Prologue

There was never any doubt that I would be a geologist by profession. At the age of five, I collected rocks at various stops on our summer vacation through the western United States and dutifully put them into the backseat of our '35 Plymouth. My parents encouraged this semi-rational activity, and our family developed into a full-fledged nest of rock hounds and mineral collectors. Soon our home in Austin, Texas, was full of specimen cabinets containing agates, petrified wood, and various types of crystal groups, and I was learning a little bit about them. My father, a carpenter and general building contractor by profession, became interested in cutting and polishing stones. He was particularly fond of agates and petrified wood and set up a bench with machines to cut and polish these materials. I soon learned to use diamond rock saws as well as grinding and polishing laps.

The father of one of my childhood friends, Dr. Virgil Barnes, was a geologist with the University of Texas Bureau of Economic Geology. He had been awarded a federal grant to study tektites. Tektites are small glassy rocks that superficially appear similar to pebbles of obsidian or volcanic glass (Photo 1); however, they are not associated with volcanoes, and their origin was in dispute for many years.[1] Some investigators believed that tektites were pieces of the Moon. Barnes needed some tektites cut into thin slabs for micro-

[1]E. A. King, "The origin of tektites: A brief review," *American Scientist*, March–April 1977: 212–218.

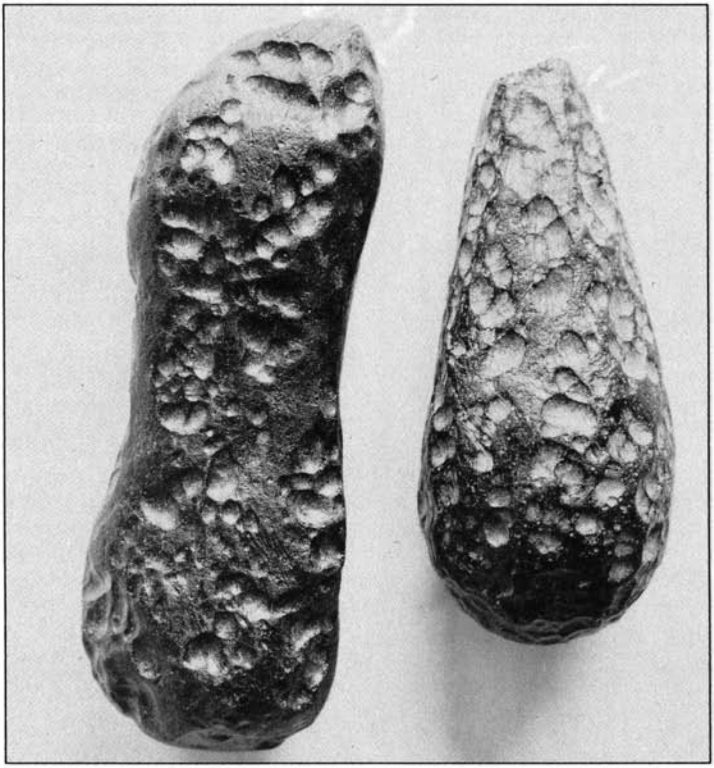

Photo 1. Typical tektites from Thailand. These are very dark brown glass and appear black in reflected light. The length of the largest piece is approximately six centimeters. (Photograph by the author)

scopic examination, but he did not have access to a suitable rock saw. He sent word through his son that he needed some specimens and asked if we could cut them for him. He offered to pay for the work, but we agreed that I could keep a portion of each specimen that we cut for our collection. I picked up the tektite samples from Barnes at his office in an old brick building at the Bureau of Economic Geology, a building I would later get to know very well. I had seen tektites before in museum displays and in the hands of private collectors, but I had never been afforded the opportunity to examine them so closely. I even cut my finger slightly on a sharp glassy edge of a fragment. Strange material to be possible pieces of the Moon. Barnes' tektites were mostly from the Philippines, but a few were from areas in Texas not too far away. It seemed peculiar that pieces of glass from two such widely separated localities should appear nearly identical. I was fascinated by the controversy over their origin. At that time a wide difference of opinion existed about their mode of origin in addition to the argument over whether they had been formed on the Earth or on the Moon. Barnes generously shared his knowledge of tektites with me at the level that a high school student could understand—he was a very patient man.

About a year later, in 1952, a summer job became available at the bureau, and Barnes asked if I might be interested in taking it. The work would entail a variety of tasks, but would mainly involve operating a large rock saw to cut hundreds of feet of oil well cores from deep wells in West Texas for Barnes' paleontologic examination. Of course I was interested! In order to get the job, however, I had to interview with the director of the bureau, Dr. John T. Lonsdale. I was somewhat nervous about meeting Lonsdale, an "old school" gentleman—tall, straight, and cleanly shaven except for a thin moustache. He had served as an artillery officer in World War I and had done a lot of geological field work in Big Bend National Park and other parts of West Texas. It seemed to me that he had been everywhere and done everything, but during our first conversation, he wanted to focus on my interests and experiences, which seemed very meager indeed. Lonsdale was an extremely gracious and interesting man whose eyes twinkled as he spoke. We had a pleasant conversation for more than half an hour, when he announced that I could have the job. He appeared to be totally fascinated by geology, and I hoped he had formed the same impression about me. Although he tended to be a bit stiff and formal, Lonsdale had put me very much at ease, and I liked him at once.

For the next several years, during my high school and undergraduate days, I worked at the bureau for Barnes and Lonsdale during the summers and part-time during academic semesters. Much of the work was mindless drudgery, but frequently something exciting or interesting came along.

It was at the bureau that I came to know an aging UT doctoral student, John Dietrich, an expert photogeologist who was working on volcanic geology in West Texas. Dietrich later joined NASA and worked at the Johnson Space Center. He became involved in the interpretation of lunar orbital imagery and surface photography and was eventually appointed as curator of lunar samples.

My first face-to-face contact with a meteorite came about through my association with the bureau. One hot summer afternoon while working in the laboratory with Lonsdale, we took a break from some tedious microscope work and began to chat. Lonsdale lit a cigarette—even though he had been trying to quit. I took the opportunity to ask him about a very peculiar rock in a nearby specimen cabinet. He took the rock of eight or 10 pounds from the cabinet and handed it to me. It was covered with a thin cream-colored fusion crust (an atmospheric friction-melted thin layer) on two sides and contained large gray crystals. It turned out to be a fragment of the Peña Blanca Spring meteorite that had fallen in West Texas in 1946. Lonsdale had published the original description and analysis of the stone.[2] As he told me the story of how 150 pounds of rock had fallen on an August afternoon into a partially

[2]J. T. Lonsdale, "The Peña Blanca Spring meteorite, Brewster County, Texas," *American Mineralogist*, vol. 32 (1947): 354–364.

dammed, spring-fed creek that was used as a swimming pool at a ranch headquarters near Marathon, Texas, I was enthralled. The fall of the stone was accompanied by "sonic booms" and occurred within plain sight of a dozen people, some of whom were badly frightened by the event. That a meteorite should choose a swimming pool out of all of West Texas as its place to fall seemed truly remarkable. "Where do meteorites come from?" I asked. Lonsdale told me that not all scientists agreed on the matter, but that most believed they were fragments of asteroids that had been deflected into Earth-crossing orbits by gravitational interactions with Jupiter, Mars, or another large asteroid. I knew the asteroid belt was between the orbits of Mars and Jupiter, but the fact that I was standing there in Austin, Texas, with a rock in my hands that had come from outside the orbit of Mars seemed truly miraculous. Lonsdale said the Peña Blanca Spring meteorite was a very rare type of stony meteorite containing huge crystals of enstatite, a magnesium silicate mineral, and he pointed them out to me. I asked Lonsdale if he had pieces of any other meteorites. He didn't, but he was certain that Barnes did. Barnes was out of town.

For several days I had to content myself with reading about meteorites in textbooks and general publications, but I also looked up both Lonsdale's and Barnes' publications about meteorites. Barnes had described at least three meteorite finds: a stony meteorite from Cuero, Texas; another stone from Kimble County, Texas; and an iron meteorite from Nordheim, Texas.[3] In addition, he had compiled a catalog of Texas meteorites. Not only were the Cuero, Kimble County, and Nordheim meteorites very unlike those from Peña Blanca Spring, they were also different from each other. It seemed curious that four pieces of asteroids should be so different.

When Barnes returned to his office I was impatiently waiting to

[3]V. E. Barnes, "The stony meteorite from Cuero, Texas," University of Texas Publication No. 3945 (1939): 613–622; "The stony meteorite from Kimble County, Texas," University of Texas Publication No. 3945 (1939): 623–632; "The iron meteorite from Nordheim, Texas," University of Texas Publication No. 3945 (1939): 633–644.

see him. I knew he would have lots of mail to catch up on, so I asked to see him on his second day back in the office. I explained how I had developed some interest in meteorites and asked him if I might see some of his specimens. Barnes seemed genuinely pleased by my enthusiasm. He showed me all his meteorites and tektites and allowed me to handle and examine many of them. We spent more than an hour discussing the subjects. I was elated. A well-prepared display in a case or cabinet can be interesting, but the real magic of discovery comes with the intimacy of touch and contact with specimens. Good teachers know this technique and use it shamelessly.

In 1954, during my sophomore year at the University of Texas, I took a required mineralogy class taught by Dr. Fred Bullard. Bullard was a superb lecturer and teacher. I was interested in and well-prepared for the subject, so I excelled in his class. Bullard asked me to serve as teaching assistant for the mineralogy labs the very next semester—quite an honor for an undergraduate. Of course I could not refuse. Before accepting, however, I had to clear it with Lonsdale, who was counting on me for the same semester. Lonsdale understood completely, and I had the impression when talking with Lonsdale that he had already discussed the subject with Bullard.

Working as a teaching assistant in mineralogy labs for Bullard was enjoyable. Bullard was a kind, rational, and fair man. One day while preparing specimen trays for the labs, I mentioned something about meteorites to Bullard. Bullard took me into his office and, to my amazement, showed me several specimens that he had obtained through the years (Photo 2). Meteorites are rare objects, yet three men I knew and respected had samples and had worked with them. They were fascinated by these rare objects as much or more than I was. In any event, I was hooked on the space-related aspects of geology.

Not all my undergraduate classes went so well. I tended to work on the classes I liked and slid through the rest, as many students do. Structural Geology was a required course for geology majors. It wasn't my favorite class by any means, but it was ably taught by

Photo 2. The Rosebud, Texas, stony meteorite (chondrite) showing a well-developed ablation surface due to heating by friction with the atmosphere during entry at high velocity. Maximum dimension is approximately 40 centimeters. (Photograph by the author)

Dr. William Muehlburger, who had recently joined the UT faculty from Cal Tech. Muehlburger, who was a large, very physical man, had played football for Cal Tech. He was an accomplished pianist and was very articulate. Muehlburger also taught the required summer Field Geology course, a stimulating course held in the summer heat of the Marathon Basin of West Texas. The course was physically and mentally demanding and, in retrospect, was exceptional experience for young geology students, though few students were fond of the course. Muehlburger later participated in the astronaut geology training courses both in the field and at the Johnson Space Center. He also led the lunar field geology team that kept track of the astronauts' lunar surface activities during the later Apollo missions. In any event, my undergraduate studies progressed fairly well, although I generated a rather undistinguished academic record with a transcript full of Cs.

During one semester when I was again working part-time at the bureau, Barnes asked me to accompany him on a field trip to some of the Texas tektite occurrences. He needed an assistant to help with the driving and sample collecting. We would be gone only a few days, so I could probably arrange to miss a couple of classes. Naturally, I was excited! Not only would I get to see where tektites were found, but I might even find one myself. Furthermore, I would be able to discuss meteorites and tektites with Barnes at length. This sounded like a good deal, but to top it off, the bureau would pay me wages for eight hours a day for the duration of the trip and would reimburse me for meals and lodging.

The trip was a grand success. I tried not to be a pest, but I completely exhausted my store of questions after the second day. I didn't find any tektites myself, but Barnes did. I resolved to return to some of the localities later and improve my score. Over the next two years I made several trips to collect tektites, both alone and with my parents, who were still avid rock and mineral collectors. I brought many of the tektites to show to Barnes, especially if they had any unusual feature or were of exceptional size. Eventually, my parents and I collected about two hundred specimens, some from localities previously unknown to Barnes, who carefully recorded them on his maps. In the scientific literature, the argument about the possible lunar origin of tektites was warming up.

In June 1957, I graduated with a B.S. in geology, got married, and was commissioned as an officer in the United States Naval Reserve. My ROTC enrollment caught up with me, and I was assigned to sea duty aboard the U.S.S. *Saint Paul,* a heavy cruiser whose home port was Long Beach, California. My life-style changed dramatically. The Navy wasn't a bad place to be. I learned a lot about people, politics, and bureaucracy, but it didn't seem necessary to study these subjects full-time for two years. I could not imagine doing this for the rest of my life. I presumed large corporations probably worked much the same way. If I ever got out of the Navy, I resolved to be a damned serious graduate student.

I was released from active duty in the Navy after 22 months. No conflicts, police actions, or wars occurred while I was on active duty. I had been very lucky. I was conditionally accepted to geology graduate school at UT because of my mediocre undergraduate performance. However, I would not be taking any classes until the fall semester, and I desperately needed a job in order to support myself and my young family. I went to see Lonsdale at the bureau. Lonsdale was glad to see me and agreed to put me on the payroll right away. He wasn't exactly certain what I would be doing but was sure he could find plenty for me to do. I was relieved. It felt good to be back in Austin in familiar surroundings and working with people that I already knew well.

My graduate work at UT progressed the way it was supposed to. I worked hard and did well. My master's thesis involved a combination of field work, stratigraphy, and sedimentation. My interest in tektites and meteorites continued, and I made many friends among the other graduate students. One of these was Uel Clanton, who was working with clay minerals. My own interests leaned toward mineralogy, so Clanton and I frequently met each other in the X-ray diffraction laboratory in the course of our separate research projects. At that time, the fields of planetology and space geology didn't exist. Clanton was soon the first geologist hired by the NASA Johnson Space Center. He would work on lunar hand tools, astronaut geology training, lunar image interpretation, high altitude micrometeorite collection and analysis, lunar sample analysis, and numerous other Apollo-related topics.

I had made up my mind to continue graduate study for a doctoral degree. The faculty at UT counseled me to select another graduate school for doctorate-level work; it was considered too inbred to earn three degrees from one school. After determining that my research interests were in mineralogy, my faculty advisors made a list of the schools they thought were in the forefront of mineralogical research and suggested I apply to the three or four that most appealed to me. I did as they recommended and requested letters of reference from my graduate school professors. They must have written very generous letters indeed, because I was accepted with a tuition scholarship at my first choice—Harvard.

May 1961 was a pivotal time in the course of the United States space program. Al Shepard took his gutsy suborbital ride on Mercury-Redstone 3 on May 5. Like so many others, I was glued to a television screen for the event. The United States had its first manned space mission—only 15 minutes and 22 seconds of actual flight—but it was a critical step.

John F. Kennedy's message to Congress in late May 1961, in which he stated his support for a manned mission to the moon, came while I was preparing to move to the Boston area. It was electrifying news! Would we really do it? In the following months, I carefully followed news items related to the "moon mission." Arguments about the terrestrial vs. lunar origin of tektites suddenly assumed greater importance, and the debate heated up. If tektites were lunar material, a number of important conclusions could be made about the origin and history of the moon and what kinds of rocks might be present on its surface. As far as I could tell, the arguments for a lunar origin of tektites were weak, but a lot of information was missing. I decided to try to work on some aspect of tektite research for my dissertation, but I was unsure about whether the geological sciences faculty at Harvard would accept a topic in this area. I would have to wait to find out about that. My first priorities were to get moved and settled (one wife, two daughters, a dog, and a cat). Our 1953 Plymouth and an unstable rented trailer hauled us to Boston. Meanwhile, Gus Grissom took a more elegant trip on Mercury-Redstone 4 on July 21.

My family and all our stuff were temporarily housed in a dingy little apartment in a poor section of Boston. After three weeks we found a more comfortable old two-family house that we could afford. The next order of business was to find Harvard. Armed with a street map, I set off in the car. The first time I drove right by the university without recognizing it. It looked nothing at all like the University of Texas. Classes would begin in a few days. I found the department and checked in with the departmental secretary, who ran her eyes over me with an experienced and appraising gaze, but gave me all the right information to get settled. I had only one goal—to survive the first year!

The Geosciences Department at Harvard had a reputation among the students for being demanding. At the end of the first year, the faculty gave a critical review of each new student. They considered course grades and the general progress each student had made and determined whether each student could stay and work toward the Ph.D. If not, the student was asked to leave but was awarded a master's degree in recognition of having completed a year of course work. The percentage of students asked to leave at the end of the first year varied from year to year, but usually the number was high.

In the hall I met a tall, soft-spoken student named Gene Simmons, who said he was the only other Texan in the department. He showed me around and introduced me to the other graduate geology students. Simmons later joined the MIT faculty, became a principal investigator for lunar sample analysis, and served as chief scientist of the NASA Johnson Space Center.

The other graduate students were a really mixed bag. Some of them would finish in three years; others had been there for more than seven years. Some had completed every requirement for the Ph.D. except the foreign language reading exams. They seemed to have one thing in common—each of them knew a lot about something. I had a lot to learn from my fellow students as well as from the faculty.

During the course of introductions I met Jeff Warner, an extremely bright and active student with abundant nervous energy and a strong New York accent. Although we seemed an unlikely pair, we became very good friends. Warner later joined the faculty at the University of Alaska, then Franklin and Marshall College, and eventually went to the NASA Johnson Space Center as an expert in petrology and computer systems. He became a member of the Lunar Sample Preliminary Examination Team and a very active researcher with lunar samples as well as terrestrial geology.

In another office I was introduced to a student named Jack Schmitt from New Mexico. Schmitt handed me a rock and asked me to identify it. It was a high-pressure, high-temperature rock called eclogite, composed chiefly of the pyroxene mineral omphacite and garnet. I identified it correctly. Schmitt was impressed and surmised that probably I would be a successful Harvard graduate student. Schmitt was doing his dissertation on Fennoscandian eclogites and had an inflated opinion of their importance. Schmitt later went to work for the U.S. Geological Survey (USGS) and was selected as a NASA scientist-astronaut. He reached the Moon as a member of the Apollo 17 crew, the only geologist to do so. After the Apollo program, Schmitt was elected to the U.S. Senate from New Mexico.

Things went well. Only one course caused me any problems during the first semester, and I completed the reading exams in French and Russian. The Harvard mineral collection was fantastically beautiful and complete. The department also had a magnificent collection of meteorites. I was eager to get started on dissertation research, so I decided to approach my faculty advisor, Dr. Cornelius S. Hurlbut, Jr., about working with tektites. Hurlbut had been on the Harvard faculty for many years and had a worldwide research reputation. He was a sensitive, friendly man who was genuinely interested in helping students, particularly if they were interested in mineralogy. He thought about the topic for a minute, remarked that none of the faculty was particularly knowledgable about tektites, but suggested I summarize the tektite literature and bring him a written proposal in the next few months.

Dr. Clifford Frondel was another well-respected member of the Harvard faculty and whose presence there was one strong reason I chose Harvard for further graduate study. Frondel later became a member of the Lunar Sample Preliminary Examination Team for the early Apollo missions and a principal investigator for the mineralogy and petrology of lunar samples.

Though not the best formal lecturer I had ever known, Frondel's mind was stuffed with an incomparable accumulation of mineralogical knowledge. He was at his best in small seminars and classes that he held at his home, over cookies and hot coffee provided by his wife, Judy, also an accomplished mineralogist, who later compiled a volume on the mineralogy of lunar rocks.[4] Typically, Frondel's class presentations provided a thorough historical background for each topic, summarized what was known about the topic, and carefully pointed out what was not known about each topic. He frequently passed fabulous specimens among the students to illustrate his points.

I prepared the proposal on tektites that Hurlbut had requested and turned it in to him. Several days later he called me into his office and said the proposal looked fine and I could start on the work whenever I had time. He also suggested I talk with Dr. Bill Pinson, a member of the MIT faculty, who had a student working with him on tektites. I didn't find Pinson on my first trip to MIT, but I met his student, Charlie Schnetzler, with whom I exchanged some ideas and conversation. He was working with the geochemistry of tektites, strontium-rubidium mass spectrometry in particular, which was unrelated to what I wanted to do. Also, he leaned strongly toward a lunar origin for tektites (as did Pinson), so we interpreted many facts quite differently. He was an amiable young man. I enjoyed talking to him, but we had little further contact. Schnetzler later joined NASA at the Goddard Space Flight Center and provided some of the most convincing support for the terrestrial origin of tektites prior to the actual return of lunar samples.

During the fall, a local Boston television station wanted to air a debate on the lunar vs. terrestrial origin of tektites. Pinson was chosen to argue the lunar side, and the station wanted to recruit someone to argue the terrestrial side. They contacted me about doing so. After briefly thinking it over, I declined. If I lost the debate, I might come off looking bad, maybe even stupid. If I won the debate, I might make Pinson look bad. I didn't want to risk making any enemies on the MIT faculty. It clearly was the safest decision for a new student in the Boston area.

The first semester was over. Christmas and the New Year's celebration had come and gone. The Boston winter was in full swing

[4]J. W. Frondel, *Lunar Mineralogy* (New York: Wiley-Interscience, 1975).

with snow, subzero temperatures, and brisk wind. In February 1962, John Glenn led the American way into low Earth orbit. I was hanging on every picture, event, and word from the flight. My heart still has a warm spot for Australians simply because the City of Perth turned on its lights for Glenn. We may have been lagging behind the Russians, but for me, the orbital flights of John Glenn, Scott Carpenter, Wally Schirra, and Gordon Cooper were inspirational, and I was confident we would catch up. Project Mercury flights ended with Cooper's splashdown on May 16, 1963. It was nearly two years before the first Project Gemini flight. At the time I never dreamed I would not only get to meet the Mercury astronauts but would have the opportunity to work with them as well.

One morning when I came to the department, I noticed several students looking at the ceiling above one graduate student's desk. They were looking at what was purported to be a geologic map of a portion of the surface of the Moon.[5] They had tacked it to the ceiling to depict the normal orientation of the lunar surface to earthbound observers. It was not a complicated map, but using photogeology, the author had interpreted the major time-stratigraphic relationships of a part of the surface of the Moon in a way that looked very convincing. The map stayed on the ceiling for several weeks and provoked a lot of thought and discussion.

I wanted very much to get involved with NASA programs, particularly with the exploration of the Moon, but I didn't know how to go about it. Besides, I still had classes to worry about, and I was going to take my oral exams in a few months. The exams were absolutely critical. I had to take one step at a time, and I planned to get my oral exams out of the way. Then I could finish the research and writing of my dissertation during the next academic year, 1963–64. In the early spring of 1963, I decided it was time to start looking for a postgraduate job, preferably in a space-related geology field. Dr. Eugene Shoemaker had emerged as the public spokesman for

[5]R. J. Hackman, Geologic map of the Kepler region of the moon, U.S. Geological Survey, Misc. Geol. Inv., Map I-355 (1962).

space-related geology, particularly for the lunar program. Although I had never met him, I had listened to him present technical papers. I knew he was an enthusiastic and influential player in the game. Also, because he was setting up a new organization in Flagstaff, he might have some jobs available in a year or so. Shoemaker had earned his Ph.D. from Princeton University, where he had worked on the impact mechanics of Meteor Crater, Arizona. He had joined the U.S. Geological Survey and served as a consultant to NASA headquarters in Washington, D.C. Shoemaker became the principal investigator for the Lunar Field Geology Experiment for the early Apollo missions and later joined the Cal Tech faculty.

I carefully drafted a letter to Shoemaker and prepared a professional résumé to accompany the letter. The résumé was brief, but I had a few publications to my credit and tried to make the best of it. This could be an important letter for me, so I sweated considerably over the organization and exact wording. I still did not have a draft with which I was entirely happy when I came home and found a note to call Uel Clanton in Houston—IMPORTANT!

I returned Clanton's call and asked what he was doing in Houston. I thought he was still in Austin working on his dissertation. He had taken a job with the NASA Johnson Space Center (or Manned Spacecraft Center, as it was then known) and was helping develop instruments and tools for use in space, training astronauts in geology, defining the lunar surface environment, etc. He was right at the heart of the action! He was calling me because there was a job available at NASA in his organizational element (which became the Lunar Surface Technology Section). His NASA bosses wanted a geologist who knew a lot about the lunar surface. I didn't know anything about the lunar surface! Clanton told me that I must know about the lunar surface because I knew about tektites, and there was a prominent senior NASA scientist at the Goddard Space Flight Center who had convinced almost everyone at NASA that tektites came from the Moon. The NASA scientist was, of course, John O'Keefe, whose work with tektites was familiar to me. I told Clanton I did not share O'Keefe's view of tektites, but he said it really