to equal *young* for the terrae. This led Henry Moore to map the Cobra Head source of Schröter's Valley as Copernican because it is bright, and he inferred therefrom that the volcanic flows that cut the valley were also Copernican.[34]

An astronomical study that was particularly relevant to lunar geology was conducted, at Kopal's suggestion, by John Saari and Richard Shorthill of the Boeing Scientific Research Laboratories during a lunar eclipse on the night of 19 December 1964 with a large (1.9-m) telescope at Kottamia, Egypt.[35] The observations, in the "thermal" infrared (10–12 μm), revealed many spots that reradiated the Sun's heat more quickly than did most of the Moon's surface. Naturally these apparent "hot spots" suggested active, or at least warm, volcanoes to the lunatic fringe. The Boeing investigators interpreted them more rationally as surfaces relatively free of fragmental debris, which retains heat that bare rock would radiate. Because most of the infrared hot spots coincide with bright-rayed craters, known by this time to be young, the spots presumably

represent surfaces exposed relatively recently. Astrogeologists could thus use the infrared data to divide fresh-appearing craters into youngest (Copernican) and less young (Eratosthenian) categories even when rays were not obvious. A few years later, high-resolution Lunar Orbiter photographs of the spots revealed blocks and boulders that had been quarried from cohesive target materials like mare basalt. Relatively clean rocks therefore cause the hot spots. One would think that the presence of all this dust-free blocky material would have weakened the Gold-dust theory, but no amount of data can shake a theoretician deeply committed to his ideas.

PICKING THE LANDING SITES, ROUND 2

Bellcomm personnel observing the USGS work knew very well that engineers understand numbers better than maps and were impressed by the photoclinometry data being generated by Jack McCauley's project as a source of the numbers. As a result, Colonel Arthur Strickland of the U.S. Army Corps of Engineers, who was serving as chief of the cartography program at NASA Headquarters, made available almost by magic a large pot of money for McCauley's project. The project could then expand into a major terrain study of the lunar equatorial belt and was the means of hiring several new geologists. The first of these was Lawrence Calvin Rowan (b. 1933), recruited by Wilmarth, hired by McCauley in August 1964, and destined for a major role in selecting exploration targets. Rowan had become interested in remote sensing during his Ph.D. fieldwork in the mostly soil-covered rocks of the Beartooth Mountains of Montana and preferred related lunar work to oil companies or teaching. He and McCauley took the lead in converting the geologic maps of the LAC areas into separate maps whose units were expressed in quantitative terrain terms understandable to the engineers.[36] Their purpose was not to certify landing areas — the telescope could not do that — but to eliminate areas unfavorable for landings. NASA in general and Strickland in particular had nothing against the terrae at that point, but the project eliminated blocks of terra and large craters on the principle of additive relief. The maria would be the targets of early Surveyor and Apollo landings.

Where in the maria? Locating the best spots within them was partly a simple matter of looking for hills and pits. Subtler means descended from the telescopic work. Because crater rays and steep slopes are bright under high Sun illumination, and the telescope, later confirmed by Ranger photographs, showed that small craters and other roughness elements are aligned along the rays, they were excluded as landing sites. This conclusion was extended to bright surfaces that lacked resolvable individual rays. The dark = young equation for the maria led to favoring dark spots on the maria as landing sites on the assumption that

these have the fewest telescopically unresolved craters and other rough texture. Lists of sites for Surveyor and Lunar Orbiter, discussed in chapters 8 and 9, were drawn up (together) very largely on this basis. Seven of these sites became candidates for early Apollo landings. Subsequent study, however, revealed a flaw: except where rays or small islands are the brightening factor, the albedo of a mare unit has nothing to do with how many hazardous craters it contains. Nevertheless, the ultimate landing targets for four successful Surveyors (1, 3, 5, and 6) and two Apollos (11 and 12) evolved from this simple method. Right and wrong, this USGS work of the early 1960s sowed the seeds for the scientific exploration of the Moon.

One of the louder criticisms of Kennedy's end-of-decade deadline was that it might force the unmanned program and preparations for Apollo to overlap. They did. While the ground support for Apollo was shaping up, the first successful American spacecraft, the Rangers, were already streaking toward the Moon.