So now we knew about the maria. But they were just the prelude, the simplest scientific and technical problem that confronted geologists and NASA.

Apollo had shown it could gently set pink bodies (Mike Collins’s term) down on the Moon and bring them home safely. The other part of the Apollo entity, the cold metal, was upgraded for the later scientific flights called J missions but wasn’t replaced by shiny new developments of the type that could keep the engineers and the manufacturers interested and rich. That left science as the rationale for any continuation of the program. NASA and MSC were now genuinely interested in finding the scientifically most productive landing site safely accessible to the third landing and exploitable by an H-type walking mission. It need no longer be on a mare.

Geologists were sure they knew where that site was. In the list being considered for the remaining Apollo missions, one part of the Moon stood out. The chain of discovery traced earlier in this book led to the description by Shoemaker, Hackman, Eggleton, and a cleanup crew, including me, of the most extensive stratigraphic unit on the Moon’s near side: the distinctive hummocky blanket that surrounds the Imbrium basin and was interpreted with little doubt as its ejecta. An H mission to this ejecta, called the Fra Mauro Formation since November 1963, would address several objectives at the top of all scientists’ wish lists. Samples returned from the Fra Mauro would represent a large part of the Moon’s surface. Since Imbrium was such a large basin, the samples also would be derived from a substantial depth—tens, conceivably hundreds, of kilometers. Their absolute age would date the formation, a goal of major importance because it would bracket the ages of everything else that touches the
blanket. All of Gilbert's "antediluvian" craters and all the known lunar basins except Orientale could be put into a box labeled "older than x years." All the maria, fresh craters, and Orientale could be put into a box called "younger than x years," which could be divided later into separate age compartments by crater counts at the leisure of the counters. Astronomers were as eager as geologists to know age x, for it would suggest how long the early premare impact barrage lasted. Rock textures should reveal how major impacts brecciate, metamorphose, melt, and redistribute their target materials.

All the Lunar Orbiter 1, 2, and 3 prime Apollo sites were now obsolete. One of the smooth-looking ones had become Tranquility Base, and one of the rough-looking ones had provided the landing field for Conrad and Bean. Fra Mauro had never been far from our thoughts during Lunar Orbiter target selection, and geologists were ready when the opportunity arose for the Lunar Orbiter 3 mission to shoot a "supplementary" site in its vicinity. Dick Eggleton suggested a specific photographic footprint south of Orbiter 3's ground track that would include a good landing site. We also wanted to sample the Fra Mauro Formation's appearance, so we recommended the "slow" sequencing mode that yielded discontinuous coverage of the four H frames (132-135) of this site. The sampling showed that the site offered the desired relatively smooth topography over a large area which made it relatively safe for an early mission, this being before the Apollo 12 point landing. It was safely situated in the equatorial Apollo zone. Alternates we frequently considered for an early mission were Hipparchus, another extensive and even smoother tract, and Censorinus, a small "drill hole" in the terra. But Fra Mauro was superior scientifically and seemed ideal in all respects. On 10 June 1969, six weeks before the landing of the Eagle, the Apollo Site Selection Board tentatively approved Fra Mauro as the landing site for Apollo 13 pending the results of Apollo 11 and 12.

Several alternative landing ellipses were suggested in August 1969 by Dick Eggleton, Farouk El-Baz, and Lou Wade of the MSC Mapping Sciences Branch. The premission geologic maps were assigned to Dick Eggleton and Terry Of­field, an experienced and competent geologist who had come to Astrogeology from a foreign assignment four years earlier. The Apollo 13 and 14 crews were also picked in August 1969. Deke Slayton chose all crews, subject to approval by the Office of Manned Space Flight, and outsiders were never sure what his criteria were. One wit (San Francisco Chronicle columnist Herb Caen) suggested that it was alphabetical order, five of the six Apollo 11 and 12 crew members having been Aldrin, Armstrong, Bean, Collins, and Conrad. As a long-suffering WI can believe this, and it is as good a guess as any. Slayton regarded all his men as created equal and claims that they simply
joined a crew when they came up in the normal process of rotation; and it is true that most backup crews became prime crews three missions later. By this rule Gordon Cooper, who had backed Apollo 10, would command Apollo 13.

But Slayton clearly had one bias. He wanted his fellow Mercury astronaut and fellow medical case Alan Shepard to fly to the Moon. Shepard had been grounded by an inner-ear ailment that caused dizziness and ringing in the ears (Ménière's syndrome) and since 1963 had been serving under Slayton (chief of flight crew operations) as chief of the astronaut office. After a successful and secret operation, Shepard was reactivated to flight status in May 1969, making him the only Mercury astronaut flight-qualified for Apollo except the maverick Gordon Cooper because of Slayton's heart problem, the death of Grissom in the Apollo 1 fire, and the resignations of Glenn, Carpenter, and Schirra. Slayton presumably figured that if one desk-bound supposed cripple could fly, he could too—as, in fact, he did on Apollo-Soyuz in 1975. He therefore planned to name Shepard as the commander of the Apollo 13 crew, along with Ed Mitchell and Stu Roosa, backed by Gene Cernan, Ron Evans, and Joe Engle.

Shepard's version of the sequel is that he felt unprepared to fly a mission for the late 1969 or early 1970 date then set for Apollo 13 and asked Jim Lovell, slated for Apollo 14, to trade places with him. However, Astronaut Walt Cunningham's book and historian David Compton's interview with Deke Slayton trace the decision to switch crews to George Mueller. Shepard might leapfrog over better-trained crews, but Apollo 13 was just too soon. Moreover, the mention of Shepard raised a general expression of cynicism among NASA watchers, including the Space Science Board. Shepard had never concealed his disinterest in the geology training and other scientific aspects of the missions. On 6 August 1969 the geologically enthusiastic trio of Jim Lovell, Fred Haise, and Ken Mattingly became the prime Apollo 13 crew, backed by John Young, Charlie Duke, and Jack Swigert. Dale Jackson had hoped Gordon Cooper would fly a lunar mission. They had much in common, including enlistment in the Marines at age 17. But Cooper got the message from the Shepard and Lovell assignments and bitterly announced his resignation.

THE FIELD GEOLOGY TEAMS

Starting with Apollo 13 the voice of science was heeded as never before. The pace of lunar exploration accelerated dizzyly for the geologic support teams, which were assembled under NASA contract in official NASA science experiments and which through Apollo 14 were officially called the Apollo Lunar Geology Experiment Team. The teams did not work directly with the Moon rocks unless
they individually contracted for projects. What they did do was oversee the fieldwork during which the rocks were collected. First, they had to train the astronauts in the geologist’s way of extracting information from rocks, a process that Dale Jackson’s group in Houston had begun way back in 1964 and now was getting down to cases. Second, they mapped out the EVA traverses, located stations along them deserving of close attention, and worked with MSC in preparing detailed time lines to guide the astronauts’ activities. Third, working with the astronauts during frequent field exercises, they simulated the missions in appropriate localities on Earth. The fourth task came during the missions, when the teams oversaw what was happening from the back rooms at MSC, earlier called Science Support Rooms but as of Apollo 13 officially called the Science Operations Rooms. Last, but far from least, they prepared reports that ranged from first-reaction judgments about what happened on each EVA to elaborate U.S. Geological Survey professional papers, which are really books. I did not participate directly in this activity and so can say without conceit that it was an all-out exertion of competence and devotion in the finest tradition of cooperative endeavor. Most geology team members put their tasks ahead of their egos. With their help most astronaut crews extracted every possible drop of information from the rocky Moon that could be obtained within the limits of hardware and time.

For five years intensive preparation for these tasks had been under way by the manned-studies group of geologists, geophysicists, photogrammetrists, electronics specialists, draftsmen, and secretaries headquartered in Flagstaff who, since August 1967, had been collected in the Surface Planetary Exploration Branch (SPE). They knew that the astronauts would be on the Moon for a precious short time and that what the geology teams did might make the difference between wasting and exploiting an opportunity that would come only once. What the SPE personnel could not know was whether they themselves would constitute the field teams. NASA was far from accepting their participation as inevitable. The USGS was anathema not only in much of NASA but in much of academia, presumably because the USGS was a little too aware of its leadership in American geology. This feeling has a long history. I quote the following letter, dated May 1906, to D. M. Barringer from J. C. Branner, who had recently resigned from the Survey: “Survey people have a way of knowing it all that is quite convincing to themselves and to a large part of the rest of the world. That you dare to call into question the conclusions of a member of the Survey will be looked upon with suspicion and strong disapproval you may be sure.”

Recent history was also against USGS leadership of the ground support effort. The squabble between Dale Jackson and NASA in 1964 had left an indelible mark in the collective memories of MSC. Although he was good at briefings, Al
Chidester was more inclined in the field to point out something and assume the astronauts understood it rather than to develop the problem-solving skills: self-reliant observer would need.

The laboratory courses in mineralogy administered by MSC were, as always, even more questionable in their relevance and were boring the astronauts stiff. Jack Schmitt and Gene Shoemaker were well aware of this state of affairs; the astronauts and lunar geology both deserved the best they could get. Jack persuaded Alan Shepard that the astronaut office should take over the training. It should be tuned more to the missions and less to geologic generalities. Apollo 12 had begun to move in that direction, but the future of the training was unclear.

At this point the Caltech connection reasserted itself once again. During his undergraduate days at Caltech between 1953 and 1957, Jack Schmitt had been particularly impressed by an enthusiastic and versatile professor of geology and geochemistry by the name of Leon Theodore Silver (b. 1925). Actually the connection was older than that: Harrison Schmitt, Senior, had introduced the 11-year-old Jack to Silver during a visit to the Schmitt home in Silver City, New Mexico, in 1946. Silver had also met Shoemaker on the Colorado Plateau in 1947, had attended the dedication of the Flagstaff Astrogeology building in October 1965, had participated in the Santa Cruz conference in 1967, and was a lunar sample investigator starting with Apollo 11. He is fully at home in the laboratory, the field, and the classroom. Jack had taken as his mission the enlargement of the role of science in Apollo and was trying to activate the interest of the Apollo 13 crew in geologic training. He was sure that Silver was the man to do it and called him in August 1969. Silver was willing but felt he had to get permission from his department chairman. No obstacle there; the chairman was Gene Shoemaker.

Jack's motivation of the prime Apollo 13 crew of Jim Lovell and Fred Haise and the backup crew of John Young and Charlie Duke had been so successful that they took leave and paid their own expenses for a long week during September 1969 in the Orocopia Mountains of the southern California desert, a non-lunar but fascinating area rich in easily visible geologic relations among colorful rock units where I also once worked. Schmitt came along for about three days. Also there were geologist John Dietrich of MSC and, for one day, old-time, no-nonsense Caltech geology professor Bob Sharp, also recruited by Schmitt. Silver crammed everybody in one Carryall, drove the vehicle, and did the cooking. The Orocopias are sizzling hot in September and the astronauts asked for and got a day off in nearby Palm Springs, but otherwise the fieldwork was intense and rich in geologic education. Silver invented the techniques for teaching field geology to astronauts as he went along. Apparently he succeeded; this
week in the Orocopias appears to have been decisive for the future of Apollo geology. The four astronauts generally took pride in doing as well as possible in all aspects of their mission, and they now saw that a well-managed geologic program could be part of it. Geologic training could expand.

Gene Shoemaker had ably chaired the Apollo 11 and 12 geology teams, except that he never got around to writing the expected professional papers summarizing the mission results, and he remained as chief of the Apollo 13 team at least in title. Still not established was who would lead the teams after Apollo 13. It would definitely not be Shoemaker. During a talk at Caltech on 8 October 1969 that he considered informal, not knowing a reporter was present, he announced that he was withdrawing the following March from formal status as an Apollo experimenter and gave the reasons. One was his deep commitment to his proud post as chairman of the Caltech Division of Geological Sciences. The reasons that made the newspapers, however, were his criticisms of the way Apollo was conceived and operated: NASA had never made much of an effort to accommodate science into the lunar program; all they wanted to do was build ever bigger and better hardware; Apollo had become just a transportation system, and its scientific job could have been done earlier and more cheaply by unmanned spacecraft. Shoemaker foresaw that NASA simply wanted to use up its remaining spacecraft as fast as possible without making the major changes needed to exploit Apollo scientifically.

Needless to say, his comments were not well received by NASA. Homer Newell, Shoemaker's early supporter who had himself struggled to insert science into Apollo, never forgave him. Newell complained that NASA had lifted Shoemaker from a young unknown into the leader of a major program—financed by NASA. Now, however, Shoemaker "seized every opportunity . . . to castigate NASA." Shoemaker's point was that the astronauts should be instruments of scientific discovery, not just passengers. A field geologist could get down on his hands and knees and intelligently sample layers of the regolith, which contains a detailed record of solar and galactic as well as lunar history. He felt strongly that NASA had failed to exploit the scientific opportunity presented by Apollo, and he did not feel as beholden to NASA as the agency might have imagined; his great outpouring of lunar discoveries came before it gave him or the USGS a dime.

That his basic reproach had some merit is shown by a particularly plaintive note in Newell's list of grievances: "And, anyway, what good was all the criticism going to do? NASA lacked the funds to continue Apollo landings much longer. Moreover, voices on the Hill were asking why the agency didn't just stop all further lunar missions, since each new flight exposed NASA and the country to a possible catastrophe."
Shoemaker was not alone in his views. Coming from him, however, they struck particularly tender nerves. Mention of his name is unwelcome in many quarters of NASA to this day.

The leadership of future geology teams would be established by the usual NASA process of formal proposal and review and was up for grabs. University professors and everyone else could have a crack at the grand adventure, and it was no secret that the acceptance of a principal investigator and coinvestigators from the universities would be greeted by widespread relief. Lee Silver could have done it, but he was deeply committed to his teaching and laboratory work at Caltech.

In the month of Shoemaker's infamous talk, one of the stalwarts of the subsequent field program submitted his proposal to be team leader for the remaining H missions (half of the remaining missions). Gordon Swann had been at the Houston Astrogeology office and since October 1964 had been with the Manned Investigations Group at Flagstaff that became SPE. Gordy, a country boy from western Colorado with an accent and manner to match, can deliver a good joke better than anyone else not employed in show business. Effete intellectual types therefore assumed at first that he was not worthy to participate in the noble Apollo program. In fact, however, Gordon is plenty smart, geologically astute, and personally secure and sensitive enough to lead both the political and scientific aspects of a major geologic program.

MSC had favorable reports from the astronauts about the Orocopia trip and paid the way next time. Between the final briefings and the launch of Apollo 12 in November 1969, Gordon Swann and Tim Hait of SPE led the Apollo 13 crew to the Kilbourne Hole maar near the Mexican border in New Mexico. In December they went to Kilauea, Hawaii, accompanied by geologists from MSC.

But Apollo 13 was not supposed to go to maars like Kilbourne or volcanic terrain like Hawaii's. Early in the Apollo 12 mission Dick Gordon had aimed the 500-mm lens of his Hasselblad at the Fra Mauro site to check its topography at the same low 7° Sun angle that Apollo 13 would encounter. The official announcement that Apollo 13 was cleared for Fra Mauro came on 10 December 1969. No backup site was trained for; it was Fra Mauro or bust. Swann's proposal to lead the H-mission geology teams was accepted at about the same time.

The other successful proposer was William Rudolph Muehlberger (b. 1923) of the University of Texas. Bill also presents a deceptive exterior. Although educated at Caltech, he does not show it except that he mercilessly perpetuates physical and psychological practical jokes. He played football at that major athletic center and does show it—he looks like the fullback he was while crashing through the mighty lines of the Pomona Sagehens or the La Verne Leopards, or the linebacker he was when trying, more often, to prevent the reverse. After
Apollo 11, Don Wise, wearing the hat of the Lunar Exploration Office of NASA Headquarters, had called Bill to tell him that the request for proposals was soon to go out and to ask him to put together a team from the universities to manage all Apollo science. The USGS was becoming too dominant. Bill thought this was too much to do and said no. Next, Gene Simmons, a geophysicist from MIT and chief scientist at MSC since late 1969, called with much the same thought except that Bill’s scope would be restricted to geology. That sounded better. At the time of the Apollo 11 Lunar Science Conference in January 1970, Bill attended a meeting in a smoke-filled room at the Rice Hotel in Houston that was also attended by the other candidates, some NASA management people, and Arnold Brokaw, who had moved to Washington in July 1969 as deputy chief geologist for Astrogeology. Bill realized that he could not assemble the necessary expertise from the universities, and he proposed hiring USGS people from SPE. That broke up the meeting. Soon, however, Brokaw called with another scaled-down request: would Bill join the team for the J missions, Apollos 16–20, as a coinvestigator? Later, Brokaw reescalated the offer to principal investigator.

I was serving on the Planetology Subcommittee of the Space Science Steering Committee of OSSA that was charged with reviewing space science proposals when it met in February 1970 in the Caltech library to review Muehlberger’s proposal. Swann’s had already been accepted and he was also present. Noel Hinners went into a long discourse to the effect, “Dr. Muehlberger, Dr. Urey believes that geologists have no place studying the Moon because it’s a simple chemical object not suited to their line of inquiry, etc. etc., and they haven’t had enough experience with selenology, etc. etc., and anyway they’re not very bright, etc. etc.” Bill shifted his bulky frame on the chair and intoned his first words of the day, “Waal, that’s bullshit.” His proposal was quickly accepted, and the Apollo field geology experiment acquired another astute geologist who expertly and effectively led his team and fended off those who were trying to cripple the effort.

I refer to a resumption of the USGS-MSC rivalry that had begun in 1964. The Apollo-era phase of the conflict was a major distraction for the participants. It was personified by space physicist Anthony John Calio (b. 1929), who replaced Wilmot Hess as the director of MSC’s Science and Applications Directorate after Hess resigned in September 1969. Hess was intelligent and competent but had proved unable to prevail against the other, antiscience MSC directorates. Calio’s name still raises the hackles of the USGS survivors of that era, and I suspect the feeling is mutual. Two problems among other, apparently more personal, ones were that Calio wanted his own people to take over the field geology teams, and he hated the USGS. I had encountered similar sentiments on the part of some members of the Space Environment Division of MSC while I was there in 1963 and 1964. Humans were going to the Moon—the Moon—and yet were squab-
bling as they did when their survival depended on excluding another tribe or clan from their hunting grounds. Calio and his directorate were constant thorns in the sides of Swann and Muehlberger and the entire USGS operation. The real and adopted westerners from Flagstaff on the field teams usually wore string bolo ties, which became known at MSC as "spy ties." However, they had the respect of the astronauts and also of the flight directors and controllers of the Flight Operations Directorate, and this closeness saved many a day.

Shortly after the Apollo 12 mission Calio brought in the respected petrologist Paul Gast to be chief of the Planetary and Earth Sciences Division and his chief science adviser—a move, I am told, designed to help Calio in his own competition with the other directorates at MSC and the Apollo Program Office at NASA Headquarters. Many geologists (including me) found Gast obnoxious, but he was straightforward (often a positive aspect of obnoxiousness) and an effective manipulator who could get things done. No one doubted that he was an intelligent and dedicated scientist who thoroughly understood the petrology and chemistry of rocks. Swann is positive that Gast helped more than hurt the program, though he does not say the same for Calio.

One important interaction went smoothly between the USGS and Calio. Shortly after his appointment as principal investigator and geology team leader, Swann noticed a fresh crater 370 m across only a few kilometers west of the nominal Apollo 13 landing point, and suggested in a letter to Calio that the landing point be moved downrange to be close to this probable drill hole in the Fra Mauro. Calio readily accepted the suggestion, and the rocks exposed at Cone crater became the mission's principal objective.

In March 1970, the month before Apollo 13 lifted off, Silver, SPE geologists, MSC geologists, and a cast of supporting characters from NASA Headquarters and Bellcomm carried out a major exercise with the prime (Lovell, Haise) and backup (Young, Duke) crews at a month-old crater field created by SPE in the Verde Valley in central Arizona as a winter training ground. The exercise closely simulated a lunar mission. Swann, half a dozen other SPE geologists, photogrammetrist Ray Batson, and two court reporters brought in at the time of Apollo 11 by SPE geologist Dave Schleicher were sitting back in Mission Control in Houston as the astronauts' observations were radioed from the field. Astronaut field exercises after the Orocopias trip were generally accompanied by an astronaut called the mission scientist, who had an advanced degree in some scientific subject but was also a pilot. The mission scientist for Apollo 13 was Anthony Wayne England (b. 1942), an astronaut since August 1967 and a geophysicist by training (Ph.D. from MIT, 1970). The mission scientist, selected by the chief of the astronaut office, was a Janus who could talk both to the other astronauts and to the scientists. He would later serve as capcom during the EVAS of his crew.
on the Moon. England supplied important glue during the training, but unfortu­

Specific training for visual observations from orbit was instituted for Apollo 13 under the guidance of Farouk El-Baz, assisted by others of us who had performed a similar function for Apollo 8. Ken Mattingly and Jack Swigert, the CMPS, were the main recipients of this training. As the countdown for the Apollo 13 launch began, one of Charlie Duke's children came down with the German measles and exposed the prime crew. Only Mattingly among the three tested as nonimmune, so he was replaced as CMP by his backup, Swigert. Mattingly was, of course, crestfallen. In different ways, Fate would mock the postponements of Shepard's and Mattingly's flights.

THE AX

Apollo was at the divide between eras at the beginning of 1970. The day on which the Fra Mauro Formation was confirmed as Apollo 13's target, 10 December 1969, was also the effective date of the resignation from NASA of George Mueller, a major force in Apollo planning and a supporter of a vigorous space program—including the science. Mueller was one of the Apollo giants who had moved over from private industry long enough to carry out the grand enterprise and now was returning, as had or would most of the others. In a maneuver that had proved crucial to NASA's success in the 1960s, these people had been hired under special Public Law 313 that enabled the Administrator to circumvent some civil service requirements and pay them decently. NASA had burned too many bridges to permit all 10 of the landings that had been foreseen in 1967. The prime driver within NASA for the manned lunar landings had been George Low, the manager of the Apollo Spacecraft Program Office at MSC since the Apollo 1 fire. On 4 January 1970, a day before the first Lunar Science Conference began, it fell to Low to announce that Apollo 20 had been canceled. No funds were available to reopen the Saturn 5 production line, and the Saturn 5 that was to have launched Apollo 20 was needed for the launch planned for late 1972 of the Earth-orbiting space station, Skylab. Skylab and the Apollo-Soyuz joint mission with the USSR in 1975, also already gleaming in the eyes of planners in 1970, were the sole survivors of AAP. The last glimmer of hope for a post-Apollo lunar program had flickered out. Reassuringly, however, Low also observed that cancellation of any more Apollos would waste the great investment in the program and diminish its scientific return. The remaining seven missions (Apollos 13–19) would be stretched out to place the Apollo 18 and 19 launches in 1974, after Skylab, a pace that suited mission-support
scientists and engineers better than the constant fire drills they had come to know. Apollo 13 was moved from 12 March to 11 April 1970.

But the budget cutters were just getting warmed up. In February 1969 Nixon had appointed a very-high-level panel with the same name as the Langley group that had designed Project Mercury, and in effect the manned spaceflight program of the United States, Space Task Group. The new STG, chaired by Vice President Spiro Agnew, entertained such money-be-damned options as space stations in lunar orbit, a lunar base, an Earth-orbiting space station capable of supporting 50 to 100 people, and a manned Mars landing in the 1980s (!). They also realized, however, that the public was balking at great expenditures for manned flight and proposed cheaper alternatives such as a robotic Mars landing possibly followed by human crews by the end of the century. In March 1970, just before the Apollo 13 launch, Nixon pronounced himself in favor of a middle ground emphasizing the space shuttle. NASA Administrator Thomas O. Paine had bucked the growing trend to small thinking and tried to put a good face on the decision. But he had already confirmed publicly that the Saturn 5 production line had been irrevocably shut down. The future road to the Moon was closed.

**UNLUCKY 13**

The objectives of visiting the Fra Mauro highlands were achieved, but not by unlucky Apollo 13. Of course, no scientist believes in that ancient superstition, but Apollo 13 did lift off at the thirteenth minute of the thirteenth hour (by Houston time) on 11 April 1970, and almost 56 hours later, on 13 April, a loud bang and a drop in voltage elicited the comment first from Swigert and then from Lovell, “We've had a problem.” Indeed they had; one side of the service module had blown away when one of its two oxygen tanks exploded, damaging the other one. Apollo 13 had to return to Earth after looping once behind the Moon. The crew had to depend on the oxygen, water, electric power, and air-cleaning systems in the lunar module Aquarius and space-suit backpacks for the flight back to Earth. The problem was that the trip would take something like 90 hours and Aquarius theoretically had consumables for only half that time. The free-return trajectories having been abandoned after Apollo 11, trajectory corrections would be needed; but because the usual performer of this job, the service module engine (the SPS), was thought probably damaged (and in fact was), Aquarius would have to fill in here too. The interest of the press and the public in Apollo was suddenly renewed.

Good luck and brilliant work by a small army of engineers from NASA and Grumman, the LM’s builder, saved the astronauts’ lives and, undoubtedly, the
future life of the Apollo program itself. Almost 142 hours after launch and 86 hours after the explosion, the lifeboat *Aquarius* was jettisoned, and an hour later, on 17 April 1970, the well-named command module *Odyssey* splashed down with three live but cold, deeply fatigued, dehydrated, and generally miserable occupants. A board of inquiry later established that the wiring in the oxygen tank's heater system was damaged before launch because it was designed for a lower voltage than was employed at Cape Kennedy. Nobody had been grossly stupid or negligent; only one tiny detail among literally millions was overlooked. The many problems with space shuttles in the 1980s make us marvel that Apollo suffered so few glitches of this type as it pioneered a new and enormously complex technology.

The mission was not a total scientific loss. Haise and Swigert were willing and able to shoot pictures of the Moon's central far side as they passed over it—so near (254 km) and yet so far. Also, the seismic experiment begun by Apollo 12 was followed up as Apollo 13's S-4B third stage crashed on target 137 km from the Apollo 12 seismometer, only 23 minutes after the docked CSM and LM emerged from behind the Moon. The seismometer shook for four hours as the signals tailed off very gradually. The geophysicist experimenters eventually interpreted this unearthly behavior, which had also been noted when the Apollo 12 ascent stage hit, as arising from the looseness, heterogeneity, and complete dryness of the lunar crustal material. In other words, the rocks of the Moon are waterless breccia—not a surprising conclusion to geologists familiar with the Moon's multitudes of impact craters and basins. The S-4B impact was also noticed by other sensitive instruments of the Apollo 12 ALSEP. Beginning 20 seconds after the impact the suprathermal ion detector and the solar wind spectrometer detected ions of a tenuous gas cloud from the S-4B's residual fuel.

THE AMERICANS TAKE A BREAK

The Apollo 13 explosion caused a 10-month delay in the Apollo schedule, the only significant delay suffered between the flights of Apollo 8 and Apollo 17. As always, history did not stand still during the interval.

Our Menlo Park astrogeology office received a surprise visitor during this period, one who would not have asked to meet a bunch of geologists 10 years earlier: Harold Urey. We discussed the forbidden subjects of politics, religion, and geology. He sided with the liberal majority among us and told us with a chuckle that his favorite deity was Aphrodite. I have told of his rare ability to admit when he was wrong. By now, he had admitted he was wrong about geologists—why, they had gotten certain facts about the Moon right even before he
did! They, and not he, had known the maria were volcanic lavas. They, and not he, had realized that the Iridium crater was not the entry hole for the Imbrium projectile but a separate post-Imbrium crater in its own right. He promised to work with us to see if we could devise a better model of the Moon on which we could all agree. We never did, and Urey fades from this book's history at this point, though he kept in touch with us for several more years and I saw him at the Apollo 17 launch.

When Arnold Brokaw moved to Washington in July 1969, Al Chidester had returned temporarily as branch chief of Surface Planetary Exploration. However, Brokaw and Chief Geologist Hal James, the creator of SPE, had witnessed the straightforward Chidester's impatience in dealing with Calio and Gast and concluded that relations with NASA would improve if he were replaced. In June 1970 they yanked him as SPE branch chief, whereupon he left the lunar program entirely. To make a clean sweep, Brokaw and James canned Hal Masursky as chief of the Astrogeologic Studies Branch because his free spending and jurisdictional dispute with Chidester had caused them and other Survey managers endless grief. The USGS benefits from an admirable and almost unique practice of rotating administrators, including directors, up from the ranks and then busting them back down to the working level before they get stale or develop Potomac fever. Thus, a memo from James dated 19 June 1970 greased the skids under Chidester and Masursky with the wording, "Following our policy . . . of rotating personnel in administrative positions . . ." Chidester "has agreed to head up our cooperative work in Colombia," and "Hal . . . will act as consultant and adviser to the two Branch Chiefs and [Brokaw]." Further: "In dealing a new administrative hand, we open with a pair of Jacks." Jack McCauley eagerly accepted the chiefdom of Astrogeologic Studies, moving from San Francisco back to Flagstaff, and Jack Strobell became chief of SPE. McCauley served Astrogeology well until he burned out in 1975. Strobell did not work closely with his branch, but the geologists of SPE were so confident of themselves and the value of their mission that they surmounted all obstacles erected by the USGS or NASA. James and Brokaw never did succeed in their effort to rein in SPE, which continued to function independently of the century-old rules, regulations, and customs of the USGS.

Jack Schmitt suggested a visionary program of four spectacular landings to regain public support: Tycho, the Orientale basin, the north pole, and the far side. But the Apollo 13-14 interlude was not a happy one for the lunar program. Money was getting ever tighter as the Vietnam War escalated, and Robert Gilruth and others in NASA were also worried that their luck might not hold out and they would lose a crew if the flights continued too long. The future belonged to the shuttle. Administrator Paine acceded to further cuts in Apollo, and on 2 Sep-
tember 1970 the Apollo missions originally numbered 15 and 19 were canceled. Missions 16, 17, and 18 were renumbered 15, 16, and 17. Because the necessary Saturn 5s had already been built, the cuts saved possibly as little as $20 million per flight—an amount that Harold Urey justifiably called "chicken feed." As both a visionary and a Democrat, Paine felt out of place in the Nixon administration and resigned on 15 September 1970, to be replaced temporarily by George Low as acting Administrator and then permanently in April 1971 by the unimaginative Utah Republican James Fletcher. Wernher von Braun had moved from Huntsville to Washington in February 1970, and he watched with increasing dismay as his adopted country's vision shrank from the unlimited vistas of cosmic travel to the cramped perspective of the short-term bottom line.

Apollo 13's failure to land at Fra Mauro also greatly affected the selection of all the later sites, including the very last. And it put Alan Shepard in the position of commanding the first mission dedicated mainly to science and sent to what I consider the best of all the point targets accessible to an Apollo landing.

**THE RUSSIANS FILL THE GAP**

Entirely by coincidence, and certainly without international planning, two successes of the Soviet unmanned lunar program came along in time to alleviate the boredom between the Apollo 13 miscarriage and the consequently delayed Apollo 14. Lunas 14 and 15 apparently had tried to return samples in April 1968 and July 1969, and in September 1970 Luna 16 registered the first unqualified success by an unmanned spacecraft in collecting a sample from the Moon and returning it to Earth. Luna 16 landed in Mare Fecunditatis just south of the Crisium basin rim (0.7° S, 56.3° E), in the eastern near-equatorial zone where Luna 15 had expired and the later successful sample returners Lunas 20 and 24 also landed. It drilled a hole, extracted a core 35 cm long and weighing (on Earth) 0.1 kg, and blasted off for home. The sample consists of fine material and small rock fragments, mostly basalt, as would be expected by now from a mare landing. However, the basalt is of an aluminous type intermediate between the Apollo 11 and Apollo 12 types in titanium content and also in age (about 3.4 aeons). It was beginning to look as if every spot on the Moon was different, to the surprise of the simplistic model makers. Jack McCauley and Dave Scott described the site setting for the volume of mission results, emphasizing the evolution of the entire region and tracing to their sources the rays that cross it.

Those rays brought an important alien to the mare site, just as other rays had to Tranquillity Base: small pieces of the terra. Americans who analyzed part of the Luna 16 sample identified five shocked, brecciated, and recrystallized soil
particles by the rock names anorthosite, norite, and troctolite, leading to coinage of the acronym ANT. Compositional analyses of the Luna 16 ANT particles are like those from Tranquility Base and much larger samples of true crystalline rock from subsequent Apollo landing sites. During the early 1970s ANT was a common designation for the main constituent of the lunar terrae.

The other Soviet success, or probable success, was the last lunar flight of the peculiar Zond series in October 1970. Like its predecessors, Zonds 5, 6, and 7 (September and November 1968, August 1969), Zond 8 looped once from the northern near side to the southern far side before returning to Earth for a braked landing. The Zonds obtained good stereoscopic coverage of strips of the far side, regional coverage of parts of the west limb barely covered by the U.S. Lunar Orbiters, and color photographs (Zond 7). Some of the photographs were used for constructing profiles that revealed the existence of a giant depression some 5–7 km below the average surface elevation and in the middle of the far side (centered at 56° S, 180° W). The Russians were going to help Bill Hartmann look good again. In his definitive 1962 paper with Kuiper on basins, Bill had written of the Leibnitz Mountains, at 8–9 km elevation the Moon’s highest, which are just visible over the south limb of the near side during favorable librations. He predicted that they were part of a very large basin because “all major mountain arcs on the visible lunar surface are associated with mare basins.” The Leibnitz range turned out to be the rim of the giant (2,500 km in diameter) impact basin called the Southwestern mare by its Russian discoverers, the Big Backside basin by informal usage, or the South Pole–Aitken basin by Desiree Stuart-Alexander and me. Otherwise the Zond data have been used little in the United States because Americans obtained them only in part and only after Lunar Orbiter and Apollo coverage of the same areas was already available. The Soviets were not as forthcoming with their lunar data as they now are with their Venera (Venus) data.

Another Soviet success during the Apollo 13–14 lull, and the first of a second type of mission, was achieved by Luna 17 in November 1970. Luna 17 landed Lunokhod 1, which continued to function for 10 months after its landing. It crawled 10.5 km across the surface of Sinus Iridum, far from any other Soviet or U.S. landing site, transmitting television photographs and other data by lunar day and resting by night. It stuck a penetrometer into the lunar soil to measure its density, analyzed the soil chemistry with an X-ray spectrometer, and was tracked by a laser reflector. It returned no samples, and I am not sure it added much to the findings of Apollo 11 and 12. The Soviets may have agreed. They flew only one more Lunokhod, equipped with the same instruments plus another camera, a magnetometer, an ultraviolet sensor, and an astrophotometer. In January 1973 Luna 21 carried this Lunokhod to the crater Le Monnier, north of the Apollo.
THE SECOND ROCK FEST (JANUARY 1971)

If Apollos had been fired off according to plan, attendees at the Second Lunar Science Conference, held on 11–14 January 1971 at MSC, would have feasted on a banquet of delicacies from the Fra Mauro Formation, a dark mantling deposit at a site called Littrow in eastern Mare Serenitatis, and possibly the Davy crater chain or the small young crater Censorinus on the northern Nectaris basin rim. Instead, the fare consisted of the crumbs brought back by Luna 16 4 months earlier and the rocks, regolith fragments, and Surveyor components brought back by Apollo 12 a long 14 months earlier. The reports of the Luna 16 findings came directly from Alexander Pavlovich Vinogradov, vice president of the USSR’s Academy of Sciences and director of Vernadsky Institute for Analytical Chemistry in Moscow. Vinogradov’s paper was given the honor of occupying the first pages of the three-volume conference proceedings. In it he accepts the impact origin of the regolith (reluctantly, it seems to me) and is bothered by the concentration of the maria in a belt on the near side. The paper shows the classic signs of confusion between an impact basin and its volcanic filling.

The second rock fest might be described as the KREEP festival. Read any technical account of Moon rocks and you encounter the catchy acronym KREEP (K for potassium; Rare Earth Elements; Phosphorus). The abundance of these elements in the samples from Apollo 12, and later Apollo 14, astonished the chemists and has had important implications for the extent and style of lunar differentiation that are still not resolved. KREEP turned out to be the “magic component” that seemed to raise the Tranquility Base soil ages to the age of the Moon. In the naming paper and another influential paper in the conference proceedings, the fathers of KREEP, geochemists Paul Gast, Norm Hubbard, and Charles Meyer, suggested that the KREEP-y fragments found in abundance at the Apollo 12 site came from an older, nonmare basalt that lay somewhere beneath the mare basalts that constitute the local bedrock. Geochemists love basalts because they are the usual product of planetary melting and reveal the nature of their hidden sources. Since basalt made the maria, the geochemists thought that a different basalt might have made the highlands (an old suggestion by Baldwin), and they suggested that the name KREEP might someday be replaced by highland basalt. But KREEP is the most highly radioactive lunar material because of its potassium-40, uranium, and thorium. If it composed the highlands or large parts of the crust, the Moon would be the hotbed of volcanism that the hominopers once thought it was but that we now know it is not. Instead, it appears to be concentrated in the Imbrium-Procellarum region — another puzzle.

Only two weeks after the conference, the geologists and the laboratory people got a second chance to see what actually does lie beneath the mare basalts of the Apollo 12 site. There was KREEP, and much more.