

**USGS Open-File Report 2005-1190**  
**APPENDIX A**  
**(Selected Interviews with Branch of Astrogeology personnel)**

What follows (except as indicated) are relevant portions of selected interviews conducted by this author (2000-2004) as background in completing this work. The interviews were mostly with retired or current employees of the U.S. Geological Survey, Branch of Astrogeology, and/or the Branch of Surface Planetary Exploration (the latter Branch was in existence only from 1967 to 1973). The persons interviewed are listed in alphabetical order by surname. There were, of course, many people that contributed significantly to the notable achievement of the Branch of Astrogeology and Branch of Surface Planetary Exploration during the Apollo Era.

Although not all personnel who worked for U.S. Geological Survey in two branches prior to and during the Apollo Era could be specifically mentioned in this work, or were interviewed as part of this appendix, I personally thanked them all on behalf of the U.S. Geological Survey and (if I may) NASA for their outstanding contributions. This acknowledgment includes scientists, support personnel, and administrative/secretarial personnel alike. All of you can be very proud of the job you did.

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**Raymond M. Batson** (planetary cartographer; b. 1931; B.S., 1962, University of Colorado)

The following information was taken from a phone interview with Ray Batson (Hurricane, Utah) by Gerald Schaber on 19 January 2001:

Batson: "I recall being interested in space exploration at the early age of five. Working with the [U.S. Geological] Survey in Denver while going to the University of Colorado, I spent every minute he could watching the early launches---often missing a few classes trying to keep up with them. I became "physically ill" when the Vanguard launch vehicle blew up on the launch pad [December 1957]. I worked at the Federal Center for about five years, starting in 1957—including a year in the photogrammetry office, and two years in the field-doing field controls. Then, as the space program started "cooking"—in 1962—I saw a job vacancy announcement from Shoemaker's new Branch of Astrogeologic Studies in for Menlo Park, California.

I went through map-sales office in the Denver Federal Center and saw this [Bob] Hartmann map--an engineering [terrain] study of the Moon. I bought that using personal funds because I was interested in it. Then a job vacancy announcement appeared on the bulletin board that they were going to hire a photogrammetrist-geologist in Menlo Park, California for eventually moving to Flagstaff, Arizona. That would have been in 1962. I had just taken my first vacation time since I'd been with the Survey---and the vacancy announcement had closed. My wife Rhoda said to go ahead and give it a shot. So I went ahead and sent in an application--and heard zip! Then I got a call from Don Elston who was over at Carl Roach's house. He called me up and asked me to come over there for an interview. So I did. We hit it off all right. Then I went back to work and kind of bided my time. I didn't hear a thing for a long time. Then all of a sudden, I get this notice that I'd been accepted.

I was instructed to report to Menlo Park, and they said as soon as he walked in the door--they would fill out the paper. That was February of 1963 when I walked through the door. Elliot Morris was there and showed me around. I met Mike Carr, Henry "Hank" Moore and Richard "Dick" Eggleton. I didn't meet Gene [Shoemaker] for a while because he was on leave to NASA Headquarters. I just kept hearing about the legendary Gene Shoemaker. The first job they gave me was to compute a triangulation net on the Sierra Madeira structure. Shoemaker, Elston and Eggleton had been down there and they had done a bunch of triangulation in the field with a T2 theodolite, but were not quite clear on how to reduce the data. The data had not been gathered very systematically, and I had never computed a triangulation net from field data. So, the job was really a nightmare!

The Branch was so broke at that time that you couldn't even get an adding machine, let alone a calculator. So I went to downtown Menlo and bought myself some eight-place log tables. And I would sit there, calculating all of the triangulation net with log and place tables by hand! I called a couple of office supply stores in Menlo and talked them into 30-day trials on different adding machines, turning them back in and getting another. I finally scrounged a loaner for a longer term to finish the net. It took months! I got that done and started compiling on the Sierra Madeira project, on a Kelsh Plotter.

It wasn't until he was busy working on the Sierra Madeira [near Ft. Stockton, Texas] mapping that I actually would meet Gene Shoemaker; Gene would ask me to transfer the geologic contacts from his detailed mapping at Meteor Crater to a map base. Well, Gene had already mapped the Crater. He had mapped the thing on old Trimetragon aerial photos; and they were just terrible—and he expected all of this geology to be transferred down to the nearest resolution element of the photos. You know, Gene was always optimistic about things like that!

That was actually the first time I met Gene; I was sitting there working away on the log tables for the Sierra Madeira map, and everybody came walking through with this "kid." I asked somebody, who's that kid out there—he's growing a mustache, but he still looks about 19 years old?" Someone said that's Gene Shoemaker. I guess after I met him, we probably shook hands—that's about it. Then I was trying to transfer that damn geology to these ancient old photos, and just having a hell of a time. Don Elston wouldn't even let me buy a gooseneck lamp to put on the Kelsh Plotter. It was a going to cost about three dollars, and the Branch didn't have any money. I needed to pop the lamp on and off so I could look at Gene's drawings [Gene wrote extremely small on his geologic maps with a 3-0 Rapidograph pen as I recall]—and then look at the stereo model on the Kelsh Plotter. So that [project] muddled along—and I never really did finish that transfer. Gene was kind of bummed out about it—but I did finish a map.

Gene did finish his thesis [1960, Princeton] on Meteor Crater, but he did it showing his geology just on the air photos. He wanted it transferred to a topo base- which didn't exist [at that time]. So the first thing I did was make the topographic base [of Meteor Crater] from Gene's old photos. In the mean time, I talked them [the Branch] into finally springing for about \$700 to get a guy named Mark Hurt to fly new photos out at Meteor Crater.

It was six months to the day that his family] moved to Flagstaff (in September 1963). And that was right about the same time that they had finished our Astrogeology wing on the Museum of

Northern Arizona. Russ Wahmann had moved there [to MNA] earlier, and had stayed in the "Whoopee House," as he called it--actually the Hopi House at the Museum. Jack McCauley and Chuck Marshall were already there."

Before the wing at the Museum was built, Jack McCauley and Chuck Marshall had space on the second floor of the Arizona Bank Building for about a year. The initial contingent of Astro people from Menlo, consisting of Don Elston, Elliot Morris and Russ Wahmann, moved into a little building on the museum grounds called the "Hopi House." That was probably the summer of 1963. There were only a few weeks until the completion of the Astrogeology wing in the museum

I didn't get any lunar cartography to speak of [at first] because Col. Strickland was so proprietary about having ACIC to do the lunar maps. However, one of the first jobs [of that kind] that Gene wanted me to do was to use the stereoplotter for the Ranger pictures. That was tricky because the baseline was vertical. They wanted to use it on the approach pictures. There wasn't much of a horizontal component to it. You could get stereo out of it but the parallax is radial to the approach vector, and that was tricky because the baseline was vertical and, of course, we didn't have computers then.

In the meantime, before the first Ranger actually was successful, Dave Roddy working on Flynn Creek (an impact structure in Texas) as a graduate student at Cal Tech. He needed a lot of horizontal control, and there wasn't any way that I was going to be able to do that and some of the other stuff that Gene wanted me to do. So Don Elston and I went up to Denver—and we ended up hiring Jim Alderman. He was a photogrammetrist. We interviewed both Sam Prieb—a very easy-going guy; quite competent, but never very aggressive. Both Elston and I were both kind of worried about Alderman. But you know-Shoemaker kept putting it to us—well, who is really the most aggressive and most competent? Well, that had to be Alderman, at least the most aggressive part. So, we ended up hiring him. Later, we also hired Sam Prieb. Alderman went down to Flynn Creek and spent about three months there—about 1965 I would guess.

Anyhow, Alderman came back and was going to set up a topographic mapping unit [at the Branch of Astrogeology]—and he considered himself in charge of everything. By that time, Shoemaker had put Hank Holt, Elliot Morris and me on the Surveyor Project as Co-Investigators. I was supposed to come up with ways of making maps of the near-field from the two cameras that were supposed to be on the Surveyor spacecraft for science. The so-called engineering flights only carried one camera. So that- was--you know--not really much to do with that. And Shoemaker had also got Bob Altenhofen, who was head of the topographic Division in Menlo Park and an author of one of the early chapters in the manual of Photogrammetry--and quite a very bright guy--also very aggressive and impatient with some of Gene's antics, and some of the NASA antics. He got really pissed off a couple of times when we'd go to JPL and nobody would show up at a meeting we had there. He just had a hissy-fit on a couple of occasions. But he died, I think he was a diabetic--I think that's what got him before he had time to do anything. And it was never quite clear what he and I were doing on the Surveyor Project except to extract stereo out of this two-camera system--which never ended up happening."

*Schaber: "I heard that NASA panicked because of all the crashes with Ranger, so NASA just decided to take almost everything off of Surveyor."*

Batson: "Yes; there was some discussion about that. They did have two cameras on the spacecraft--they had an approach camera. That thing--would have really simplified the mission because they were set to turn that thing [approach camera] on as the thing was in descent trajectory, and take pictures like Ranger--all the way down. And they didn't find out until all the design was locked-and-lit that they didn't have the bandwidth to operate the approach camera. So, it rode along on the first three Surveyor flights but it was never turned on. Well, they did turn it on after the spacecraft landed. Tom Birch suggested well, you might have a focus problem, the camera is set on infinity, and it's only going to be about a foot of the ground. So, that didn't work.

But I remember we had this big Surveyor camera field test out at Bonita lava flow. Let's see, Surveyor 1 landed in 1966 on Memorial Day, and so we had been out there on the Bonita lava flow close to a year or so before that (1964-1965) with a prototype --not a prototype-but a simulated camera. We were engaged in making survey photos [mosaics], and working out photographic sequences. And it was while we were out there that Ranger 6 was going down--and I remember Jay Rennilson listening to the radio--and hearing that it failed. That was just about the time that Nikita Krushchev got tossed out in Russia.

Another of the things that I did was work with Gene when we [the U.S.] one-upped the Soviets--was with the images returned by their Luna-9 spacecraft on the lunar surface [February, 1966]. Bernard Lovell had secretly recorded the signals from Luna 9 at Jodrell bank in England. He and noticed that the signals looked a whole lot like the signals transmitted by newspaper fax machines. So, he ran the signals through a newspaper fax machine and came up with pictures of the Moon. And so he released it but the aspect ratio was wrong. [*Schaber- it was compresses!*]. It was compressed in one direction but not the other. Anyhow, Gene and I got on that one and we made up a map from the pictures. We figured out the aspect ratio early on, and we made a map of the surface from the Luna-9 pictures and wrote an article for Aeronautics and Astronautics which appeared in 1966--before the Russians! There was some muttering and grumbling about stealing pieces of other people's data, and writing reports on it. But, of course, in those days anything was fair where the Russian were concerned.

In the meantime, one of Steve Dwornik [NASA Headquarters] good buddies, who was the best man at his wedding, worked for the CIA back then. We were talking to Steve on the phone; he said I'm not going to identify him because-I don't know if it would cause a problem now. In any case, he [Steve Dwornik] would not feed us any information directly--but he obviously knew something. So Gene or I would figure out where the mirror was on the spacecraft, and what it was looking at. Gene spotted that one. We'd ask Steve about it, and it was like twenty questions. Steve would get back to us and said "Well, you're getting warm--or well, that doesn't look so realistic." But anyhow, our article came out and it was about a year or two later that somebody asked Gene Phillippi [a colleague at the Branch of Astrogeology in Flagstaff], where in the heck we had gotten all of that information. I got back with the guy and said, "We figured it out. Well, they "gave" that to us. This was somebody from CIA, and he said, well, all we can say is, it is damn hunchy."

Schaber: "Well, in Gene's interview with Harry Ryan [from Flagstaff who did some documentation on Early Branch history for the USGS], Gene said that the DOD also secretly downloaded (from our CIA bases in Iraq or Iran) much better resolution version of those [Luna-9] pictures."

Batson: "Yes; we were doing pretty good work even though we had a lot less to work with. After Gene found out about those CIA tapes of the Luna 9 images, he did a whole lot of politicking and finagling to get copies of them; and in the end, they [the CIA] finally sent them through. However, they purposely scrambled them; there was nothing but noise on them. So I don't know that we ever did see what was on them."

*Schaber: "So what were the big exiting things and problems we had on Surveyor? I know it was real surprise to everyone when Surveyor 1 actually landed. Gene said that we were ready; however, Elliott Morris [another Astro member of the Surveyor Television science Team] said that we weren't ready."*

Batson: "Well, it depends on what you mean by ready! We certainly weren't prepared for 11,000 pictures [from that first Surveyor]. We were sort of ready [one could say] because my team had been preparing to lay down frames at four second intervals. That I believe was the framing rate on the thing. And those cameras only had a six degree field of view; so any one picture by itself didn't mean anything. You had to put them down in mosaics. So, we had a system worked out--and we had simulated this out on the Bonita Lava flow [near Sunset Crater just a few miles east of Flagstaff, Arizona] with data packages of pictures--and we'd feed them to the mosaickers at the rate we expected. So we were prepared for a much faster rate. However, we didn't get them anywhere near that fast because they were messing with the sequences in real time, and that kind of stuff."

*Schaber: "Was pasting them [the Surveyor pictures] inside the half hemispheres known from the beginning? That was the kind of geometry you had--right?"*

Batson: Yes, that's the way it worked out, especially a small picture. We didn't lay them down on the hemispheres at first. We put them in segments-- which looked like a map projection."

*Schaber: "So was putting them in a hemisphere our idea?"*

Batson: "Well, actually maybe not. I think Tom Bird [JPL] came up with that. He got a couple of big plastic globes; and that was kind of a pain. Of course, you have to adjust the picture size to fit. We fiddled with that a little bit and then Blecha [Bob Blecha-a machinist at Astrogeology in Flagstaff] and I got together and figured out the exact size we needed. Blecka managed to get a whole bunch of fiberglass globes made up. We started putting the Surveyor images down in the hemispheres in the very best surveys. There were a total of about 89,000 images from all six of the Surveyor missions. Up until Magellan [NASA' Venus, radar-mapping spacecraft (1990-1994)], I think, that was the largest data set that NASA had.

As far as being ready goes, it was a bitch to try and figure this stuff out--and it was a real bitch to try to map with them, because we only had the one camera--and it was tilted."

*Schaber: "Especially when you're trying to put them down in a mosaic at first."*

Batson: "Yes. You pretended like the surface didn't matter. In other words, we knew the azimuth and elevation setting of the camera."

*Schaber: "Yes, that's right, it made the surface sinusoidal."*

Batson: "Yes, it made a sinusoidal surface. And the amplitude of the sine wave was the tilt of the spacecraft. You can compute that quite accurately."

*Schaber: "And then you had fun when one of them [Surveyor Landers] landed sideways or was tilted, right?"*

Batson: "Well, no, they were all a little bit crooked. The camera was crooked--we already knew we had a 16-degree tilt. And so we had to take that out, and what was left over after that was the tilt of the spacecraft. And Surveyor 3 landed inside that crater and kind of crow-hopped down because the retro-control jets didn't shut off properly."

What we were not prepared for was the volume of information we were going to get---and really how to do something with it. There was a lot of flailing around. JPL had no place for us to work. So, we ended up working in the Control Center--not the big one with the big maps--but it was a side room. It was called TSAC (sp?); I forgot what the acronym stands for. We had Ray Sabala and Jim Van Diver [from our Branch of Astrogeology in Flagstaff] down there [at JPL], and we sneaked into the room that was dedicated for Lunar Orbiter. It had a carpeted floor, and no tables, nobody was working in there."

*Schaber: "That's right, Lunar Orbiter--was going on at the same time?"*

Batson: "Well, one of them [Lunar Orbiters] came online at the same time during on of the [Surveyor] missions. But we could get access to that room. We had to be sneaky because your badge was good only for a particular room in the SFOF [Space Flight Operations Facility--at JPL]. So Shoemaker, and somebody else, had a badge for one of these other rooms. And we'd keep sneaking that same badge back and forth. It was driving the guard nuts--he couldn't figure it out and he couldn't catch us at it. Those guards are really paranoid out there."

Jim VanDivier and Ray Sabala were trying to draw circles with a beam compass--on paper that was sitting on carpet. They finally ended up using a pencil for the center, a string and another pencil to draw a circle. And Jody [Lowman/Swann] had a typewriter set up on one of the consoles, and she was typing mission reports on that. And we had an old Freidan rotary [mechanical] calculator in there that Shoemaker was cranking out grand computations. And Hank [Holt] and Elliott [Morris] were doing crater counts--and I was trying to figure out how in the hell to make maps out of these. We made some sketches but I wasn't at all confident--especially the way that Luna-9 worked out--that they had any dimensional integrity at all. We had come up with a scheme for making measurements from the photos--like how far way things are by changing the focus on the camera. It had 50 discrete focus steps that were calibrated. They wouldn't let us mess with it on Surveyor I, but on subsequent Surveyors--On Surveyor III-- it didn't matter because the

pictures were so crappy because the lens was dirty. But on Surveyor V and subsequent ones, we would start off and survey as if the ground was on a flat surface. We would compute where the picture would have been taken if it was on a pool table. It wasn't of course--it was out of focus. So they would let us range the focus in and out until we got an in-focus picture. And then we knew what focus setting that was on, -so we knew how far away it was. So we made a bunch of reasonably good sketch maps of the near field. It was only good out to maybe 30 or 40 feet from the spacecraft. But that was better than we had before.

But most of the science they did was looking at the surface and making pronouncements based on what it looked like. Also, on Surveyor V (I think) that was the first time they had the alpha scattering instrument that Tony Turkevich came up with. But that got stuck-- got stuck on deployment. Maybe that was on [Surveyor] VII. I can't remember which the first one was on. I think the soil sampler was on three (Surveyor III). So we got a lot of pictures of scratching on the surface; and that had strain gauges that could give you strength of the regolith information. Ron Scott [Caltech] was the PI on that.

From then on, I think [Surveyor] V had the alpha-scattering box [for determining the chemical composition of the surface]. They got the first measurements on that and proved [Harold] Urey dead wrong--much to his [undecipherable.] They were getting together with Turkevich, Urey, and some other people in a motel room preliminary to the news conference. Turkevich went over his calculations, and you could see Urey's face just sagging as he saw all of his pet ideas [like the absence of basalt on the Moon] going down the toilet. And finally when it was over, he [Urey] got up--went to walk out of the motel room and he shook his head and said "Well, I guess Mother Nature knows best."--And he walked away."

*Schaber: "So, after Surveyor-- how did the Lunar mapping program and then the planetary mapping program go- and talk a bit about you ace airbrush artists Pat Bridges and Jay Inge."*

Batson: "The Air Force Aeronautical Chart and Information Center [ACIC] in St. Louis got Pat Bridges working on this map of Copernicus map [about 1957-1958]. It's a military institution but other military agencies can get in there; so they didn't want anyone to see her making that map--less they get ideas and try to one-up them. They set Pat up in a little office hidden under some stairs in the old building [at ACIC in St. Louis]. So she did that map in hiding so that AMS [Army Map Service] wouldn't see it and get any bright ideas (see main text, 1959-1960). Pat was from St. Louis. She and Bill Cannell [also from ACIC in St. Louis] came to Flagstaff in 1959 and got things set up with Lowell. They used the telescopes up there and started the ACIC operation before the Survey's Branch of Astrogeology ever got going."

[Author's Note: Shoemaker had for several years by then been using lunar telescopic photographs to map Copernicus crater. However, during a visit to JPL in 1960, Gene was astonished to see a copy of the ACIC prototype LAC (Lunar Aeronautical Chart) of the Copernicus region, that Pat Bridges had been working on for ACIC in St. Louis, lying on a table in the trailer office of his former Caltech classmate Manfred Eimer, assistant chief to Albert Hibbs of JPL's Space Science Division. Gene immediately had the Branch copy Pat's prototype LAC chart map of Copernicus with the goal of transferring his geology to that LAC base; see main text, 1960.]

Batson: “Pat joined ACIC at Lowell in 1960 [Author’s Note: and stayed there until 1967; see Pat Bridge’s interview below; see main text]. Pat proceeded everybody in Astrogeology to doing lunar mapping in Flagstaff, including McCauley and [Chuck] Marshall.

Jay Inge came on at Lowell after Lunar Orbiter started sending stuff down [in 1963]. He kind of set up a production line operation for doing the airbrushing off of the Lunar Orbiter until someone said why are we doing this--those LO photos are so good that we don't need airbrushing. So, they fired all the airbrushers. So in 1967, Jay went to work for National Geographic, and Pat went back home and had kids.”

[Author’s Note: Pat eventually got back into the cartographic airbrushing when we hired her at Astrogeology in 1970. Lowell hired some of the people that worked in the Flagstaff ACIC office when it down-sized. Some of them were working for Lowell and some of them were working for ACIC under contract. Jay Inge came back from National Geographic in 1971 to once again work for Lowell Observatory; and about a year later began working for the Branch of Astrogeology in the Arizona Bank Building in downtown Flagstaff].

Batson: “When Mariner 9 got to Mars [1971]--there was the great dust storm on the planet, and it started clearing from the South Pole first. So we managed to put together a half-assed mosaic. Pat and Jay worked days, nights, and weekends with all those pictures and made an airbrush map of the South Pole. That really kind of set the credibility in the airbrush operation because none of the actual [photo] mosaics looked like anything. And, having that map--you know it really did something. Then we did a map of the equatorial band. Then, we were off and running.

*Schaber: Then, you started getting into Apollo, like talking to the astronauts about taking photos on the lunar surface. How did that happen?”*

Batson: “Well, Gordon sort of got me in on that. Because Surveyor was sort of winding down and it really didn't look like there was going to be anything after that. And at the same time, Gene had come up with this cockamamie idea about the lunar stereo camera--LGEC. Yes, the Apollo Surveying Staff, the little camera with the vertical base on it. So Gordon and Gene approached me to get onto that and start doing some work with that. Hank Holt and I both ended up in that. Hank was going to do some photometry and colorimetry with it. And we worked along with Tim Hait and Gene Phillippi--in the early stages-- getting that thing built--going back to Goertz Optical in Pittsburgh that got the contract to build the thing. It never did work, of course. The camera kept jamming. It wasn't going to be ready in time for Apollo 11, and on Apollo 12. Then, for Apollo 14, we thought it might work. Then we went to Hawaii with Al Shepard, but we had Lovell working with it for Apollo 13. Then the thing kept freezing up on Shepard in Hawaii and they couldn't make it work--even in the terrestrial model. It was pretty clear that it was way too complicated.

In the meantime, Warren Borgeson [Branch photogrammetrist] and I had worked up procedures for using a Hasselblad, starting with Apollo 11.



*Schaber: "Where did we get Warren Borgeson?"*

Batson: "Borgeson ["Borgy"] came on just after Jim Alderman [who arrived 12 June 1964]. Alderman found him, I believe in Washington D.C. He never worked for us you know. He was always on detail from Topographic Division [USGS]. He came out and worked with Alderman, then [later] he kind of took over the photogrammetry operation down at Fourth Street [in East Flagstaff] until Sherman Wu showed up. Sherman wouldn't work with him. Alderman hired both Borgeson and Sherman.

*Schaber: "So Apollo astronaut Training. What part did you play in training these guys?"*

Batson: Well, we started off working on developing lunar surface photo procedures. Early on, we worked down at the Apollo Data Facility the Branch [of Surface Planetary Exploration] built down on at Fourth Street [in East Flagstaff in 1967]. I got in on one of the last few tests in that facility.

*Schaber: But you actually went with them in the field and talked to them about photography. Did you ever go down to Houston and talk to them?"*

Batson: Oh yes, I had briefings pretty often at the Cape [Canaveral] starting with the Apollo 11 mission although I really never got to brief the Apollo 11 crew face-to-face [Author's Note: Very few people were allowed to brief the Apollo 11 crew; especially lowly scientists ["anti-engineers"] who wanted the crew to do any "science"—God forbid]. Tim Hail, Gordon [Swann] and I developed some photo procedures that we recommended that they use. These procedures were relayed to them and they agreed to use them to some degree.

*Schaber: "The gnomon we made them take along [to the lunar surface]; the Apollo crews hated that darn thing didn't they?"*

Batson: "Yes, and let's see--who's idea was that? I was kind of credited with that but I didn't come up with it. I think it might have been Danny Milton. [Author's Note: Gordon said it was Danny]. He named it anyway. I was worried how we were going to establish any vertical control on the lunar surface. And, of course, Henry Holt wanted a photometric target. So we had both on the gnomon. Actually, the gnomon never worked right."

*Schaber: Tell me about the perspective grid that you came up with that we used down in Houston after every mission to try to locate distances."*

Batson: In photogrammetry it is called a Canadian Grid. The reason it's called that is that they used it to make reconnaissance maps of the Canadian Shield from air photos-oblique air photos. --because the Canadian Shield is so flat, you can get away with that. Actually, I did invent it without realizing that it had already been invented. I think I came up with that and someone said--Oh, that's a Canadian Grid. Oh, well, okay, I thought it was a Batson grid. Yes, we also used that [perspective grid] on Surveyor.

The other mission that I got involved in was as a team member on Mariner-Venus-Mercury (Mariner 73). So I went out there on all three encounters on Mercury--and the Venus encounter.

The image processing was pretty crummy in those days, and we got a global mosaic of Venus through the UV filter--of the classic cloud patterns. Pat [Bridges] retouched that mosaic, and that retouched image made the cover of *Science* [Magazine]. Then I had Bill Sowers and... out at JPL doing the Mercury and Venus encounter mosaics-- In 1973--right during the big gasoline crisis.

*Schaber: "Describe the process by which your cartographic group and the VanDivier Group in Building-two on the Mesa got the maps made. That appeared to be really efficient."*

Batson: "Well, yes, it started essentially during the Apollo 12 site mapping from the [surface Hassleblad] cameras; we had to figure out where the cameras had been located [and to take the panoramas, etc]. We got Hugh Thomas [photographer] in the act making various overlays. And also, of course, with the Mariner 9 pictures we [together] made that series of uncontrolled photomosaics of Mars. We didn't try to re-project the pictures--we just scaled them--because we had footprint plots of where they should be on the surface. Those were all trapezoids and diamond shapes--and the pictures were all rectangular of course--and we didn't have any way to change the projection on them--at least in time to do any good for the scientists. So I thought--anything is better than nothing if we can get at least close to the right scale, we can probably make some decent [if not absolutely-controlled] mosaics.

The problem was that we had the old X-Y plotter then--and we were plotting up the footprints, and so we made a bunch of templates for Karl Zeller and his photolab gang to use to enlarge the pictures to. There was a different template for each picture of course. And we borrowed Joe Remy from Jim VanDivier to come down to the bank building to trace these plots--and boy he hated that!

Of course Jim Vandivier and Russ Wahmann before him had already been doing all the scribing and mapping stuff for the lunar geology maps. The geologists were doing geologic maps of the Moon since about 1962.

So the only time that I really got into the systematic mapping (planetary cartography) was on Mariner 9 and on Mariner 73. Up until that time Strickland [Army Map Service, AMS] was absolutely adamant that Flagstaff was not going to be doing any topographic mapping. That was not their turf--they could do geology if they wanted to.

*Schaber: "Even though the Survey had the biggest topographic mapping Groups in the World."*

Batson: Well, the Survey really dropped the ball on Lunar Orbiter because--they were approached [with regard to making maps]. There was stereo on the Lunar Orbiter images, and you could do some mapping [despite the fact that the images sent back to Earth were actual, dry processed, and scanned photographic strips that had stereographic steps between them]. When the Survey was approached on that they said--oh no, those pictures are all wrong--their no good for that. So NASA had no choice but to go to ACIC and AMS.

Bill Cannell at ACIC was a good manager who kept the ball rolling and all of that. But that AMS gang---there was one big fuss where AMS said that they were going to do all of the Surveyor mapping and photo-mosaicking-. They told the press that. They didn't have anything to do with it.

I heard it on the radio. Then, Steve Dwornik [NASA Headquarters] got the word that they were making stereo maps from single camera pictures-- which isn't as magic as it sounds because the pictures are taken through the mirror-and every time the mirror would take a step sideways, the virtual image in the camera would move another perspective center. So, there was a stereo in those pictures, but the base line was only a couple centimeters. But they, of course, had these first-order plotters---these hundred to two hundred thousand dollar jobs. So, you could stick those pictures in there and duplicate the geometry--and yes, you could see the stereo effect. And the operators are convincing themselves that they could put a dot down. So Dwornik told Shoemaker what was going on and Shoemaker said, well, we're going to send Batson out there and check it out [at AMS in Washington DC]. I wrote up this memo-trip report-- to Shoemaker in which I eventually said that this stuff was bullshit and showed the reason it was bullshit mathematically--you know the resolution element was smaller than the amount of parallax.”

*Schaber: “I found looking at stereo over the years that your brain can fool you into seeing things that really aren't there.*

Batson: Oh yes, you can look at two pictures that are no stereo and convince yourself that your seeing stereo

Depth perception is not just stereo--you use a whole bunch of clues. When you fly across country in an airplane--especially over mountains--even if you way the hell up--your eye base is no way near enough to resolve stereo--yet you are quite well aware of the three-dimensional aspect of the ground below you. Some of the reasons for that is that you are changing perspectives constantly, and your mind is assimilating the parallax as you move---and telling you that's rough down there. The parallax is there and the brain knows it. Now that's one of the reasons why Pat Bridges was so successful [despite the fact she had trouble seeing stereo in images] is because she can keep storing these images in her brain from all different perspectives--and she wasn't exactly seeing them in stereo--but she was getting a very accurate picture out of her brain after she had looked at several pictures.

*Schaber: "So, we're into Apollo and we start making maps of landing sites. How did the planetary mapping come about?"*

Batson: What happened was Elliott [Morris], as Dwornik designee back there (at NASA Headquarters for two years), and I wrote a letter to Steve whom I'd known during the Surveyor thing--detailing the different phases that one would have to go through to do a map series for Mars. Parenthetically, I said that we had done 89,000 pictures on Surveyor that we knew something about handling data. I outlined how the geodesy had to be done and the mosaicking and the final mapping--about a three page letter-- showing the different phases and how you could farm out some of the work, and how some of the work had to be done in the same place--and all of that. I did not hear directly from Steve, but when Elliott came back, he said that that letter really made a hit with Steve at headquarters. [Batson later told this author--this event was at the very start of Mariner 9]. So, it was at that point that Steve jumped on the bandwagon to get the Survey to do all of the planetary maps. That was a big jump. And I think that my letter [to Dwornik] was historic. That may be my own personal conceit, but I think it was. So, anyhow, Steve came down very firmly on the side of the Survey [this was during Mariner 71].

In the meantime, Hal was working on Steinbacher at JPL and the Mariner Project Office to have us do two or three prototype maps under Project funding, rather than headquarters funding, and they went along with that. And at this time we also got involved with astronomer Gerald de Vaucouleurs at the University of Texas. Gerald was the head of the Mariner 9 cartographic group on the Mariner 9 team. And Gerald's main interest was in verifying the telescopic observations he had made of Mars over 40 years or so. In fact, one of the experiments he wanted JPL to do was to compress each one of their images down to a single pixel.

He was having trouble with all these details. He was very impatient with craters. He said the interesting part of Mars is the markings--the craters are always there-- they don't mean anything scientifically. So you can see why we might have had some disagreements. Gerald did a couple of maps for sky and telescope. They were very attractive, but they showed only enough craters to set the scene--mostly albedo markings. They published two or three of those but NASA wouldn't fund him for any of that. In the meantime, we had funding from the Project, and Dwornik flailed around Headquarters and set up the program to get us the money to do the rest of the Mars maps. We were also doing these mosaics--which didn't have the status of the published maps. So we open-filed the Mars maps with the USGS. That kind of ticked off Strickland that we dared to do that too--but he finally backed down."

*Schaber: "So what were your biggest challenges in your Cartography Group at Astrogeology in those days?"*

Batson: Well, internal problems-- my biggest problem was that the Survey would never let me hire anybody. I had everyone working WAE. Then, every September they'd say--that's it-- they are going to count WAE's. Everyone goes home and we don't pay them. And that way it looks like we don't have any WAE's. And that included Pat [Bridges] and Jay [Inge]--and the whole damn gang. It wasn't working very well for morale. We were keeping WAE's working 80 hours--and sometimes even more--per pay period. They couldn't get time-and-a-half if they went more than 80 hours because they weren't supposed to be working that much. Somewhere in the Survey--not in Astro--somebody blew the whistle-- and the Survey came down and said "You can't work WAE's full time. You have to give them a full-time appointment--and were not going to give you any slots. For example, Jay Inge didn't get full time until Mike Carr was Branch Chief (1974-1979).

*Schaber: "Bottom line here is that your Cartography group mapped an area in the Solar System that was over seven times the area of the Earth."*

Batson: "Basically I was in on and did mapping on the first complete reconnaissance mapping of the Solar System--twenty-three satellites and seven planets [excluding Earth and Pluto].

*Schaber: "So your definitive work was the one that you got published with Ron Greeley [Arizona State University]?"*

Batson: Well, we did one "Planetary Mapping" it came out in 1990--and it showed all the phases in mapping--and the last one--that is just now going into yet another printing--I'm still working on

it-- the NASA "Atlas of the Solar System."--that has also been published in Polish would you believe?

*Schaber: "The final question then. If you had it all to do over again, what would you do differently and why--or would you?"*

Batson: Oh, I think I would do the same thing. Basically, I think it is the career that I had always hoped to do. I felt like a cartographer--I never felt like a geologist. I studied the courses they made me take, but never really enjoyed it that much except for the geometric things--structural geology and all of that. Stratigraphy just about drove me crazy, and so did mineralogy.

*Schaber: We all fell into what seemed like a niche in those days-- to what fits us. It was an amazing group of people.*

Batson: Yes, but even as a kid, one of my favorite books around the house was an Atlas of the World. And I sat down with all those maps and figured out voyages I was going to take--which I am now doing. But anyway, to get to map the planets, the frustration was not to be able to get all three dimensions. You had to be satisfied with planimetric maps and I had to be able to explain to people why they didn't have contour lines on them.

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**Norman G. ("Red") Bailey** (b. 1927; geologist/driller) arrived in Flagstaff to work with the Branch of Astrogeology on 2 April 1964. During the Apollo Era, the USGS, Branch of Surface Planetary Exploration in Flagstaff used Red's rather impressive and broad range of knowledge on many subjects, as well as his contagious enthusiasm and ability, to "get tough jobs done very effectively. Red left Flagstaff in 1976 to work at Wood's Hole [Massachusetts] for the Branch of Atlantic and Gulf of Mexico Mariner Geology where he worked for ten years before retiring from the USGS. Red currently resides in Dewey, Arizona.

The following information was taken from an interview with Norman "Red" Bailey (Dewey, Arizona) by Gerald G. Schaber on 22 January 2001:

Bailey: "I was working for the Ohio Division of Water as an Engineering Tech. I hadn't finished college, so that was the best position (and money) they could get me. But essentially, I was working as a ground water geologist and an engineering geologist doing work on dam sites; preliminary foundation studies, and operating a drill rig.

*Schaber: "Where in Ohio was this?"*

Bailey: "All over the State of Ohio. I worked for them about eleven years, from about 1953 until March of 1964 when I left for Flagstaff. The way I came upon the job [with Astrogeology] was that Joel Watkins, the geophysicist from Washington D.C., came [in the early 1960s] over to work on a co-op deal between the USGS and the State of Ohio doing refraction seismics. I drilled the shot holes for these crews that would come in either from the USGS in Denver, or over from Washington, D.C. They would come in and spent two or three weeks in Ohio in the summer time; sometimes in the fall or spring. We would go out in the field and carry out refraction seismics.

Essentially, we were trying to outline the buried bedrock valleys in the state--beneath the glacial cover. Of course, this was important to us in water resources because the buried valleys were great sources of ground water.

So, working with Joel--he came into the state [Ohio] twice; the last time was 1963--the year before I came to Flagstaff. That December, he called me and said that there was a project coming up out west and he was interested in hiring me. He couldn't tell me any more about it because it hadn't been firmed up as yet. It wasn't until the following January [1964]--early in January--that Joel either called or wrote about the project--the *In Situ* Geophysics Project out in Flagstaff. Joel was transferring out to Flagstaff from Washington as Project Chief. The In Situ Project was about a two-year, NASA-funded, project, to determine the feasibility of using refraction seismic methodology to determine the engineering properties of lunar-like materials, especially volcanics. I was to find out later that geophysicist Marty Kane had something to do with this project. Marty was interested in finishing some work on his Doctorate and couldn't take over the project himself [at Flagstaff], so Joel ended up with the project there.

So, lo and behold, I came out to Flagstaff, and started work in on April 1 in 1964--April Fools day. I was located up at the [Astrogeology wing of the] Museum of Northern Arizona. I was the first one on the In situ geophysics project. Joel Watkins showed up about a week or so later. Meanwhile, I started writing up the specs to get the drill rig. Joel and I occupied the same office [at the Astrogeology wing at MNA]. Shortly thereafter, Dick Godson came on board [17 April 1964]. Joel also hired a young geophysicist out of the University of Oregon--James H. Whitcomb arrived 9 September 1964]. We also had hired [WAE starting June 1964] a consultant Claude Jean De Bremaecker out of Rice University. As Joel set the project up, my job was of course to get the test drilling outfit set up--order the drill rig, set up a drill shop, etc.

But when we out in a bid to the Survey to buy the drill rig, it bounced from Washington with a note that the Survey wasn't allowed to buy drill rigs. Can you imagine the Survey not having drill rigs? They do have them, but they called them dosimeter tube installers, and other names. So, that requisition bounced, and we had to turn around and talk to NASA. Then, NASA bought the thing for us, and simply assigned it to Flagstaff. Anyway, that caused a delay of six months or more to get a drill rig. In the meantime, I was doing nickel-and-dime administrative work for Joel--trying to get the project going. I was going down to Phoenix, interviewing perspective employees for the project--the working stiffs that we were going to hire. And Joel, of course, was busy writing reports and trying to get the In -Situ Geophysics Project staffed up.

What I understand was that two years later we essentially found was that the results of In Situ's experimentation with refraction seismics, magnetometer studies, and gravity meter studies in volcanic materials, were somewhat inconclusive and confusing with regard to what one could really tell about the engineering properties of such materials.

We did work on the [very blocky] SP lava flow [extending north from the base of SP Cinder Cone, from Hank's Trading Post on highway 89 north, north of Flagstaff]. We also worked out at the [rugged, aa-type] Bonita lava at Sunset Crater [northeast of town], and over at the Nevada Test Site [NTS] in Nevada. We were at the NTS on an astronaut training exercise at the same time that

Joel Watkins was there with his crew doing research work for the Branch's *In Situ* Geophysical Project.

[Author's Note: Red Bailey's actual personal log of daily activities during June 1965 includes the following assignments:

2/8-17/65	Nevada Test Site In Situ Work; shot hole drilling, astronaut training (2/16), test hole over an uncollapsed, underground, test
2/22-24/65	Nevada Test Site; In Situ work; shot hole drilling, astronaut training (2/23)
2/26/65	Move to new office on Mesa (Building One)
2/28/65-3/3/65	Nevada Test Site; In Situ work; shot hole drilling, astronaut training
3/12-25/65	Core drilling at Amboy Crater (CA)
4/1-7/65	Meteor Crater; In Situ work; shot hole drilling
4/12-29/65	S.P. Flow; start core drilling
5/10-19/65	Kingman, Arizona area, Yucca area and Sacramento Valley, California; In situ Work; Shot hole drilling.
5/23-28/65	Tucson area; In Situ work; shot hole drilling

*Schaber: "Tell me about this incredible road that you built up on SP Lava Flow. That was always amazing to me!"*

Bailey: "Joel [Watkins] came into me one day and said I want you to build a road on SP Lava Flow. The two of us went out and climbed up on the darn thing, and Joel said, I want you to build a road up here. I said, how far? He said, well, at last it has to be about a thousand yards or more into the flow.

In the meantime, he had borrowed Bob Loney from the Survey [arrived in Flagstaff in April 1964]. Bob was a good California field geologist--an old timer and good field man. I had been out with Loney studying the geology. We'd walked across SP Flow--halfway out-- collecting rock samples in our backpacks--and backpacking rocks back out again.

When I got back into the office the next day, people like [Jack] McCauley stuck their heads in the office and said I hear your going to built a road up on top of SP Lava Flow! I said Yeah. They said how are you planning to do that?" I said, I don't know!"

*Schaber; "So you obtained a big bulldozer as I recall."*

Bailey: "I believe it was a D-8 Caterpillar; we leased it from a local contractor."

*Schaber: "So had you ever driven such a large dozer before?"*

Bailey: "No, I didn't---I hired a contractor. So I took him out there in a Government truck and showed him where I wanted this road."

*Schaber: "And he did what-laugh?"*

Bailey: "No, he just stood there, and then popped his cowboy hat back off the back of his head. He stood there on top of SP Flow scratching his head you know. I thought he was having some doubts and I said, do you think you can put a road up here. And he said, hell Red, I don't know. He said, but I've got a guy named Johnny [something]; and he pushed coral around on all the Islands out in the Pacific during WW II, and if anyone can do it, he can. So, we arranged the following Monday I believe that I would meet him out there and sure enough--he showed up with the big D-8 on a low-boy truck.

*Schaber: "I'm really surprised that he didn't get killed going up that rugged and blocky slope."*

Bailey: "We figured a way. You know the edges [of SP flow] are kind of grassy [in places] where the lava flow kind of plowed out over junk that was along the sides and edges of the thing. I picked a way up where he could wind back and forth to get up to the top-- and then a straight shot. I guess it was 1,500 feet long-- enough to lay a 1,200 foot seismic line

You know, I worked out there three days picking up rocks off of SP Flow and throwing them under the steel treads of that dozer so he could crush them down to make a road bed. All he did was level out a cut-and- fill-operation. He cut the flow tops sticking up, and pushed all those blocks and stuff in to fill out. We made the "road" as flat as we could make it with the materials we had to work with (see Fig. 47).

*Schaber: "So, once they got the road built, they built some kind of wooden platform there--I guess it was a drilling spot-right?"*

Bailey: "Well, we took the drill rig up on the flow when we finally got the road finished. Joel [Watkins] had a sign put up out there [at SP Lava Flow]. Is it still there?"

*Schaber: "No, the cattle have knocked it down. It's all weathered and chewed up; you can hardly read what it says anymore."*

[Author's Note: The sign said something to the effect that this road was constructed by the USGS, Branch of Astrogeology's In Situ Geophysics Project in 1965.]

Bailey "In fact, I started the first drill hole up there out at the end of the road at the 1500-foot marker. What we did there was make a big flat platform and a parking spot for the seismic trucks that Joel Watkins wanted to get up there and of course the support trucks with the crews, the dynamite, and stuff like that. So, we needed the road for that, essentially, so they could lay the seismic line out and record the shots.



But, I started the first drill hole. We had a hell of a time with that busted basalt [on S.P. Flow]. Then, we got called off of that [S.P. Flow drilling project] to go over to California or to the Nevada Test Site. So, I only got maybe down about thirty feet or so. What we had to do was drill--and then grout--and then drill out the concrete grout to leave a neat hole. Then we would go through those blocks--and anytime a block moved--you know—the drill bit would jam the hole--and then, all kinds of other problems.

Bob Elmer finished that first hole a year or so later. He was a driller I hired [January 1965] for Joel [Watkins]. He ran the rig when I left, and stayed on with the *In Situ* project. He took that first drill hole on S.P. Flow down 80 feet. He went through the entire flow. He even took it deeper than that I think [through the Moenkopi Formation (Triassic sandstone/siltstones) and into the Permian Kaibab limestone/dolomite].

[Author's Note: Red Bailey left the In Situ Project on 30 June 1965 and transferred (on 7 July 1965) to the Manned Lunar Exploration group of the Branch of Astrogeology (Project Chief-Gordon Swann). Here, Red was assisting project chiefs setting up and directing operations in the field, and office scientific data centers. Red was refining maps generated by [Apollo and Post-Apollo] tests and simulations. He was also Branch consultant to NASA in the development of lunar drills.]

Bailey: "What Gordon Swann said that he wanted to do was to help him analyze the early field tests. He had some early tapes--tape recordings--of guys like [Don] Elston and [Al] Chidester out at Meteor Crater."

*Schaber: "They were not in suits at that first Meteor Crater test as I recall."*

Bailey: "No, not in suits. They were in shirtsleeves playing astronaut out there. And all of them were busy arguing about this, that, and another--so they were quite disorganized. So Gordon was brought in there to take over and start running these tests. The very first suited test that I took part in was AES [Apollo Extension Systems] Test-1 [and Apollo Test 5] [20 October-1 November 1965; see 1965 in main text] out in the Hopi Buttes [Volcanic Field]. I acted as Capsule Communicator (Capcom) for the simulated lunar traverses. Gordon Swann, Joe O'Connor and Dave Schleicher were the suited test subjects [see Fig. 31].

In fact, Gordon and I went out there the first time together to get permission from the Navajo grazing committee chairman, and to explain what it was we'd be doing, and where. They would pass the word to the local Navajos as to who we were, and to keep their sheep in our way. So that was my first introduction to the Navajo Reservation.

*Schaber: "So, how did the idea about constructing our simulated crater fields come about?"*

Bailey: "Hans Ackermann was with the geophysics crew blowing some holes out there in the cinder flats just east of the Flagstaff city dump [east-northeast of town]; they were still doing geophysical work out there. They had the drill rig, of course. The city was at that time digging the trenches [for trash] in the south-southern part--the extension--of Cinder Lake. If you went straight

east on that road, you got out to where we eventually built the first Crater Field. Hans Ackermann and those people didn't want to get involved in building crater fields--and it became my chore.

Schaber; "Gordon says that the simulated crater field was originally John McGonigle's idea. He saw the geophysics guys working to blow these craters out by the city dump and said why not make a simulated, lunar-like, crater field out here for the astronauts."

*Schaber: "So, Gordon says that he's got a job for you. How did you get that thing [the Crater Field at Cinder lake] started?"*

Bailey: "Well, I took the Lunar Orbiter photo that they wanted me to pattern [copy] you know [a very small area within the anticipated Apollo 11 landing ellipse]... We simply blocked off a 500 X 500 foot area out there [at Cinder Lake] and turned that over to the Surveyors--[Jim] Crossan and his gang---to go out and survey the shot hole locations. Then, I had to come up with the logistics of how we were going to do it, including how we were going to get rid of all the truck tracks, footprints, and that stuff before we blew the craters. You remember, we went and got WW II aircraft landing mat that we dragged behind the trucks. It was what they had used to make remote airfields during the war. I was essentially the head honcho for logistics, and with the ordering of the explosives and this sort of thing. I had to devise how we were going to proceed --step by step. On top of that, Tim Hait-came up with the idea to geologically map the geology of the shot holes [craters], and do a report with [the authorship of] Hait and Bailey. I said, hell, I don't have time for that Tim; I'm too busy getting this thing built. So we got Bill Colton involved. And Colton actually did the geologic fieldwork. We eventually wrote a report on the crater field construction and geology that that should be around up there in the Astrogeology library, with an authorship of Colton, [Ivo] Lucchitta, and Bailey. This report was completed after we constructed the 3<sup>rd</sup> third Crater Field in Cinder Lake

The first crater field we built at Cinder Lake was 500 X 500 feet; but we later expanded that and added craters to the north end of it. Gene wanted to see the biggest hole we could possibly blow up, so I took all the explosives left over. By then, we had pretty good idea--from just experience--that the depth-to-diameter ratio of the crater you make is dependent on the depth of the charge and the amount of explosives we used [nitro-carbo-nitrate, which is ammonium nitrate soaked in diesel fuel, and dynamite sticks]. We actually did some experimental shooting out there at Cinder Lake just to determine that.

So, we finished the first crater Field [at Cinder Lake]. Then we did the second one later--which was a bigger one. Instead of 500 X 500 foot, the north field was 1200 X 1200 feet I believe.

And then, interestingly enough, with all the genius we had out there in Flagstaff it never occurred to any of us that we would be training crews in the winter time when the crater fields up there would be covered in snow. So, we had this desperate need to construct a third crater field somewhere down in a warmer environment. I know a whole bunch of us drove down to the lake Pleasant area. I think there were two vehicles of us--Al Chidester, Thor Karlstrom, Gordon Swann, George Ulrich, and myself. I not sure who else; however, there were perhaps six or eight of us. And, we drove all around there. Of course, they wanted me along for logistics---if we can

do this or not in any specific location. I didn't like the logistics at all down there, and we didn't see any particularly good sites of the type, flatness, and size that we wanted.

We gave up on the lake Pleasant area. Chidester had to go to Washington for a meeting. He told me when he left that he'd give us three days to find a site in the Verde Valley. Jack Strobell was the Branch Chief by then--and Al Chidester was the Center Chief. Strobell and I drove down to the Verde Valley; we drove back and forth through the country between Route 179 that goes over to Cottonwood off of the Interstate [I-17]; and then the route further up north that goes across to Sedona. We came down almost to the bridge crossing into Cottonwood, and then turned up and went northwest back up to the red rock country on back roads. I just couldn't find a site that I wanted. Strobell realized that he was no help and begged off. I went down alone the next day. I drove across the Verde River and looked over there at the site where the Black Canyon Crater Field was eventually constructed.

The site was nothing but scrub. And what I saw there, I liked. It was long, it was flat; we could fit in the size field we wanted. My biggest problem was that in the stream cuts just south of it is the Verde Lake beds and they case-harden on the outcrop over thousands of years into a hard limestone. I looked at that and thought--oh my God, we'll never be able to get a backhoe hole into that stuff.

*Schaber: "It was probably that fresh-water limestone from the big lake that was down in the Verde Valley long ago."*

Bailey: "Yes it was; and so the only way I could prove to myself was to get Bob Justice and In Situ's drill rig down there. And the third day [of the 3 days Chidester had given him] I hauled him Justice and his drilling rig down there. We left Flagstaff about 5 O'clock in the morning I think it was--and we started up on the upper part of that long sloping area just punching holes. I wanted that alluvial rubble to be over those Verde beds. If we could get through those Verde beds--it would be okay... What I suspected was--I'd seen the Verde Lake beds on road cuts where it's real powdery, where it is not case hardened--Just white powdery stuff--it's a lake deposit. And it doesn't harden until you get in on the outcrop apparently. And that's exactly what happened; I found that I could punch down into those lower beds okay with the drill bit. They were soft enough that, if we had to, we could dig down into them--if we had to--with a bulldozer.

And so, I kind of took a deep breath and said this is where we are going to do it. In three days, we found--had picked a spot-- and I thought boy, I'm going to really look bad if we have any problems at all. Well, we couldn't get charges down a drill hole without going to nitroglycerine, and I didn't know anything about handling nitroglycerine. Of course, by practical experience, I knew how to work with dynamite and some of the other explosives we had used earlier on the Cinder Lake Crater Fields [in Flagstaff].

Our Black Canyon Crater Field was constructed in February 1970. Henry Moore came over [from our Menlo Park office]. Here, of course, we were working in much more complex and compact materials that we had at Cinder Lake-- a big alluvial fan pouring out into the Verde Valley there. We did some test shooting there where Henry Moore worked out the mathematics for us.

The Black Canyon Crater Field was made for the Apollo 13 crew [who trained there on 13-16 March 1970; see March 1970 in main text]. When we cleared that thing and finished the work there, I left the next day to go down for the [Apollo 13] SIM in Houston. Then, when Apollo 13 didn't land, we trained the 14 crew down there [16 November 1970].

Bailey; "However, a problem did come up in that a couple of the Survey's water resources guys-- heard about us wanting to blow the Black Canyon Crater Field. They were afraid that we were going to dry up Montezuma's Well or collapse Montezuma's Castle [National Landmarks that were that were located 12 miles north across [the Verde Valley from the planned crater field]. They went to the Survey's Director and the Director's office sent out a call to Al Chidester [Chief of the Surface Planetary Exploration Branch at that time] to put a stop on it. So, that came up; and Chidester called me up over the car radio. I was down at the proposed Black Canyon Crater Field site supervising the contractors working to clear the crater field. Chidester told me to come back up to Flagstaff immediately. I get up there and Al asks me, what do you think about that? They told me that you are going to dry up Montezuma's well, or collapse Montezuma's Castle. I said that that's bull! I said the only thing that I'm worried about is the water well up at that Indian School which is only a quarter of a mile up the road.

*Schaber: "You certainly weren't going to use enough explosives to do that."*

Bailey: "No, and I just couldn't imagine it. However, we had never bothered doing any seismic recording on the two earlier crater fields [near Flagstaff], so we had no idea what kind of energy we were setting off.

*Schaber: "You would have gotten "Montezuma's Revenge!"*

Bailey: "So, we placed two seismic rigs; one by Montezuma's Castle; and we had a seismic suitcase recorder set up with Bob Regan up at the Indian School [nearby the actual crater field site]. They would record seismic energy-- recording just to prove what actually happened. We had to go to all this trouble. Then I got on the phone and called geophysicists all around the country. I had to describe the size of the shots--the fact that they were scattered over five or six hundred craters--or whatever, and then the biggest charge would be X number of pounds of nitro-carbo-nitrate, which is ammonium nitrate soaked in diesel fuel. I also informed them that the smaller ones would be dynamite, 60% high-velocity dynamite. Everybody [all the geophysicists they called] said the same thing; no way that you are going to bother anything. "How far did you say that thing was?" I said 12 miles away.

*Schaber: "So, you got the Flagstaff and Verde Valley crater fields constructed, and the [astronaut] crews really like them. So, after the crater fields were completed, what jobs did you do for Astrogeology?"*

Bailey: "Most of it was the post-mission reports and data analysis. You know my big chore during the mission was putting into the computer everything the astronauts said on the Moon about geology. All the traverses on the Moon, we loaded into a text-editing program [SEARCH; see David Dahlem, October 1965 in main text].

We started on the AP 12 mission with the computerizing. I went down to Houston Mission Control with Dave Dahlem on Apollo 11.

On Apollo 11 with Dahlem, my big chore in the Science Support room [at MSC Mission Control] there was to write up on a big blackboard the station numbers, and the samples they collected-- this sort of thing. Well, you know how sketchy [science wise] Apollo 11 turned out to be. Some effort of putting up an organized display, you know, rocks sample numbers, the colors, the size, the shape and that sort of thing.

The one thing I didn't mention before that is that I was, of course, also involved in some of the astronaut training trips. Actually, quite a few of them, starting with one of the first ones [3-6 June 1964] to the Philmont Scout Ranch [near Cimarron, New Mexico] with the *In Situ* [geophysics] Group [see June 1964 in main text]. Marty Kane and Joel Watkins were there representing our In Situ Geophysics group from Flagstaff (see Fig. 15). So, there were those things going on too. There was always something going on.

As you are well aware, at the field tests out at Hopi Buttes we would be analyzing the previous tests, and I would be helping Gordon as he was preparing the reports. And as those field tests went on [at Hopi Buttes, Meteor Crater, and other local sites], Gordon Swann would put other people in charge of them.

*Schaber: "Yes, I was put in charge of Field Test AAP 3 and AAP 8."*

Bailey: "Yes, okay, so you know how those things worked. We were all one big team, but it was somewhat interchangeable on the part of you guys, as to who was going to write up the report, and playing Capcom [capsule communicator] on those things. I took notes, as you recall, but I always had guys behind me with maps, and guys trying to make maps from what was being described-- and that sort of thing. But we were agonizingly trying to figure out ways of not only doing geology on the Moon--with the suits, but especially, how we were going to handle NASA people listening down on Earth. We didn't really know that we were actually going to be in real missions at that time.

*Schaber: "Well, we had hoped so. That's what we were doing it for--but we weren't sure!"*

Bailey: "The other interesting story about--I remember AES [field test] One, Gordon's first test out there at Hopi Buttes {20 September-1 October 1965}. Gene Shoemaker came out the second day--we were out there three or four days testing--he came out in a truck and wandered around and watched for two or three hours--and turned around and took off without saying a word. And Gordon turned around and said to me, we must be doing something right, because that's the first time he came out that Gene's come out to a field test where he didn't have to take it over, in order to get something out of it--you know. That was a real compliment. Gordon was real happy about that.

I remembered that because Gene didn't say a word. He wandered into that big van--we had that big truck van out there we were calling our data center [science support room]. He just stood there silently and watched you guys behind me working. And I remember. I was always listening with

the headset trying to keep track of what station they were at, and the sample numbers and things like that. But there were guys behind me that were hollering--ask them this, ask them that!

*Schaber: "This is what Gene wanted to see the astronauts do on the Moon--so he was happy with what we were doing. But unfortunately, it didn't happen on the Moon. Their time was so constrained and that's what really got Gene angry, and to decide that there wasn't going to be enough real science done."*

*Tell me what you did after each Apollo mission down at MSC in the Apollo Experimenter's trailers that were set up there."*

Bailey: "I would have information --at least, hopefully, the straight information on the stations and the timing, and all of those sorts of things that I was keeping track of--and then whenever those of you that were writing NASA's [mandatory] Interim reports, my data was available to them.

Starting with AP 12, we started putting all their data into the computer. We were using the big IBM computer at the National Institute of Health at Bethesda, Maryland. It was an IBM-360-50. In fact, they dedicated that to us on weekends when NIH wasn't doing anything else. They kept their computer Center up and kept that one computer running just for our purposes.

But what happened was, NASA had a whole batch of gals typing the transcripts--but, unfortunately, they had no technical editing done on the things. They had the wrong guy [astronaut] saying the words. They had terminology all wrong--and things like that. I would take those transcripts--and clean them up using my gals [the secretaries from the Survey in Flagstaff].

*Schaber: "We had Keith Welch, Don Thacker and Ben Leesman doing typing and transcribing--starting with Apollo 12, right?"*

Bailey; "Yes, they were the court reporters. Yeah, that was a different ball game. We were inputting our data into a distant computer starting at AP 12. Actually on AP 12, we didn't use the NIH computer--we used Stanford University's computer--and a text-editing program called WILBUR after one of the Wright brothers. It was a text-editing program--and what we finally decided to do was to just input all the [astronaut's] words [from the lunar surface] in there, and then to key-word each line from what station they were at--what sample they were talking about, and this sort of thing. So, it was a text of what they said, who was saying it, the exact time it was said-. That became my chore-- doing the key words, etc.

*Schaber: "Dave Dahlem [geologist with the Branch in Flagstaff] had a lot to do with this computer program, didn't he?"*

Bailey: "Yes, Dave was interested in doing computer modeling of geology. But, because he had taken computer programming and could program, he became head of that programming thing.

At the beginning of 1968 I got assigned to Dave Dahlem's activity in that regard, which was to develop a [FORTRAN IV] program to put all this information into--which we called SEARCH.

SEARCH was a data storage and retrieval program, which we developed testing it at the IBM 360-30 up in Flagstaff.

*Schaber: "At our ADF facility that our Surface Planetary Exploration Branch had built [in 1967] on Fourth Street in East Flagstaff, right?"*

Bailey: "Yes, on Fourth Street. Dave Dahlem somewhere found Jack Fife, and hired him on. Jack was-a very fine mathematician and programmer. Jack could not only program in FORTRAN (that was the big language at that time), but in Assembler language, which is an earlier language that only engineers and mathematicians used. It turned out, that SEARCH ended up having to have six Assembler Routines in it because you couldn't do the logic using FORTRAN at the time.

Our getting permission to use our program during Apollo 11 mission involving a NASA programmer named Delton Costa down at the mission control [in Houston]. Dave Dahlem got hooked up with Delton to arrange for us to use one of the NASA computers for further developing and testing our SEARCH program. During the time when we weren't going to be busy with the missions, we'd be busy messing around at Mission Control with our SEARCH program.

Down in the real time computer center in the Mission Control Building (MSC, Houston), there were five, state-of-the-art IBM-360-75 computers. There were two for each mission. They had a live system running the mission. They had a backup system that was taking in all the data information; and in case anything happened to the prime computer, the backup simply was kicked in and took over. And, essentially, since they never used both Mission Control Centers at the same time down there, the other three computers were used for testing, running SIMS at the same time etc.

So, they got us on one of those state-of-the-art [at the time] IBM-360-75's. The first time we fed our program in those big fancy computers, they couldn't read it. The reason was that it didn't have a compiler. They had never written a compiler for assembler language. They could apparently compile FORTRAN okay.

So once the NASA programmers got a compiler for assembler language installed--we were going in. We were the only two guys allowed in there. That was a very highly [secured] area.

*Schaber: "In between Apollo missions--right?"*

Bailey: "During the missions even--during the Apollo 11 mission. This is when we were going to use SEARCH."

*Schaber: "You mean they were going to let you use this other backup computer DURING the mission?"*

Bailey: "One of the other three computers at Mission Control--if we needed it. We were still developing SEARCH during Apollo 11 mission. Jack [Fife] was still up at Flagstaff writing routines for us.

Dave was the boss-- the big idea man--and Fife was our master programmer. I was the practical guy who came up with how we were going to get organized geologic information inputted

*Schaber: "It really never worked-right--I mean we never got it working to a useful level?"*

Bailey: "Well, we had the program working fine, but we weren't getting the data back. We realized from Apollo 11 that we were getting such fragmentary data [from the astronauts].

*Schaber: "They just picked them up [the samples] at random. They didn't number them or anything."*

Bailey: "Well, they didn't describe things [until the later Apollo missions]. In fact, we found out from our own field testing--with our own guys with doctorates in geology--guys with a lot of experience in the field--that nobody knew how to go out there and systematically describe--for organizing data in an organized fashion. Essentially, you picked up a rock and whatever came to your head--you said out there."

*Schaber: "So, the SEARCH program idea--sort of petered out before the later Apollo missions."*

Bailey: "Yeah, we decided that, since the data was going to be so fragmentary coming from up there anyway, putting it into this very organized program would be an embarrassment--not for the program [the SEARCH program], however, the program worked fine. It was just an embarrassment of how sketchy and fragmentary the information was that we were getting back [especially during the early Apollo missions]."

*Schaber: "It turned out that you've got [Tim] Hait, and all these other people---including myself--keeping track of where they were and all this. By writing things down--just the human interaction--everybody having their own task to do, sketching--whatever--that actually worked out pretty well--right?"*

Bailey: "Essentially, the team did it pretty much the old fashioned way. I still remember when we went out to map the SP Mountain quadrangle north of Flagstaff [1965-66]. We had planned to use all these modern methods [developed by the Branch of Astrogeology for lunar geologic exploration]. I was sitting out there in a chair [in an air-conditioned trailer] when you guys were out collecting samples, etc. This was another test to see how you would feed data [via audio and video] into a data center, and maybe have a rapport going between experienced people in the data center and guys fresh out of school out there doing field geology. This was sort of the concept that Gordon and I talked about.

Well, none of it ever came to fruition. The practical thing, starting from Apollo 12, was to just load every darn word they say into that WILBUR word processor [the computer program]--because it was a big program, we had a big computer at Stanford we could put it into on AP 12. Starting with AP 13, we were going to be using the NIH computers. Well, AP 13 didn't land-- and by AP 14, Dahlem had left, and Gordon simply put me in charge of the computer thing."



*Schaber: "You know what I think? We got all bound up in those days in the "new thing" on the block-- computers. This automating the world thing was a mindset--doing everything with a computer--I think we kind of over did it. But, it was a good idea--in theory. First of all, we didn't have time. Between the time [President] Kennedy said we are going to the Moon and getting there--there wasn't that much time, and computers were in their infancy."*

Bailey: "It won't be practical for Mars because the time delay alone wipes out any interactive."

*Schaber: "No, you would have to do it from a base there."*

Bailey: "Yeah, actually on Mars--when they finally do it someday--the interactive part would be the guys up in the spaceship circling Mars talking to the guys on the surface--and they'll be playing mission control for them."

*Schaber: "What were you doing toward the science missions--starting with AP 15, 16?"*

Bailey: "Apollo 15 used Coconino Point and the Little Colorado Valley—north of Flagstaff—for their final geology field training exercise [25 June 1971; see 1971 in main text]. It was my chore to be the guy out in the field during the day they came up here. You were along on the AP 15 exercise at Coconino Point, weren't you?"

*Schaber: "Yes, I did the photogeologic mapping for the Apollo 15 crew's field traverses for that Coconino Point test."*

Bailey: "Remember, we met them [the Apollo 15 prime and backup crews] at the Flagstaff airport when they landed and took them up there to the fifth floor of the Bank building to brief them. In fact, I had you do the briefing--not on the real geology, but on the photogeology, and on what they would be seeing on their traverses--that kind of thing. My essential chore was to plan and run the logistics out there as the support for training these guys. And we always had one or more of our senior geologists [like Lee Silver from Caltech] as the critiquer who followed them around on the traverse.

Bailey: "The Apollo 16 [final test] was up south of Las Vegas over by Henderson, Nevada [17-18 February 1972; see 1972 in main text]."

*Schaber: "Yes, we called that the Boulder City Test. You basically made sure that we had the logistics, the trucks, and the other things we needed to get there organized, and all that stuff."*

Bailey: "Someone else, of course, did the traverses and all that. It was the typical team effort."

*Schaber: "I believe that I did the traverse planning for that trip--and for two of the Hawaiian trips.*

*When did you actually leave Flagstaff?"*

Bailey: "I left in 1976."

*Schaber: "So what did you do from the end of Apollo [December 1972] and 1976?"*

Bailey: "Post-mission data work essentially. George [Ulrich] and I also published the transcripts pertaining to all of the lunar surface geologic activities for all of the Apollo missions. That was a six-volume set--just for a reference-type thing. In 1974, Dale [Jackson] and Gordon [Swann] picked up some NASA funding to do the same thing you are doing right now--to write up the history of astronaut geologic training. They sent letters out to everyone involved in those early days in the astronaut's geologic training program. Then, I believe it was me that popped up with the idea that Gordon Swann and Dale Jackson should get facts and remembrances from our own guys--people like Joel Watkins who no longer worked the Branch at that time. They sent in their personal notes, but everything was in total conflict [with regard to events, activities, and dates]. I suggested to Gordon that the one place we could get accurate records of the astronaut training trips would be at the astronaut office down in Houston; so, they send me down there to see what I could get.

Well, I got to the astronaut office, and they welcomed me with open arms. They took me and said here are our files Mr. Bailey. So, I spent a day down there. You know what I found? They had two complete sets of files. The big problem was when I compared file-1 with file-2; they were in total conflict too. So, their records didn't agree any more than our guy's memory.

[Author's Note: Unfortunately, Dale Jackson, who was to lead the Survey effort in writing of the history of astronaut geologic training, passed away in 1978; and the document/history never got beyond more than an expanded outline. Years later, in 1999-2000, William Phinney (MSC/NASA retired) was successful in obtaining a small amount of NASA funding to begin his own version of the astronaut training history—from the MSC point of view at least. At the time of this writing, Phinney's manuscript has been completed and submitted for review, and then publication, by NASA, to the Johnson Space Center in Houston. Bill Phinney's work has been referenced and quoted in this work with Bill Phinney's permission].

Bailey: "Then we got into writing the USGS Apollo Mission Professional Papers on the geology of each landing site. I did more work on the Apollo 17 professional paper than any of the earlier ones. Ed Wolfe was leading that effort, and he asked me to do the traverse geology part of it. So, I put all of that part of it together for him. Ed finally realized when he had six or seven of us writing up various aspects of the mission that they often didn't read well --when combined. So Ed sat down and laboriously rewrote it in his own words. Of course, he gave us credit for it in the authorship.

So there was work on various aspects of the Professional Paper. For example, [Bob] Sutton would come in and spend time with me on the lunar samples, where the samples were collected, photographed, etc. I would go and get on my computer in Flagstaff and list out sample number, exactly what they said, etc. for Bob. Sutton really straightened out a real mess they had with the sample documentation. Fortunately, Bob was very methodical, working carefully through the transcripts.

Another thing I did during the missions was audiotapes--starting from Apollo 12 on--what they [the astronauts] said. So, I had my own audiotapes, and this enabled me to take the NASA transcripts that were not corrected for say which astronaut was talking, and correct them.

*Schaber: "Yes, that's right; we did take audio and video tape of the mission EVAs. The video tapes were those early, 3 or 4-in wide, Sony tapes. Johnny Nuttall, head of our Electronics Group in Flagstaff, made those tapes for us during the missions, as I recall."*

Bailey: "Yeah, Johnny was doing that. And I took these audiotapes and corrected the NASA transcripts [of the audio from the lunar surface activities]. This is what the gals that we had working down there on our IBM-Selectric typewriter terminals were working on. By then, it was up to the NIH computer starting from Apollo probably 13 or 14. I know on AP 12 we were using the Stanford computer, but starting AP 13--which didn't land, and definitely AP 14--we were using the NIH computers.

After the astronauts took off from the Moon and were coming back, our secretaries who would input the data into our SEARCH program would come down [to Houston]. Lynda Sowers [later Lynda Ballissime] was the first one during the Apollo 12 mission--by herself."

*Schaber: "What did you do with the transcripts that our [court] stenographers did--the transcripts that they typed in near-real time at Mission Control during the Apollo EVA's? Were they of much value to us at all?"*

Bailey: "I finally gave those original transcripts to Don Thacker. [Keith Welch, his partner, later died in an airplane crash.] Don Thacker has the originals. This was on paper, and it was three or four sheets on a roll together, and one sheet got ripped off and taken into the Mission Controllers in the Mission Control room so they could see what they [the astronauts] had said. The way we worked that--of course neither Don nor Keith--as court stenographers--weren't up on the [geologic] terminology either. As soon as we tore off say five feet of their paper, I hastily ran them [down the road from MSC to one of our Geology team's apartments over at the Colonnades Motel on NASA Road One in Webster where our gals were located with their typewriters]. So, I was still listening and tape recording what's being said from the Moon in real time by the astronauts, while at the same time correcting Keith and Don's sheets on what the crew members had said five minutes earlier. So that is probably why I am still talking to myself even today!"

*Schaber: "Where did you go when you left Flagstaff in 1976?"*

Bailey: "I left in April 1976 and went to Woods Hole [MASS] to the Branch of Atlantic and Gulf of Mexico Mariner Geology where I worked for ten years. What I did was set up an oceanographic data library there, and used my computer capabilities and knowledge to set up a computerized inventory; every piece of marine data they collect. These guys were going out on ships practically every week and collecting refraction seismics, uni-booms, soni-bouys, and just tons of other data of all kinds.

[Author's Note: Red Bailey was invaluable to the Branch of Surface Planetary Exploration in the Pre-Apollo and Apollo Era. His kind demeanor, personality, prior field experience, and ability to

get things done without question, are exceptional. Red, like many of us, was strongly driven by a love of what he was doing during the hectic, but wonderful, days of Apollo. Red typified the can-do attitude that was essential for success. I am proud to have worked with Red, and to still call him a good friend.]

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**Lynda (Sowers) Ballisime** (Clerk Typist and Computer specialist), for many years one of the key players in the Survey's image processing group at Flagstaff, began work with the Branch of Astrogeology on 11 September 1967. She started working with Gordon Swann and Tim Hait on the 5<sup>th</sup> floor of the old Arizona Bank Building in downtown Flagstaff. Lynda shortly thereafter started working with George Ulrich and Red Bailey on the Branch's WLYBUR and SEARCH computer program in preparation for Apollo missions. Between 1967 and 1969, Lynda [from the ADF on Fourth Street] also practiced inputting the verbal input from the astronaut's EVAs in the field during the SPE Branch's pre-Apollo field tests in Hopi Buttes and other local test sites. In November 1969, Lynda participated with the Survey's Apollo Lunar Geology Experiment Team in Houston during Apollo 12, inputting astronaut verbal information from the lunar surface into the Branch's SEARCH program. Lynda transferred to the Survey's EROS Data Center in Sioux Fall, South Dakota in January 1972, but returned to Flagstaff soon after Apollo 17 to work for Larry Soderblom (originally in Astrogeology, and then the Computer Division in Flagstaff). She managed image and data processing for various planetary missions, including Viking, Voyager, etc. Lynda retired in 1996 and moved to Cottonwood, Arizona, and then Las Vegas, Nevada, where she still resides.

The following was abstracted from an interview with Lynda Sowers by Gerald G. Schaber on 24 April 2001:

Ballisime: "I was born in Gallup, New Mexico, and graduated from high school there. I lived there most of her life, and moved to Flagstaff in 1966 when my husband (Bill Sowers) was going to school at NAU. I got a job at Flagstaff High School for that first year. The typing and shorthand teacher told me that I was wasting my time here; she said you should be looking into the Government. So, I took the shorthand test and typing test, and went and took the Government [Civil Service] test. I was offered four different jobs--one at NAD (Navajo Army Depot, west of Flagstaff), one at the Forest Service; one at Wapatki National Monument, and one at the USGS. I thought NAD and Wapatki were too far to travel and that the Forest Service would not be exciting I guess--so I went down to the Branch of Astrogeology at the Arizona Bank Building downtown.

I interviewed with one of the administrative personnel and was hired. I started working with Gordon Swann and Tim Hait. I started on the fifth floor of the Bank Building. I was up there with Dorothy Kubach [hired in June 1967]. Anyway, we were in the front of the Astrogeology office on the fifth floor. Al Chidester was the Branch Chief and Doris Ferren was his secretary [hired by Astrogeology in September 1965]. Then I got involved in work with George Ulrich and Red Bailey. I worked on that computer program [WLYBUR and later--the SEARCH computer program in preparation for Apollo, and later during the Apollo missions, the WYLBUR program.].

Jack Fife [Author's Note: computer programmer for Astrogeology; first hired by Astrogeology in May 1967; then transferred to the Flagstaff Field Center's Computer Facility in August 1967; then returned transferred back to the Branch of Astrogeology in November 1968] was working with

Red Bailey and Project Chief Dave Dahlem on that. Dave Dahlem was the one who first got me interested in computers because he was writing computer programs, and I would go back and forth to [the smaller of the Fourth Street buildings] Astrogeology's Apollo Data Facility (ADF), and run his programs over there; and then physically bring them back to the bank Building. Back then I think we still had the PDP 11/44 computer or something. I asked geologist Dave Dahlem how I might get into computer programming. He said you would have to take some classes. So I started taking computer classes and some science classes, and started working on Apollo."

[Author's Note: Between 1967 and 1969, Lynda [from the ADF on Fourth Street] also practiced inputting the verbal input from the astronaut's EVAs in the field during the SPE Branch's pre-Apollo field tests in Hopi Buttes and other local test sites.]

Ballisime: "The first Apollo mission I went on was Apollo 12 (to Houston MSC). I was supposed to go down for Apollo 13 but the night before I was going to leave, it blew up [Apollo 13 had an explosion onboard the Service Module and had to circle the Moon without landing, barely making it back safely].

During Apollo 12, Darline E. Johnson (another Astrogeology secretary) and I were inputting data from the astronaut's verbal transcript [from the lunar surface] into the computer to be sorted into geologic context and categories by the SEARCH program that Jack Fife, Dave Dahlem, and Red Bailey at Astrogeology had developed." [The SEARCH program was a pioneering precursor to modern "relational" database programs that would soon become routine.]

[Author's Note: The author then recalls to Lynda that that the people at the Mission Control Center at MSC (Houston) actually let us use some of their mission backup computers [IBM 360/75s] there in the Mission Control Room for their computer work [that is, the inputting of the astronauts' commentary into the Astrogeology computer program. It was amazing because that was a very highly secured area; see main text and Norman (Red) Bailey above for more details].

Ballisime: "We were set up in the Colonnades Apartments [just down NASA Road One from MSC in Webster, Texas. I just sat there and typed everything that the astronauts said [from the lunar surface]. Red Bailey was also part of this activity."

[Author's Note: Lynda and the others would type from the transcripts of Keith Welch and Don Thacker, the Flagstaff court stenographers and typist whom Astrogeology had brought down to MSC to support the Lunar Geology Team starting with Apollo 12. Keith Welch was an amazingly fast typist and could type 150 words per minute at his top speed! Dave Schleicher was in charge of the court stenographer activity for Apollo 11 through Apollo 12. Then, Red Bailey was given both Dave Dahlem and Dave Schleicher's chores (after Dahlem and Schleicher left) for Apollo missions 14 through 17.]

Ballisime: "It seems like when Keith was typing it--as soon as they got to a certain point, they would bring it down to the apartment, and then I would type it in [into the WYLBUR program]-and they would go back and get another section. So, it was being put in the computer as fast as I could. I remember Dave Dahlem putting in key words, and then they'd have me go back and underline something.

Apollo 15 was the mission that we really got the entire transcript [inputted]. That's when things really started going well. I got a full transcript for them. [Author's Note: Red Bailey says that by Apollo 15, we were using NASA-generated transcripts, and we corrected the mistakes using audio tapes made in real-time during the EVAs—off the TV screens].

Before Apollo, I worked for Gordon Swann. I was Gordon Swann's secretary. Jody went to Albuquerque I guess, and then she came back. So, we were both working on the fifth floor of the Arizona Bank Building."

[Author's Note: Jody Swan left for New Mexico before Apollo 11 in March 1969 but came back on duty with the Branch of Astrogeology while still located in Albuquerque in October 1971.]

The following was abstracted (but quoted) from an interview with Lynda Sowers Ballisime by Gerald G. Schaber on 24 April 2001:

Then, the Survey's EROS Data Center (Sioux Falls, North Dakota) was opening up--and Lynda's husband Bill had just graduated from college. So, her family moved up there in January 1972. While she was working in Sioux Falls, her boss, Gary Selner let her go back to Houston to work during the Apollo 16 mission with the Branch of Astrogeology's Apollo Lunar Geology Team. Her boss would not let her go for Apollo 17 for whatever reason.

Lynda moved back to Flagstaff right after Apollo 17 at the request of Larry Soderblom--and because it was "too dang cold" [in Sioux Falls]--according to Lynda.

Upon her return to Flagstaff, Lynda first worked for Larry Soderblom. Subsequently, there was an opening in the Survey's Computer Division [in Flagstaff], and she transferred over and worked for Jim Crawford [head of the Computer Division operation in Flagstaff at that time]. She worked at Fourth Street for maybe a year or so after she came back. Lynda did work on the Viking-Mars data in the summer of 1976. There was a whole group of people who, along with Lynda, took 12-hour shifts so they wouldn't miss any of the computer tapes that came in nightly on the train from JPL.

Lynda subsequently worked on imaging processing of the Voyager data, acting as sort of the coordinator for the image processing flow. In 1996, Lynda retired and moved to Cottonwood, Arizona for a couple of years. Then, when her daughter Lisa and her family moved to Las Vegas, Nevada, she decided to move to Vegas as well--to be closer to the grandchildren. Then, of course, her daughter Lisa moved to Denver! Lynda Ballisime still lives in Las Vegas.

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**Fern Beeson** (Administrative Officer) started work with the Branch of Astrogeologic Studies in Flagstaff on 12 July 1964 as a GS-4. In 1966 she went to the Administrative Office at the Flagstaff Field Center as an Accounts Maintenance Clerk. In 1968 Fern was promoted to a Budget and Accounting Clerk. In 1972 she was promoted to a Physical Assistant, and in 1979 she was promoted to a Fund Management Analyst. In 1981 she was reassigned as an Administrative Assistant, and then soon thereafter promoted to the Administrative Officer at the Flagstaff Field

Center. Fern retired—as Administrative Officer for the Flagstaff Field Center in March 1985. She now resides in Fischer, Texas.

The following quotations were taken from an interview with Fern Beeson by Gerald G. Schaber on 13 August 2001:

“I came from Chillicothe, Missouri, and in 1941 moved to New Mexico and worked for the Santa Fe Railroad for a while. Then, in 1954 through 1958 I worked for the Navajo Army Depot in Flagstaff. I was hired as a Clerk-Typist, and when I left there in 1958 I was an Appointment Clerk.

In 1959 through 1963 I worked for the U.S. Atomic Energy Commission out of Grand Junction, Colorado, but the Branch office was in Grants, New Mexico—and that’s where I worked. I was a Secretary. We calculated uranium ore reserves in the mines there and monitored the processing mills there.

In 1963 and 1964 I worked for the U.S. Bureau of Reclamation. Phoenix was the Headquarters, but I worked at the Field Office in Flagstaff as a Secretary to the Chief. I was so bored with that job at the Bureau of Reclamation that I decided to go and check with the Branch of Astrogeologic Studies Office there in Flagstaff to find out what happened to my application, because I had turned it in months before then.

One thing was kind of funny. When they called me to come in for an interview at the Museum of Northern Arizona—because that is where the Branch of Astrogeology office was then. Gene Shoemaker was in his usual hurry, and told me he had to go downtown to another Astro office, the “Dance Hall” or ANNEX building on old Santa Fe Avenue [now Highway 66 [see December 1963 in main text for more details on the Dance Hall.]. Gene told me to get in the car with he and Don Elston and they would interview me on the way down and back. So they asked me a few questions going down and coming back, and Gene asked me why I wanted to leave my present employment. And when I expressed a desire for a position where I would be kept busy--and not be bored—Gene’s answer was a laugh. He said I would never be bored again!

I started at Astrogeology on 12 July 1964. My first assignment was a strange one. They handed me some processing papers; it turns out that they wanted me to do “my own” employment papers! The second job given me was Gene Shoemaker’s travel for months. His travel file was made up of notes, scraps of paper; well, you can guess what it was like.

By the end of my first day, I was a wreck, and ready to quit. Anyway, it took me a few days, of course, to get Gene’s travel voucher done, because there were so many other things that needed to be done. The Astrogeology files in the Museum of Northern Arizona were in boxes, stacked everywhere—with no rhyme or reason.

I shared an office with four people. There was myself, Don Elston, Jack Schmitt, and one other I can’t recall. About the second week I was there, Don Elston walked in and told me to get my things together because he was going downtown to find office space to rent for part of us [there at the MNA]. He came back and he said that he found offices in the old building at 119 E. Aspen

Street—"the Burris Building" across the street behind the Arizona Bank Building (now the Bank of America Building).

So, we took everything we could find that belonged to the one's who were moving—and moved down there the Burris Building on Aspen.. Of course there was no elevator, and we were on the 3<sup>rd</sup> floor.

And when Don went back to MNA to tell Wanda Wilkerson [Administrative Officer at that time] that he had gotten more space, I can remember Wanda saying, you didn't even request it—there is no approval! That is the way we did things in those days. Don told Wanda to go ahead and do your thing—and I'm moving! And, of course, we didn't have any telephones—hardly anything you know. From The Burris Building, we soon moved into the nearby Arizona Bank Building—and then things really got started.

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**Ramona L. Boudreau** (photographer), another talented mainstay in the Branch of Astrogeology's photolab for many years, joined the Branch of Astrogeology in Flagstaff on 15 March 1965. Ramona, whose parents were both French Canadian, grew up in Vermont in a little farm town of 1,500 people. She was preceded in the Branch photolab by photographers Jim McCord (her old boss in Tucson) and Karl Zeller. All three individuals became incredibly hardworking and dedicated professionals, turning out a truly amazing number of high quality lunar and planetary photographic products for decades to come.

The following quotations were taken from an interview of Ramona L. Boudreau by Gerald G. Schaber on 19 March 2001).

Boudreau: "After graduating from high school in 1946 I enlisted in the service and spent four years in the U.S. Army Radio Communications Division during Korean War working at the Pentagon. After I got out of the service I and a friend went to New York, sat in the middle of Times Square and flipped a coin as to where we would go next. So I went west. I went to Davis-Monthan Air Force Base where I had heard they were hiring people for the Titan Missile sites all over the west. So I put my name in there, and a year later I started working on those missile sites as a photographer. They were in North Dakota, South Dakota, Wyoming, Denver, and Washington State. Actually, it was taking pictures during the missile site construction. Then, when the missile site business was over, we all were returned to Tucson. We all said this is it--no more. This was in 1958.

I then happened to get a call from Jim McCord [later to join Astrogeology in Flagstaff as a photographer]. He said I hear you are looking for employment in the photography business. I said, yes I am. I don't have a job now, but I was hoping to get one. So I worked in his Photographic Industries business in Tucson. He did a lot of construction photography, building of skyscrapers and everything like that. I stayed with him from 1958 until 1964, and he decided that he wanted to sell the business. So he did. I found another job taking photos and portraits of women's bridal dresses.



Then I got a phone call in September 1964. It was Jim McCord from Flagstaff. I said in God's name, what are you doing up there? He said, well, I came up to look around. His mother also lived there, so it was a convenient way to get up--to see his mom. He said Gene Shoemaker was getting together this so-called Astrogeology Branch and that they needed seven photographers. I'm the only one right now.

Well, I went up for an interview in January 1965, and Gene [Shoemaker] was the one who interviewed me. There was nobody at all in the whole building--Building-One [only recently completed] on the Mesa. Nobody was there; nothing but bare walls and bare floors. I went up in March and it was cold--God it was cold! I stayed with Jim and his mom. I talked to Gene Shoemaker thinking he was the janitor, and almost insulted him. So anyhow, I started to work up there in March 1965, and that's how I started to work in Flagstaff. Of course, in the photolab at that time there was only Karl Zeller, Jim McCord and I.

The first thing I did was Ranger--the lunar impact spacecraft missions--was going on, and we were making all of these tiny postage stamp-sized prints. Karl said I'm just about to ditch this job with all of this stuff. We pasted them down into mosaics. This is how we got the first actual spacecraft photographs of the different areas of the Moon. We did that for a year to two years. Of course, it was very boring! So when Ranger was over, there was Surveyor. That was a little more exciting because you had the lunar surface pictures [for the first time].

With regard to Ray Batson's simulated Surveyor camera out on the Bonita Lava flow at Sunset crater [starting in the summer of 1964]; we were there as extra bodies in case they needed us for anything. We were also assigned to go into the higher areas of the Bonita Flow and take photographs looking down on the training itself. Anyhow, we eventually got our seven photographers."

[Author's Note: During my interview with Ramona Boudreau (retired Branch photographer) on 19 March 2001, she recalled the following information about Walt Roeder who was Chief of the Branch of Astrogeology's Film Documentation Unit in Flagstaff during the Apollo Era]:

Boudreau: "Walt Roeder had been working down in Tucson with Jim McCord for his Photographic Industries business, and then he left. He went I believe to Wilcox, Arizona to do ranch-type photography, and making documentaries for industries. He was invited to come to Flagstaff because Jim had known him. He started to work in May 1965. So that's how the film Documentation Unit got started.

Walt was born in Germany (birth date unknown). He did photography over there, but he left Germany just before WW II. His mother said if you want to leave--you should. He said why? She said because your brother is in the Nazi Party, and he had just as soon put you in the Nazi concentration camp as anybody. So Walt went north to Finland, took a freighter over to the U.S., landing in New York. He worked in New York for a while, and (I believe) did a lot of men's fashion photography."

[Author's Note: Ramona Boudreau is now retired and still resides in Flagstaff.]

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**Joseph Michael Boyce** (b. 1946; geologist; M.S, 1972, Northern Arizona University), who was born in Mesa, Arizona, joined the Branch of Astrogeology as a Physical Science Tech (PST) in February 1969. Joe started out working in the Branch's Lunar and Planetary Image Data Facility that was then set up on the Fifth floor of the Arizona Bank Building in downtown Flagstaff. Boyce was soon assisting several of the more senior geologists in the Branch, such as Dave Roddy, Bill Colton, Dick Eggleton, Jack McCauley, Larry Soderblom, Ivo Lucchitta, and others. Joe participated in the geologic mapping of--and the geologic training of the Apollo 14 astronauts on--Astrogeology's Black Canyon Crater Field in Verde Valley, Arizona in 1971. Working with a technique that Larry Soderblom had originally developed that used crater shadow lengths and photometric properties to relatively date surfaces on the Moon (the so-called DL technique), Joe extended this research and, in collaboration with Larry, eventually contributed several widely referenced and important publications on this subject. He would later work on the Mariner 9 and Viking-Mars missions at JPL in Pasadena.

[Author's Note: In the summer of 1976 Boyce replaced Russ Wahmann (Astrogeology-Flagstaff) as Steve Dwornik's USGS Staff Scientist at NASA Headquarters for two years. Then in 1979, just as Joe was starting to plan to come back to Flagstaff, Steve Dwornik took early retirement and asked Joe to take his place as Discipline Chief for the Planetary Geology Program (later renamed the Planetary Geology/Geophysics Program) at NASA Headquarters. From 1983 through 1992 Boyce stayed on at NASA Headquarters and became Chief of Planetary Geoscience which included the Cosmochemistry Program, Geology/Geophysics Program. In 1992 Boyce took over NASA's Outreach Program at NASA Headquarters, and became its director. In 1995 Joe became the Mars Program Scientist at NASA headquarters. In 1997, he became manager of NASA's Cosmo-chemistry and Origins of the Solar System program. Joe retired from NASA in August 2002 and became a member of the research faculty at the University of Hawaii.

The following was taken from an interview with Joseph M. Boyce by Gerald G. Schaber on 13 February 2002:

Boyce: "I was born and raised in the Phoenix area, in Mesa, Arizona in July 30, 1946. I grew up basically in North Phoenix and went to North Phoenix High School, and then went to Arizona State College (ASC) [now Northern Arizona University] in Flagstaff. That was in 1964. Towards the end of my tenure there I had always heard about the Branch of Astrogeology.

Actually, my first encounter with Astrogeology was through *Arizona Highways* Magazine. They came by the College and were looking to take pictures of Flagstaff, the College, the Museum and some of the activities that Astrogeology was doing-

They decided to tie in some of the ASC students with what they were doing. So, it was me and a couple other students. They said well do you guys just want to come and stand around? So we went up and pictures of us taken up by the skeleton of the giant sloth up at the Museum of Northern Arizona and at NAU, and then we went over to where Astrogeology had that big Mobile Geologic Laboratory [MGL or MOLAB]. That was over at the Survey's building they rented over on Huntington Road in Flagstaff [now occupied by Border Products, Inc.)

Well, for a few years after that, that's about all I hear about it in the news was what Astrogeology was doing up there. Of course, I was a geology major and I was interested in things that were going on up at Astrogeology-- but it seemed to be so distant for me. I did know a few classmates who were part-time (WAE) field assistants and Physical Science Techs at the Survey. It was very clear that that was real plum assignment--if one could ever get hired up there--then you were really doing great.

It turned out that just as I was graduating in January 1969 I had a friend, Louie Middlestorb who worked up there in the Data Center [at Astrogeology]. I actually applied with Astrogeology in 1968, but there wasn't anything open; I was told to perhaps come back when we have some other positions. Now Louie was telling me that there were going to need somebody and perhaps he could get me an "in."

So I applied and went up and talked to Jo Ann Higley in the Lunar and Planetary Data Center [then on the 5<sup>th</sup> Floor of the Arizona Bank Building]. It turns out that they hired me to basically help in the Data Center. That was February of 1969. That's when I started to meet people like Hal Masursky, Sherman Wu, and others.

I started pitching it to people as they came in that perhaps I could perhaps assist them in some of their geologic projects while I am holding down the fort at the data Center. About that time [August 1970], Jo Ann Higley left, and that left me sort of running the Data Center [in Building One on McMillan Mesa].

So I started asking people like Dave Roddy and Dick Eggleton if they needed help. Well, Dick Eggleton asked me to do a couple of things, and of course Masursky mostly wanted me to do graphing. However, Roddy actually started using me for real science. I did some field work with him and worked my way into doing more advanced geological geological activities. That would have been about 1969-1970.

In June of 1969 I realized that I had to do my thesis fieldwork [for his Masters at NAU]. That was at the bottom of the Grand Canyon; so I resigned and Astrogeology hired me back in October or November. So there was a little break in service there for several months.

About the same time, around the early 1970s, I also met Larry Soderblom [Soderblom had been hired WAE while he was a post-doc at Caltech in August 1970.]

When I returned to Astrogeology, I actually got to do more scientific research. Dave Roddy started his drilling program at Meteor Crater around 1971--he and Bill Colton--so I worked on that.

Jack McCauley was quick to jump at any opportunity to let anyone help him that wanted to help. So things went along and there were actually a lot of people that I did have the opportunity to work with. So they started funding little pieces of my time—McCauley, Roddy, and Masursky.

When Larry Soderblom got to Flagstaff [in October 1970], we were already friends because I had met him through some mutual Caltech friends. I really started working a lot with him. In fact, in the Apollo 16 *Preliminary Science Reports*, I co-authored a paper on the Orbital part of the

mission--with Soderblom. I was the junior author. On the Apollo 17 paper I was the senior author."

*Schaber: "Was that paper on the DL relative age measurement of lunar features?"*

Boyce: "Yes, it was on the relative ages [on the Moon]. It was a technique that he [Larry] had developed. Soderblom is so smart that he developed these things, and then he would take off and do something else and let someone else--like me--carry it forward.

Occasional, I would work with Ivo Lucchitta, or would be borrowed by various other researchers at the Branch. For example, the Apollo 14 crew was being trained [on 16 November] in 1970 in the Branch's Black Canyon Crater Field in the Verde Valley south of Flagstaff. I helped with that, including doing some geologic mapping in that area. John P. "Phil" Shafer [who arrived at Astrogeology in November 1969] and I went out and mapped the ray patterns [from the explosion craters]. The nice thing about the mapping was that it led to me being down there when they were actually carrying out the astronaut training exercises.

The only other astronaut training exercise that I was on was one at Meteor Crater with the Apollo 16 prime and backup crew on 25-26 February 1971 [see 1971 in main text for more details of this field test]. It was cancelled partway through it because the wind was blowing. I remember that the weather report in Flagstaff that day indicated that the wind gusts were up to 75 miles per hour. I remember standing there in the silica pits [on the rim of the crater]. You can imagine what the silica dust was doing. They [the astronaut crews] were trying to navigate through the rim ejecta. Finally, it was so miserable that they decided to say that we had a mechanical problem and had to go back to the LM [Lunar Module].

I've got to admit that I was in kind of an interesting, fun, position being that young and not being a professional scientist in a sense at that time. But it gave me an opportunity to work with a lot of different people and do a lot of different things."

After this Apollo 14 training test [in Astrogeology's Verde Valley Crater Field on 16 November 1970] Henry Holt also took me down to the Cape in Florida. In fact, James W. Langman, Jr., who is an old friend of mine [and who arrived at the Branch of Astrogeology in June 1969], drove the big instrument truck with all of the big light cannons and all of the photometric calibration stuff down to Florida. That was just before Apollo 14.

Oh yeah, this was also my chance to "almost" give the Apollo 14 astronauts colds--so they couldn't to the Moon! On the way down to the Cape I picked up a raging head cold. When we got down there--as you will remember-- with schedules you never had time to sort of recuperate--you just sort of forged ahead. It was also close enough to the time to launch that they were in quarantine up in their living quarters at the Cape. We were at the bottom of that building trying to do calibrations in a clean room, and we needed extension cords. So Holt found out that there were some in a certain place up on the top floor. It was also the place where the astronauts were. He said go get them. I said, well I've got a cold in case I run into these guys. Henry said no, you'll never run into them."

Boyce: "So I'm up there digging around in one of the rooms getting the extension cords, and in the next room all of a sudden I see all three of the astronauts with some of their buddies doing something. I am thinking that I should get even farther away from them in the next room because I didn't want these guys seeing me. They met me at the Verde Valley Crater Field EVA, so they might remember me--and then they would want to come over and shake hands--and remember that is the worst thing you can do with a cold. Then, what do you think? I mean they followed me into that next room.

Then of course it was hey, how are you doing? I was thinking go away, go away."

*Schaber: "Al Shepard would have disemboweled you if he knew you had a cold."*

Boyce: "I'm thinking we don't want to shake hands you know. So anyway, I tried to make my way out of there as quickly after saying hi without shaking hands. So that was my brush with almost destroying someone getting to go to the Moon!"

*Schaber: "So you didn't actually get to go to any of the Apollo mission operations at Houston."*

Boyce: "No, I didn't. The only time I went to MSC [now JSC] was with Henry Holt to calibrate one of the gnomons. I may have actually gone down there twice. There were some funny things that happened down there too. Remember the mathematician Rae Schaul [joined the Branch of Astrogeology in June 1970]?"

*Schaber: "Yes."*

Boyce: "Well Rae was one of Henry Holt's people for a while. Rae of course was beautiful. Hank had brought her down to MSC at the same time that I was down there when we were calibrating the Apollo surface cameras. Henry decided one night that he wanted us to go to Ellington AFB to the officers club--him and some of his old buddies from MSC. They had a bunch of other guys that went over there and drank. And of course, all of Hank's friends wanted to dance with Rae, but she didn't want to dance with any of them. So I was in the wonderful position of having Rae ask me every dance if I would dance with her. She is really a nice person." [Author's Note: Sadly, Rae Schaul Harvey, who after Apollo became a well-loved math teacher at a local high school in Flagstaff, passed away in 2004 from complications of breast cancer.]

Boyce: "You have to remember also, going back to 1971-1972, that Mariner 9 was going on. Larry was very instrumental in getting me to work on Mariner 9. Mike Carr had quite a bit to do with it too--and Jack McCauley. That was tremendous experience because what I eventually ended up doing was going over to JPL in November 1971. It encountered one raging dust storm, and it took us a few months before we could start seeing the ground. We didn't fully realize until Viking that clear through the end of the Mariner 9 mission we were looking through kind of a hazy atmosphere [with Mariner 9]. But the exciting thing was that this was really the first orbiter of another planet!

Anyway, I got what I thought was absolutely a plum job. Every other week I was on the targeting team. Danny Milton, Mike Carr, George McGill, and I shared a two-bedroom, two-bath, place on Hudson Street in Pasadena. We would just take shift crews at JPL.

So Danny and I, being on the targeting team, would get up early in the morning and go down--and the Mariner 9 pass from the night before would have been processed. We would call it up on these big film viewers and target B-frames--the high-resolution [Mariner 9] frames. So the absolutely great thing about this--for a kid basically--that this was the first time that anyone had ever seen Mars at high resolution. Nobody else had seen these pictures because they were basically done automatically. So Danny and I thought, if something is going to show up that is really spectacular--we are going to be the first to see it! That was really cool. So I did that for a number of months before I went off to Colorado.”

*Schaber: "So you went back to school at the University of Colorado in Boulder, right."*

Boyce: "Yes, that was the fall of 1972. I got my masters [from NAU] in May of 1972. So I went up [to Boulder, but still maintained my employment with the USGS. There were two persons who continued to pay me [from Astrogeology]. That was Soderblom and Roddy. I should also bring Henry Holt into this also. Henry was doing photometry, photometric calibration of things [like the Apollo surface cameras].

I probably didn't endear myself with the faculty at UC who were really not space buffs anyway. Periodically--because I was still working on the Apollo stuff--I would fly down to Flagstaff. And you can imagine people that basically think that grad students are slaves did not appreciate a grad student being flown somewhere to work as a professional scientist. I thought this was a thing of pride and it turned out--you don't know your place--do you? And they showed me where that was later by kicking me out!

Anyway, I was there [at the University of Colorado] until January of 1974, then came back to Flagstaff and started working for Soderblom---and a little bit for Masursky. But I must tell you, it wasn't clear that I really had a job in Flagstaff. I kind of just came back out of desperation--thanks to the kindness of Dave Roddy and Larry Soderblom.

Even though I had a bad time in Colorado, I thought I needed to get moving along and do something. It was like March of 1974 when I went to my first Lunar and Planetary Conference. It was the first real paper that I gave; it was on the Soderblom-Boyce DL surface age dating technique. I senior authored a paper for that conference. It was my first real, peer-reviewed, outside, full-blown paper you know--for the Proceedings of the Conference. That was good.

Then, right after that I started writing proposals to NASA; because I knew the best way to stay in Astrogeology is to have your own money. So I started doing that. Larry helped me and Dave Roddy helped me write these things, and I actually started winning approval for these proposals.

While I was in school I had been working on Roddy and Soderblom's projects. I thought well if they don't have much in the way of extra money for me, I will just get in here at Flagstaff and dig out a spot for myself--I'll have to."

*Schaber: "What about Masursky--did he help you?"*

Boyce: "Well, Masursky was always willing to. I actually started doing some things for Hal too. The problem with Hal is that he was not willing to put anything into the pot to pay your salary, but he was always willing to have you work for him. Art and I did some Viking Rover studies for him, and one of them was published in 1974."

*Schaber: "Then you, Art Dial and I somehow got into Viking thanks to Hal [Masursky]."*

Boyce: "Yeah, I don't know how you got involved, but Art and I and some of the others were working the backroom at USGS in Flagstaff to backup Hal because they already had this team out there that was designated, and Hal wanted his own team that he could rely on. So we would do crater counts and all sorts of stuff."

*Schaber: "Of course I was already a member of the radar team [for Viking Site Selection] also because I had a background by then in radar geology. I was working with Len Tyler, Dick Simpson from Stanford, and Masursky looking at those radar spectra of the surface trying to figure out what it meant. And they [the radar guys] are the ones who really identified the true small-scale roughness of the landing sites.*

*Remember, Art Dial was doing the crater counts, and you were plotting them up on that little TI-51 handheld computer with a small paper printer. Then I would present [to the Site Selection Committee] the results for the different candidate sites, with regard to the crater distribution."*

Boyce: "Yes, that took place after Hal invited us to join the Site Selection and Certification Team over at JPL. But I do remember what finally got us over there was the fact that the teams who were there--the original designated teams--were starting to wear down--so they left for the most part; and that's when Hal said why don't you come over [to JPL]. That's when the good stuff happened. Those guys left at the wrong time--much to our good luck actually.

*Schaber: "And then there is the story of how Jim Martin [mission director] changed the landing site to the one you and I picked."*

*We had preferred a site. Well, NASA HQ called and said you guys on the Site Certification Team have got to land this damn thing because it costs us a fortune each day of delay. We want you to meet every four hours 24 hours a day and vote until the Site Selection Committee has a "unanimous" vote on a site. Yet, there was this conflict between the radar team, who liked this one site that had larger craters on it, but they said it didn't have the degree of small-scale roughness than did our preferred lava flow site on the lava plains. That probably made sense.*

*We were obviously biased by our earlier work counting craters on the Moon."*

Boyce: "Well you are right; the bias was simple in retrospect. There is a process on the moon that breaks up small rocks--it is micro-cratering-. So rocks don't stay long on the Moon because you have this full range of small debris that comes in and breaks all of that stuff up--and it doesn't

happen on Mars. You may have a few little things coming in but you sure don't have the same numbers. In the younger areas, like the ones we were looking at, it was not good. In the older areas the regolith sort of matures a little bit."

*Schaber: "So we ended up voting for a landing site for Viking Lander 1. The one you and I preferred didn't make it, but a second lava plains site did because it was slightly better from the radar spectra point of view[with regard to small-scale roughness.] So we all finally left the SFOF out there at JPL and came back the next morning; and damn if Jim Martin hadn't changed the site. Martin came up to us and said I hope you guys are right. We said why. He said because I changed the vote to the site that you guys preferred. I remember my heart sunk!"*

Boyce: "I remember that well. We had gone through this day after day period initially of having that four o'clock meeting [to vote for a landing site]. When it started to wear on even longer [without a unanimous vote] and they were spending a million dollars a day-and he was getting a lot of heat, and he wanted to get that spacecraft down. That is when he started having the meeting every four hours! I remember the vote. In fact we were in the orbiter room and we had been doing geologic mapping--you and I and John Guest. Remember one day we were mapping from three different corners of the map towards the center--it was in Chryse Basin. We were all standing around the map as I remember and Masursky came in with Jim Martin, Carl Sagan and Mike Carr. I remember Masursky saying, tell us about this. So we briefed them on that area, and I think that is what swung old Jim Martin. Martin was swayed by our argument."

*Schaber: "I wonder if Masursky had something to do with that."*

Boyce: "Oh, I am sure he did. I am sure that he argued for it."

*Schaber: "But when we saw the site--when the first pictures came down from the surface--I almost wet my pants!"*

Boyce: "Yeah but it was the second landing site that I really laughed about. I guess we weren't there--it was Carr telling me about this. Martin came over the loudspeaker, because Carr had been fighting everyone about how this site was going to have sand dunes on it, etc. And here it was rougher than hell--even more than the first site. Martin says Mike Carr well in the hell are the sand dunes? He was just really yelling at him [over the intercom]."

*Schaber: "That's right; there were more sand dunes at the first site than the second site. You know Joe, those were the luckiest two days of our lives, I will tell you that right now!"*

Boyce: "Oh we could have been the big goat--Oh yeah. We could have looked like total morons. But instead we came off looking pretty good because they landed safely."

*Schaber: "So after we lucked into saving Viking, what did you do?"*

Boyce: "After Viking I continued to work on Viking things. In fact you remember the channel paper that basically you wrote most of for Hal Masursky. I wrote the chronology part, but you wrote everything else. And that turned out to be a Benchmark in Mars Geology papers that next



year. I don't know if you knew that or not. They sent me some notice--there is a publication called Benchmarks in Geology, and that was one of those papers."

*Schaber: "I'll be darned, I did not know that!"*

Boyce: "So, it was one of those things--like I said--a Benchmark, it laid down the channel story on Mars. In fact, when you go back and read what people have written since that original Mars channel paper was published, they haven't improved on it a lot--which makes me smile a little bit. So you did a good job.

Well, I continued to work research and helped Soderblom a little bit on the Voyager things he was doing because they were trying to decide on things like camera focal length and all of that. Larry was right in the middle of that fight--and really doing the right thing. He was trying to prove to them that you really wanted to put longer focal length lenses on there--even though they were heavier--because it gave you so much longer to look at things at high resolution in order to let things turn under you and all of that. It made a big difference in the coverage.

In the fall of 1976 I also began to realize that in order for me to best survive --even though I was doing quite well in terms of peer review and bringing in two to three times as much money as I actually was spending myself-- I might want to understand the system at NASA Headquarters. So, I volunteered to replace Russ Wahmann here at NASA HQ as the USGS staff scientist, which Steve Dwornik normally had. He liked to bring somebody from the USGS--and the USGS liked to give them somebody. You know, NASA got away from that agreement, which I think was horribly shortsighted.

Russ's tenure at NASA HQ ended in the summer of 1977, so that's when I was set up to come back to NASA headquarters in D.C. So I came back to D.C. to be staff scientist for Steve Dwornik for two years in the Planetary Geology Program--which also of course supported the cartography [that was being done almost exclusively at Flagstaff.]

Then, as I was starting to plan to come back to Flagstaff in 1979, Dwornik sat down with me and said you know I have an opportunity to retire early and go off and enjoy life a little bit more. Would you be interested in staying on and taking my job? He said I would recommend you if you want. Tom Young had also been our Division Director, and then it became Gus Taffero (sp?)--two Viking guys. So I thought this doesn't sound so bad--old home week-- and these were the good guys, and Jeff Briggs was the Deputy. He was a Viking guy too. So I applied. Steve Saunders applied, and so did Jack Sevier (from MSC). I knew that I had better apply, and try to get this job because both Jack Sevier and Steve Saunders during those two years I was staff scientist, had said in meetings that if they had the opportunity they would "shut down Flagstaff"-- as they saw no reason for it to be there!

I wasn't surprised at Steve, but I was surprised at Jack Sevier because he was Gordon's [Swann] good buddy. But he didn't like Flagstaff. He might have liked Gordon--but he didn't like Flagstaff.

Things went on down the stream and I kind of became the Chief of Planetary Geology--and then things were reorganized and I took all of Geoscience. That took place in 1983. After that I did all sorts of things.

*Schaber: "Then after that you kind of got out of the geology into the Mars program or something."*

Boyce: "Well, in 1995 Briggs decided that there should really be a lead scientist for the Mars program--so I became the Mars Program Scientist at NASA Headquarters [Author's Note: the job that Jim Garvin had before his recent assignment as Chief Scientist at NASA Headquarters.]

I've gone through a lot of different iterations of titles. From 1983 through 1992 I stayed on as Chief of Planetary Geoscience, which was the Cosmochemistry Program, the Geology/Geophysics Program, and the Mars Data Analysis Program, and a few other little programs including Lunar Data Analysis and all of that. I had responsibility over all of it. I had several people working for me like Ted Maxwell, and Don Bogart--so I had a number of managers working for me.

But by 1992 I had had enough. I finally sat down with Wes Huntress and said well, I'm going to JPL--I've had it. And Wes--bless his heart--stopped me and said what is it that you want to do? I said, well I don't want to do this anymore! He said how would you like to do NASA outreach? You do a lot of outreach stuff. And I said well, that sounds good. He said you can be the Director of Special Programs. So my official title was Director of Special Programs. That was in 1992 or 1993, and extended until 1997.

In 1997 Henry Brenton came and said I know you are having a wonderful time being Director of Special Programs, but because of all the downsizing, we need your expertise back in the Programs. So I have a number of programs I would like you to manage. He gave me a choice of either going back in geology/geophysics or Cosmo-chemistry and Origins of the Solar System. So I decided to take Cosmo-chemistry, and not geology/geophysics where I came from because I thought that sample returns were going to happen again. So, I thought this was a going concern--the Origins Program. Since then I have eight programs now [Authors Note: at the time of our interview]--and almost 50 million dollars worth of funding.

My projects are all under Origins and Evolution of the Solar System. That is the element that I manage. In the meantime when I was Director of the Special Programs, I was also put in as acting Assistant Associate Administrator for Outreach and Education in Space Science--what Jeff Rosendahl is in now--but I never actually got the title, just the function. I said, well am I acting or not? They said well no, we can't have you acting in that position--but you need to do the job. So I did that at that level. That was like a 25 million dollar a year program of education. It turned out that it was a lot of fun because it put me on some national educational boards--like for National Academy of Sciences. They had a board to put together the science standards for the next 25 years. It was fun. The people who were on that were just amazing--educators and legislators and all that. I felt like I was getting a real education."

*Schaber: "I will tell you the truth Joe, our colleague at Astrogeology (both at Flagstaff and Menlo Park) are all amazed with your tenure up at NASA, and your tenacity to stay with it and do such a*

*good job. I think everyone who knew you when you first left here for D.C. thought that is one hell of a job for him to be doing. Well done!*

Boyce: "One can only hope that you get into a position where it matches what you like to do."

[Author's Note: At the end of August 2002 Joe retired from NASA and moved to Honolulu Hawaii. He is now part of the research faculty at the University of Hawaii working on Mars research and enjoying life. He is spending about a quarter of his time as a rehired annuitant for NASA managing planetary Instrument Development Definition Program.]

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**Patricia Marie (Mitchell) Bridges** (b. 1933; Cartographer/illustrator Fine Arts degree, 1955, University of Washington, St. Louis, Missouri) secretly drew the very first shaded relief map of the Moon in 1959 while working for the Air Force Aeronautics and Chart Center (ACIC) in St. Louis (see 1959-1960 in main text). She subsequently moved to Flagstaff, Arizona in September 1961 to work for ACIC at Lowell Observatory on Mars Hill. At the request of Ray Batson, Pat Bridges formally joined the Survey's Branch of Surface Planetary Exploration in 1970. She was located in Building-One (Astrogeology's administrative headquarters) on McMillan Mesa in Flagstaff; but after a very short time. Pat and her associates were moved down to the 3rd floor of the Arizona Bank Building. Pat started out on the Apollo landing site maps, but she was also doing some mosaicking from the images returned from the Mars [1964]-flyby mission. Pat went out to JPL with USGS colleagues Sue Davis and Jay Inge to make a 1:1-scale model of one of the Surveyor landing sites. She also participated at JPL with the Branch of Astrogeology Science Team during the encounters of the Voyager spacecraft with Jupiter, Saturn, Neptune and Uranus during the late 1970s and 1980s. Pat Bridges officially retired from the USGS in 1990, but stayed on working part-time for another couple of years finishing up some maps from the Magellan radar mapping mission (1990-1994).

The following was taken from an interview with Pat Bridges by Gerald G. Schaber on 12 April 2001):

Bridges: "Well, I grew up in Illinois, just across the river from St. Louis. I went all the way through grade school and high school in Wood River, Illinois. Then, when I went to the University of Washington in St. Louis, I received a scholarship to the Art School--which seems far-removed from anything that I would do in Astrogeology later on. Anyway, I got my degree from the Washington University School of Fine Arts in 1955. Actually, before I was finished with my degree, I worked for one year for the local Government restoration of murals on the old Court House in St. Louis. I did that just before I finished school, and ending up the following year."

We were cleaning and restoring the murals that were up in the dome in the old courthouse. That was part of the Jefferson National Expansion Memorial. That was a neat job, but of course it was temporary; so when we were done--that was it.

After that I got married. My husband and I were living in Illinois. He started school and I wanted to work. So, I finally went to work for what they called then the Chart Plant, which was eventually called the ACIC--the U.S. Air Force Aeronautical Chart and Information Center in St. Louis. So I

was commuting back and forth from Illinois to Missouri. I was hired to do shaded relief, which is the standard shaded relief with contours, but with the airbrush. That is where I learned to use the airbrush. I didn't use it in college.

*Schaber: "You were using the airbrush at that time on terrestrial photographs?"*

Bridges: "Yes, that's right; in fact, they weren't even photos, they were just shading from the contours. We could do a little research if we needed to look up something about the form of features, whatever--but basically, it was based on the topographic contours. Then along came special projects. I did a few of those; I even did one of Arizona that included the Mogollon Rim—and I thought that would really be neat to see, not knowing that I would live near the rim later.

Finally, a special project came along from the Development Office, and I did some drawings of the Moon from pictures from the Kuiper Atlas [of the Moon.] So that was the beginning of my doing shaded relief from photography, with telescopic observations and pictures--and that's what started all of that for me."

*Schaber: "How did this relate to the early lunar terrain mapping that Bill Mason and Robert Hackmann were doing at the USGS in Washington, D.C., and the early geologic mapping of the Moon that Gene Shoemaker and his fledgling group of astrogeologists were doing out in Menlo Park, California, at about the same time?"*

Bridges: "Yeah, in fact, the first map that I did [the Crater Copernicus] they wanted to quiet about because there was some competition among some of the mapping agencies [e.g., the Army Map Service] and so they had me doing some drawings hiding under some stairs with the door between me closed, because they didn't want it known that there was that kind of work going on in our group. [See 1959-1960 in main text for more details]."

*Schaber: "So was it Bill Cannell, or someone else, you were working for?"*

Bridges: "Actually, at that time, Bill Cannell was not involved in it. There were a couple of other guys who were in the Development Office [at ACIC in St. Louis]. They kind of started me; they showed me some of the pictures and got me input on what I could do with it. So I started with them. I can't even think of their names right now, but they were pretty sharp."

*Schaber: "Do you suppose that the Air Force started this because they got wind that the Russians were growing very interested in the Moon?"*

Bridges: "I don't know. I know that we were competing with the Army Map Service for the projects--and I don't know whom else. But different people at ACIC were planning different projects, and then submitting them. I don't know who made the decisions, but it was somebody in NASA [NARPA at that time]. Anyway, about that time, the managers at ACIC started talking about going someplace to do lunar observations-- using a telescope--and they were casting around for places to go."

*Schaber "What year were you doing this secret mapping of the lunar crater Copernicus*

Bridges" This was probably 1959 or 60 because I made my first trip out here [to Flagstaff], and that is when Bill Cannell became involved; that was the first time I actually met Bill. He came out here [to Flagstaff] around 1960, and we went out to use the Naval Observatory's telescope."

*Schaber: "Was ACIC in Flagstaff at that time?"*

Bridges: "No. Nobody from ACIC was stationed at Flagstaff at that time."

*Schaber: "So how did Bill Cannell get into the act with starting the ACIC group here at Flagstaff?"*

Bridges: "We came out to Flagstaff in October 1960 to observe a little and, more importantly, to look over various observatories. They were kind of settling on Flagstaff.

Well, Bill was also from ACIC in St. Louis, but it was a big organization and I just hadn't met him before that.

Anyway, that was the first lunar observations that I got to do. I flew out and did the hop skip and jump thing--lots of little short flights because we didn't fly directly into Phoenix then and then fly up to Flagstaff. So I spent all day flying to get to Flagstaff, and then went out and observed all night--which was sort of a shock.

I also did a little bit of drawing there n Flagstaff. Bill did manage to round up an air tank for me and I did some drawing using the air tank and my airbrush in a hotel room at the Monte Vista Hotel downtown [see Fig. 5 in main text]. We were here a couple of days, and then we were supposed to fly back. We still had really nasty weather, and it ends up being a long overnight stay at another place in between because we couldn't fly to various towns to get back to St. Louis."

*Schaber: "Let me go back here a minute. What were you doing in Flagstaff during that trip, exactly? Were you looking through the telescope doing airbrush on contour maps, or annotating photos?"*

Bridges: "No, there were no contours maps of the Moon in existence at that time."

*Schaber: "So you were just using a photomap--a base map?"*

Bridges: "Yeah, what we had kind of a blue-line copy of one photo for a base. They were usually pretty distorted, and you had to kind of interpret what it would look like looking directly on it.

*Schaber: "So you were airbrushing details onto that photo base."*

Bridges: "Right. What we would do was to make a lay-in drawing just showing where the terrain forms were that we could see from the photographs--and then refine it from observations from the telescope. Now, when we worked at the telescope, I made notes on the photographs we had--and

so did the other observers later on--trying to clarify features so that we knew what the features really looked like when you actually observed them."

*Schaber: "So you got back to St. Louis and start working on this map--under the stairs."*

Bridges: "Well, by that time it was a little more known, so it wasn't so secretive. We had gotten the contract for lunar mapping and it was pretty well established that we were going to come to Flagstaff."

*Schaber: "So when did Gene [Shoemaker] get into the picture? Did he ever come to ACIC in St. Louis?"*

Bridges: "I didn't see Gene until we were already out there mapping in Flagstaff. When we came out, there were only three of us. There was Bill Cannell and his family, and my husband and I, and Jim Greenacre and his wife and kids."

*Schaber: So you're back there [in St. Louis], and they decided to come to Flagstaff.*

Bridges: "In fact, ACIC had kind of a little contest among some of the other people who were interested in doing this sort of work. A bunch of us did drawings. Of course I had a little bit of an "in" because I had already been looking at the pictures. At any rate, they had Gerard Kuiper [University of Arizona, Tucson] rate those drawings--and mine came up on top. However, they weren't too sure that they wanted to have a woman involved in this. This was about 1960.

*Schaber: "So Kuiper was just being asked to check them your drawings out?"*

Bridges: Yes, Gerard Kuiper, Ewen Whitaker, and Dai Arthur came out together [all were at the U. of Arizona at that time; Dai Arthur would soon start working for Astrogeology in Flagstaff]."

*Schaber: "So the people at Lowell provided that little place for your ACIC lunar mapping operations on the Lowell Observatory campus, right?"*

Bridges: "They were little wooden shacks basically. When we first got there, that is all we had. It was three rooms and a bathroom. I think it was already in existence; it was like a storehouse or something--and later they put a wing on it and made it more of an office."

Shortly after we got to Flagstaff, ACIC brought out a couple more observers because Bill was in charge of the operation and he was also doing lunar observations. Jim Greenacre was doing observing in addition to the photography, and making the prints. I was doing the airbrushing. So they brought another airbrushed out-- Fred Dungas. Anyway, he was the second airbrusher to come out. He had also been doing airbrushing out in St. Louis. Then they brought out [from ACIC St. Louis] an observer, Leonard Martin. Then they hired a few others like Bruce Faure."

*Schaber: "So you are her, and then Gene must have showed up to go up there [to ACIC at Lowell Observatory} and see you."*

Bridges: "Yes, I remember Gene coming up, and various others coming up at various times, and we would send them our maps for review because they were looking at the area too."

*Schaber: "Before the Branch of Astrogeology formally moved from Menlo Park to Flagstaff [in 1963]."*

Bridges; Yes, I think so."

Usually we would go to the telescope when the map that we were working on was at optimum illumination for us to look at it. We would go up and observe during those periods, like two or three nights in a row. But the observers were also using the telescope."

*Schaber: "What did the observers do at the telescope; just make comments on your maps or lunar photographs*

Bridges: "yes, what they were doing is making notes with an overlay on the lunar photograph, and we would transfer their notes and talk with them about what they had seen on the surface."

*Schaber: "You all were using the old Percival Lowell scope, right?"*

Bridges: "Yes, the original, historic one built by Percival Lowell."

*Schaber: "It is still a good telescope!"*

Bridges: "Oh yeah. It has excellent optics in it, and it was great for that lunar work."

*Schaber: "So how did it come about that you ended up coming to Astrogeology?"*

Bridges: "In 1963 I had a baby. I stayed home just about a week or so, and then went back to work. Bill Cannell's wife Ann was babysitting my first baby. Then the next child came along in 1966, and that was when things were starting to wind down. There were still observers and airbrushers there, but we knew it wasn't going to last a whole lot longer."

*Schaber: "As I recall, they kind of gave that whole lunar mapping activity over to the Army Map Service-right?"*

Bridges: "They did! They were going to have the Lunar Orbiter, and they figured that they wouldn't do to have all of this observing because they would have the Lunar Orbiter pictures; so that was coming into play. In 1966 I went back to ACIC at Lowell for just a short period of time, till 1967, and that is the time the work was beginning to slow down. Then I decided it was time to stay home. The whole operation was going to close anyway, so little by little, people were leaving at that time." That would have been 1967-68--about then."

*Schaber: "So you stayed home for a while?"*

Bridges: "I was home for a while and I had a third child in 1969. I enjoyed staying home with the children but I was beginning to think that I would really like to get back into some mapping--and there was some talk about doing Mars mapping.

I talked to Bill Cannell about it a little bit. He was living back east at that time. The way he tells it--I didn't know how it happened--but he says that he talked to Hal Masursky and told him that I was interested in doing some work. So I got a call from Ray Batson in 1970.

So I went up to Astrogeology [to Building-One on McMillan Mesa] in 1970 and my first assignment was an airbrush map of the Apollo 11 landing site for a post Apollo 11 final report or something. That work was for Gordon Swann who was in charge of my first project.

I was there at Building-One a very short period of time, and then they moved us down to the third floor of the Arizona Bank Building."

*Schaber: "You started working on some of the Apollo landing sites but were also doing some Mars preliminary maps--the Mars flybys were also about that time."*

Bridges: "Yeah, I did some preliminary work on various things, but I also did some mosaicking when those first pictures were coming in from Mars."

*Schaber: "So when did Jay Inge show up; he worked for Lowell didn't he?"*

Bridges: "Yes. Well, what happened was that Lowell hired some of the people that worked in our ACIC office at Lowell when we closed down. They were employees of Lowell; and they also hired some of the ACIC observers. It turned out that some of them were working for Lowell and some of them were working under contract for ACIC. Jay came in about 1963 and started mapping there at ACIC until the office started closing up; then he went to work for the National Geographic Society in D.C. for a while. Later, he came back and worked for Lowell for about one year and then, about a year or so later, he came to work for Astrogeology."

*Schaber: "So Jay was at the Bank Building also when you got down there?"*

Bridges: "Yes, he came down there and worked part time at the Bank Building?" I started there in 1970 and he [Jay] arrived about 1971."

*Schaber: "Then, Ray Batson started building up an airbrush group at Astrogeology. It was you, Jay Inge, and who else?"*

Bridges: "And Sue Davis. She started there... Her married name was Davis; I can't remember her maiden name. She was just out of school. She had gone to school to learn how to be an art teacher. She was working at Astrogeology part time when she was going to school, and then she stayed on and did airbrushing at the Bank Building."



*Schaber: "You all developed this technique. You were probably the first person to use the airbrush for that purpose [shaded relief [lunar mapping] back in St. Louis then. Then, you kind of modified the tools and technique a little bit."*

Bridges: "Yes, I had learned to use the airbrush, but of course I was using it a little differently than I did enhancing topographic contours earlier. It was more of an illustration-type thing--trying to make it look as realistic as possible."

*Schaber: "So somebody came up with the idea of using the electric erasers to make the highlights or the sun lie side of the craters."*

Bridges: "We were using electric erasers even back when we were making shaded relief maps from the contours, but we weren't using them for the kind of detail that we gradually got into when we got much better images to work with."

*Schaber: "Did you use standard airbrush instruments, or did you have to modify them in any way?"*

Bridges: "We were using Paasche AB airbrushes, and they were used for the finest detail that you could do with airbrushing. There were other airbrushes around, but these were the best."

So it grew out of the basic airbrushing I learned to use in the first place. Then, when we got into more detail, we modified the needles. We sharpened the needles so we could get finer detail. We got to be pretty good mechanics with them because they had to be working just right."

*Schaber: "So what happened after that? You're working on some Apollo things and some Mars things, and then I guess your big thing came when Mariner 9 started taking Mars picture and you started doing airbrush-shaded relief maps all of the Mars Quadrangles. First, there was a series of uncontrolled Mars mosaics. Then you did a series of semi-controlled, and then controlled photomosaics. Is that right?"*

Bridges: "Right. Batson's group did those photomosaics, and those were our bases, and we augmented by comparing different photographs taken from different angles and views--and combining them."

*Schaber: "Did you ever go out to JPL for Surveyor with Batson's group?"*

Bridges: "We did go out to JPL once to help put together the base scale 1:1 models for Surveyor. We made the foam lunar rocks to scale, etc. Jay Inge and Sue Davis also went out. We made and tried to make a real copy--just like the lunar surface looked. We made Styrofoam rocks and cut them with a hot wire, and tried to make them look like the real rocks. That was fun."

*Schaber: "Then it was the Voyager 1 and Voyager 2 encounters [1980-1989]. That was really exciting. Besides doing a lot of the airbrush maps for Voyager, did Ray ever take you out to JPL?"*

Bridges: Let me think. Yeah, we did the preliminary maps when we were out there for those missions. We were working long hours out there; we'd be out there about a couple weeks at a time."

*Schaber: "So you just do preliminary airbrush maps from the encounter or approach pictures."*

Bridges: "Yes, whatever we could get our hands on we would try to make a map out of them."

*Schaber: "So you actually retired in what year?"*

Bridges: "I retired officially in 1990, and then I worked another couple of years part time finishing up some things."

*Schaber: "So what was the final thing you were working on?"*

Bridges: "The Magellan radar images of Venus."

*Schaber: "Everyone says that this was such a remarkable period of time--not only for Flagstaff and us--but for the space program. We were just ordinary people going to school, and all of a sudden we ourselves in the middle of this remarkable period of history."*

Bridges: "I really felt fortunate. I never thought I would ever make a living doing any kind of artwork --period! I had an art degree, but what do you do with that--you know. But to do work to use those talents, and be so exciting and interesting, it was just a real opportunity for me and I'm really glad that I was able to participate in it."

Anything that anybody did, Gene [Shoemaker] was so appreciative too, and he made you feel like--Golly I just had to do that for him."

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**Michael Harold Carr** (b. 1935; geologist; Ph.D. 1960, Yale University)

The following quotes are taken from an interview with Mike Carr (Menlo Park, CA) by the author on 16 May 2001:

Carr: "I was born 26 May 1935 in Leeds, England, and went to University College London and majored in geology [B.S. Geology, 1956]. At the end of my last year there, I decided that I would like to go to the United States, at least for a year. I applied for a scholarship to go the US, and received one to go to Yale. I went to Yale intending to stay there for one year--then go back to England. However, I ended up staying for four years and getting my Ph.D. at Yale [in 1960]. I was on a student visa so I had to leave the US. I had already met my wife Rachael by that time so I didn't want to go back to England. Besides, there were no opportunities for work in England. So I went to Canada. There also very few opportunities in Canada at that time. So, I ended up getting a job that I knew nothing about--in shock waves.

The Chairman at the Department of Geophysics at the University of Western Ontario had noticed a paper on a mechanical way of generating shock waves. He had proposed to the National Defense Council of Canada for support because, at that time, there was an interest in the shock properties of rocks as it related to the building of nuclear-hardened facilities. So I ended up doing that. My thesis at Yale was the geochemistry of cobalt, which involved neutron activation analysis and spectroscopy. So, I jumped from that to shockwave work. After I went to the University of Ontario, and had been there about six months when I realized that this technique was totally inapplicable to rock materials. So I devised a totally different technique using explosives to generate shock waves. The problem was that dealing with shock waves was very difficult work. There is no way that you can do it on about \$2,000 a year, which is what I had in spending money.

What I had to do was to measure the speed of shock waves in a rock over the distance of a few millimeters. That involved measuring the shock arrival times within tenths-to-hundreds of a microsecond.

*Schaber: "The technology wasn't there then."*

Carr: "It was, but people elsewhere were spending millions of dollars, instead of a couple of thousand doing this. The work that was going on at SRI [Stanford Research Institute in Menlo Park, California] was incredibly more sophisticated than what I was doing. I couldn't get the precision that was really needed. Anyway, I was interested in shock wave stuff at that time.

I noticed a paper in *Geotimes*--an account of Ed Chao and Gene Shoemaker's discovery and work on Coesite [the high pressure phase of Quartz first identified at Meteor Crater, Arizona]. I was wondering where I was going to go after Canada. This was 1961. So I wrote to Gene and told him of the work that I was doing, or trying to do, on shock waves. He called me when he got my letter, and two days later he was on my doorstep in London, Ontario. Just amazing! He had a geologic map of Copernicus and a copy of his work on the shock or dynamics of formation of Copernicus, and comparing it to nuclear craters. So he gave a talk at the University of Western Ontario there.

*Schaber: "That is all you needed--right?"*

Carr: "That's all I needed, I was kind of hooked. I sent my application in, and he was kind of hot to hire me. So, after I had completed my two year exile from the U.S., I was able to come back.

*Schaber: "So when was Gene's visit?"*

Carr: "The visit must have been in the fall of 1961 or spring of 1962. So in spring 1962, I joined the Branch in Menlo Park, California. Gene [Shoemaker] left to go back to Washington (NASA headquarters) close to the same time I arrived--within two weeks of my arrival."

*Schaber: "So Don {Elston} took over?"*

Carr: "Right, Elston took over."

*Schaber: Carl Roach [with Gene's Astrogeology group in Denver, Colorado] was also involved in shock work on rocks. Did you interact with him, and if so, how?"*

Carr: "Well, Carl was doing work on thermal luminescence. Gene had this idea that I could take rocks, and shock them to some known magnitude, and then send them to Carl Roach in Denver where he would look at the effect using thermal luminescence. But I was never able to calibrate things accurately enough that that really worked."

*Schaber: "But Carl did [eventually] find out that there was a shock effect in rocks that was detectable with thermal luminescence]."*

Carr: "Yeah, Gene got really excited. Carl actually sent a guy out [to Menlo Park]-whose name I can't recall--from Denver to learn the techniques that I was using to do the shocking."

So I built this lab and did the experimental work; however, that was pretty well terminated when I had this accident [Author's Note: Mike eventually lost one eye as a result of a serious explosion in his lab]. I think it was about the time when Kennedy died [November 1963]."

Carr: "That was when my shockwave work stopped. Now right at this same time--in fact, I was in New York at a meeting at the time Kennedy died. The meeting was on cosmic dust. Gene had asked me to take on another project. He was excited about the inflow of cosmic dust into the atmosphere. He had read a paper by a guy [Crozier??] in New Mexico who had been collecting what he called cosmic spherules-- iron-rich spherules. He had just been putting trays out various places and collecting and sifting through what landed in these trays in isolated areas.

Gene thought that this was something that we might do. He had also been talking with an ex-Survey guy who had moved to the Department of Defense, and who was very much interested in World Peace. He had thought that the biggest obstacles to world Peace and working with one another was ignorance of opposing technologies, etc. And he had been talking with Gene about the possibility of collecting atomic debris in the upper atmosphere and analyzing it with the hope that one could determine the technology used in making nuclear bombs in different countries, and the techniques that we use for refining uranium, etc.

Anyway, this was a classified project and Gene was persuaded to establish a large project within the Survey. It was split into two pieces. I headed it here in Menlo Park and Mike Duke headed it in Washington. A substantial amount of money was spent on this project. This was not a small project. A large amount of money was spent, particularly in Washington on microprobes and electron microscopes, and other instrumentation --and clean labs and what have you by Mike Duke. And this was in the mid-1960s!

I set up an electron probe lab here in Menlo Park. I didn't have clean room- facilities, but we had laminar-flow benches, etc--semi clean. Anyway, it turned out to be an interesting project, really. I had various people working for me, mainly three people: Charles Meyer, Sue Pradford, and Lewis Calk. Lew retired just about six months ago [December 2000]. He's been here ever since running the Microprobe lab. He got involved in the microprobe work as a consequence of this project in the 1960s.

In addition, I got involved in a couple of experiments to collect cosmic dust. One was an experiment called LUSTER that was run out of Ames (research Center, Mountain View, CA) in which a sounding rocket had a series of arms that opened up when it got in the upper atmosphere, and there were various panels on these arms that collected cosmic dust. I had one of these panels and I could design the collection devices. We put an electron microscope grid in there, etc. I used to go back to Washington during the early stages of this work because--particularly with the LUSTER Experiment--

*Schaber: "What years are we talking about here?"*

Carr: "I would guess about 1965. We did have an electron microscope at Menlo Park. I went to [Ed] Dwornik's lab--this is Steve Dwornik's brother--in Washington, which was in the Interior Building at that time. He was really good guy, and I did a lot of work with Ed Dwornik in this period. It must have been in the mid-1960s.

In fact, you know, that accident I had [I Menlo Park], I damaged my eyes, but I could see with both eyes for a while afterwards.

*Schaber: "You damaged both of them?"*

Carr: "Yeah, I damaged both of them. And a year after that accident, while I was back in Washington, I had my retinal detachment in my right eye, and lost my vision in that eye. I was doing electron microscopy. I was peering down onto this screen, really long hours just looking for these cosmic particles. That is when I had my retinal detachment and lost the right eye. So, that must have been 1964-1965.

So, that project went on and between the cosmic dust stuff and the classified stuff, it consumed a very large fraction of my time during the 60s."

*Schaber: "Wasn't the truth of the matter though that all of the money that showed up from the military was related to the test-Ban Treaty*

Carr: "Yes, it was the Test Ban Treaty."

*Schaber: "Trying to find cheaters?"*

Carr: "No, it wasn't detection of nuclear events. It was determining the technology that was used to do two things: to build the bomb and to refine the uranium--which is very different from detection of nuclear weapons."

*Schaber: "Well Carl Roach [and Gene] was involved in the other aspect of it [detection of nuclear events]. That came to him because the military thought what he was doing (detecting strain in rocks related to shock effects using thermal luminescence) could be useful in detecting [or proving] nuclear shock events. So he was in the other side of that."*

Carr: "Well, this was a big project, and we were only a small cog in it. What we did was to isolate the particles that contained fissionable components, and they were extremely small, one to two microns. Then, we would send them off for isotopic analysis to other labs. Yeah, it was a nationwide effort.

We would have these meetings to talk about our results and then I would have to leave because I was a non-citizen. And in fact, it was really strange because I would write a report; then the report was classified--and I couldn't read it! So it was bizarre, really!"

*Schaber: "Only the U.S. Government!"*

Carr: "Once it was classified, you needed a Q clearance. For a Q-clearance you needed to be a citizen, and I could not be a citizen until I had been a resident for five years, which would have been 1967.

Anyway, that consumed most of my time. However, I was still doing lunar work. We were all assigned lunar quadrangles, and we would drive up to Lick Observatory. Now, I am talking back to 1963 and 1965. We would drive up to Lick Observatory when our quadrangles were near the terminator--and do the mapping. I found it very difficult to do because the [lunar] image would come and go--and just transferring it to paper. It was impossible. Don Wilhelms loved it! However, I just found it frustrating.

Hal Masursky had arrived--I forget exactly when--but he was very much intrigued with the idea of getting better photographs. He had this camera that he bought for Lick Observatory. He got money to get a camera. So he spent a lot of his time looking into buying a new camera for Lick Observatory, so they could use the camera to improve our [lunar] photo collection."

*Schaber: "So you never really got into the lunar thing then?"*

Carr: "Well, we all did lunar stuff. I mapped the Timocharis Quadrangle and was involved in Lunar Orbiter, and I did go back and forth to Houston two or three times to brief astronauts --but I never really got into the lunar stuff. Because I did not relish going up to the Observatory every month staying up all night disrupting everything else I was doing--because I was doing all of this other stuff."

*Schaber: "So then lunar Orbiter. You had nothing at all to do with Ranger or Surveyor?"*

Carr: "Oh yeah! With Ranger, there was a group of us that were involved with mapping--doing some post mission analysis. I was interested in the crater Alphonsus, and made sort of a geologic map of the floor of the crater."

*Schaber: "That was one of the later Ranger missions."*

Carr: "Yeah, that was the last Ranger I think. That's was sort of my only involvement with Ranger."

*Schaber: "What about Project Surveyor?"*

Carr: "I wasn't involved in Surveyor at all!

Now Lunar Orbiter, I really was involved in that. Those were my first trips to JPL--going down there and being in SFOF (Space Flight Operations Facility) at JPL. In fact, one thing we had to do, as you recall, was put together those strips [of Lunar Orbiter images of the moon] on a light table. The strips would come in and we'd lay them down on the light table and gradually build up the picture. And I recall particular those pictures taken at an oblique angle. It was really exciting seeing the picture of Copernicus slowly build up.

That is where I first met Tom Young and Jim Martin [Author's Note: who later would be Mission Director for the Viking-Mars missions in 1976, and subsequently, the Magellan mission to radar-map the surface of cloud-covered Venus] who were involved in that--and Norm Crabell--all of those guys from Langley. And of course we went back to Langley two or three times to--what they called "screening". We spent weeks back there. Mainly what we were doing was making geologic maps, because this was the first mission of importance for finding potential landing sites [for Apollo]. So we started that program that Newell Trask ended up being in charge of--making geologic maps--P2P, 3P3--that was the most useless period of my whole professional career.

We must have made between 50 and a hundred geologic maps [from the Lunar Orbiter images]. Totally useless, and they printed hundreds of copies of all of them. They ended up not being used I think. Anyway, that must have been 1966-67--that era.

I did go back to Houston just a couple of times I think. What we did was go over the flight path of the Apollo spacecraft over the lunar surface, step-by step--just talking with the astronauts as to what to anticipate--what to look for.

*Schaber: "Did you ever do anymore work on the Moon after that?"*

Carr: "Well, I did some work on the lunar samples. I had gotten regolith samples--and really didn't do much. What I did was--because we have this microprobe here--was look at the distribution of different components in the regolith--you know, the anorthositic components, and the basalts and glass."

*Schaber: "Who did you work with on that project?"*

Carr: "Well, it was just me and our group here in Menlo Park that was still working on this cosmic dust project. It was Lewis "lew" Calk who did most of it; Charlie Meyer did most of the microprobe work.

We would just make these slides; we'd go over them and make diagrams of rock-type distribution, and see how the distributions changed with location within the landing site."

*Schaber: "What did you find interesting with those distribution; anything particular?"*

Carr: "It was obvious--within the distribution of the different particulate--that the distribution of a particular particulate was very dependent on the local geology. So, as you go away from say the Apollo 15 site--you go away from the Apennine Front, the proportions would change. But there really wasn't a lot in that. That work was done about 1971-1972."

*Schaber: "You didn't do anything with the cosmic aspects of the particles?"*

Carr: "No."

*Schaber: "So then what--you got involved in Mariner 9; it overlapped Apollo."*

Carr: "Hal [Masursky] got us involved in that [mission]. There was Don Wilhelms, Danny Milton, Jack McCauley, and myself. We were in the planning, and as you recall, there was originally going to be two missions for Mariner 8 and 9. One was a mapping mission, and the other was going to be a variable features missions--where it was going into a resonant orbit and look at the same place on the planet time and time again. It was to look at the wave of darkening--the "growth of vegetation." [Author's Note: a dig on Sir Percival Lowell's predictions] Carl Sagan was behind that--and Geoff Briggs was a proponent of that too.

Then, after we lost Mariner 8-, we had to redo all of the planning. It is sort of fun to think back on that time, because now we have all of these sophisticated planning tools. However, back then was we had this big ellipse painted on the table, and we had a sphere representing Mars. Then we would walk around the table and look on Mars from various points in the ellipse to see what we could see."

Carr: "I found that it [Mariner 9] was a very demanding mission. But fortunately, there was a dust storm [the great dust storm of 1971-72]. We had no idea where we were, or where anything was located. We'd see things in the wide-angle camera, and we had an opportunity perhaps ten or fifteen days downstream to re-photograph that same area. So, we'd pick targets in the wide-angle camera. But then, we've got to locate them you know--was their latitudes and longitudes were. The only maps we had were basically Lowell's maps showing Lowell's so-called canals."

*Schaber: "And we all know that was wrong!"*

Carr: "It was just incredible. There were some strips of the Mariner 6 and 7 [Mars flyby images from 1969], but there were so few of those; they were mostly just shading you know. It was really very difficult and very demanding. No feature had formal names. We had this big blackboard up in the planning area there where we kept track of all this stuff. Anyway, it was not easy. The dust cleared about January of 1972."

*Schaber: "Tell me the famous story of when the three giant spots--the big volcanoes--started coming out [becoming visible in the Mariner 9 images] and some people thought they were impact craters."*



Carr: "Well, my recollection of it [the story] may be different from Larry [Soderblom].

However, as you know, there were these three spots. The image processors were rattling around--the people from IPO--they didn't know what to do because there wasn't anything to do--so they sort of did some processing on those spots. It brought out the craters--you could see them; there were intersecting craters that looked like calderas that were poking out above the dust top. However, there was a group from Caltech who had proved that it was impossible to have volcanism on Mars. You know, they had done theoretical calculations [Author's Note: Boy, were they wrong!]

Jim Cutts, Larry Soderblom, Bruce Murray and several others, absolutely refused to believe that these three spots were volcanoes. They would insist that the whole planet was cratered, and we were just seeing some high part or something that was cratered."

*Schaber: "That just goes to show you what the influence of the Mars flybys [Mariner 6 and 7] were!"*

Carr: "This was my sort of first experience with the Caltech group. They were so pugnacious. They were so nasty to each another. Jim Cutts, Larry Soderblom and Bruce Murray would get into these fights. We would just pull back with eyes wide open looking at these guys just going at each other."

*Schaber: "Did Hal [Masursky] believe that they were volcanic?"*

Carr: "Oh yes, Hal was very strong advocate of the volcanic origin for the craters"

Incidentally, prior to this time, 1969 or 1970, I had been made team Leader of the Viking Orbiter imaging Team--prior to getting into orbit with Mariner 9.

*Schaber: "So the Viking Project went way back."*

Carr: "Yeah, it started about 1969-1970. You know, I ended up being Team Leader by default. What happened was there was a small Viking Imaging Team originally team selected. There was Gene Shoemaker, Larry [Soderblom], Bill Baum from Lowell Observatory [Flagstaff], Geoff Briggs, and maybe another. Anyway, it was a small group.

So, the question came up as to who should be the Imaging Team Leader. Well, Gene said that he was just too busy. In fact, he was going to resign from the Team. Hal [Masursky], who was Team Leader for Mariner 9, could not spend the time necessary. Bill Baum was a possibility, but Bill was an astronomer. Yeah, it ended up being me --by default.

They were very concerned about it. Mike Mitz at NASA Headquarters thought that I was too young and inexperienced, and they ought to try a new search... Anyway, I ended up with it.

During this period from 1969-1970 on Viking started taking up more and more of my time. But there was a big group at JPL of Viking people that called it VDAP (Viking Data Analysis Team).

They were looking at the Viking data with the eye to figuring out how difficult it was to land on Mars, and all this kind of stuff.

So this Team was at JPL around 1971-1972. A lot of guys were involved; Tim Mutch headed it and Henry Moore was on it. The Mariner 9 Team thought that these [Viking] guys were trying to horn in on the science--so there was some tension between the two groups.

That was a watershed sort of period for me, because I ended up being Team Leader on Viking, and you are well aware of all the stuff that came in on mariner 9."

*Schaber: "That was quite a plateful. So, the Mars atmosphere started clearing up for Mariner 9, but we know that it hadn't cleared up as much as we thought until Viking took better pictures in 1976."*

Carr: Yeah, it started clearing up in January 1972."

*Schaber: "The surface dust was still there, but we weren't really sure exactly how much."*

*Now there is a story about Hal fighting for the high-resolution camera on mariner 9, and having all kinds of problems. People said you won't see anything, etc. etc.--and of course we didn't for a while [because of the massive dust storm]. They didn't want it [the HR camera]."*

Carr: "I don't recall. I probably wasn't a part of it. But Hal was fantastic on Mariner 9. He ended up chairing this sort of Science Integration Team, where they would meet almost every day and talk about results. But there was the UV spectrometer, there was the IRIS Experiment--the thermal experiments, etc.--and Hal would pull all of these people together to talk about what the results of the different experiments were."

*Schaber: "Yes, he apparently wanted to be in charge of all of the different Mariner 9 experiments."*

Carr: "Yes, and he was very good at that--in getting integrated observations, but also integrating the interpretations. He really was quite impressive."

Carr: "He was so articulate, orally."

*Schaber: "We are up to Mariner 9. It was successful. Tell about the channels--how you got into the channel research. There were a lot of people interested in the Mars channels after Mariner 9--up through Viking."*

Carr: "Well actually, that came later. We divvied things up--Jack McCauley, Danny Milton, and me--in volcanism you know. Really between Mariner 9 and Viking, that's mostly what I worked on, the volcanic features, although I did look into the formation of the valleys by lava erosion and decided that it didn't work very well. But that was my interest at that time. It wasn't really until Viking that I really got hooked on the water story because of all those fantastic pictures that we got during the Viking site selection you know."

*Schaber: "So you get through Mariner 9--and then Viking.*

Carr: "There are some stories that have to do with the camera. Well, we almost lost the Viking Orbiter camera. Let me tell you a story. You can find actually it in Ed Ezell's book on the history of Viking. Well, we were always having money problems, mainly because of the growth of the experiments, GCMS, and biology experiments--the growth costs kept growing and growing.

There was one particular meeting at Ames. The meeting started out with Jerry Soffen saying at this meeting, we have a \$19 million problem to solve. Then, he said the Viking Orbiter Imaging camera costs \$20 million dollars--does anyone have a suggestion?

This must have been about 1972. The Viking Orbiter camera started out with a very different design from what it ultimately ended up. What we had was a vidicon camera with an image intensifier up front. An image intensifier could be used for image motion compensation. What an image intensifier does is to transpose the image into a stream of electrons. This stream of electrons can be accelerated to give a more intense image on a screen, but also you can divert the stream of electrons so as to compensate for the motion of the spacecraft over the ground. The reason for this was for site selection because we needed high resolution. Given a vidicon without image compensation, you couldn't get the exposure time for high resolution (e.g., 3 or 4 meter) because of the poor sensitivity of the device. So you had long exposure times, and you blew it out your image. So this was a potential solution to that. Well, we had all kinds of technical problems with this image intensifier.

So we had this meeting at Ames, and the pressure was really there to compromise significantly on the camera. In fact, there was a motion on the floor to reuse the Mariner 9 camera. Well one attribute of the Viking Orbiter Camera was that it takes pictures very quickly, every 2.2 seconds--you get a picture as you run over a landing site--and immediately start seeing what's on the landing site. Well, the Mariner 9 camera had a 33-sec gap between pictures. So coming up on the landing site you'd point forward and take a picture and then pass over the site and look back and take another picture--so slowly, you could build up a picture of the landing site.

Well, this pressure from Jerry Soffen and others in the Project to kill the Viking camera, and substitute the Mariner Orbiter camera, was intensified by Conway Synder who was the Viking Orbiter Chief scientist. So I was left isolated and Is Tayback (sp?), the project Engineer, pulled me aside and said we're going to lose this [Viking] camera unless you do something. I thought what in the hell can I do? This meeting you know, we are already into it.

I thought what I can do is do some simulations of what would happen with the Mariner 9 camera and Viking Orbiter camera on the Apollo 14 landing site. So I came back to the Survey here (Menlo Park). The meeting was [nearby] at Ames. I got the Lunar Orbiter picture of the Apollo 14 landing site and had Noel Prime (sp?)--who worked in our photolab here-- cut out sections simulating what would happen with the Mariner 9 camera as you slowly build up over days and days of getting these little pictures and putting them up. And then I did a simulation with the Viking camera.

Noel Prime and I stayed up until about three o'clock in the morning here making those view graphs and the simulations; and I went back to the meeting the next day and showed those simulations, and it was just so compelling. I never heard anything about it again. What we did was we ended up dumping the image intensifier and we cut back on some IPL stuff--but we kept the Viking camera."

*Schaber: "Where did they come up with the \$20 million then?"*

Carr: "Well, piece meal from other places. We gave them about \$5 million from IPL."

*Schaber: "I would imagine that Jerry Soffen had [carried] a pretty big stick with the biology-life experiment."*

Carr: Oh yeah, they kept saying this is a lander experiment--this is a lander experiment--not an Orbiter experiment. Anyway, that was a major success! Well, it was a much better camera, the point-spread function on this camera was just so much better than on Mariner 9 was."

*Schaber: "But there were entire surface features that were showing up [on the Viking images] that weren't there before [on even the late-mission Mariner 9 images]. I think it was a combination of the better camera and the fact that there was still some dust in the [lowermost] atmosphere all throughout the Mariner 9 mission---but it had cleared up by Viking."*

*Let's talk about the actual Viking mission (summer 1976)."*

Carr: "Well, it was consuming incredible amounts of my time. I was living at JPL. We had all kinds of problems with the on-line image processing system--all kinds of things. We would have the operation-readiness tests, and I would be down there. As the data came in and you could only process two lines at a time. You'd process two lines, and then you had to delete one line out, and add another [line] and process that. All the image processing had to work on those two lines, and progressively move down the image. You had to devise filters and so on that worked in that mode; and it was tough.

But this guy Johannsen, who headed that and wrote all of the software, was an absolute marvel. And he had problems with JPL. JPL was fighting him because he was wanting do what the scientists wanted--and do it very well; but there were all of these requirement documents, and JPL was saying let's stay with the requirement documents. They had that conflict. But it turned out a great system; those real-time products that we got [during Viking] were fantastic. I had spent all of my time at JPL that spring. It got to be on the top-ten problem list of the whole project--the first-order processing system. It got solved shortly before we got into orbit [around Mars]."

*Schaber: "Tell me briefly some of the planetary missions that you have been involved in after Viking."*

Carr: "I have only really been involved in several aspects, one is Galileo, the outer planets--and in Voyager. I was peripherally involved in Voyager. I was a Team Member for the early encounters with Jupiter, and worked mostly on Io in 1979 or so. I was not so involved in the subsequent

[Voyager] encounters. I got involved in Galileo. I wrote my Galileo proposal [to be on the Galileo Imaging Team] in 1976. My God, it's still going on. I've been involved in Galileo forever. As far as Mars missions, there had hardly been any [because of the three failures]. Mars Observer--I was an interdisciplinary scientist on Mars Observer [lost].

*Schaber: "Very frustrating!"*

Carr: "Yes, it was very frustrating, and that went on to MGS. I am now a member of MGS."

*Schaber: "Were you on the one that crashed--the Polar Orbiter?"*

Carr: "No, I was not on the Polar one. I'm on MER [Mars Exploration Rover]. I was on Athena that got delayed, and now I'm on MER.

*Schaber: "Briefly, tell me what these missions were!"*

Carr: "Athena was a rover mission to collect samples by drilling into rocks, and also making observations as it went across the [Martian] surface. It's evolved into MER, and being downgraded somewhat--and delayed. MER now goes in 2003; there are two rovers. They are very simple, scientifically. A week before last I was involved in these FIDO tests. We had a rover in the Mojave [Desert, CA] and we were simulated what we are doing on Mars. It is very demanding to run these rovers.

I am involved also in the Mars Express high-resolution camera-, which is built in Germany. It was originally on the Russian Phobos Mission, which was launched in 1996.

But during the 1980s and early 1990s, I was just about on every advisory committee that NASA has with respect to Mars. I headed up the Mars Rover sample return Study Group--which went on all through the 1980s. I was on the International Mars Express Working Group, on the International Council for the [Soviet] Phobos Mission. I was on the Joint US-Soviet Working Group for Planetary Exploration. I was on just about everything."

[Author's Note: Mike Carr also participated in the selection of the site for the 4 July 1997 landing of Pathfinder on Mars. Carr has published more than 170 publications, including two books, "The Surface of Mars," and "Water on Mars," among his many awards are a NASA Medal for Exceptional Scientific Achievement (1977), the Department of Interior Meritorious Service (1979) and Distinguished Service Award (1988), and the Geological Society of America's G.K. Gilbert award (1993). Mike was also the 1994 recipient of the National Air and Space Museum's Lifetime Achievement Award in Air and Space Technology. On 16 February 2002, Mike Carr was installed as a "Fellow" in the American Association for the Advancement of Science at the AAAS Annual Meetings in Boston, Mass. At the time of this writing, Mike Carr is formally retired from the Branch of Astrogeology in Menlo Park, California, but he is active in several Mars-related projects.]

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**Roger Carroll** (1942-2002; cartographer), a talented, friendly, and dedicated member of the Branch of Astrogeology's Cartographic Support Unit throughout the Apollo era and well beyond, arrived in Flagstaff on 14 March 1966. Roger was originally located at the old city Annex Building, or "Dance Hall" as it was also called [see main text for details] which the Branch had leased for a short time on Santa Fe Avenue (now re-designated Route 66) in Flagstaff. He started out working there along with James Vandivier, Ray Sabala, and James Hart. Roger, who retired in 1997, unfortunately passed away of a heart attack while working on his commercial fishing boat in Alaska in June 2002.

The following was taken from an interview with Roger Carroll (Flagstaff, Arizona) on May 14, 2001 by Gerald Schaber:

Carroll: "I was born January 3, 1942 in Thermopolis, Wyoming, and went to high school at the Hot Springs County High School there. I went to the Casper Junior College (Casper, Wyoming) for two years, and received an Associate of Arts degree. I worked for the Superior Oil Company in Casper from 1962 to 1966 and came to Flagstaff March 14, 1966 to work for the Geological Survey (Branch of Astrogeology).

I worked with Ray Sabala in Casper for Superior Oil Company when Jim VanDivier [who was working with Superior Oil Company office in Denver] came to Flagstaff with the USGS in the fall of 1964. In 1965, Ray Sabala left Casper and I came to Flagstaff with the USGS, Branch of Astrogeology. Jim contacted me in Casper and said that they needed another draftsman down in Flagstaff. So that's how I came down to Flagstaff and went to work for Jim Vandivier at that time.

*Schaber: "You knew Jim at Superior Oil right?"*

Carroll: "I worked at Superior Oil Company with Ray Sabala but I didn't know VanDivier at all; he had worked with Sabala in Denver. Sabala and Vandivier worked together in Denver; and then Sabala came to Casper when they closed the Denver office down, and VanDivier went to Farmington, NM. Then at that time they close the Farmington NM office down, and that's when he [Vandivier] went to work for the Survey. So that's how I came about working for the USGS."

*Schaber: "So when you arrived in Flagstaff, in what facility were you located?"*

Carroll: "When I came here in 1966 I worked under Jim Vandivier; and our first location was down on Santa Fe--what was that ANNEX Building down there on Santa Fe Avenue [now Route 66] by the old A&W Root Beer stand. Jim Hart worked down there with Sabala, Vandivier and me—perhaps some others I can't recall. Then after we left [the Building on Santa Fe we went to the Building on Fourth Street [in East Flagstaff] [about 1967 or 1968]. We had our office down there. Then again I believe John Robey was in that building with us. He was a draftsman.

We were doing illustrations and other things; for example, we worked with Dave Roddy doing some scribing for his geologic map of the Flynn Creek (Tennessee) impact structure. We did some illustrations of Meteor Crater for Shoemaker. Most of the work that we did down there [in the

Building on Santa Fe] was illustrations, including some design-layout work for some of the prototype Apollo Lunar Tools, and other equipment such as lunar surface cameras."

*Schaber: "What about lunar maps?"*

Carroll: "Well, after 1968, when I left Jim VanDivier's group and went up to work with Russell "Russ" Wahmann in Building One [on McMillan Mesa], we started doing the 1:1,000,000 lunar maps [geologic quadrangles]. We did all of the preliminary maps at 1:1,000,000-scale."

*Schaber: "So how did Wahmann's group up in Building-One differ from VanDivier's group down at the ANNEX on Santa Fe Avenue?"*

Carroll: "Because the group that Sabala and VanDivier were in did mostly illustrations, and Wahmann's group did mostly lunar map-making at that time."

Our group did all of the 1:1,000,000 preliminary maps. At that time, the published maps (final maps prior to printing) were prepared by the USGS Publications Unit in Menlo Park, California. Then, after I had been there maybe a year or so, we started doing some of the final maps for publication.

Actually, the first actual publication map we did was for Apollo 11. We hand-colored the large-scale Apollo 11 landing site map and VanDivier and his group were doing some of the small-scale maps at that time. We hand-painted a bunch of the large-scale maps (1: 250,000. 1:1,000,000, 1:500,000) for the Apollo 11 crew in Houston. We did them at the request of Hal Masursky, if I remember right. We also did some hand coloring some of the small-scale maps in VanDivier's shop also."

*Schaber: "The Apollo 12 data package [geologic maps] ended up being hand colored at the Cape. Ray Sabala told me that that he and Jim VanDivier went down to the Cape and hand colored them right before the mission."*

Carroll: "Yes. I went down with them as well. Sabala and VanDivier were already there when I was asked to come down to help out [before the deadline to get the maps onboard the Apollo 12 spacecraft; see November 1969--Apollo 12--in main text].

We did some earlier hand-painted maps at the time I was down in Van Diver's shop on Fourth Street; then in 1968, when I went up to work with Russ Wahmann (in Building One. Wahmann was VanDivier's supervisor at the time that I went up to Building One. Then, he went in as the Chief of Technical Support, and I was made the supervisor of lunar mapping in Building-One on the mesa. We were in the area where the administrative group was for a long time after we left). That's where our drafting room was.

In Building-One, the group that was there working on lunar mapping cartography included Mona Moeller, Linda Wester, and myself. Our main job at that time was to prepare the preliminary 1:1,000,000 lunar maps; and they used those for the annual reports. I remember we ran about 300 copies of each one we finished -and they were put there in the back of our Annual Reports that we

put out for NASA. The old Ozalid machine was there; and we had the printing press back there also. We printed the Annual Reports, and we printed the Semi-annual reports there.

Russ Wahmann eventually went back to Washington [to NASA Headquarters] as you recall.

Then, in late 1969--Apollo 12 I believe--was when we did our first cut-and-peel-coat for our first formal publication map [of an Apollo landing site; the Apollo 11 maps were never published because of NASA deadlines]. That was the first formal publication map at that time that our group did in color--and it was of an Apollo landing site. We hand-peeled all of the peel coats; and usually, there is maybe 15 or 20 peel coats for a map. So we hand-cut all of those peel coats. Bill Hazel, a supervisor for the [USGS] Publications Group in Menlo Park, came out and assisted us with that first published map that we did.

We did all of the work in about two or three weeks--we hand-peeled them and worked day and night almost. Then, we hand-carried the material back to Washington D.C. and we had them printed. We stayed there while they printed them;-we set up a special schedule for printing the maps with the Publications Division there."

*Schaber: "The USGS printing presses were still in the GSA Building back then-right?"*

Carroll: "Yeah, it was still downtown D.C. We went down there and stayed with the printers until they [the maps] had them all printed, and then hand carried several tubes back to Flagstaff so that they could use these for the Apollo 12 mission.

We did the same thing for Apollo 13 and I believe Apollo 14. I know, during all of the Apollo missions we did these large-scale maps. Then we finally got into the etch-coat-type of peel coat where it would actually etch the line on the peel coat, and you could peel it. Before, we were cutting them by hand. Anyway, that was the first ones that we did."

*Schaber: "So, when you came up and started doing the lunar maps out of Building-One, who was doing the drafting work that used to be done down there at the Annex then?"*

Carroll: "That was Ray Sabala, who stayed with Jim VanDivier, and John Robey. There was an Indian fellow also who worked with them too but I can't recall his name."

*Schaber: "Tell me about the Apollo Data Packages. You guys produced all of the actual Lunar Surface Data Packages [that included the site geology, pre-planned traverses and station information, etc.] for all of the missions."*

Carroll: "Oh, those were the small-scale maps [8 X 10 inches]?"

*Schaber: "Yeah, the ones that they took to the lunar surface."*

Carroll: "Right. Ray Sabala and Jim VanDivier did most of the work on those; I helped some, but they were pretty much in charge of the small detailed Data Packages."



*Schaber: "So, until Building-Two was completed [on McMillan/Switzer Mesa] about 1968 or so, drafting was pretty much done down at Fourth Street then?"*

Carroll: "They worked first down at the Annex; and secondly in the Fourth Street Building in East Flagstaff; then, when Building-Two was completed about 1968-1969, we all moved over to Building Two on the Mesa. That was when VanDivier, Sabala, and all of them came up from Fourth Street, and we all moved into Building Two at that time.

After moving into Building Two, we continued to work on lunar mapping--the large-scale maps. VanDivier and Sabala continued to do mostly illustrations and the small-scale maps for the Surface Planetary Exploration Branch (SPE)."

*Schaber: "In addition, of course, to preparing everyone's illustrations for oral presentations, publications, etc."*

Carroll: "That's right; they did most of that work."

*Schaber: "So tell me how and when it came about that VanDivier became head of the Cartography Group?"*

Carroll: "I believe it was when Russ Wahmann went to Washington [to be detailed to the geology group at NASA Headquarters for a while]--then, VanDivier became the Chief of both groups. I believe at that time that Ray Sabala was supervisor of the illustrations Group, and I was supervisor of what was called the Cartography Group."

*Schaber: "So, when Wahmann was at Astrogeology, he was in charge of both those groups--the drafting group and the cartography group?"*

Carroll: "Yes, I think he was VanDivier's supervisor. Then--at one time--early on--I think that VanDivier was not under Wahmann, but Al Honka, who was head of all technical Support for a while. At that time, he was supervisor over Wahmann and VanDivier. Then, Wahmann became Chief of Technical Support when Al Honka left, and I became head of the Cartography Group and VanDivier became head of the Illustrations Group. Then, VanDivier took over for Wahmann (when Russ went to D.C.) and VanDivier was my supervisor and Sabala's supervisor. Actually, what they did was kind of put us all in one group--then, VanDivier separated it out so it was Carto Group and Illustrations Group at that time. Then it continued that way for quite a while.

Some other people who worked with us in cartography/illustrations up there in Building-Two included Hugh Thomas [arrived July 1965], Linda Wester [arrived 1965], Robert B. "Bob" Skinner [a May 1966 transfer to Astrogeology from Vandenberg Air Force Base, California], Frank Dunhour [arrived June 1966], Mona Moeller [arrived August 1966], Joe Bullmore [arrived February 1967], Joe Remy [arrived March 1968], John Hallinnan [arrived 1972], Joe DePadre [arrival unknown] and Angel Marcia [1972]. Bill Miller worked with Hugh Thomas, but he worked with our group also for a while.

Linda Wester is now in Alaska. Joe Bullmore is now supervisor of public schools in Winslow, Arizona. He got tired of the work with Astrogeology and decided to go back to school; he went back to NAU. Joe married Jonie Wheeler, sister of Branch employee Charlotte Wheeler. Robert B. "Bob" Skinner, who worked for us in the photolab with Jim McCord for a while, married Charlotte Skinner. They moved to Denver, and then left Denver. While John Hallinnan worked with us in cartography, he invented for us the first circular-type scribing tool that we used to make grids for the Polar Regions, as well as for some of the Lambert projections. John Hallinnan was working both for us and as the Branch pilot for a while there. John did some drafting for our group."

*Schaber: "Ken Stice mentioned that Bill Miller having worked out at JPL on Surveyor with him in Ray Batson's group."*

Carroll: "Bill Miller worked with Ray Batson. Later, Bill worked with us and went over and worked in the photolab. Yeah, I went out there [to JPL] and worked with Ken and Bill for a while. I was out there [at JPL] maybe once or twice."

*Schaber: "I recall that your group did a lot on Surveyor."*

Carroll: "Yeah, we went to Pasadena and did some mosaicking on Surveyor. I went out, and [Jim] VanDivier went out there. I guess Sabala went too. I worked with Batson an awful lot on the Surveyor maps, and we put a lot of preliminary maps out for Surveyor and the Mariner 9 program. We were also working on the big 1:1,000,000 lunar maps at the same time."

*Schaber: "Yes, let's talk now about the Mariner 9 [Mars] maps. Gosh, you guys made about five series of those."*

Carroll: "That's right; we did the uncontrolled, the semi-controlled, the controlled, and the airbrush. The actual mosaicking was done in Ray Batson's shop; and then we did the map-collar work and made prints of the final product for the NASA Planetary Data Facilities around the country (and world)."

*Schaber: "After Mariner 9, along came Viking-Mars (1976), Mariner10 (1973-Venus-Mercury flybys), Pioneer-Venus (1978), and Voyager 1 and Voyager 2 encounters with Jupiter, Saturn, Uranus and Neptune between 1980 and 1989."*

Carroll: "Yeah, there again, we did the entire collar work and published all of those maps too. Batson's group put them all together, and then we did the collar work and pre-publication work [for the geologic maps]. We did all of the final work here [in Flagstaff]."

*Schaber: "What was your actual position-title under Jim VanDivier?"*

Carroll: "I was a Supervisory Cartographer. There at the end [after Jim VanDivier passed away] I was in charge of all of the large-scale mapping--and all of the published work. We also did a bunch of scribing of topo maps [for the USGS] during the time that the lunar mapping and Mars mapping slowed down."

*Schaber: "When did your cartography group move over to the Survey's National Mapping Division?"*

Carroll: "Yeah, we went over to the National Mapping Division, and I'm not exactly sure what year that was. VanDivier was still there when that happened. At that time we were doing a lot of terrestrial maps. Before I left up there we were doing mostly terrestrial maps it seems. We did a lot of those."

*Schaber: "The amount of work that came out of that shop with regard to the preparation and publication of maps is pretty phenomenal."*

Carroll: "There is a file over there--that is still there. It shows exactly because I put every map that we published--I had a little card and put that in the file. They are just individual little-- maybe 1.5 X 1 inch-- cards that I put in that gray file over there. Everyone that we published and the date that it was published are there. The majority of the lunar and all of the planetary maps were published in Flagstaff. Only the earliest 1:1,000,000 lunar maps were published in Menlo Park (CA).

*Schaber: "Then, eventually, they even gave us those to get ready."*

Carroll: "That's right; we did a lot of those later on. I worked an awful lot with Don Wilhelms. Don and I were very close in working with the 1:1,000,000 lunar maps and some of the other mapping projects.

The other thing that we did during the Apollo days was the Apollo landing site geology maps. Remember Phoebe Bernat (later Phoebe Hoffmann) (arrived February 1966) our Branch editor at that time? Anyway, on some of the first Apollo maps she would actually read the text material [from the Apollo maps] over the telephone and Jim Pinkerton would review it--over the telephone. Jim was an editor in Menlo Park. Phoebe worked with Bill Colton.

Oh, I have to tell you a story about Jack McCauley. We were working on an illustration, and it had to do with some lunar or Mars photo; I'm not sure exactly what it was. But it had been printed reverse, or backwards. We were almost totally done with all of the annotation work on it. That's when they realized that the photo was flipped. So McCauley, me, and several others were in the bathroom of Building-Two using mirrors trying to reverse this image --in the bathroom against mirrors.

*Schaber: "Except the printing would be wrong (backwards)."*

Carroll: "Oh, there was no way we were going to be able to correct it--we were going to correct it--we were going to have to redo it. But we were in the bathroom with two mirrors (to reverse the image to right-reading), and holding a hand mirror up here and looking in the main mirror in the bathroom. We were trying to get this image to flip for him."

*Schaber: "So you worked on all of the missions up through Voyager, Magellan (1990-1994), and Clementine (1994), and then you retired in 1997."*

Carroll: "Yes, and at that time we were doing all of our maps on [Mac] computers; probably about three or four years before I retired. When we first started doing most of our work on computers, I didn't even know how to turn one on. Darlene Casebier from our group was the first one to start getting up on the use of computers in publishing, and Alex Acosta helped us an awful lot."

[Author's Note: Roger Carroll, a good friend to everyone who ever met him, retired from the USGS in 1997 after thirty-one years directing [from Flagstaff, Arizona] the production of large-scale maps of the Moon and virtually all other significant solid bodies in the Solar System (except Pluto and its moon Charon) for the Branch of Astrogeology. Roger died unexpectedly in June 2002 while working on his commercial fishing boat in Alaska. Roger's wife Linda Carroll worked for the Branch of Astrogeology for many years following the Apollo Era as a Secretary and Travel Agent.]

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**George W. "Bill" Colton** (b. 1925, d. 2005; geologist)

Bill Colton passed away in Cottonwood, Arizona on Sept. 17, 2005. He served in the 104th Infantry Division during World War II and was awarded the Bronze star. After the war, he earned a degree from Yale University. He worked for the U.S. Geological Survey for 28 years and 16 years with the Arkansas Geological Commission. Bill Colton married Jane Miskimen on April 9, 1952 in Philadelphia Ohio; they had twelve children.

Bill arrived at Flagstaff in November 1966; and in August 1969 became acting Assistant Chief of the Branch of Astrogeologic Studies. He headed up the Technical Reports Unit at the Flagstaff Field Center. Bill was appointed formally as Assistant Chief, Branch of Astrogeologic Studies in November 1969 under Branch Chief Hal Masursky. Bill worked with David Roddy on three of his explosion cratering test in Colorado, Utah and Nevada (Nevada Test Site), and participated in Roddy's drilling program at Meteor Crater, Arizona. Bill was engaged in the geologic mapping of the Branch's Cinder Lake Crater Fields. Colton also participated in geologic mapping of some of the Viking-Mars landing sites and various craters on the Moon. Bill was a Co-author, along with Hal Masursky and Farouk El-Baz, of the very popular NASA Special Publication titled *Apollo over the Moon*.

Bill left the Branch of Astrogeologic Studies In 1978 and returned to the Branch of Oil and Gas—but stayed in Flagstaff. He eventually ended up in the Branch of Central Environmental Geology while in Flagstaff. Bill retired from the USGS in June 1980 but couldn't live with retirement, so he went to the Arkansas Geological Division. He left Flagstaff in March 1981 and lived in Arkansas. Bill finally did retire for good near Christmas in 1997. He had served 48 years in the Government all together and now lives in Cottonwood, Arizona

The following was taken from an interview with Bill Colton by Gerald G. Schaber on 3 June 2002)

Colton: "I was born 5 June 1925 and live most of my life in New Haven Conn. I went to Hill House High School in New Haven. After only about two weeks at Yale University, I went into the Army in the summer of 1943. I was discharged after the war in December 1945. A month or so

later, I started back in school. I graduated I believe in January 1950. Yale was close by and my father had taught there for years (Engineering, strength of materials, engineering mechanics, etc.), so, it seemed like a logical place. After graduation I reported to duty with the USGS in May 1950. I was stationed at New Philadelphia, Ohio for some years. That is in Eastern Ohio. There was a Survey Office there, and I worked for the Branch of Oil and Gas. In 1958 our Office was transferred to Washington, D.C. In 1966 my Branch Chief asked me if I would be interested in going to Flagstaff. I had heard something about Gene Shoemaker and his Branch of Astrogeology, so I yes. I was transferred to Flagstaff, Arizona in November 1966 and I headed up the Technical Reports Unit there. At the same time, I informed them that I was preparing to go to graduate school at John Hopkins—but I gave that up because I already had 7 or 8 kids by then.

So, I went to Flagstaff. Two years later in August [1969], I began as active assistant Chief of Astrogeologic Studies, and was appointed as the Assistant Chief in November 1969 under Hal Masursky [Branch Chief at that time.]

In 1978 I left the Branch of Astrogeologic Studies and returned to the Branch of Oil and Gas—but stayed in Flagstaff. Then, for some reason other, I ended up in the Branch of Central Environmental Geology—but my field work was back in the eastern States.

In June 1980 I retired from the USGS. By March 1981 I couldn't live with retirement so I went to the Arkansas Geological Division. I left Flagstaff early in 1981 and we were living in Arkansas. In 1997—almost Christmas time—I finally retired, and I am still retired.”

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**David H. Dahlem** (b. 1935; geologist; Ph.D., 1965, University of Michigan) arrived in Flagstaff to work for the Branch of Astrogeology in October 1965. He worked under the supervision of Joseph O'Connor on the Branch's Advanced Lunar Programs Project. Over the five years that Dave was with Astrogeology [until August 1970], he was in charge of putting together [for potential use in Advanced Lunar Base missions] an alpha (radioisotope-source) spectrometer (Fig. 31), as well as the small, portable X-ray diffractometer (that was originally developed by Philips Corporation to be part of the instruments on the Surveyor Landers but never flown) (Fig. 32). These miniaturized analytical instruments, as well as others to be used in analyzing lunar rocks and soil on future lunar bases, were then being seriously studied by NASA. Dahlem was part of the Apollo Lunar Geology Experiment Team from Astrogeology (Flagstaff) for Apollo missions 11 and 12, and participated in these two missions down at Mission Control at Houston.

Dave's main job during the Apollo 11 and 12 missions was to help develop, facilitate, and monitor the use of SEARCH, a primitive database program (in Assembler and Fortran IV programming languages) and WLYBUR a large, computer text-editing program, developed primarily by Dave Dahlem and USGS computer programmer Ed Fife in Flagstaff. WYBUR, first used by the LGE Team on Apollo 12) was a large computer text-editing program, then residing in the Stanford University computer system. Both programs were used by the Lunar Geology Experiment Team. [WLYBUR was a pioneering predecessor to the now commonly-used Word Processing programs.]

The following quotations were taken from an interview with Dave Dahlem by Gerald G. Schaber on 23 March 2002:

Dahlem: "I was born on 7 August 1935 in the Los Angeles area and went to school there. I then left to go to the Montana School of Mines, and received my BS there in June 1957 and MS in June 1959 in Geological Engineering. After graduation I went to work for the Anaconda Copper Mining Company, and then to Allis-Chalmers. After that I went to the University of Michigan where I majored in geochemistry and mineralogy. I graduated from there in October 1965 with my Ph.D.; and that overlaps the time that I joined the Survey [Astrogeology] in Flagstaff. My thesis topic was on the carbonatites in the Front Range of Colorado. It was basically a mineralogy thesis.

I wanted to get into the Astrogeology program, but I didn't have any particular contacts. I didn't know Gene Shoemaker at that time. I got hold of a space science committee person. I think he was a Senator from Massachusetts or someplace in that area of the country. I can't remember his name. I just wrote him a letter--cold--and said that I wanted to join the Astrogeology program. He put me in touch with Shoemaker's group in Flagstaff. I can't remember the person who called me [from Astrogeology] originally. It may have been Joe O'Connor, or Gordon Swann. Anyway, they set up my coming [to Flagstaff. When I arrived at Astrogeology I was assigned to Joe O'Conner with the Advanced Lunar Programs Project."

[Author's Note: Dave participated in several geologic field exercises out in Hopi Buttes and Meteor Crater with fellow SPE Branch personnel his first couple of years with the Branch, where he helped set up and worked (with Joe O'Connor and Gerald Schaber) a small mobile analytical laboratory set up in the field to support real-time analysis of returned samples, etc.]

*Schaber: "How did your leaving Astrogeology and Flagstaff come about in 1970, and where did you go?"*

Dahlem; "Well, I got a chance to teach at my alma mater--Montana School of Mines. And the Advanced Post-Apollo Programs at Astrogeology was going through some budget reviews at that time, and they were threatening to zero us out in another year or so. But, of course, that didn't happen. But I took the opportunity. Actually, I was offered the opportunity maybe four or five months before I actually took the job to teach; I kind of sat around and thought about it. I liked the Astro program, but if they were going to zero it out you know I didn't know what the future was there."

*Schaber: "Are you referring to NASA canceling the Advanced Lunar Programs; Apollo missions 18-20, and beyond]."*

Dahlem: "Yes, that's right. It was the Advanced Program that was to be cancelled--and that's the program I was working on. After sitting on it for four or five months, I called the guy and said is that job still available? He said yes; so I went [to Montana]. That would have been 1970.

I taught there for about five years and I taught mineralogy and engineering geology. Then, I nearly starved to death. I started up a consulting business while I was there, and it got to the point where I was working seven days a week just to make a living. In fact, at one time I had enough jobs to hire the entire faculty of the Geology Department [at the Montana School of Mines]."

*Schaber: "So you never did get back into the Survey then?"*

Dahlem: "No, I never did. Actually, there's a sad story there. I was contacted by a fellow in D.C. in the Strategic Metals program--I believe it was in 1975. I was tied up in consulting for the summer; but I was set to go back with that Geological Survey group back there. Then, at the end of the summer when I called him and said that I was done with the consulting job that I had, he said well, we can't use you now. At that point, I went to the Grand Junction (Colorado) DOE (Department of Energy) offices that at that time was part of ERDA, and had charge of the hydro-geologic program there. That ran for about five years (1975-1980 or 1981). And then that program was also threatened to be shut down. I was in a position at that time to forecast the budgets for my part of the activity. I had a major part of the activity there. I think it was the Dingle Committee, one of the Congressional Committees that was reviewing the activity and decided that this was a uranium resource program, and the hydro-geology program was prospecting basically using hydro-geology techniques. They said that they felt that they had enough uranium at that time under tabs; and so they were going to zero out the program.

I happened to mention the fact that my job at ERDA was going to be zeroed out to a guy I knew in International engineering out in San Francisco and he offered me a job, almost on the spot. So I went out to San Francisco and was there for three or four years. I had the geothermal program, and was doing a small amount of exploration and trying to develop that program for International. International was a subsidiary of Morrison-Knudsen; and Morrison-Knudsen was trying to get into the geothermal energy business. So I had a small project, basically looking at the potential in different areas and trying to give them an idea as to where they could put their emphasis in developing a program. I also got involved a little bit in the petroleum logging work--kind of an advisor to other project offices. I was in San Francisco from 1981 to 1984.

Then in 1984 I joined the basalt waste isolation project [for toxic waste siting] for DOE. Basalt was one of the three candidates at that point---basalt, salt and the Yucca Mountain site in Nevada. I was there until I think 1990--maybe the end of 1989 or early 1990.

Then I came across an opportunity with Waste Management to do some work in their waste program. And for the first year I was a project manager on small geochemical projects. After a few months, I went out to California to run a program on at a RECA [government's toxic waste reclamation program] site in Union City. Anyway, I ran a small project there for about a year; then they asked me if I wanted to go foreign. So I went over to Czechoslovakia to Prague and was formally employed through one of their sub-companies which was Waste Management International. We were over there for a couple of years, and then the program was somewhat de-emphasized, and we came back in 1992--I guess it was. I came back into the Grand Junction field office--but the contractor side of that. That contractor was Rust Engineering. I was still with Waste Management because Rust Engineering was part of Waste Management. Then I was basically let go as part of this early retirement business you know (from Waste Management). That was in 1995.

Then, at that point we looked around for things that would have some technical bent and some people-oriented activity; so we picked up a franchise in Terminex Pest Control. We started that franchise in 1995 up in Spokane, but soon found out it was too cold up there to run the business.

So we transferred our interest down to Kingman, Arizona in the middle of 1996--and we've been here ever since.

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**Kenna (now Krista) Edmonds** (B.S., psychology, 1962, University of New Mexico) came aboard to work with the Branch of Astrogeology in March, 1966.

The following was taken from an interview with Krista (Kenna) Edmonds by Gerald G. Schaber on 11 May 2002:

Edmonds: "I was born in New Mexico on a ranch in the southern part of the state. I grew up in a ranching family. I went to the University of New Mexico and graduated in 1962 in Psychology. I married Jack Edmonds that fall and moved to New York. I lived there for a year. I had a job doing technical writing for one of the Tech-Hughs Company--Johnson and Johnson Companies

We lived there for about a year and then came back to Albuquerque for Jack to work on his Doctorate (in geology). Now we are up to the fall of 1963. We stayed there for an academic year and then went to the University of Arizona (Tucson) for a year and a half. At that time I was working in administration for engineering companies. We ended up moving to Flagstaff in the fall of 1965 when Jack went to work for the Water Resource Division of the USGS. I then started into graduate school at Northern Arizona University.

Then, Jack ran into Branch of Astrogeology geologist John MGonigle on the street one day. He and John had been at the University of New Mexico together, and Georgia [John's wife] and I had been in the Psyche Department together at UNM. So they came over for dessert and coffee one night shortly thereafter--and that point they were the only people we knew in town.

John said to me one day, we are looking for a human resource-type and Paula Ables [the then present Human Resource, or time-and motion person at the Branch of Astrogeology] is leaving. At that time there was a [Survey] hiring freeze; so, I got my paperwork together, and the whole thing had to go through Secretarial approval. Udall was the Secretary of the Interior then.

I actually came on board with the Branch of Astrogeology in March of 1966. I worked for [Al] Chidester through Maury Brock.

What the Branch needed were a bunch of human factors-based time-and-motion studies on all of the processes that astronauts would use doing geology on the Moon---how do you pick up a rock, how long does it take, what's the traverse time from point A and point B, how many feet was that; how long would that take in a space suit; how long would that take in 1/6 G?--and we didn't have a clue!

At that point we were still using a tablespoon to pick up dirt samples--and canvas sample bags. Then it went to that little rock-grabber thing—as we advanced the lunar tool concepts. We were just making the whole thing up as we went along



By April 1966, only a month after my arrival, I was already attending the aerospace medical association meetings. In May 1966 we had three weeks of field test in the Hopi Buttes [9-27 May 1966]. Only one week they had the spacesuits there, but the rest of it was pretty intense too.

I also worked with several people [at MSC] in Houston to try to extrapolate--or predict timelines--from that. But that wasn't based on] just that first test; that was a result of the first two years of data

George Ulrich told me later was that when NASA started planning the real lunar missions (from Apollo 13 on); they (at MSC) looked around, and the only data they had to predict how long each activity was going to take were my reports. I have always been kind of pleased about that. And I could bring in all of my human factor work to bear on it. But the timelines themselves turned out to be probably what paid my tariff and half the rest of the group's anyway, because I had shown ways that things could be done more effectively.

I left the, by then, Branch of Surface Planetary Exploration in September of 1968 after Arnold Brokaw arrived as Chief at the Branch in Flagstaff. I was in Flagstaff for two and a half years (until September 1968). At that time Jack had got a teaching job at a college in California.

Now one of the things that I did get to do--and very few people recognized it. I got to go out to Scripps Institute of Oceanography. They had a lunar lighting lab that was the best in the world. I did a study on what the effects of lunar lighting (the columnar lighting with no diffraction) was going to be on examining lunar rock samples.

Yeah, I went out and played in the lab out there for a couple of days, and then Dave Schleicher came out and we worked for another couple of days. We came to the conclusion that columnar lighting was going to be a real enhancement--which was not what we were expecting. It really enhanced the details. We looked at basalts, and the other types of rocks that you guys expected to find on the Moon."

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**Kay (Larsen) Edwards** (b. 1942; cartographer/computer programmer) was another early arrival at the Branch of Astrogeology in Flagstaff, reporting for duty at the Astrogeology wing at the Museum of Northern Arizona on 2 December 1963. Kay would become a mainstay in the Branch's computer and cartography groups for many years; she would be a driving force in helping to create the excellent reputation of the Branch's lunar and planetary cartography and image processing groups. After working 33 years with the USGS, Kay retired in 1996.

The following was taken from an interview with Kay [Larsen] Edwards (Hearne, TX) by Gerald G, Schaber on 8 May 2001:

Edwards: "I was born in 1942 in Palm Beach, California, and went to school there through high school and the first two years of college. In college I was majoring in mechanical engineering. Well, my mom wanted me to go into accounting and my dad wanted me to go into engineering, so I took some of each, and I said forget the accounting. My dad was into math and that sort of thing, and he got me interested in math."

So I had the two years of college, and then I got married to Fred Larsen in Flagstaff--that was in October 1962."

*Schaber: "What was Fred doing in Flagstaff?"*

Edwards: "He was a student also; he also had only two years of college, and then he decided that he wanted to go into Forestry--and Flagstaff had a good Forestry School at Northern Arizona University. So that's how we ended up in Flagstaff. My first year there, 1962, I worked at Babbitt's Department Store as a billing clerk for a year. Then we went back to California for the summer because Fred could make more money than in Flagstaff. And then we came back to Flagstaff again, and I was determined that I wasn't going to work in another clerical job because I couldn't type even 30 words a minutes.

So I ended getting a job for a month as a replacement for a secretary at an engineering surveying firm in Flagstaff. The boss was away about two or three days of the week all the time, so the guys would cover for me while I went out looking for another job. This was in November 1963, and a friend of Fred (another Forestry guy)--his friend's wife, worked at the Survey (USGS).

So I wanted to go find the Survey—and well I can't find it. So I called the Chamber of Commerce, and they didn't know where it was. I happened to see a USGS truck and I followed it. Fortunately it wasn't going out into the field. So it went out to the Museum of Northern Arizona (NAD) where Astrogeology was at that time, and I interviewed with Gene Shoemaker. He hired me. I interviewed with him I believe on a Thursday, and he said come to work Monday. That was 2 December 1963. So, that's how I got started at Astrogeology in Flagstaff.

I was working for Jack McCauley "weighing shadows" on the Moon--you know trying to figure out the percentage of slopes and that kind of things back when we didn't have any computers at all.

Not long after that I started working with Ray Batson, doing image mosaicking and getting ready for the Lunar Surveyor Project. We did a lot of field tests out there on the Bonita Lava Flow [in Sunset Crater National Monument just east of Flagstaff]."

*Schaber: "That was back in 1964-right?"*

Edwards: "Yes; we had a Surveyor-type camera set up on the aa lava [Bonita Lava Flow] and we were taking panoramas that had the same field of view as the cameras that was going to be on the Moon on Surveyor Lander. So, we were practicing how to deal with all this stuff; how to map it, and what kind of projections we should put it on, and how fast we could do it because the images were going to come down every 3.6 -so we could keep up with it."

*Schaber: "Who were you working with out there?"*

Edwards: "There was Ray Batson of course."

*Schaber: Was Jim Alderman there?"*

Edwards: "Alderman came in a little later. I didn't really work with him.

Hank Holt showed up not long after. There were 14 people when I started at Astrogeology. Jody [Lowman] Swann started on the same day. At first there was just Ray and I working on the Surveyor thing. We had Bob Blecha [Astrogeology's first machinist]. Blecha was building things for us. So Blecha would build us that framework you know, that looks very much like the Surveyor. We had prototypes of the real Surveyor landing pads placed the right way so that we knew if we were going to see the feet [landing pads] at certain camera elevation--and that type of thing. And we had our USGS photo van out there at Sunset Crater as well."

*Schaber: "So the first Surveyor Mission was in 1965? So you then went out too JPL and worked on that"*

Edwards: "Yes, and of course before we out to JPL we hired a bunch of kids to help--Rich Tyner, Alvin "Al" Dale, Charlotte Wheeler, and others. [Author's Note: Also working on the Surveyor project Team from Flagstaff were Elliot Morris, Ken Stice, R.A. Henry, Henry Holt and Ray Jordon; (see Fig. 55).

*Schaber: "So tell me about your experiences after Surveyor-1, actually landed—surprising everyone."*

Edwards: "Oh yeah!"

*Schaber: "and you had 11,000 pictures. However, one person told me you were ready, and another person told me you weren't ready for all that."*

Edwards: "I think we were ready! I thought we did pretty well. You know, we went out to JPL before the mission several times for simulations, or SIMS, and they had the Surveyor test vehicle on top of a building, and were taking pictures in a panorama of the whole area around JPL you know. And we were trying to put those together.

Of course, most of the time we went out there, they couldn't get the camera to work or we couldn't get the machinery to work, you know, and we just sat around playing cards and that sort of stuff. But eventually, they finally got that working. But with our simulations in Flagstaff and everything else--we were ready. I think we kept up with that mission just fine."

*Schaber: "So you get out there [at JPL], and your job basically was to sort of coordinate these kids putting together [the Surveyor photo mosaic, for example]."*

Edwards: "Yes, and I catalogued everything. I did it all by hand. Well, actually, we had a computer by then, an IBM-360. I punched [computer] cards with all of the azimuths and elevations--and time-- for every image that was taken."

*Schaber: "It's hard to believe, isn't it?"*

Edwards: "I think we ended up with 65,000 images for all of the Surveyor missions. There were 30,000 on Surveyor-VI, I know that."

*Schaber: "Well, you must not only have not only catalogued [images], but you must have done some of the trajectories, the aiming, and that sort of stuff."*

Edwards; "Oh yeah, I did the planimetric map showing a rectified map of where rocks and craters were located around the Landers on the Moon. I put out where the craters were, and you know--did that kind of thing as best I could. And I also helped locate where the Surveyor's landed. You know, locating the craters on the horizon-- and trying to resect. I did this for all of the missions--that landed.

I remember the first day after Surveyor-I landed. I didn't sleep for 40 hours. We worked as long as the Moon was up. Then, I would go to bed--but I couldn't sleep. My parent lived not very far away in Long Beach. I was here I was at JPL. So, I drove down there and visited them, then came back to Pasadena, and went for another 16-hour shift at JPL.

And maybe 3/4 of the way through that shift I was getting so "goofy" I remember Ray Batson had a cookie in his hand. So I just walked by and just took the cookie out of his hand and ate it. You know I was so goofy that I didn't know what I was doing. And they would say oh, maybe you had better go lie down and take a nap now!"

*Schaber: "I know what you mean!"*

Edwards: "So I finally conked out about after that, but it took almost two days before I could go to sleep --I was so excited!"

*Schaber: ""I remember during Apollo 11 at Houston Mission Control I started seeing things."*

Edwards: "Yes, I never quit being excited on those missions."

*Schaber: "So you were going out to JPL during that period what--several times a week or something?"*

Edwards: "Oh yes, we were going out twice a week. We'd take the train out and go out to JPL for the day--and sometimes we'd come back that night on the train. Then, we'd go out again--say, like that was Sunday night and Monday night--and then we'd go out Thursday night and Friday night. We knew all of the porters on the train. We knew everybody in the Dining Car. I'd go in [the Dining car] and they fixed my breakfast for me--I didn't even have to order it."

*Schaber: "What did you do after Surveyor?"*

Edwards: "Lets see; I took a FORTRAN-2 programming course on an IBM 360/65. We got a plotter, and we could draw grids. We were drawing grids for Surveyor, and then we started doing

stuff for Apollo. I was mainly writing one program, on cards again. Then, we went into Apollo; so I'm working on Apollo most of the time, but I am learning how to do programming at the same time. But we didn't use much for Apollo; programming was still too primitive at that point. So, for Apollo, I did the mosaics and the planimetric maps of where the astronauts took their samples.

*Schaber: "So, you were involved with us down in Houston during Apollo 11--right?"*

Edwards: "No, actually I wasn't. I didn't get called on until after the Apollo 11 splashdown; then I got called out to Houston to help. So the first one I was actually in on for the mission was Apollo 12."

*Schaber: "So, what were you doing then during the Apollo 12 mission in Houston then?"*

Edwards: "The same thing--like I did on Surveyor."

*Schaber: "Keeping track of what they [the astronauts] were doing, etc.?"*

Edwards: "Yes, we had a Polaroid camera that was taking pictures off of the TV. So, we make panoramas and put them down [as a pan mosaic], and make planimetric maps, and locate again on vertical photography where each station was when they started getting away from the spacecraft--and that kind of thing--and just mapping where they went."

*Schaber: "Using the Polaroid pictures."*

Edwards: "Yeah, at first, and then when they brought back the actual photographs, we would do better maps."

*Schaber: "We used some of those Polaroid pictures [surface pans from the TV] in some of the earlier [NASA] reports."*

Edwards: "I think so--sure, for the 8-day or whatever report that was."

*Schaber: "So you were with the Geology Team down in Houston for all of the Apollo missions except 11--right?"*

Edwards: "Yes, I was---all of the way through. I remember that Jack Schmitt came in the USGS trailers down at MSC and helped us figure out where things were. And I remember--let's see Dave Scott was on Apollo 15-- he came and helped us locate things. That was cool."

*Schaber: "So after Apollo, you helped write up the reports after all of these missions. I mean you helped do some of the illustrations."*

Edwards: "I did some of the illustrations, and I did the map and that kind of thing."

*Schaber: "So about the middle of Apollo, we started getting Mariner 9 images [from Mars]. Did you work on that data?"*

Edwards: "I didn't work on Mariner 9. I think I had moved over the computer group by then. I worked for Jim Crawford for a little while in what they called ISD. I didn't start working on some of the Mars pictures until Viking [summer of 1976]."

*Schaber: "So right after the end of Apollo you moved over to the Computer Division at the Flagstaff Field Center."*

Edwards: "Yes, because I really wanted to get into computer programming.

Then, I went back to work for Larry Soderblom with Astrogeology again. I didn't work for Jim Crawford very long--but I stayed in programming."

*Schaber: "So you stayed in programming. What were you doing when you started working for Larry?"*

Edwards: "I was always in [cartographic] geometry. I always got into that part of the problem. So my whole thing was trying to get the images in the right place. And that was my job for the rest of my career.

*Schaber: "You had to learn that stuff, right?"*

Edwards: "Larry Soderblom tutored me through all of that. I didn't learn any of that in school. I remember him spending a lot of time helping me learn this stuff."

*Schaber: "Then, I guess Voyagers 1 and 2 came along, starting with Jupiter flyby encounters in 1979, followed by Saturn in 1980 and 1982, Uranus in 1986, and Neptune in 1989."*

Edwards: "I started out developing the programs for Viking, and then we applied them to Voyager and kept improving them. There was a whole team of us, including Eric Eliason, and others. Each one of us had a section of the processing. I had the geometry, and Alex Acosta did the reseau-finding [the small X's on the images], and Eric did a lot of the image filter kinds of things. I think he worked on the photometry with Larry Soderblom; so we each had our own specialty which I though worked very well."

Once I got into the programming, we never did much traveling at all, except when Eric and I went out to support on of the Voyager encounters--Uranus I think it was.

We did all of the programming and all of the mapping; we pretty much scooped JPL I know that. We got the first good map of Triton. That was fun; and it was so exciting."

*Schaber: "Well, Voyager was remarkable in what it did and what it saw."*

Edwards: "I know; those satellites of Jupiter were awesome."

*Schaber: "So after Voyager, did you do anything with the later missions like Magellan?"*

Edwards; "For Magellan [1990-1994], I did basically an automated, mosaicking, bookkeeping kind of jobs--much different than anything I had done before. JPL had already geometrically corrected the images. It was radar, which I didn't know how to do yet. And Larry [Soderblom] told me not to do that, but to take their stuff and put it down in the right spot. So it was basically a major bookkeeping job--you know kind of liked database stuff? There were some database programs out there, but they were too slow. So I wrote some real specific stuff to deal with all of that."

*Schaber: "When did you leave Astrogeology, and what were you doing about the time you left?"*

Edwards: "So, the last six months I was working there at Astrogeology they let me do anything I wanted to. So, I was trying to map lumpy things, like Phobos one of the moons of Mars] and asteroids, but I kept losing my funding and I would have to quit again."

*Schaber: "Did you actually retire when you left Flagstaff, or did you go to another job?"*

Edwards: "I retired [in 1996]! I didn't go to work with Mark until after a year after I was here [Hearne, TX] (and retired)."

*Schaber: "Kay, please summarize for me your thoughts about the time you worked with Astrogeology."*

Edwards: "Well, let's see; I hated Flagstaff; absolutely hated it."

*Schaber: "Because of the cold, or what?"*

Edwards: "Yeah--and it was in the middle of the desert; but, I wouldn't go anywhere else because my job there was just too different and interesting. I can't say anything else besides that--Where else could you do what we did?"

*Schaber: "The Branch of Astrogeology [and Gene Shoemaker] kind of got together a group of people at the right time and the right place that seemed to want to get things done, and work hard. Overtime wasn't even a question. People worked so many hours over their ordinary eight-hour day obligation- and they wanted to! You know, you couldn't find people today who would do that."*

Edwards:" No; most everybody was really good. And we did fun and exciting things all the time."

*Schaber: "And we had to invent new things."*

Edwards: "Yeah, we were always breaking ground on new problems. One of the important reasons that we succeeded like we did was that leaders like Gene Shoemaker and Larry Soderblom let the job fit the person instead of making the person fit the job; they didn't try to force a square peg into a round hole. It made all of the difference!"

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**Richard Elton Eggleton** (b. 1932; Ph.D. University of Arizona 1970), one of the earliest of Gene Shoemaker's recruits for his fledgling Astrogeologic Studies Unit, arrived at the USGS' Field Center at Menlo Park, California, on October 11 1960. At that time, the Astrogeologic Studies Unit had only been in existence for ten days (since October 1 1960). Eggleton was first hired by the USGS in the fall of 1955 and was formally assigned to roster of the Minerals Branch, although they wanted him eventually assigned to the Engineering Branch. Dick would later receive his Doctorate at the University of Arizona in Tucson.

The following quotations have been selected from an interview with Dick Eggleton by Gerald G. Schaber on 3 and 15 March 2001):

Eggleton: "Until I was ten or twelve years old, I spent my summers in northwestern New York State with relatives. My mother and brother and sister--and the relatives--lived up there in Lowville, New York. Both my parents came from the vicinity of Lowville. Then, after the summers that I spent there, I spent summers at the U. of Michigan Biological Station, located about twenty miles south of the Straits of Mackinaw on Douglas Lake. That from somewhere around age ten to 1953 (last summer there)--that was grade school, junior high, high school and college years (many of them but not quite to the end of college)"

The camp [at the Biological Station] had a roster of some 200 people, typically. There was a commissary where everybody ate--and I worked in that about three years in high school, and maybe early college. Then, I also ran the supply stock room for three or four summers up through 1953.

*Schaber: "What interests did you have as a youngster; were you interested in geology at all?"*

Eggleton: "One of the last few summers that I was at the Biological Station, a quaternary geology professor From the University of Michigan--James Zumberge--came up and conducted a field trip, and I got to go along on that. That was my first real encounter with geology. My dad was an invertebrate zoologist and limnologist, and he had kind of a side interest in geology. He had worked on some glacial stream -water fall-plunge basins in northwestern New York.

*Schaber: "How old were you when this professor came to give this talk at the Biological Camp?"*

Eggleton: "Around twenty. As a kid, my dad shared with me a book on general science, I think, from the University of Chicago. I think it was probably multi-authored. There were chapters in there on astronomy and geology, and other things. I also remember- (in grade school now)--in regard to that book. I remember articles about the Solar System, particularly in the National Geographic, with paintings of the different planets--and about the time I was seeing those things, I remember sitting up in my room and looking out the window at the Moon. And as a grade school kid, I drew pictures of the Moon and some of the other planets. So I did have that interest. Then, I didn't have any particular interest in geology or astronomy until late college years.

However, in high school, I did take a chemistry course and a physics course. I was on a college prep program and took all of the mathematics that was available up through algebra in high school.



To back up a little bit--in eighth grade we did a career study project. My dad had always had a lot of frustration about his teaching career and the conflict of spending his time on teaching and working on research. He worked a lot of long hours--and I thought that I didn't want to follow that path. So in this career study I decided that I should head for mechanical engineering, engineering in particular--I didn't get strong feelings about a choice [of engineering type].

Earlier, had an earlier general science class that I enjoyed a lot I recall.

The class went out for bird walks in the morning, starting about 5:00 AM once or twice. I remember the teacher asking us if we could figure out why the Moon looked reddish during eclipses. I think I came up with the right answer on that. I generally enjoyed the science class--but again, it wasn't particularly astronomy or geology that grabbed me at that time

Then in high school, my physics class was in my senior year, and chemistry before that in my junior year. I really got a charge out of that physics class. It was really pleasing to me to study so much material that explained how so many things worked.

So, as a result of that--sometime in high school--probably my senior year--I was making moves to get into the Engineering School at the University of Michigan. Because of the physics class--when I found out there was a physics major--I made that selection. So, I launched into college on that course. I had quite a few courses in chemistry, engineering, mechanics, electrical, mechanics, physics, and mathematics.

It might be worth commenting that the engineering school had its own series of English classes that had to do with reading some literature, and writing. One or two of them were speech writing. But other than that, I had only two other electives that weren't math, engineering and science. I took one course in anthropology and one in economics. I had quite an aversion to economics but I thought I should learn something about it. I succeeded in not learning very much!

My college education was extremely technical, mathematically and scientifically inclined. I started out great. I was always a slow reader and had trouble writing. I probably had all A's and B's the first couple of semesters. Because I was kind of a slow student, I was real studious all the way through school--at least starting with Junior High when we started getting homework. The physics major in Engineering School and a Mathematics Major in Engineering school were very similar. To do an Engineering School degree it was a minimum of 140 hours, I think. So that required either nine semesters--four and one half years-- or an extra semester or an extra summer school. And since the math and physics programs were so close,-and I was taking a lot of both, I ended up choosing to get both degrees and I actually went five years. I started in the fall of 1949 and finished in the fall of 1954. They [the degrees] were both called Bachelor of Science in Engineering--then in parenthesis it said Mathematics one.

I was engaged to be married in the fall of 1953, and was married in June 1954. I finished up one BS in January 1954--and the other in June 1954. I married one of my dad's students that I had met the summer of 1953 at the Biological Station. She graduated with the BS in Zoology. The fifth year of my Bachelors work, I took one semester of general geology and a second semester of

historical geology. In the second semester I also took a beginning mineralogy and petrology course.

Well, what happened was, I had --I think the year of 1952-1953--second semester--I got extremely depressed [over romantic matters].

I started to shift toward geology my fifth year of my Bachelor work. Then I went on the following year to take two semesters and the geology field camp--and did a thesis project the year of 1954-55, and finished up that Masters in the fall of 1955-56 [University of Michigan].

The geology department really didn't have a geophysics program yet, but they had a geophysics professor who was trying to get one cranked up. I think, maybe that year that I was in the department, he was chairman of the department. So, the program I did was kind of geophysics. Viewing it as geophysics made it easier for me to do complete the degree that quickly.

My thesis problem was under him [the geophysics professor]. What I did was install some devices on a small ice-covered lake in the wintertime. I would chop a hole through and hang a weight down at the bottom. I would string the top of the weight to the top of a dial gage that measures small distances. This allowed me to measure the up and down movement of the ice surface in fractions of a millimeter. What I was looking for was seiches in the lake (Swiss word)--that's the wave mode in which the whole body of the water slops back and forth from one side to another. Lake Erie and Lake Ontario have that sort of motion.

During that year, I had job offers (1954-1955) from Shell Development in Houston where I would have done research on special techniques to milk the maximum amount of hydrocarbons out of oil and gas fields. Then I had another job offer from Texaco in field geophysics--and another from the U.S. Geological Survey. It was the Survey's Engineering Geology Branch that was interested in me (headquartered out of Denver, Colorado). I took the Civil Service Placement Exam. That exam and my degrees got me the employment. As soon as I finished my geology field camp (out of Boulder, Colorado) the summer of 1955, I did the final flap of getting my thesis together and came on with the Survey in the fall of 1955 (probably September). Up until that time I had been living in Ann Arbor.

The USGS actually put me on the Minerals Branch roster [that included one Gene Shoemaker at that time]--even though it was the Engineering Branch that wanted me in the survey. I had a temporary assignment [in Washington DC] on the review staff of the Geologic Names Committee. There were, I believe, four reviewers; Walter West was the Secretary. George Coey (sp?) was head of the Names Committee. The actual Committee members were various geologists from P&S (Paleontology and Stratigraphy Branch) and other parts of the Survey. So, this gave me a year and a half that I just spent my time reading and reviewing reports for consistency in the use of stratigraphic names, and the Names Committee kept a file of all the names whose usage of which they had approved. The Geologic Names Committee oversaw the evolution of the stratigraphic nomenclature relative to accepting the proposal of new names and changes and revisions, etc.

*Schaber: "It is interesting that you would later use that experience in developing lunar stratigraphy and nomenclature."*

Eggleton: "Yeah, it was a help in that. So, I did that from September 1955 to June 1957 and was then transferred to the Engineering Geology Branch in Denver. Our first child was born in October 1956, so he was about nine months old when we moved to Denver. I was assigned to a project on the Sun River about 50 miles south of Glacier National Park, which was being run by geologist Melville R. Mudge. I spent that summer and a good part of the following year with him. So, my son spent his ninth through eleventh month camped out up there. Mel had his family up there too. So that was a pretty neat introduction to field life in the Survey.

In 1957 I finally got connected with Astrogeology—and with Shoemaker.

I did a field trip with Gene into New Mexico and Texas in the summer of 1957. We visited Odessa Crater and Sierra Madeira [near Ft. Stockton, Texas, where we watched the comet Mrkos overhead at night]. We talked to Glen Izett who had done geologic mapping at Odessa Crater. That crater is just a few hundred feet in diameter; however, it [was thought that it] might be a companion to Meteor Crater. The meteorites are pretty similar. Generally it is of the same age [around 50,000 year]. We found out that Glen had done the geologic mapping there, and we were trying to get him to get it published. He has a report in one of the cratering Conferences. AEC was partly behind that cratering conference in which Gene and I released our global survey of terrestrial impacts.

In the spring, he and I did an engineering geologic study for the Dulles Airport site. That was in the Virginia Triassic. The airport construction was already under way, but we did some studies of the jointing, etc. June of 1957 was when I moved to Denver; so we did this study the spring of 1958. Then I assisted him in calculating the volume of some big landslide deposits and talus accumulations at the foot of the mountain front up in the Sun Mountain area.

After that, I continued working on the geology of the 7.5-minute quad that included the Dulles Airport; I went back there for two or three field seasons.

However, ever since the Sputniks went up in [4 October and 3 November 1957] I started thinking we are going to be doing the geology of the Moon one of these days, as space exploration unfolds. So, I wrote up a one or two page outline of some of ideas of things that we could do from the earth in support of getting ready for that.

*Schaber: "Just on you own?"*

Eggleton: "Right. And I think I did that one of the times that I was out in Virginia doing fieldwork--probably in the summer and fall of 1958. I was there in the spring doing engineering geology with Mel Mudge. Then, it was decided that I would finish up the map of the quadrangle, so I went back in the summer of 1958. It was at that time that I wrote out this outline [of lunar exploration ideas], and I shared that with the Engineering Geology representative [from D.C.] who was Alice Allen.

I've got a correspondence file here--a couple of them. I made a file of activities that I was involved in prior to the establishment of the Branch [of Astrogeology]. [Author's Note: Dick still has his original outlines of lunar exploration ideas dating back to that summer in 1958].

It was either summer or fall 1958 that I went out to finish up mapping the Herndon quadrangle [near Reston, Virginia] where the Dulles Airport was being constructed. At that time was when I wrote out my notes of my ideas and shared them with Alice Allen (the Engineering Geology representative at DC). She heard about Gene Shoemaker's stirrings [i.e., Gene's putting together the astrogeology Branch] and put the two of us in touch. I think he and I got together for an introductory conversation.

[Author's Note: Dick Eggleton's personal files from that early period [1958] includes notes that he made at a rather important meeting -- a first meeting in Washington D.C. organized in D.C. by Gene Shoemaker (and others like Assistant Chief Geologist Montis Klepper and William Fischer from the Survey's Photogeology Branch) of survey scientists who might be interested in getting into lunar research. The meeting probably dates from 1958. That was the year that the Survey's Plateau Uranium Project folded, so the USGS was looking for new sources of funding; see main text 1958].

*Schaber: "Did he [Gene] invite you to that meeting in D.C. to discuss lunar exploration ideas meeting?"*

Eggleton: "Yeah. He knew all about my educational background and was anxious for me to be part of the [Astrogeology] program. He was trying to dump the program responsibility onto Ed Chao, but Chao outmaneuvered him. At least, this is my understanding of it. There were about fifteen people from a bunch of different Branches. I've been trying to find those two documents and haven't done so yet. I don't think that Wilhelm's book [Wilhelms, 1993, 477 pages] includes any data regarding that meeting."

*Schaber; "So, you attended this meeting Gene and Klepper Organized in order to get together people at the Survey that were interested in geologic mapping of the Moon, and other forms of Astrogeology research? Is this right?"*

Eggleton: "Correct, all kinds of astrogeology-type research was discussed. Frank Senfle (solid state geophysicist from the National Bureau of Standards), Ed Chao (USGS Geochemistry and Petrology Branch), and William "Bill" Fischer (USGS Photogeology Branch) were active participant at the meeting.

He [Gene Shoemaker] was with Mineral Deposits [Branch]. I think he may have been living in Grand Junction. He had already been working on Meteor Crater and mapping Copernicus Quadrangle.

Gene had a connection to Bob Carter at ACIC, who were making the Air Force base maps--the early LAC Charts. They were just getting started on that and the two of them were developing their ideas at the same time and Gene was mapping [the crater Copernicus] on that Mt. Wilson photo.

Gene may have real busy getting his thesis finished up [in 1958], and otherwise wheeling and dealing. So I think he didn't necessarily want to run the thing himself, but to get somebody else to carry a lot of the administrative load. But, of course, it evolved directly into him [Gene] having the reins, and being in charge. I think he principally spearheaded writing up the concepts for the research group [Astrogeology] and getting it through the Survey. I think Gene must have done most of the wheeling and dealing--you know--talking to Survey people, NASA people, Air Force people at ACIC, and everybody else. Well that's what turned into the Astrogeology Branch.

There was, however, a parallel effort going on in the Survey's Military Geology Branch. Arnold Mason was the principal actor in that, but he was a close collaborator with Bob Hackmann. The two of them authored that four-sheet special study of the Moon [see 1959 in text]."

*Schaber: "Where was Bill Mason and Bob Hackman's work being done?"*

Eggleton: "In Washington. I have personal notes and correspondence. My notes start in 1957 or 1958. Well, Dick Van Horn with Engineering Geology here in Denver was interested [in lunar research], and I believe I have some correspondence with him from 1958. The first memo that I wrote to Dick Van Horn with Engineering Geology was April 3, 1958. Mason and Hackman were either in this Photogeology Unit or in Engineering Geology [with the USGS]. The Military Geology Branch [Mason and Hackman] got a contract with the U.S. Army Corps of Engineers to do an engineering special study [of the Moon].

*Schaber: "It was basically a terrain map wasn't it?"*

Eggleton: "Well, they did a simplified geologic map. It has, I think, pre- and post-mare craters and mare units, and the old highlands. It had a basic stratigraphic format."

*Schaber: "Were Mason or Hackman in attendance at that first USGS meeting on lunar work in D.C. in 1958 you talked about?"*

Eggleton: It's likely that Mason was there, but maybe Gene felt he was kind of in competition with them [Mason and Hackman], and managed to dodge them.

*Schaber: "So the general result of that 1968 meeting in D.C. meeting was that there appeared to be a number of people interested--right?"*

Eggleton: "Right. This evolved into the definition of the lunar research group program that was proposed to NASA. I have references to Andy Anderson who was Chief of the Mineral Deposits Branch; and he was a good help to Gene in getting this stuff going. So Shoemaker was instrumental in drawing these interests together and describing them as a research program, and getting the approval and agreement of the necessary administrators in the Survey--including Andy Anderson. Vince Mckelvy (as Chief Geologist) was in the picture at that time too.

And then Gene also was negotiating with the people at NASA, and finding out whom to talk to. He gradually became acquainted with people like Orin Nicks [deputy to NASA director Homer Newell, and director of the Lunar and Planetary Programs.]”

*Schaber: "So after the 1958 meeting in DC on lunar geology interest in the Survey, did Gene contact you and offer you a job?"*

Eggleton; "Well, he couldn't. He made it clear, however, that he wanted me to be part of this group. I have a letter from Ed Chao--I think it's dated July 1960--saying that they were going to hire me when they got the money.”

*Schaber: "The Branch was just an Astrogeologic Studies Unit out at Menlo [Park] at that time."*

Eggleton: "Yeah, it took a while for it to be established as a Branch.”

Eggleton: "I first reported in Menlo 11 October 1960. I guess that the group [Astrogeologic Studies Unit] began its existence October 1, 1960, and they wanted me there as soon as I could get there after that. That was the beginning of the Fiscal year and the funding [from NASA].

Since it had started 1 October 1960, I think that the [USGS hiring] papers had already been completed for Chuck Marshall and Hank [Henry] Moore. I think Elliott Morris was a tad later. I have some correspondence about him coming on.”

*Schaber; "So when you first got there, who did you work under--Gene obviously--and what were your assignments?"*

Eggleton: "I worked on the Unit's first semi-annual and annual report [of Astrogeology]. I became a co-author on the paper entitled "*Interplanetary Correlations of Geologic Time.*" [Author's Note: A "classic" in astrogeology]. Then I did some editing, calculations, and illustration work for a couple of the other early things that Gene was working on at that time. I was doing some crater counts.

Oh, Gene and I did a compilation of known and probable impact structures [on the earth] I think in 1961-1962 that came out as a gray-literature Summer Study Report.”

*Schaber: "You were involved in lunar mapping also-right?"*

Eggleton: "Right. He [Gene] had me serve as the coordinator for lunar mapping in 1961.”

*Schaber: "Because Wilhelms wasn't there-right?"*

Eggleton: "Right. Don came out a little bit later. So in 1961 and 1962, I started working on the Rhipaeus Mountains (later renamed Montes Rhipaeus) Quadrangle.”

*Schaber; "You were using the Lick telescope and other telescopes out there [in California] until Lunar Orbiter came around-right?"*

Eggleton: "Yes. When we got going on the observing program, we used both the Lick 36-inch refractor at Mt. Hamilton and the Lowell Observatory 24-inch refractor. Primarily we were mapping off Gerard Kuiper's [U. of Arizona, Tucson] Lunar Photographic Atlas. He had both similar Atlases by the Air Force, and another one published by the University of Chicago.

At that time, one of Elliott Morris' main responsibilities, along with Hal Stephens, was making copies of telescopic lunar photos from various observatories. So I worked on Rhipaeus quad. I was coordinator for at least a year [on the lunar mapping project]. Hal [Masursky], I think, took that over temporarily, and probably passed it along soon thereafter to Wilhelms."

*Schaber: "When did Hal arrive? He showed up sometime after you, right?"*

Eggleton: "Oh yeah. [Don] Elston, [Don] Wilhelms and [Hal] Masursky (and Mike Carr) all came along about a year or two after October 1, 1960. McCauley came on in the Flagstaff office."

*Schaber: "Yes, Jack came over to Flagstaff to work with Chuck Marshall on the telescope "seeing" project and they had an office on the second floor of the Arizona Bank Building in 1963.*

Eggleton: "We did crater counts in these early papers with Gene; these first few months. It involved counting the astroblemes in the Mississippi Valley area. We did a thousand square miles or kilometers there with about ten impact structures in it, then compared them to some lunar counts. I think we used Hackman's crater counts--the traditional negative slope cumulative size-frequency distribution curves. I spent quite a bit of time--and Gene did too--culling literature and compiling this information about terrestrial impact structures--and global distribution [on impact craters]. I was seeing his Copernicus reporting and Meteor Crater stuff. I don't think I reviewed those two, but was handed them to study. The Hopi Buttes stuff was significant too--for characterizing the maar-type craters --analogous to what we called "black halo" craters on the Moon].

Gene and I, and our families, once again camped out in tents the summer of 1961 at Sierra Madeira [Southwest part of Texas, near Ft. Stockton and the Glass Mountains] for six weeks, and started the geologic mapping."

*Schaber: "So, this was a crypto-volcanic structure of some kind--or they were trying to figure out if it was an impact structure?"*

Eggleton: "Well I think Robert Dietz [in addition to Shoemaker, Mason, and Hackman] had visited Sierra Madeira [in 1959] and found shatter cones [indicative of the high energies related to an impact structure]. Therefore, it was considered to be a probable impact structure by us. So Gene and I did an early abstract for the 1962 GSA meeting about the breccia lens that makes up most of the central peaks. The central mountain/peak may be about a mile in diameter, and then there is a plain surrounding that, and then there's a ring with about a 7-mile diameter (or kilometers) with low-relief hills and folding and faulting in the minor stuff. So the central mountain is pretty close to being the central peak--and not too much eroded--but I don't think anybody has ever found

crater floor deposits. So, it's pretty heavily eroded. The location is off the NE corner of the Glass Mountains--maybe 10 or 20 miles from the Glass Mountains."

*Schaber: "So, Gene--as everybody said-- was going in eighteen directions at once. He was interested in mapping the Moon, he was finishing up his Meteor Crater thing, and that got him into the terrestrial craters and astroblemes."*

Eggleton: "Yes, and he [Gene] started some really high-class geological mapping. Mason and Hackman had a primitive geologic map [of the Moon], but Gene's idea was to define the whole stratigraphic column: Pre-Imbrium--Imbrium. He had a Procellarum System as well."

Hackman was doing the same sort of thing, and started as soon as October 1, 1960, he [Hackman] was probably ready to start mapping Kepler Quad. Then he did Apennine Bench Quad after that.

The Copernicus LAC chart was made first as a prototype-a preliminary one. Then, the official format--the final format-- started with Kepler Quad that Hackmann did. Shoemaker and Hackmann together published the original report on the stratigraphic nomenclature of the Moon. Then, there was an annual report update that added the Apennine Bench Formation that split the Imbrium System. Gene first had the Mare separated out in Procellarum; that was later turned into the Upper Series of the Imbrium System, and the Apennine Bench was the lower one--the smooth stuff--the Cayley-type."

*Schaber: "What were you doing between 1961 and 1962 out there in Menlo?"*

Eggleton: "Geologic mapping on the Rhipaeus Quadrangle, an overall survey of terrestrial impact structures, and a comparison of the cratering rates on the earth and Moon. I was working along with Gene, except on the general cratering studies, and then Rhipaeus Quad, which was my own, project of course. I think the sequence in which the maps came out were Hackman on Kepler Quad and then Marshall on Letronne Quad, and I think Hackman got out the Apennine Mountain Quad.

*Schaber: "So Gene's original Copernicus work never got published until later?"*

Eggleton: "Well, it was published as a photograph of the map. It may have been in his thesis [it was]. There is an overlay for that photograph that maps all of the secondary craters from Copernicus. He numbered every darn one of them--about 600 of them--with pen and ink. Wilhelms ought to know where that [original Copernicus] map is, and may have secured it somewhere.

Gene, using a mathematical formula from cratering mechanics, traced the secondary craters all back into the crater--the loops lined up in straight lines with joint blocks within the crater.

So Shoemaker figured out Meteor Crater. He figured out how to do lunar geologic mapping--he did it in the Copernicus area. And he was doing studies of cratering mechanics from this Copernicus secondary crater study, and relating it to the cratering mechanics that other people at NASA and Ralph Baldwin had worked up.



So, Gene was doing at least those three kinds of things at that time. So I assisted him a lot and traveled with him. When we went to the GSA meeting in 1962 in Cincinnati we visited a bunch of impact structures in that area.”

[Author’s Note: As fate would have it, as a graduate student at the Department of Geology at the University of Cincinnati (UC) in 1962, I was hard at work as part of the hosting committee [from UC Department of Geology] at that GSA meeting. I really wanted to attend the field trip to the local crypto-volcanic structures (Kentland, Serpent Mound, etc.)--most of which I was familiar with through field trips with the department of Geology at UC. But, unfortunately, I couldn't attend the field trip because of my obligation by the Geology Department to work every day at the GSA meetings. If I had attended that field trip, I more than likely would have met Gene Shoemaker and Dick Eggleton three years before I actually would end up arriving at Flagstaff to start work with the Branch of Astrogeology in July 1965.]

I left Menlo for Houston for the summer of 1963 for the astronaut training program. I went down with Dale Jackson. He and I were the first ones in the Survey’s [Houston] Astronaut-training office. I was there maybe a month or two, and then I left for the University of Arizona. After I left Astrogeology’s astronaut training group at MSC, Don Wilhelms went down [from Menlo Park]. They gradually built up to about a half a dozen I guess. I don’t know how many years they ran that thing down there.”

*Schaber: "Not many, they didn't really get along very well!" So did you lecture to the astronauts while you were there?"*

Eggleton: "No. I was only there a month or two. They were just getting it started; hiring a secretary and arranging--trying to elbow into some space. We [constantly] had faceoffs with the NASA guys; so I hardly did any professional work at all.

*Schaber: "So were you planning to go to graduate school in Tucson, or did it just strike you while you were there?"*

Eggleton: "Gene and I talked about that. He encouraged me to think about going back to school and get a Doctorate. I got a NASA Fellowship that gave my family and me \$5,000 per year I think.

I spent a month or two [with my family] in the summer of 1963 at Clear Lake near MSC in Houston with the Branch’s astronaut training group. So I helped launch that [the Astro astronaut training work in Houston] before I reported in Tucson for school. I was there for five semesters [September 1963 through February 1966]. The intervening summers of 1964 and 1965 I was in Flagstaff. While I was in school, I was getting my Rhiphaeus Quadrangle completed.

I was [in] leave-without-pay [status with the USGS], and with a \$5,000 NASA fellowship. I received summer pay when I was back up [on duty] at Flagstaff.

*Schaber: "What were you doing in Flagstaff during the summers of 1964 and 1965?"*

Eggleton; "I think the summer of 1964 I was still working on the Rhipaeus Quad.

In the summers of 1964 and 1965 at Flagstaff I made sketch maps of secondary impact craters around three or five lunar impact craters of different size--Langrenius, Petavius, Tycho--to get a range of sizes. I was convinced at that time that the chain craters on the south side of mare Imbrium--ten to twenty kilometers in diameters--were Imbrium secondaries, and I was trying to establish, using telescopic photographs, a scaling between the size of the secondaries to the size of the crater through that series of craters. My maps are overlays of those photos. I would measure maybe thirty secondaries, and then look at the size distribution, and the size in relation to the primary. I gave a talk at Houston about that work at one of the Lunar and Planetary Science Conferences (for Apollo 14). The paper would have been given in connection with Apollo 14. I never got out a [formal] paper on that; however, it did lead into my interpretation of the impact geology of the Imbrium basin which is a redo of the Wilhelms/McCauley mapping of the front side of the Moon. That work appeared in the Apollo 16 report. What I did there was interpret two or three facies of the Fra Mauro Formation and map the secondary field. It's not the whole front side. It's about a sixty-degree sector between radii of the Imbrium basin. I plotted the Imbrium concentric- and radial- fractures on that. So this work that started in the summers of 1964 and 1965 ultimately led to that product."

*Schaber: "What was your [Doctorate] thesis on down there at the University?"*

Eggleton: "I think the title was *"Probable and Possible Volcanoes on the Moon."* It includes my Rhipaeus and Rumker Quadrangle maps, and three or four detailed maps based on Lunar Orbiter imagery of volcanic features."

*Schaber: "When you came back in those summers, were we at the Arizona Bank Building? And you put up all of those Lunar Orbiter-IV pictures like they did at Menlo Park. Was Howie Pohn there [at Flagstaff] when you came up here to work in those summers?"*

Eggleton: "Yes, Well I don't remember exactly when he came. But I was up there during those two summers, and then continuously starting February 1966--until I left in July 1976."

*Schaber: "Dick, Did you go out to JPL and work on the Lunar Orbiter missions?"*

Eggleton: "I went to Langley [with the screening group]"

[Author's Note: Astrogeology had personnel both at JPL [for mission operations] and at Langley for the Apollo landing site screening effort using Lunar Orbiter photographs].

*Schaber: "So you come to Flagstaff finally after getting your Doctorate from the University of Arizona?"*

Eggleton: "Well, I didn't get my thesis in and defended until the spring of 1970. It ended up with those two maps. Oh, I included Lunar Orbiter photographs and descriptions of all the different kinds of volcanoes that I could come up with."

*Schaber: "Who was your thesis advisor down there?"*

Eggleton: "Spence Titley [Author's Note- who would work WAE for the Branch of Astrogeology in 1963.]

To go back to the beginning of the whole matter of me going back for my Doctorate, Gene started talking to me about it probably in 1961--certainly by 1962. Gerard Kuiper had already moved from the University of Chicago to the University of Arizona, establishing the lunar and planetary laboratory there--a research institution. He and Titley were both working toward establishing a Ph.D. program in lunar and planetary science. They kind of went their separate ways, so Dale Cruikshank, Bill Hartmann and Allen Binder worked mostly with Kuiper in the Astronomy Department. Actually, Binder may have gotten a geology degree. I had an office in the geology department, and I was most closely tied with Titley. But the reason that I went there [U of A] in the first place was because [Gerard] Kuiper was there. I had a meeting or two with Kuiper before I arrived on campus in Tucson. While I was working on getting there, I perceived it as an interaction with him [Kuiper] but by the time I got there. Titley had already begun to deal with the Survey.

Spence Titley spent some summer time [1963] up in Flagstaff working. He effectively worked for us as a professor at the University of Arizona, and had mapping projects. He mapped one or two of the Ranger-7 Map series--the nested ones. So, he authored one of those. I made quite a few contributions to one, as did Hank Moore and Maurice Grolier... So, they gave the authorship to the Branch of Astrogeology--and none of us got individual credit. I guess there probably a big hassle between Spence Titley and the Survey over what the authorship should be.

At that time I was mapping the 500,000-scale map. They redid Montes Rhipaeus Quad from the Ranger data, and then they made a 500,000 map called Gerick--that's the large crater at the south end of the Fra Mauro highlands, and it was in my Rhipaeus Quad. I never published that. Too many other things came along."

*Schaber; "So you finished your course work. You came up here [to Flagstaff] before you wrote up and defended your thesis. So you got back To Flagstaff in February 1966. Besides finishing up your thesis, what were you doing?"*

Eggleton: "I worked on this Guericke Quad. I actually did a fair amount of mapping. And for that, I took the photometric scans that [Maurice] Grolier made. They were 11.5 X 14 paper sheets from the Isodensitracer (IDT). The patterns were alternating parallel lines with different spacing. They were real hard to read. I carefully traced out the contours covering this 500,000-scale quad, and the other more detailed quads. I traced the contours. Grolier developed that program and he ran all of those scans, but the things weren't calibrated. So I achieved a calibration by matching my contours to contours on the calibrated Pohn/Wildey map, but mine were much more detailed than the ones that Grolier produced. So I calibrated my contours against Pohn and Wildey which gave

them an absolute calibration. Howard Pohn and Bob Wildey did one big photometric map of the whole front side. It was much less detailed than the one that Maurice Grolier compiled.

So I used this photometric study. From that, I extracted photometric map covering one of these Ranger quadrangles--and it went into the map. But it wasn't credited to me--it was credited to Grolier. He had a big program, and he ran out probably the whole Moon on these pieces of paper with uncalibrated contours. I did the calibration, and the work to make an illustration out of it. So, the photometric map on this Ranger Quadrangle authored by the Astrogeology Branch was produced by me and credited to Grolier.

Anyway, that mapping and photometric study was one of the things that I did probably after February 1966. I think the Ranger-7 mission was the summer of 1965. During the mission, I didn't have a charter to work on it. Gerard Kuiper was the Principal Investigator and Shoemaker was a Co-Investigator--and they both did their reporting independently."

*Schaber: "Did you have anybody working with you?"*

Eggleton: "The blond girl. I am thinking of the girl that eventually married Bob Sutton, Gale New. Gale New Sutton was in there as our secretary [in McCauley's Lunar Orbiter area with the big round table on the 3<sup>rd</sup> floor of the Bank Building]. I am quite sure that this was the situation as of February 1966; however, I'm hazy on the two summers before that."

[Author's Note: Gale New (later Gale Sutton) was hired by the Branch of Astrogeology in June 1966; see June 1966 in main text.]

Eggleton: "I've got about a page here in my PTR [Professional Technical Record] about what I was doing 1966 to 1975 in Flagstaff.

It mentions this 500,000-scale Guericke Quad. I worked on mapping the Rumker quad 1966 to 1968. I think you asked if people were working with me. Steve Saunders was in our office there one summer--and one or two other people--Gary Fuiz. There were one or two other guys like that. That guy who mapped in the Hellas basin on Mars -Dave Pierre I think maybe that he was there too.

Then, there was a study of outstanding examples of a variety of probable lunar volcanoes scattered over the sub-terrestrial hemisphere and numerous probable volcanoes and major structures in the Rumker and Montes Rhipaeus Quads. In Rumker, I worked out a massive lineament-type map based on the distributions of pre-Mare rocks sticking up through the Maria and volcanoes. I think I added those onto my Rumker map.

A student at the University of New Mexico, Gene Smith, assisted me on the Rumker Quadrangle. I mainly worked on the ejecta from the Sinus Iridum crater [mapped by this author]; the west edge of that [Sinus Iridum]; the ejecta and the secondary craters distinguished it from Fra Mauro.

I did a basic map of the mare area [of the Rumker Quad], but Gene Smith did mapping of the mare of the Rumker Quad from Orbiter photography because I was pretty much finished. But most of the volcanic features in the Rumker were ones that I interpreted.”

*Schaber; "You worked on that right up through the Apollo Program, or what did you do different during the actually Apollo missions?"*

Eggleton: "While I was in the crunch to finish up my thesis, geologist David [H.] Scott [see Appendix A] came along [about 1967] to help. He sort of got the job of going from the preliminary map to the published map for Rumker Quad--so he is senior author and I am second author.

For the Lunar Orbiter Project in 1966 and 1967, I mapped the A-7 site of mission I which covered the Ranger 7. Well, the Rhiphaeus Quad that I mapped included the Ranger 7 landing site, and all the Ranger 7 maps, and the Apollo 12 and Apollo 14 landing sites. The Lunar Orbiter Apollo target site (A-7 map) I mapped I believe covers the Ranger 7 impact site--or at least close to it. So I did a map in the initial Lunar Orbiter Report. I think I was in Langley for both Orbiters 1 and 2. For the second mission, I did research on the lunar soil depth as revealed by the shapes of small craters. There was some kind of report in there about that.

Then for the Apollo program, I did the pre-flight landing site 250,000-scale map for Apollo missions 13 and 14. The 13 ones got printed in color, but I don't think it was really a full-scale publication. There wasn't time to have it go clear through the USGS publication mill. There was a 25,000-scale and a 5,000-scale map in the same envelope, and I think Terry Offield did those.

I made one trip to the Cape before the mission for Apollo 13 or 14.

I contributed to the analysis of geologic results for Apollo 14 and 16. You know, you and I worked together on the 16 results--on the Cayley. The main thing I got into the Apollo 14 report was the regional geology of the landing site. Then with regard to the Apollo 16 site, I have that whole front side report of the Facies of the Imbrium ejecta and secondary crater field.”

*Schaber; "Take one minute and tell me about the Branch's error in the pre-mission geologic interpretation of the so-called volcanic unit at the Apollo 16 site in the Descartes Highlands."*

Eggleton; "I was down there to participate in the real-time targeting of orbital photography [during Apollo 16.] I wasn't in the Field Geology team area there in the trailers. I was somewhere else. It was sort of informal. I wasn't a Co-Investigator or anything, but I had contributed through the years to sites-of-interest lists, along with Wilhelms and others all around the Moon. I spend lots of time scanning them, and was aware of interesting spots to shoot at. So I was there contributing to that because I had made written contributions to the list. Then, when the astronauts started picking up all the breccia, I stayed up all night one night, writing up a hypothesis that it was a phase of Imbrium ejecta.

I talked to Bill Muehlberger about it I guess, and he had me come down to the trailers and talk to the team for an hour or two during the mission.”

*Schaber: "I would like now for you to talk about what you did after Apollo 16-- until you left Flagstaff."*

Eggleton: "You pretty well know what I did on Apollo 17 [the Lunar Sounder Experiment with you] so there is not much add to that."

I have down in my notes that you and I worked on the 3.8 and 70-cm radar images of the Moon in 1969 and 1970. It actually didn't accomplish much with that. Tommy Thompson [JPL] and Stan Zisk [Haystack radio observatory] supplied that data to us.

Okay, 1975 and later, I did some things I called applied planetology. Most of it was applied impact geology; and actually, I got a series of about eight proposals that I worked up. One was a big one to search for terrestrial impact structures, find them in the subsurface, study ejecta blankets, look for secondary craters--some in the subsurface--using it as a chronology-type tool. One of the objectives was to mount a serious program to find impact structures. This was 1975 and later, so I was already in Building One on the mesa [in Flagstaff].

These were just proposals and none of them ever got approved. The last one was in July 1973.

In 1975 and 1976 I went back to the Herndon Quad [Dulles Airport area in Virginia] and did some more work. I was also writing these proposals for applied impact geology--the study of the impact history of the earth and the utilization of the fallout layers for tephra-chronology-type work. I carried on an interest in that and in writing proposals and working on data some until 1975 when I retired."

*Schaber: "But you left Flagstaff and went to Denver didn't you?"*

Eggleton: "Yes, I left Flag in July 1976."

*Schaber: "To what Branch?"*

Eggleton: "I think it was either Central Regional or Environmental. I've forgotten the history on that."

*Schaber: "So you were working on this impact work you wanted to do under Central Regional?"*

Eggleton: "Well, I wanted to. In August of 1975 I submitted a large proposal to do these applied impact studies of searching for craters and impact horizons, etc.

*Schaber: "Before you left Flagstaff?"*

Eggleton: "Yes, about a year before I left. I gave it to [USGS Director] Dallas Peck. I don't remember if I gave it to Shoemaker or not. But I told people to keep it under their hat

That last year in Flagstaff.... Oh, that's what happened. A year before I moved [to Denver] I transferred out of Astro into Central Regional (in Flagstaff). That would have been about July 1975. I was in Flagstaff another year, except that I did a fall 1975 and spring 1976 field season in the Beattyville Quadrangle in Kentucky. I worked with geologist Gordon Weir there. He was with the Kentucky Branch and I assisted him with the quadrangle. We were in the field separately but were working at the same time. So I had two field seasons there."

*Schaber: "So you worked at Central Regional in Flagstaff, then you transferred too Denver and worked for them [Central Regional Geology]. Is this correct? There you worked for the Coal Branch. What did you with them?"*

Eggleton; "I worked on a 100,000-scale quad in Northeastern Montana above the junction of the Yellowstone and Missouri Rivers. I did surficial geologic mapping. Let's see, I was with them [the Coal Branch] from 1977 to August 1986--nine and a half years. During the last two or three years of that period I worked in Bohours (sp?) lab and we were looking at volcanic ashes from Costa Rica, and he also did a lot of work on different Cretaceous ash beds. In the Montana surficial mapping, I found two volcanic ash beds out of Yellowstone Caldera--a 600,000 year-old ash (identified and correlated by Glen Izett). And I found a dirty ash at 1100 feet above the River that was probably the 1.8 million-year old one."

*Schaber: "Did you enjoy that work with the coal Branch?"*

Eggleton: "It was fairly interesting. I never felt effective as a field worker; I just didn't have enough experience in it."

*Schaber: "Then you retired from the Coal Branch when there was a riff or early out in the Survey. Is that correct?"*

Eggleton: "It was an early out opportunity"

*Schaber: "looking back at your time at Astro what would you say?"*

Eggleton: "Well, the whole Astrogeology experience for me was a thrill and a super high--and great fun. I felt like a discovery a day kind of activity. It was certainly the stellar part of my career, and I am grateful to have been part of it" (from an interview with Dick Eggleton by Gerald G. Schaber on 15 March 2001).

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**Donald Parker Elston** (geologist; b. 1926; Ph.D., 1967, University of Arizona)

[The commentary below was abstracted from an interview by this author of Don Elston on 18 June 2001.]

Elston: "I was born 27 June 27 1926 in Chicago, Illinois. I started high school in Chicago, and then moved all over during the War years. I went to school in Miami Beach, Mobile, Alabama, and Washington, D.C. (at Woodrow Wilson High School)."

It was the War years, and my dad was moving a lot. He was an Engineer, and he ended up with shipbuilding things--supervising and all. Then he ended up on price control during the war years in Washington. My final years at high school were at Augusta Military Academy in Fort Defiance, VA, and I graduated on June 6, 1944. Two weeks later I was in the Army. I volunteered for the artillery, went to Fort Sill, Oklahoma, got shipped back to North Carolina, and then shipped overseas in January 1945. I spend some time above the Normandy Beach at Camp Lucky Strike, and then--when we finally got our field pieces--which were 155mm Howitzers--we went inland and just did the last little bit of the war. After that, I was in the army of occupation, and after they reorganized everybody I didn't have enough brownie points to go home right away--so there was another year or so before I returned to the States.

I was in France and Germany. I saw quite a bit. I didn't want an office job so I ended up driving 2 1/2-ton trucks all over the place. Of course, we saw the southern part of Holland that was really destroyed. We guarded the [then famous] bridge at Ramagan for a while during the later stages of the war when we were no longer shooting. After VE day I watched the whole 8<sup>th</sup> Air Force fly the Rhine south to north all day long just burning up gasoline. Then local people began to go home, and there was the biggest mish--mash of people going north, south, east and west--trying to head home, pushing bicycles and holding on to charcoal-driven trucks--they didn't have gasoline. It was really an interesting time to go through.

I got back to the States in June 1946--so that was two years from the time I got in. In September 1946 I entered Syracuse University [Syracuse, New York]. My dad had enrolled me while I was overseas, and I didn't realize that there was a GI Bill coming up. This was pretty neat and this is really what saved this country. He put me in engineering--because that was "his thing." He couldn't get me in Purdue in Indiana because I wasn't a residence of Indiana. So, I ended up in Syracuse. I carried three jobs and did twenty credit hours. So I didn't do very well the first semester. Then, I got myself off of probation the second semester. I looked around, and found geology. So I came into geology starting my sophomore year. Shirley and I met about that time. We met in March, got engaged in April, and were married in September of 1947."

*Schaber: "Had you ever been interested in geology earlier?"*

Elston: "No, I was looking around. We had a whole year where all of these returned veterans were talking to one another, and we really turned that whole school upside down. No longer was there any [regular] freshman; we had all of these "old" freshman, and the real upperclassman were no longer upper classman. I bunked with a couple of people; one started in architecture, and he burned out on that in a hurry. And there was another one doing something else, and all three of us ended up in geology. I looked around, and said hey I'd like to do that!

I started the course work and found out they [Syracuse University] had a really good geology department. So, I started geology and I ended up taking 18 to 20 credit hours per semester for the remaining years. I was able to duly enroll my senior year in the Graduate school (1949), and I got my Bachelors Degree in 1950 and my Masters one year later in 1951.



The Korean War was cooking along about that time. I had taken ROTC for two years. I didn't have to take that stuff--except for the Infantry part--which it turns out I ended up doing in the Korean War. Just before I graduated--about March 1951--I received a notice come report for active duty. I wrote them and said hold off a little bit; and sure enough--they did.

That same spring (1951) I passed the Civil Service Exam for the U.S. Geological Survey. I passed it with over 80 points and they told me that was a good score. I said I want to work for the Survey, and I'd had been offered a job from Washington offering me a position out on the Colorado Plateau. I say great. Then, two days later came this letter from the Army. Talk about a little down time! Anyway, come June I finished and defended the thesis which was on the Poolesbrook Limestone of the Manleas (sp?) Formation in Central New York.

Then, I reported for active duty in June of 1951. At Fort Dicks, New Jersey I taught machine guns, automatic weapons, and things like that. I ran firing ranges for the Infantry. We had a good time there. For the first time in our lives we had some money; I mean enough that we didn't have to worry too much. Our son Jeff was born a year and a day after Shirley and I were married in September 1948."

*Schaber: "So the Army took you even though you had a child?"*

Elston: "Oh yeah, and Shirley was pregnant! And I had prior service, and found out that they shouldn't have done that. But they need what they called "sandbags" in those days--those are infantry line officers to go out there. Oh well! Our second child came along--that was Jay--at Fort Benning [Georgia] in January 1952. I had shipped down to Fort Benning for I don't know how many a week course, where they just burned up a whole lot of ammunition that they had left over from WW II. They brought out the tanks, and did things like that--and just to emphasis the value of firepower. Then, I almost joined the paratroopers, the decided that I didn't need that [Author's Note: Shirley Elston told this author that she wouldn't let him!].

So anyway, I shipped over to Pusson in March of 1952), took a train over to Seoul, and then joined an outfit that was on the line in the Kumwha Valley at the southeast corner of the iron triangle--and there was this huge mountain that was occupied by the Chinese. They looked down on us. Then right along side of us was something called Sniper's Ridge. This was metamorphic terrane, and that ridge came in at right angles to our ridge--and stood above it. So I got up there, and all of the positions had pretty much stabilized--thank goodness. I had a command post and I was leader of the First Platoon of Company A--224<sup>th</sup> Infantry Regiment--a Regimental Combat Team. That meant that we had some of our own artillery and a tank or two--things like that--of course a heavy weapons company and other things. They normally put two Battalions on the line and keep one in reserve. You normally do things like that in the Company, except we had everybody on line in the Company--everybody in foxholes.

They had already been dug by the time I got there. I had the north end of the line. This was a north-south line for our company. Then, we joined another Division over in the corner--and they went over to the west--and there was a natural drainage coming down between us--and a screwed up terrane! I became Company Executive Officer, as well as Platoon Leader. But on the line you take care of your own Platoon; you go up and down and check the holes at night and walk along.

What you did in those days was after it turned light--and you were sure you weren't going to be hit with a night attack, you went to sleep. Then you woke up in the afternoon and chow was brought up from down below. Wow--good climbs; you needed legs for this. So at night it would settle down and you'd put two people in each hole.

We didn't do a lot of shooting. You'd sit there in the evening as it got dark and you'd heard the Chinese above you whacking away at the trenches, as they worked their way through the metamorphic rocks so they'd get a better position above you to shoot at you. The company Commander said hey, let's have a time-on-target. So our Company and our artillery and I think the next Company. It came on in early in the evening. The whole hill exploded--the white phosphorus and everything all over the place. Then, it was quiet--there was a body or two up there--and then we didn't have anymore trouble from them for quite a while.

Then we went down to Koshido that was a prison camp for the North Koreans. These were nasty people--the North Koreans. So they wanted a line outfit to go down and pacify them. There were a few things like going in at night to see what was happening. People would disappear at night, and we figured that they had eaten them! So you walk around at night with a flashlight and a small squad, and you walk across two hundred people lying on the floor--not breathing--but would open an eye occasionally. So you'd step between them. It was a thrilling experience!

So we stayed there until almost November, then they found out that **I shouldn't have been in Korea in the first place**, So, I returned to the States in November 1952. I met Shirley, and we ended up going a GSA meeting in Boston. I wanted to find out if the offer from the Survey [that I had received the letter from before going to Korea] was still valid. Of course, the Survey had a hiring freeze in effect and they didn't know anything about my earlier job offer before Korea. Meanwhile I stopped by Texaco and inquired. By the time I got back to Shirley's place, I had an offer from Texaco.

However, I made a train trip down to Washington--the old Survey headquarters downtown. I called down and they said sure--we're interested in having me show up and go out to the Colorado Plateau. So I went down there and they offered jobs to me, and when I got back I guess the job from Texaco was there- at more money of course. So, I had my first big decision. I said Shirley we are going west! So I accepted the job with the survey and went to get a physical.

Oh, this is interesting. I had a physical from an old doctor. He had a diploma up on the wall and I am sure that he was a physician in WWI. He was on his last legs. Well, I passed the physical and then I was accepted [by the USGS for the job]. However, I wasn't feeling too sharp. It turns out that I was coming down with Polio. And within a day or two of that job offer from the USGS I couldn't walk. This was about the first week in December 1952. I got shipped up to the VA Hospital in Albany. And after about a week or so, the March of Dimes sent up one of their specialists up there--who came in... The Army didn't know what to do with Polio. I said am I going to walk? He said you'll walk again! I said thank God! So for about the two months that I was in the hospital I was using a cane and moving around reasonably well, thank to a lot of physical therapy. Then the third month, I was sort of an outpatient.

Meanwhile, here's this letter from Dick Fischer and Lowell Hillpert who had offered me the job. That arrived while I was in the hospital not knowing if I was going to walk. I wrote them back and said I'm afraid I can't report right now. Later we found out--when we talked to them--they got my letter and said what are we going to do—he has passed the physical? They said well, let's wait and see what happens,-and they waited. So in May of 1953 I showed up.

Well, the first day that I reported for work--I think May 5, 1953-- Arnold Brokaw showed up for the Colorado Plateau Project--and a year later he took it over from Dick Fischer. Arnold was a "hatchet man [for the USGS] even then.

I was functioning again, but I was on a cane, and it took me a couple of months after I reported for work before I sort of hid the cane around the corner--but the first [stratigraphic] section I did in the field--I did using a cane.

After a week or so of looking at Journals, then they assigned me to a Uranium mine called Jo-Dandy in Paradox Valley, Colorado. There was a fellow down there running a drilling program.

The Jo-Dandy mine was within the Jo-Dandy Mining District. The project had originally been started by Gene Shoemaker back in 1948. My connections with Gene Shoemaker actually start when I showed up on the Plateau and stepped into a project Gene had started back in 1948. This is where some of this early kindred spirit happened, because Shoemaker did one hell of a fine job of mapping there. He was young and wet-behind-the-ears, and he had his dad there as his rod man. His dad helped him map. Gene was too young to go in the Army at this time. This was before Gene had even met Carolyn.

Gene was really charging around about this time-and he-got together with a fellow named Lincoln R. Page. I was at that time on a drilling project trying to refine some of the geology that Gene had done earlier. I went through and did something on the section measuring. We were calculating ore reserves, etc. It was the Survey's only million-ton uranium ore deposit in the area. Gene had been arguing with Fred Cater (sp?) about the origin of the salt anticlines. And Gene, after he bailed out of Jo-Dandy, he went over to Sinbad and started to map that. There, Gene got involved in Moenkopi Formation where he named new members there. Gene was doing this while I was stuck on this [Jo-Dandy] drilling project. So I spent two years in Jo-Dandy on drilling.

Gene ginned up something called the distribution of elements project (DOE). He had Al Miesch [working with him]. The whole idea behind this --this is one of Gene's-- not failures--but it wasn't a success in the long run. The idea was that Gene decided that he could prove that the uranium-vanadium deposits were extrinsic--they were intrinsic elements that were related to the sandstone and extrinsic elements that were related to a hydrothermal source. That is where the model fell down. So they spent a couple years on that. Lincoln Page was in Boston, and he was pretty chief mucky-muck out there--pretty close to a Branch Chief--or even better. He had connection with the AEC (Atomic Energy Commission)--so he was one of Gene's "angels"-all right?

So Lincoln Page would show up there with money to run a project independently of Dick Fischer's Colorado Plateau Project (at least for that one year)--then Arnold Brokaw took over. However,

Dick was still around. Dick Fischer was a great guy. He started the Uranium-Vanadium in about 1936-1937 with his thesis out of Yale.

Anyway, the AEC got hold of him [Fischer] and that's how the USGS got into the Uranium-Vanadium business, which was an extension of the Manhattan Project. The nation needed some Uranium-Vanadium; and so for a couple of years they did mapping here and there, and then finally the AEC said can you drill here and there. Anyway, Gene had the DOE project and he pushed pretty hard on the hydrothermal origin of the uranium-vanadium deposits. Dick Fischer was a sedimentary-source-person. Everything he saw said sedimentary source for these sedimentary ores.

It's a very simple thing, Aaron Waters and a man named Granger figured it out before we left the Plateau."

[Author's Note: Years later, Aaron Waters would be asked by Gene Shoemaker to be one of the Branch of Astrogeology's senior geology advisors for the Apollo 11-13 Lunar Geology Experiment Team.]

Elston: "Anyhow, they figured out that the uranium came out of the devitrified volcanic ash, and everything pointed to a time of re-mobilization of connate waters during the Larimide."

*Schaber: "Yeah, the late volcanism."*

Elston: "And those ashes are in Jurassic and Triassic sediments--the devitrified ash--and the source of the ash was to the south in Arizona. So you kick the ash in and it devitrifies--then you remove the water. Then you precipitate the uranium with the vanadium at reducing places where there was carbonaceous trash."

*Schaber: "Yeah, where the Eh-Ph conditions were right."*

Elston: "Yep and it turns out that the vanadium does not occur with uranium in the crystalline environment--so you needed a very special set of circumstances to precipitate vanadium. But Gene pushed like hell and he was a real thorn in the side of Dick Fischer--he really was. We'd have every Monday--or every other week--we'd have a seminar--and Dick would lead it--and we'd have people whom would talk. It was a great working group and the remnants of that are still friends at this time.

Then Gene disappeared shortly, I think in 1953, to go to Princeton [to complete his second year of course work towards his Ph.D.]

I went through 1955 at Jo-Dandy with the exploration. It became part of the Bull Canyon Project, and a guy called Art Flint sort of headed it. It was because he was an older guy. They were bringing in all of the older guys; they didn't like all the young ones around.

Anyway, in Bull Canyon, which was part of the Monogram Mesa Project, there was a fellow named Carl Roach. Carl was working on Uranium-Vanadium deposits and precipitation fronts--

and he could demonstrate that things precipitated out of fluids moving through the rocks, and that there was an interface on the Ph-Eh. Carl had been there early and had been living over near Paradox Valley at Vancorum [barracks]. Vancorum was a stop on the way into Paradox Valley and Naterita. We'd have to go in and shop, and we're up on the side of this alt anticline on a slump structure-landslide. I think we met Carl Roach on our first trip in.

That was the beginning of our acquaintance with Carl. And we'd play poker every week" (see Carl Roach's story about the poker below].

Then I went for two years for two years--1956-1958-in something called DEMA (Defense Minerals Exploration Assessment) where people who had "cats and dogs" (slang for mineral prospects) would write in and ask for money from the government to explore their property. I got a chance to see a lot of the Plateau.

Anyway, Gene Carl, Lorene and Shirley got pretty close. This was from 1956 on because Gene showed up and started to make waves. So we'd go out in Sinbad Valley and look at geology—the Moenkopi Formation and other things. We would take the kids out with us. All the kids were growing u together. And, of course, there was bridge club. There would also be poker at Grand Junction.

Okay 1956-1958. While I was doing the DMEA work, Carl Roach ended up with Gene. They were mapping out in the Hopi Buttes Volcanic Field (Arizona). They were doing a uranium-type project. Gene was still pushing the idea of a hydrothermal source for the uranium deposits of the entire Colorado Plateau. He went where he probably had a good chance--he knew that the diatremes came up from below. They would look in these little lake-bed deposits [in the diatremes].

That's when Gene started to go over and divert himself--as usual--on a brand new personal obsession --Meteor Crater. So here he had money for the DOE project, and he burned out a couple of people, including Al Miesch. As you know, Gene pushed so hard. There aren't very many people who lasted around Gene more than a couple of years in those days. He would just burn them out

In June-July 1958, the Survey broke up the Colorado Plateau Uranium Project. It turns out that the nation had enough reserves.

We ended up moving to Lakewood near the Survey's Federal Center in Denver most of the people. I ended up over in the Fuels Branch. When the Uranium Project folded up, Gene wanted to move to Menlo Park--and he did. This was June 1958. Meanwhile, we had occasionally been getting together.

Gene was generating [new ideas] as usual. He ran into this physicist [Ted Taylor] who wanted to make isotopes--the secret MICE project. I didn't know about it at the time since I had transferred into Fuels Branch in Denver. Soon thereafter, Gene went by Fuels Branch, and told them I've got some money--so we can support Don Elston. You know, Gene was a champion for me; he just backed me. My Branch Chief was a famous guy--James Gilluly.

Anyway, Jim became my Branch Chief. He was a good Branch Chief--a tough old bird too. He was a field geologist. The Fuels Branch at that time was doing mostly coal work--but they did a lot of fieldwork and section measuring and the like. I never got to do any fuels work. All of the work I did was things in the salt anticlines on behalf of the salt project--the MICE project. They were looking for pure salt at great depths so they could produce Plutonium. We had it--it turns out-- at 12,000 to 13,000 feet at one end of Paradox Valley. But it meant doing all sorts of things like pulling together all of the drill hole information from oil companies, and mapping the evaporate cores of Paradox Valley and Gypsum Valley.

MICE was a top secret project. It was Gene's project; I hardly ever saw him. So that two years from 1958 to 1960 was salt; and solved the problems of the salt anticlines.

In 1960 and something called Kentucky geologic mapping came over the horizon. Charley Anderson was Chief Geologist of the Survey at that time. The survey needed money [following the close of the Plateau Uranium Project]-and Kentucky would give them money if have (the geologists) moved there to map the state geologically. So they went out and scoured the rest of the Geologic Division trying to force people to go to Kentucky and map this "layer-cake" geology they have there.

Also in 1960, Gene was thinking about starting an Astrogeologic Studies group. He came by and said me to join him? I said yes, but I'm on the Kentucky mapping list. All of this was happening at the same time.

Then Gene went to Italy [to a meeting], and was talking to the Chief Geologist, Charley Anderson. Then, he wrote me a letter saying Don I really let you down--I couldn't get you off the Kentucky list."

*Schaber: "So did you move to Kentucky?"*

Elston: "No, we were right on the fine edge of moving there. Well, I was going to go out and map the Hart Mountain thrust of Pierce up in Wyoming. That was going to be my Green Mountain project. Then, one day I was cleaning a trailer; I came home where Shirley was and said stop cleaning the trailer--we're going to Kentucky. Al Zap [Elston's boss who was Chief of the Fuels Branch] eventually got me out of the move because there was a protective association. The Fuels Branch took care of its own---so he got me over in Engineering Geology-- to go the Nevada test Site. So, I started in the Nevada Test Site [NTS] with Engineering Geology, and spent the next year and a half there--from 1960 through half of 1962. I then went from Engineering Geology to Special Projects. That was for the business of regional mapping and isotopic mapping.

There, I got a lot of introduction into volcanic stratigraphy and facies changes, and eruptive volcanic deposits. Meanwhile, Gene had run me by Teapot-S and Jangle-U, two of the nuclear-created on the NTS. He took me and on a tour of these craters, and one or two others. He was showing the fold-over rim flap and how the explosion would cause the rim strata to rotate back over on itself [causing reversal of stratigraphy].

So, Gene came back, and I was in Engineering Geology and then Special Projects. Gene and I still kept in contact, and that was mainly because Gene did it. I had plenty to do learning how to use the Kelsh Plotter and other things as well.

About a year down the pike from that, I guess, things developed with Gene where he would have to go to Washington--to NASA Headquarters-- to set up the Space Sciences Division."

*Schaber: "Yes, he had just barely started the Menlo Astrogeologic Studies Group when he had to leave."*

Elston: "Oh yeah, that's when I showed up. I went to Menlo in May of 1962 for a month. Gene wanted to try me out I guess by holding down the fort when he left for a year. Then we went back in June. Then the Astrogeologic Studies Group became a Branch [1 July 1962] while Gene was gone. I became Acting Chief Branch of Astrogeology at that time.

That same summer [1962] we went out to Sierra Madeira in West Texas--Gene, Carolyn, their kids, and me. While looking for shatter cones [indicative of a meteorite impact] at Sierra Madeira, I saw- the biggest rattlesnake I've ever seen in my life. I didn't see the back end of him, but his whole body went across a two-lane track, and the back end of him disappeared in the brush"

We had rattlesnake meat to eat [while there], but not "that" rattlesnake. He was so big. I was alone at the time and I wasn't going to chase him around. They were all over the place down there."

*Schaber: "So what projects were the Astrogeologic Studies Unit engaged in when you arrived, and were they all funded by NASA?"*

Elston: "It was all NASA funding, and the real crunch was to turn out a semi-annual and annual report--to which we could append a proposed budget for the forthcoming year's work. So I worked my butt off, and Dick Eggleton was a mainstay in those days. Hal Masursky, starting in September 1962 [when he arrived], found out about what we called GTR's (Government Transportation Requests). He could walk up to an airline counter and write a ticket. So Hal [instead of helping on that first Annual Report to NASA] ended up spending almost all of his time down at JPL because they were planning Ranger and Surveyor.

As we were putting this thing together [the first annual report] in the winter of 1962 and approaching spring of 1963, the crunch time was coming because we had to get that annual report completed so we could get some money for July 1 Fiscal year. I think at this time Don Wilhelms joined us [December 1962] and we started getting the lunar mapping going. Frank Cutitta, Bob Hackman, and Ed Chao also did his their parts for the Annual Report. Dick Eggleton worked like crazy on the interpretation of craters--cratering mechanics and how you map these things. And the hypervelocity impact results were contributed by Henry Moore and Don Gault (Ames Research Center, Mountain View, California)."

[Author's Note: The Elston's, Don and Shirley, recalled during their interview with this author on 14 June 2001 (see their interview above in Appendix A) that they were still on the West Coast [the

USGS Center in Menlo Park], and Don was finishing his first Semi-Annual Report for Astrogeology (with the budget) in the spring of 1963--and there was no money to publish, or print it. Shirley recalls Carolyn Shoemaker and her looking at Gene and Don--and them saying well, we'll just go and do it anyway. Carolyn and Shirley looked at each other and said well okay, we'll just come and visit you in Leavenworth [the Federal prison in Kansas]. Don said that we just went ahead and printed it--and then sent in the requisition. We were out of money.]

Elston: "About November of 1962, Gene [Shoemaker] showed up on one of his visits back from Washington D.C. He always stayed with us. He sat on the edge of the bed and said Don I'm tired! He was dragging and looked like death warmed over. Unknown to Gene [or anyone else] at the time, he had Addison's disease [that would reach a critical stage by the Fourth of July 1963 when it was finally diagnosed]."

[Author's Note: -The entire story on the diagnosis and treatment of Gene's Addison's disease is given in Levy, 2000, pp. 88-89; also see 1962-1963 in the main text]

Elston: "Oh, we also learned how to fly. We bought an airplane (3514-C or "Charley") three ways—Gene Shoemaker, Danny Milton, and the Elstons. We each put in a third for a four-seat, 70B Cessna. It was underpowered. It would struggle off the ground near sea level, but up here [in Flagstaff]--not so hot! Anyway, we took lessons at Half Moon Bay [near San Francisco], and Gene started to take lesson also. This is when the Addison's came in [was diagnosed], and he found that he could never get a license to fly. But Danny and I finished, and I became the main flyer of the thing. We used that airplane to go back and forth to the coast.

So when we moved in late June 1963 to Flagstaff, we moved the airplane, cars, dogs, and everything else. Meanwhile Jack [McCauley] was busy doing things for us in Flagstaff--promoter Jack.

Meanwhile, Jack started to look around Flagstaff because Gene wanted some land to set up the Branch Headquarters in Flagstaff. Fortunately, right about that time the Forest Service and the City had recently swapped some land. The City had gotten some outlying land and the Forest swapped to the city part of the land located between West and East Flagstaff referred to as McMillan/Switzer Mesa."

[Author's Note: See 1963 in main text for details on the interesting story of how the Branch of Astrogeology acquired from the City of Flagstaff the land on McMillan/Switzer on which to build its Headquarters (Building-One), and how a wing on a building at the Museum of Northern Arizona (MNA) in Flagstaff came to be built as a temporary home (while building-One was being constructed) for the fledgling Branch of Astrogeology--thanks to Ted Danson, MNA director].

At the same time, I was traveling around interviewing people to move to Flagstaff [to Astrogeology]. One of them was Russ Wahmann. Now Russ and Eloise were out in Rolla Missouri, and I got the sense that they weren't too happy there. It appeared as if Russ would be just about right for the Branch since Gene wanted to hold down the topographic end of things--map preparation, etc.. So, we brought Russ Wahmann on with a transfer. He was to also show up just about the change of the Fiscal year."



*Schaber: "How did you get hold of Ray Batson?"*

Elston: "Ray sent in an unsolicited application. He was working for the Survey in Denver doing photogrammetry while attending the University of Colorado in Boulder. He earned a Bachelor's degree in geology--raising a family--working nights. And I said, anybody like that who has that much get up and go--let's go see what he's like--and bang--we brought him on! He showed up in Menlo Park [in February 1963].

Gene came out of [his temporary assignment at] NASA Headquarters [summer of 1963] wanting to set up an Apollo research program [of our own]. I ended up setting much of it up using NASA contacts after we moved to Flagstaff in July of 1963. I got on the telephone and contacted some people who weren't in the Max Faget group down at NASA/MSC in Houston. Most of these contacts were at NASA Headquarters in Washington, D.C. My contacts there included Don Beattie, who was a real pleasant guy.

I think Jack [Schmitt] came on board in the summer of 1964, because 1964 was really going along well [see Appendix A]. We had all of these different teams doing things; Jack Schmitt was doing time-and-motion with students [from Arizona State College--now NAU]. That was Jack's project; he would go out into the Hopi Buttes, or the Bonita Flow [at Sunset Crater near Flagstaff].

The manned Lunar Exploration Studies Group at the Branch consisted of the following sections, as I recall: astronaut training, time and motion studies [Jack Schmitt's thing]; and then later it evolved into the things that Gordon [Swann] started doing [out in Hopi Buttes, and elsewhere] because Gordon's work sort of combined with what Jack was doing [by late 1964-early 1965].

*Schaber: "So Jack Schmitt was starting to work on that "Astronaut on Foot" USGFS Interagency Report. Gordon edited it when he came on and sort of took over finishing it. That was Jack's claim to fame while he was here. It contained an [early] design of the lunar tools. That early Astrogeology Interagency had an important influence of the eventual design of the Apollo Lunar Tools. Is this not right? The tools did not change much from the original design in that document. And, of course, our Branch machinists actually built some prototypes of those tools in Flagstaff."*

Elston: "That's right. Meanwhile Ray Batson was working on Surveyor --and that was within the Branch's Unmanned Lunar Studies Group. [In 1964] Ray set up a Surveyor model at Bonita Lava Flow [Sunset Crater east of Flagstaff] so he could develop procedures for handling [photogrammetrically and photometrically] the images expected from the Surveyor cameras." [See 1964 in main text.]"

*Schaber: "And they started on Building One on McMillan Mesa about that time"*

Elston: "August, 1964 was ground breaking. Frank Cutitta and Ed Chao were there [from the Branch's geochemistry group in Washington, D.C.], and Joel Watkins [one of the Branch's geophysicists] was there. Of course Gene and I were there with the shovel.

I was never actually located] in Building One--never. Al Chidester must have come back from our astronaut training group in Houston in late 1964 because I had already announced that I was going back to school [University of Arizona, Tucson]. He came in and took over for me as head of Manned Lunar Exploration Studies.”

[Author's Note: The subject of the dedication of Astrogeology's new wing on the biology building at the Museum of Northern Arizona (MNA) came up during my interview with Don Elston]:

Elston: "We know the date that we entered the Museum wing, because we were coming back, and I had left Charley [3514-C, Don's airplane that he bought with Shoemaker and Danny Milton] in Farmington, so I decided to try a night flight. Oh, I shouldn't have done that! I went and straightened a dihedral under the wings on the take off [and flipped the plane]. So, I stayed overnight and got home another way [commercial] the next day. So I missed the opening ceremony for getting into the Museum wing. [The dedication took place in September 1963; see September 1963 in main text.]

I left for school (University of Arizona) in August 1965 and turned the thing over to Al Chidester, and while I was there [September 1967] the Manned Group had become the Branch of Surface Planetary Exploration; or SPE).

Let me just outline what happened after that. I was in Tucson for three years; the first two in school (1964-1965, 1965-1966), and the third year (1967-1968) I was there sort of cleanup work. We returned to Flagstaff in the summer of 1969.”

*Schaber: "So what did you do when you first came back?"*

Elston: "First of all, I wasn't out of Astro; I was studying meteorites and getting an introduction to magnetics. And interpreting magnetics --or meteorite classifications with respect to magnetics led to the paleomagnetism lab [in Flagstaff] that we had that Gene ended up with. It started out as part of SPE.

What happened was that Bob Dubois and I had a project proposal in 1968. He was in Tucson--an archaeo-magnetism person. So we proposed a [lunar] traverse magnetometer staff--and Gene Phillippi was on it [as was Hans Ackermann and Jack Strobell as Branch Chief]. We had a gradiometer, a susceptibility bridge on the bottom, and a recorder. All you were supposed to do was walk around on the lunar surface. We made it simple for them. They'd go out there with this little tripod; you'd just set it and they [the astronauts] go do their chores. Then, they come back and go to the next station; and leave it again away from where they were working. From this they would get station magnetic data and the susceptibility of the lunar materials.

Well, it was the number one project [by NASA's Space Science Review panel in December 1969--and it essentially got stolen by Dave Strangway at the Ames Research Center (Mountain View, California). I saw some of the correspondence of that thing. Don Beattie then about a month or two months later sent me \$25,000--and that's what I used to start the Paleomagnetism laboratory [started the lab in 1970 in Flagstaff off Ft. Valley Road just north-northwest of Flagstaff]--but it had its roots in Tucson.

I came back [to Flagstaff] in 1968 to work--but commuted [from Tucson] for that year [1968-1969.] I then moved back [to Flagstaff in the summer of 1969] and I got involved in both Apollo mission planning and the magnetics research. We were on the fifth floor of the Arizona Bank Building. They were all afraid that I wanted to be Branch Chief! I didn't want to be Branch Chief!"

*Schaber: So you came back and you were working on some of the [advanced] Apollo planning?"*

Elston: "The Marius Hills region of the Moon, which included Jack McCauley. Thor Karlstrom was also around (since August 1965). Al Chidester had brought him in from Augustana College. But then something happened [earlier] whereby Al got relieved [of being Branch Chief] or whatever--and [Arnold] Brokaw came in [September 1967].

When I came back I also chased the Allende meteorite fall [in Mexico] (8 February 1969)."

We began geologic mapping and paleomagnetism studies in 1969 in the Hackberry Volcanic Center (SE end of the Verde Valley, Arizona) assisted by Gary R. Scott. This area was later used for astronaut training. We took [astronauts] John Young and Charley Duke down there."

*Schaber: "You went to Houston for some of the Apollo missions did you not?"*

Elston: "For Apollo 16, we were at Houston. I had gotten pretty handy with the stereo plotter; we had mapped the Apollo 16 site in detail. And as Muehlberger said--"You mapped every rock in the quadrangle!" I figured I could follow them on the Rover Roving Vehicle if they would talk enough. So I got into another room in Houston with one of these plotters--so I wasn't with the main group--the ALGE Team. I was down stairs then with this plotter. I tried to keep up with them [the astronauts] on the plotter stereomodel, but they lost me. On Apollo 17, I believe that Ed Wolfe tried the same trick."

*Schaber: Tell me something about Phil Shafer, Gene Boudette and Val Freeman who Arnold Brokaw had transferred [without discussion] to the Branch of Surface Planetary Exploration."*

Shirley Elston: Phil Shafer came in 1969--[followed shortly thereafter by Val Freeman and Gene Boudette as well.] Phil Shafer was later a member of the City Council in Flagstaff and died while in office.

Okay, I will fill you in on Gene Boudette and Phil Shafer. I don't know when they came, but it was enough time to get involved for planning of Apollo 16. Gene Boudette knew Phil from the Boston Office I guess—from USGS geologist Link Page. Phil, being this Renaissance man, had never been too closely controlled by any of his previous supervisors. But we worked together beautifully on the Apollo 16 mission plan.

So that gets me directly out of Astrogeology. I transferred to Branch of Central Regional Geophysics in September 1973--and stayed in Flagstaff of course.

The following details of Don Elston's career between 1969 and 1991 were taken from Don Elston's USGS Professional/Technical Record (PTR):

[May 1969-February 1971]: Don recognized that all meteorite classes (including achondrites of then-postulated lunar origin) have extra terrestrial remnant magnetism, leading to the preparation of an experiment proposal submitted to NASA (Apollo Traverse Magnetometer Staff: Co-Investigators, R.L. DuBois, H.D. Ackermann and J.D. Strobell). They field-tested a breadboard model of the magnetometer staff. As a result, the Space Science Review Panel rated it the top experiment in December 1969, but the experiment was taken over by Ames Research Center in California. Don then began setting up a paleomagnetism laboratory at Flagstaff with H.D. Ackermann in 1970 using \$25,000 provided from NASA Headquarters. In 1969 Don began mapping and paleomagnetism studies of the Hackberry Mountain volcanic center, Central Arizona, assisted by G.R. Scott. This area was later used for astronaut training. Don completed his lunar geologic map of the Colombo Quadrangle and mapped the lunar geology and prepared a reference mission plan for an area of the Copernicus Peaks region, then considered as a potential landing site for either the Apollo 16 or 17 missions.

[February 1971-June 1972]: During this period Don Elston was a Co-Investigator on the Apollo Lunar Geology Experiment Team for Apollo 16. The Principal Investigator was W. R. Muehlberger. Don prepared regional and local geologic maps of the Descartes landing site area with E.L. Boudette and J.P. Shafer. They conducted geologic training exercise for Apollo 16 crews at the Hackberry Mountain area, Central Arizona, and participated in other field geologic training exercises in New Mexico and Nevada.

During this period Elston began paleomagnetism studies of Proterozoic rocks of the Grand Canyon, and he and Gene Shoemaker in August 1971 began a detailed magneto-stratigraphy study of the Moenkopi Formation (Triassic) at Grey Mountain, Arizona. In October 1971 Elston continued paleomagnetism and stratigraphic field studies in rocks of the Grand Canyon with C.S. Gromm and E.H. McKee.

In June 1972 Elston became a Co-Investigator of the ERTS-1/Skylab investigation of the Colorado Plateau Margin (area-D North Central, Arizona). Under principal Investigator A.F. H. Goetz of JPL (Pasadena, California), Don continued sampling of Proterozoic rocks of the Grand Canyon and began a sampling [coring] to ascertain the paleomagnetism parameters of Late Precambrian rocks of Central Arizona (Apache Group and Troy quartzite). In August 1973, Elston began a Sedona (Arizona) Water Resources mapping project assisted by W.D. DiPaolo.

In September 1973, Don transferred from the Branch of Astrogeology to the Branch of Regional Geophysics under Branch Chief Marty Kane [previously with Astrogeology in Flagstaff]. He continued field geologic and paleomagnetism studies in Proterozoic and Paleozoic rocks of northern and central Arizona. He mapped (with W.D. DiPaolo) four 7 1/2 minute quadrangles in the Sedona area, Arizona, employing high altitude, high resolution RB-57 color photographs; and compared ERTS-1 images and Skylab images. [The above was taken from the Professional/Record of Donald P. Elston].

[Author's Note: Don Elston officially retired from the USGS on 31 September 1991, but was in Rehired Annuitant status with the Survey for two additional years (1992-1993). Don and his wife Shirley still reside in Flagstaff, Arizona.]

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**Walter Fahey** (Machinist)

The following was taken from an interview with Walter Fahey (Phoenix) by Gerald G. Schaber on 2-2-2001:

Fahey: "I was born and raised in upstate New York. I left there at the age of 19. Before I went into the service, I had worked as a machine operator at Savage Arms Corporation in Utica, New York --building machine-guns there. Then, I went into the Marine Corps; that was the fall of 1942. I got discharged in Jan. 1945. I went to work as an apprentice machinist in Baltimore. I stayed there for several years.

They put me to work there in Baltimore in a small shop--a captive shop--it was kind of like the machine shop we had there at USGS--it wasn't really creating something on your own; it was only building what somebody wanted--which are the best places to work. I worked out at Hughs Aircraft Company. After that, in 1952, I got tired of Baltimore--and I guess it got tired of me--and I went to Tucson, Arizona and worked for Hugh's Aircraft Company. I worked there about twelve years. I was in management there. I was a tool and die maker--and everything else you can think of. Even took a three-month trip over to Germany. I was in Germany, Holland, and Spain.

At Hughs, I was taking atomic warheads out of--I don't remember the name of the missile anymore-- but anyway, it took us about three months to get those out of the missile."

*Schaber: "It sounds like a pretty dangerous job!"*

Fahey: "It always felt like it, but it really wasn't. You had to go through quite a procedure to get one of those things out.

This was 1963 that I was in Tucson. I left Hugh's Aircraft Company, and the next job I had was with the USGS (Branch of Astrogeology) in Flagstaff."

*Schaber: "How did that come about?"*

Fahey: "Well, I was working in a small shop in Tucson. They had a big layoff up there, and I got into a small beef, and one thing and another. I was working for a small shop in Tucson. Somebody from the employment office called me and told me that there was going to be a couple of guys from the Geological Survey in Tucson interviewing for a job with the USGS at there [in Flagstaff]. Low and behold, that's when I met Don Elston and Bob Blecha [Astrogeology's original and only machinist at that time]."

*Schaber: "So Blecha was here first. When did the other guys show up?"*

Fahey: "Bob [Blecha] was buried with work at the Survey; there was just him there at that time (as a mechanist). So I went to work up there on 15 June 1964. It was getting pretty hectic up there then as far as we were concerned, so I told Bob I need some help. So, Bob Blecha got the ball rolling on that."

*Schaber: "What were you doing when you first got here?"*

Fahey: "The shop was out at the Museum of Northern Arizona then."

*Schaber: "So, you got here--and Bob was here--and you at the Museum. That was in 1964. What were you working on then?"*

Fahey: "Oh, dozens of things. They were doing quite a bit of work out at the [Sunset] Crater by that time--you know, where the aa [lava] is."

*Schaber: "Oh, Bonita Lava Flow."*

Fahey: "Yeah. We went out there. And Dave Dodgen was working there at the time. He was a physicist I believe."

*Schaber: "Batson had set up a simulated Surveyor camera set up on Bonita Flow back in 1964, and they were doing some work--taking photos before Surveyor I landed."*

Fahey: "Dave Dodgen and I together built a periscope for that plywood LM

There was a bunch of things we did out there for that. They had a camera they put together. I think it operated off the LM too--you know--the mockup. I recall that was made from. The main frame of the thing was a coffee can and I went down to a toy store in Flagstaff over on Fourth Street and found a little merry-go-round over there that had a plastic dome on it. I took the plastic dome off and put it up on top of the coffee can and the camera was mounted inside. It actually looked like somebody had planned it that way. You didn't want to examine it too closely, but the bloody thing worked."

*Schaber: "We were very innovative in those days."*

Fahey: "Well, we just about had to be. You couldn't order anything in Flagstaff. If it wasn't for a cattle ranch or a railroad, you couldn't find it in Flagstaff.

We did a lot of work up at the Museum [of Northern Arizona]. Of course, we built that heavy test stand for Hank Holt. Remember, he was testing all that stuff? He had those terrifically hot lamps in there. I don't remember when that was that he was testing that... Anyway, that was a pretty good-sized project he had going there. The one thing we did that was a real killer was that three-lens camera. This was a camera that was built there in the shop. I built the camera."

*Schaber: "You mean Shoemaker's Apollo Stereo Camera?"*

Fahey: "yeah, it had a 6-inch stereo base, and in the middle, there was a telephoto lens. They all had to be set up so they would all fire at the same time. Somehow, we got that to work. I don't know what happened to it."

*Schaber: " Well, I think it probably then evolved into that Lunar Surveying Staff that was going to be automated, where you knew what the tilt angle was and all that. Then, it got so damn expensive, and some people at NASA didn't want it [they thought it would be too hard for the astronauts to use]."*

Fahey: "They were going to have another camera made--based on the ones that I built. They took it to some instrument shop in Pittsburgh I believe; and they spent a million dollars on the darn thing--and never got one that worked. I understand they went belly up after that. I don't know if that's true or not."

*Schaber: "Yes, that was one of the things we worked on that never really flew. Gene had this idea of freeing up the astronauts of everything--keeping track of their photos; he didn't want them to worry about that."*

Fahey: "One of our big problems was that everything we built was a prototype. And we never had anybody there that understood that a prototype is exactly what it says it is--so you never got a chance to build a new generation out of it.

You know Gene [Shoemaker] was a remarkable man, but I told him one time about this. I said, Shoemaker, you remind me of a flock of quail. He said, how's that? I said, because you're flying off in all directions simultaneously. He was--you know how he was. I don't know how the man kept his sanity."

*Schaber: "I don't either. So you were working on the early camera prototypes. What kind of machine shop did you have set up originally, and where did it move too. After the Museum, where did you move it?"*

Fahey: "We went down on West Street [in East Flagstaff]. That long building on the east side [of Flagstaff at 1733 N. West Street; see Fig. 21 in text]."

*Schaber: "That would have been about 1965--I guess."*

Fahey: "Yeah, probably 1965-1966.

*Schaber: "What kind of machinery did you have out at the museum and what did you end up with down there [on West Street]?"*

Fahey: "Well, we had basically the same stuff. We had picked up a good Gordon milling machine up at the museum, and we had picked up a real fine Hardings lathe up there--a small

lathe. We had a big old monster lathe there that was too big for most of the work we did. Elliott Morris had gotten a lot of that stuff from surplus. Elliott was quite a hand with surplus.”

*Schaber: "Yeah, Elliot was very clever."*

Fahey; "He did a real good job at that. And we got a Bridgeport mill. I think that Elliott got that on surplus. Then, we bought a new one.”

*Schaber: "When you got up here to the Center--building Four on McMillan Mesa [in the late 1970's]--you had a really huge machine shop there for a place this size. So, what did you have basically--instrument wise--in the big machine shop up at Building 4 on the Mesa?"*

Fahey: "We had the same big Monarch lathe. We had two vertical Bridgeports, and we had a vertical Gordon. We had that Hardings lathe. We had a real good three-dimensional Pantograph--God only knows what happened to it. We had an excellent welding machine.

At that time, money wasn't a problem. If we needed something, we could pretty well just get it; and you didn't even have to beg!"

We bought two of the milling machines new. One of the lathes new--the big welding machine was bought new. Then, we bought a real good surface grinder. That was there in that grinding room. I don't know what happened to that either. I don't think it is there anymore--from what I heard. We had a jillion small tools, and we had several drill presses. We had a large metal shaper that I picked up at the airport down in Prescott.”

*Schaber: "The Apollo Geology hand tools. Tell me about that. You had something to do with fabricating some of those as prototypes--scoops, the hammer, the gnomon, etc."*

Fahey: "I didn't have much of anything to do with those. Earl Butler [another machinist who worked for Astro at that time] worked on those. He made- I don't remember who he made it for--kind of a nice contraption there. I don't know that they ever used it--they might have been afraid of I---but it fired squib charges [proposed to NASA to be included as part of the then proposed Apollo Lunar Surface Seismic Equipment Package (ALSEP)] experiment.”

*Schaber: "Oh yeah; I believe that that active seismic (explosive squib) experiment ended on Apollo 16; however, John Young (I believe) tripped over the multiple line that went to the seismic experiment and broke it--so they didn't get a chance to use it. However, I think it was scheduled to be fired on that mission."*

Fahey: "It was a real nice piece of equipment that Earl made. It could fire one, and then rotate the firing head on the thing. I forgot how many it held, but it held at least six or eight.

*Schaber: "This was something that [Bob] Reagan and Joel Watkins and those guys wanted for seismic reflection or refraction experiments on the lunar surface."*



Fahey: "It was supposed to work like the regular charges geophysicists set off--to get a seismic reading."

*Schaber: "You also had a fellow by the name of Green with you in the Astrogeology machine shop—right?"*

Fahey: "Yeah, Kenny Green. He lives out in Apache Junction now."

*Schaber: "So we had a tremendous group of machinists. We had a good machine shop for a place this size. It was pretty impressive. There are people who wonder how we did all of these things in such a short time period. It's because the money was there--the enthusiasm was there. There was a big program--there was a goal there--right. And everybody was excited about it. And like you say, if money is not a problem--and you have a really exciting goal--then this country needs something like that now."*

Fahey: "Yes, we had a wonderful place to work there. One of the nice things about it was that the supervisor we had at the time--Bob Blecha--would tell you what he needed--then go off and leave you alone. Mr. Butler got him [Blecha] into that mold though. He was out there telling Earl something one time--and Earl had been around quite a while. Earl turned around to Bob and said Blecha how in the hell do you think I got along for fifty years before you showed up? And that was a good deal. Bob never did that again. If we needed help, he was there to give it to you."

Fahey: "I will be 78 years old this summer [2001]. Bob Blecha must be 90 something!"

*Schaber: "What part--if anything--did you do on the designing, building or fabrication of the Explorer and Grover vehicles that Astrogeology built to simulate the Lunar Roving Vehicle [LRV]?"*

Fahey: "We did whatever machining had to be done. There wasn't a lot of machining."

*Schaber: "Tell me one of your funny stories about Gene Shoemaker in those early days at Flagstaff."*

Fahey: "One of my favorite stories concerns Shoemaker. I used to always dread Friday afternoons at 4:28 PM because Gene would show up then and he would have twenty hours of work for us [usually over the weekend].

*Schaber: "To do in five hours!"*

Fahey: "I could always depend on his. If I didn't get out of work, I could depend on Gene to be sure that I didn't have a weekend. So anyway, he comes blowing in this one afternoon and I was already scheduled to work that coming weekend."

*Schaber: "We all were!"*

Fahey: "But anyway, Gene said, I've got to have this and I have to take it Monday morning. I going to go here--I am going to go there to an important meeting or something. Anyway, I said, I've got a job here that I've got to finish up and it's got to be out of here by Monday. He said, oh, you're already working the weekend. I said, yeah, it looks like it. All the time, I'm praying to myself, go ahead Gene, you've got a good start, keep it going! So, he said well I've got to have this thing out of here--whatever it was he wanted. I told Gene that I was working on this high priority thing at the time. He said whose job is that? And I kind of said thank you God--and I turned around and said: "well Gene it is yours!" He had forgotten that he even had given that earlier job to me."

*Schaber: "That is just too typical of Gene back then."*

*Hal Stevens just raves about Elliott Morris. He says he [Elliott] could do anything. He would build anything. He was very handy that way.*

Fahey: "Well, he was a lot happier tearing things apart than he was putting them back together. I had to get after him one time about that. We'd make some instrument or other, and the guys would take it out into the field--and, of course, the first thing they'd do was screw it up. Then, Elliott would take it apart--but he didn't know how to put it back together. So, he would bring it in a tin can or a paper sack, or whatever in the hell he had. And I was supposed to remember how to put it back together--no matter who made it--there were four guys there--anyone of them could have made it.

He always had these things apart---you know--just junk coming back. I day I got to him and told him, Elliott, I want you to do me a favor. He said what's that? I said, put your screwdriver and your pliers in your desk drawer and leave them there!

Were you there Jerry when Elliott retired? Well, I had a whole bunch of miscellaneous parts--all kinds of junk there in the shop in a box. It probably measured about 2.5 feet by 18 to 20 inches and 7 or 8 inches deep. And when Elliott retired--and, of course, I always happy to help people retire. They had me in part of the deal [the retirement skit] there. I presented that box of junk to him--as part of the skit. He said, what is this? I said those are left over parts from all the things that you tore apart. He didn't know what to say, but he said I'll tell you what--I'm going to take it."

*Schaber: "You know-- after talking to everybody-, I think the success story we had with Astro, and all the work we did in such a short period of time (that nobody believes when you tell them)--is attributable to lots of drinking, smoking, and coffee.*

Fahey: "Well, everybody we had around Astrogeology was someone who was very intense about what they were doing."

Schaber: "You're right. Shoemaker was contagious. His enthusiasm was very contagious."

Fahey: "Oh, absolutely, absolutely!"

Schaber: "In addition, we had the program, we had the money, and we had the excitement. You didn't go on a plane anywhere that you didn't tell the guy--everybody sitting next to you--the whole story of what you were doing; and they asked you all of these questions because they were fascinated with Apollo. Now [today], you sit next to somebody, and they don't say a word. It's really weird."

Fahey: "They are different kind of people."

Schaber: "And it a different time—and not necessarily a better one!"

Fahey: "Well, I don't know if they are thinking about anything or not. Everybody is into the thing where if you don't make so many dollars you can't buy so much stuff."

Schaber: "And they want everything--now. And the kids want everything. When they first get married---they want to buy a new house--and then they get into debt."

Fahey: "Yeah, they're trying to retire at the age of 22. All of this stuff they want that we never got until we were in our fifties."

Schaber: Amen brother."

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**Richard H. "Dick "Godson** (geophysicist, b. 1929; B.S., Notre Dame University)

The following was taken from an interview with Dick Godson by Gerald G. Schaber on 16 February 2001):

Godson: "I was born in Saranac Lake, New York, on Nov. 20, 1929. I went to high school there and then went to the Notre Dame University and received a degree in geology in 1951. Then, I went into the Marine Corps in Korea as a Second Lieutenant, and as a forward observer. I spent two years in the Marines and got out in 1953. Then, I really didn't know what to do. I just fooled around, and then went to New York City and did some engineering-type work for a few months. My Uncle got me in to see the Chief Geologist of Standard Oil of New Jersey to see if he could get me a job. He suggested that I should go back to graduate school, suggested either Oklahoma or to Texas. So, I was able to get into Texas and went down there to get a masters degree in Geology.

During my first semester there, I took a course in geophysics--and I didn't have a clue what it was; it was an elective. I just kind of fell in love with it and decided that I really didn't want to be a geologist, but that I would rather become a geophysicist. However, I kept on working toward getting my Masters in geology. And during that spring--it was April or sometime in there--I saw this ad in the paper from a Dallas Company called Geophysical Servicing Inc. They were looking for people, and so I thought well, why don't I just try to get an interview? They hired me on the

spot; so I just dropped out of graduate school and went to work for Geophysical Servicing Incorporated, which was the major contractor to oil companies at that time. They were at the forefront of research, and actually were the first to develop digital methods of seismic a few years later.

Anyway, I went to work for them and spent the first year at Houma Louisiana, and got an opportunity to go overseas. So, I took that and went to Sumatra, Indonesia, and spent three years there--where I got married. My first daughter was born there. I spent three years there--came back to the states and spent just a few months in Dallas--and then went to Libya. We were the first crew there. That would be in 1959. I spent two years there. We had a son born there. Then I came back to the states; and I was on what they called "vacation relief." That meant that I moved about thirteen times in one year, going from Louisiana, to Californian, to Texas and Ohio. During this time I said, no, I couldn't do this the rest of my life with a bunch of kids. So, I was in my hometown and put in an application to the Civil Service for a geophysicist position. I didn't hear anything for a long time; and I was finally transferred from New Orleans to Monterey, California. While I was in New Orleans, we worked for a Chilean Company called INAP, and during this time, the Chilean representative kind of liked me--I forget when--but I was offered a job with the Chilean National Petroleum Company. When I was in California, that's when the final offer came--so I quit my job with the Geophysical Services, Inc. and went to work for Impreso Nazionale del Petroleum, or INAP for short.

So I went down to Punta Arenas, Chili, which is the southernmost city in the world. It is right at the tip of South America--right across from the Island Terra del Fuego. It never got real warm, and the wind was just horrible. Sometimes of the year it was like 100 miles per hour was common, you know. But, of course, the money was good. That's why I took the job.

So after I was there about seventeen months, I get a letter from Joel Watkins. He was then in Washington D.C. I believe--or had just gotten to Flagstaff--I'm not sure. He asked me to come and run the seismic operations at [the Branch of Astrogeology] Flagstaff, Arizona. This was in the spring of 1964. Well, I had a two-year contract with INAP. So, here I am about 8 months short of doing that--so I explained the situation down there. They said if you stay about 18 months, you could leave without penalty. So, that is what happened. I was about to leave there around April, and that's when I got to Flagstaff. That would have been in 1964.

I didn't even know what I was getting into--really. It was just a one-page letter from Joel [Watkins]. I was supposed to run a seismic field crew for Astrogeology. Anyway, that is how I got to Flagstaff."

*"Schaber: "So what month in 1964 did you arrive in Flagstaff?"*

Godson: "It would have been in March or April--somewhere in there."

*Schaber: "Was your first assignment, pulling together the seismic crew?"*

Godson: "Joel had actually ordered a big seismic truck from an outfit in Houston. I was to get that truck and get a field-crew together, and we were going to do field work at the lava fields [around Flagstaff], at Meteor Crater, and elsewhere.

The office was up at the [new Astrogeology wing] built at the Museum [of Northern Arizona]. That's where the first group of us was. There weren't many people there at that time. There was Joel [Watkins] and I think maybe Ken Watson was there--and Gene [Shoemaker]. There weren't too many people."

*Schaber: "So, you found out when you got here that what they were trying to do is plan some geophysical experiments on the Moon--right?"*

Godson: "In lunar-type terranes--you know-- to get an idea of what it would be like up there on the Moon. We went to Meteor Crater, Pisgah Crater out in the Mojave. Mono Crater in California, a few places around Flagstaff actually. We had to go out there in the Babbitt lands [around Flagstaff].

Another thing that we did was to help train the astronauts. We went with them down to the Pinacates Volcanic Field in Mexico with that seismic truck. That would have been in 1965-1966 [see Table I in text].

In the late 1960s, the seismic industry was going digital. I got very interested in that; I studied FORTRAN [Programming] on my own and started doing a little programming. Then, I actually left that the *In Situ* geophysics group at Astrogeology and joined the Computer Division there in Flagstaff. That would probably be 1968--or something like that."

*Schaber: "What did you do over in the Computer Division in Flagstaff, and when did you leave there? You were helping them built up the Computer Division--right?"*

Godson: "Fred Sowers was the Chief [of the Flagstaff Computer Division], and Gary Selner was also there.

*Schaber: "And what kind of computers did we have at that time?"*

Godson; "Oh boy, there was an old IBM-360-30."

*Schaber; "That was a big one at the time."*

Godson: "It was a real pain. You had to spend the whole night to get it so that the operation system would work.

I stayed there for two years, and then I got a chance for a promotion in the Computer Division--so I went to Menlo about 1970. I was there about a year and I got a chance to go to Jeddah, Saudi Arabia--in the Computer Division. So, I went to Saudi in 1972--I think it was

I went back to Menlo after the two years in Saudi. When I was in Saudi, Marty Kane came over for a visit. And I think he called me later when I was back in Menlo and asked me if I wanted to come to Denver to help them out in computers and geophysics. This was the time when the Branch of Geophysics in Denver really expanded about 1974. So, I left Menlo and came to Denver in 1974--under Marty [Kane].

*Schaber: "It is small world."*

Godson: "Yeah, well, he knew me from way back!" So I spent the rest of my career in Denver, and retired in 1990."

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**Mortimer ("Tim") Hait** (1931-2003; geologist; Ph.D. Penn State University) arrived in Flagstaff to work with the Branch of Astrogeology a few days before Christmas in 1965 to work with the early Apollo Group.

The following was taken from an interview with Tim Hait by Gerald G. Schaber on 16 April 2001:

Hait: "I was born in Caldwell, New Jersey (in North-Central New Jersey) and grew up in Northern New Jersey where he went to school. I went to Elementary and high school in Madison, New Jersey. I spent one year as a geology major at Vanderbilt University; however, I was not focusing there. So, I went to Lafayette College in Easton, Pennsylvania, and graduated from there with an AB degree in geology in 1955.

I wanted to be a vertebrate paleontologist, training out at the University of Chicago. Brenda (Tim's wife) and I went out to the University of Chicago to look around for a place to live--anyplace we could afford. The places where we would want to live were too far away and cost more than we could spend. So, the head of the department (Geology/Vertebrate paleontology) at Lafayette sent a letter to Frank Swartz, head of the department of geology at Penn State. So I ended up going to Penn State in General Geology--which I enjoyed.

[Professor] Dick Jahns came there (to Penn State) after a while. It was pretty neat having Dick there. Then, John MGonigle arrived, and John and I became fast friends. Dave Schleicher was there also. I taught Dave undergraduate historical geology laboratory--and of course Dave was the brightest guy in the class. Dave was always a good student, and I enjoyed him. He was smart and could think up questions you couldn't answer. [Dave Schleicher went to Penn State for his B.S. in geology, then went to Caltech for his master's degree; then followed Dick Jahns to Penn State where he received his Ph.D. (See Dave Schleicher's biography above.)]

After completing at least my class work for an advanced degree, I joined Texaco Oil in Casper, Wyoming in 1964."

[Author's Note: Tim Hait, Dave Schleicher, and John M'Gonigle would eventually work together at the Branch of Astrogeology in Flagstaff starting in 1965. Gene Shoemaker would also call on

Dick Jahns Dick Jahns to act as a senior geology advisor to help in the geologic field-training of the Apollo astronauts; see main text.)

[Author's Note: Tim Hait went to Texaco with only his Bachelors degree. He eventually received his Ph.D. from Penn State, but says it took a long time. Tim went directly from his BS to the Ph.D.]

Hait: "My thesis was on an area in the Lemhi area of East-Central Idaho that I mapped. At the same time, John MGonigle was working over in the Beavershead Range of Southwest Montana--east of where I was. So we had a bunch of Penn State folks out there working in some of those Basin and Ranges. When I was working with Texaco in Casper I got in touch with John--or maybe he contacted me--and said that he had joined the Survey on this space program project--and that it would be pretty neat.

So, I was finding out that I was not cut out to be an oil geologist. They did things differently; they needed me to be different than I was--is more like it.

So John McGonigle set it up with Gordon Swann [then at the Branch of Astrogeology in Flagstaff] to work on some papers; and it was okay for me to come and join because they were looking for people in Flagstaff. It sounded super to me. So, we moved to Flagstaff a few days before Christmas in 1965. I was sworn in by Max Troyer A few days after Christmas.

I was put to work on Early Apollo--on some of John's [MGonigle] projects, putting together the Apollo Lunar Geology Experiment.

We got with the guys out in our Flagstaff machine [instrument] shop and dreamed up [Apollo] tools, and tried to come up with plans that the shop guys could use to create these tools--drive tubes, tongs, hammers, etc. Then Gordon Swann, me, and a few others, were also qualified to fly the lunar trajectories [zero-g] in those K C-135 Air Force planes [referred to as the "vomit comet."] That was really neat--once you got past the first stages of being sick.

In the plane we had our little lunar drive [core] tubes, designed and built in Flagstaff. We drove them into a scoriacious obsidian rock, and we found that in the space suit we needed to cock the hammer head a little bit off the centerline of the hammer handle because that's just the way it was easier to do. And we scooped, and we saw how it worked to scoop loose material; and of course loose material went flying all over the place. However, we learned a lot from those missions in the K C-135 planes. We were able to feed it into the final design of these lunar tools. Of course, we were able to get together with the guys in various other companies--like Martin Aerospace--who were going to design these tools--and come up with their own design. I believe that NASA ultimately ended up making the Apollo Tools in their shops.

I remember my first trip to Houston with Red Bailey [Author's Note: Red Bailey recalled that the meeting was on 23 January 1966.] It was a Martin Hand Tool presentation at MSC/NASA. Someone said watch out for this guy Uel Clanton [one of the MSC geologists who Dale Jackson and the Astrogeology MSC astronaut training group had dealt with earlier]. Uel came across very

sternly because he was on the “NASA” geological side--and that was basically in competition with the USGS. But Uel turned out to be an okay guy.

The purpose of this first trip to Houston was to watch Clanton's presentation of a box of tools that had been built by Texas Instruments Inc. [according to Gordon Swann.]

Gordon wasn't there. He may have been working [at that time] with Joe O'Connor on some of the Advanced Apollo (AAP) missions. I think some time later after I arrived in Flagstaff, Gordon Swann came back to early Apollo and became Shoemaker's second guy in command on Early Apollo.

Anyway, we went down and looked over the tools--drank beer someplace--and met Uel Clanton and some of the other guys. So that was the beginning of many such meetings.

With regard to Astrogeology's field tests out in Hopi Butte, Tim Hait recalled for this author the following:

Hait: "Oh yeah, I remember one test when we had all the astronauts sleeping in tents out there. We could see the marks of the sidewinders [rattlesnakes] in the morning after. That was the time when we were having those good steaks that Bill Rust--our carpenter—cooked. Of course, they were just covered with sand because the wind was blowing so hard. But those exercises out in Hopi Buttes were really neat [Author's Note: The expression “really neat” was always Tim's favorite.]”

Sometime later Tim recalled to this author:

Hait: “I was in a space suit once or twice in the Cinder Lake Crater Field. I can remember that for some of those simulations in Flagstaff—after the Apollo 1 fire in 1967--we had to use regular air. But it had to be pure, and it had to be clean. It had to be tested by whoever tests air to make sure that when we breathed in the spacesuits that it was fine, and that it wouldn't choke us or anything like that. That always took a long time to get the air tested.”

The suited tests were hard work! The radios were hard to use because you had to blow in them to activate them, and then talk--but it was all part of the fun of developing the system!

One of my Survey heroes, Ed Ruppel said--you know Tim, Shoemaker was never able to attract experienced Survey guys down there to Flagstaff-- which turned out to be wonderful. Because he was unable to attract all these high-bound, older, Survey guys--the GS 15's and 16's--what he got were all these young Ph.D.s who were smart and enthusiastic, and who were willing to figure out options. So that was really good that Shoemaker couldn't attract standard Survey guys; so he attracted inexperienced guys that got the job done.

My job in the ADF during those early tests was as the sketcher; whose task it was to turn the astronaut's verbal description into some type of pictures--of rocks, craters, crater profiles, and distribution of rocky terrain--whatever it was. That was really fun!



I was impressed by the overall gradual progression of the simulations, and other aspects of the geology training session--and all of the people involved in the missions. We went on field trips with just the NASA geologists, Flagstaff geologists, and the astronauts--and a couple of jillion support people who just didn't give a damn. We started with that. But then, the whole system of simulations, geology field trips, and geology training and communications evolved into really big, complete, sessions that included everybody who had something to do with the actual missions. This included Flight Controllers (from MSC), the astronauts slated for actual Apollo missions, the various kinds of support people, the television people, the camera people, the geologists, the backroom geologists (our people in the actual Mission Control Center in Houston). Because after a while we had guys like Lee Silver, who was a verbal powerhouse--who could explain to these guys [like Flight Director Gene Kranz in Mission Control] why we needed [or at least wanted to] to talk to the astronauts straight away without any flight controller in between. Also, they wanted to know why we needed to ask the astronaut questions at all.

Gradually, the NASA people began to get the idea that, yes, it is helpful for everybody to know what is going on with the missions, and therefore it is important for all of us to be aware of how these training sessions go. We need to participate in these training sessions so we can all learn together how best to run the overall missions.

So we graduated or evolved from Apollo 11--which was us [the Lunar Geology Team] standing by [at Mission Control] and taking a few notes--into the full-blown missions later [Apollo 15-17] with Lee Silver finally actually talking to the Apollo 15 and (I believe) the Apollo 17 crews once in the LM between EVA's [on the lunar surface.] That was really neat--that whole evolution!

I vaguely remember going on one Apollo 11 astronaut field trip somewhere in Texas; it may have been Big Bend (Texas). I didn't do anything but watch. Buzz Aldrin was there, and Schmitt was there. I was there as somebody getting used to being in the astronaut training loop.”

[Author's Note: This was most likely the Apollo 8 crew trip to Big Bend, Texas on 13-14 February, 1968 with participating astronauts Borman, Collins, Armstrong, Anders, Aldrin, Lovell, Haise and Schmitt. Alternately, the trip could have been one for the Apollo 11 crew on 24 February 1969 with astronauts Armstrong, Aldrin, Lovell and Haise. Jack Schmitt was not listed as having participated on this trip by Phinney (unpublished work).]

Hait: “At that time the astronauts were fighting the idea of "older versus younger,” as opposed to specific numbers of years; [being engineering types] they didn't like the vagueness of it--these rocks are older than these other rocks. They said how much older? We want to know how much older. We did get that straightened out with them.

Preceding the Apollo 12 mission, Gene [Shoemaker] said hey Tim I'm really busy and they want me to talk before the launch down at the Cape in Florida-to give some kind of general talk to an assembled group of people [probably visitors] at KSC [the Kennedy Space Center]. He ask me to go in his place.

I said sure, that sounds like fun. So I had some slides made to show how the meteors come in and disturb the soil, and blast the soil out; where the craft was supposed to land; what the guys were

going to look for. It worked out pretty well, except of course, as soon as I started talking someone down in the front trying to keep his voice down said okay make it quick, cut it, cut it!"

Tim went to Denver about in 1973 or 1974 and joined the USGS, Branch of Central Regional Geology. He was Assistant Branch Chief for a while. Following the eruption/explosion of Mount St. Helens in May 1980, Hait became spokesman for the Survey on the activity there; he was stationed in the Vancouver/Mt. St. Helens area for the duration of that assignment. Around Christmas time he was transferred from Central Regional Geology to the Engineering Geology Branch. In late 1984 Hait was transferred out to Las Vegas, Nevada to work at Mercury on the Nevada Test site. Tim formally retired from the USGS in 1987 and moved to Green Valley, Arizona, where, sadly, he passed away in 2003.

When this author asked Tim Hait during our 2001 interview about his memories of Flagstaff --Tim replied, positive, positive, but it did take a terrible toll on the family--all of that travel--a terrible toll. But I think it was really a neat personal experience; but as far as the family is concerned, it may have been a family disaster. But personally, it was really neat, exciting experience--to work with these people who had bright ideas.

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**Don Hart** (1940-2004; Narrator/Scriptwriter), who narrated all of the Branch of Surface Planetary Exploration's pre-Apollo lunar surface geologic procedural testing and training films for NASA, started to work for the Branch of Astrogeology's Film Documentation Unit in October 1965. His pioneering family first came to Arizona in the 1880's when his great-grandfather opened up a gun shop in the infamous town of Tombstone, Arizona. Two of Don's brothers, James A. Hart and Robert W. Hart, were earlier employed by the Branch of Astrogeology in February 1964 and March 1965, respectively. James worked with Ray Batson's Cartographic mapping group while Robert worked with Jim VanDivier's cartographic/drafting section. Don Hart passed away in March 2004 in Phoenix, Arizona.

The following was taken from an interview with Don Hart by Gerald G. Schaber on 15 April 2001:

Hart: "My family first came to Arizona in the 1880s, when my great grandfather opened up a gun shop in Tombstone, Arizona. I was born in Phoenix, one of six sons of Phil and Velma Hart in 1940. We lived in Phoenix until about 1948, the winter of the big snow in Flagstaff [67"-a record until 1972-1973], which is when we chose to move there! I was raised in Phoenix up through the second grade. I was raised in Flagstaff up through the end of high school; and we wound up moving to Winslow, Arizona (65 miles east of Flagstaff) because my father (an electrical engineer) got a job there at what was then a brand new radar installation there. I graduated from Winslow High School in 1958 and went to Arizona State College (in Flagstaff, now NAU) for two years. Then, I had an accident during the summer and didn't make enough money during the summer to go back to school. So I went out looking for a more permanent job until I could earn enough money. And that's how I first got my first job in broadcast radio.

I worked in broadcast, mostly in the Tucson area, for several years, then I got married and decided that I wanted to go back and finish my degree. Various circumstances led me to decide that Flagstaff was the place that I should finish it. So I went north to Flagstaff in 1965 to see about

that. In fact, I applied and was accepted at what was by then called Northern Arizona University (NAU) to finish my degree.

In the process of also looking for a job, I found that I had two brothers employed at the USGS, who told me that they were starting a film documentation unit there at Astrogeology in Flagstaff. I thought that maybe with my broadcast experience that Flagstaff would be a good place for me to go. My background by that time was in both radio and television.

So I went and talked to a number of people--Hal Stephens, Dave Schleicher, and Al Chidester. Eventually, I was introduced to Gene Shoemaker, and I was hired in the Film Documentation Unit. They found a position for me called Scriptwriter--and that is what I was hired as. I wrote scripts, I narrated, I assisted in shooting film, and I edited film. I ended up doing all of the sound editing. In fact, after I was there, one of the facilities improvements we did under my direction was to set up a more complete sound facility to do film sound editing and dubbing. Hal Stephens was my boss at first, but eventually it was Walt Roeder.

I had two brothers who also worked for Astrogeology prior to my arrival. One was James A. Hart, who worked in the mapping section with Ray Batson's group. The other brother was Robert W. Hart. Bob did some of the cartography stuff. Bob had a tremendous artistic and engineering talent. He would up working on the design of some of the Apollo lunar tools. I remember it was his lunar geologist hammer design that was sent to NASA to be let out on bid, and so on."

After the first lunar landing [Apollo 11] in July of 1969, Don Hart decided to go to Law school. He became a lawyer in Phoenix; Don passed away in 2004.]

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**John Hallinan** (pilot) started work with the Branch of Astrogeology in 1972. John had been working at Sky Aircraft, small air taxi and flight training outfit operating out of Flagstaff's Pulliam Airport. John actually started working at Astrogeology for Jim VanDivier's Cartographic mapping group. John soon became more-or-less a full-time pilot under Putty Mills (then the chief pilot for the Branch of Astrogeology) when the Office for Aircraft Services of the Department of the Interior (OSA) ruled to require that each pilot have at least a commercial license and an instrument rating, thus eliminating many (but not all) of the geologist-pilots at the Branch who had been helping out. John flew a great many trips in various Branch aircraft, including their Comanche 260c, Cessna 210, Cessna Skymaster, and later their twin Bonanza, ferrying Branch personnel back and forth to their many meetings and Phoenix airport runs, as well as supporting other organizations (e.g., Oil and Gas, EPA, Water Resources, Forest Service, etc) with various kinds of remote sensing flights.

John left Astrogeology at the end of December in 1980 and reported in early 1981 to the FAA in the Great Lakes Region (South Bend, Indiana). He became an air carrier inspector for FAA. He left South Bend in December 1981 and moved up to Fairbanks, Alaska in December 1981. He and his wife Mary now reside in Anchorage, Alaska, where John still works for the FAA.

The following was taken from an interview with John Hallinan by Gerald G. Schaber on 8 July 2002:

Hallinan: “I am from northern Ohio. I was born just outside of Cleveland, a little place called Painesville. I just kind of lived around small towns. I actually lived up around Aurora, Ohio for a while; and University Heights for a while. I worked at a place called Barnes and Oliver. I was actually going to high school part time; I was studying engineering at the Ohio State Extension there. This was back in the 1960’s. I didn’t have enough hours to maintain a school deferment—so I got drafted. Coming out of the military around 1970, I really didn’t want to go back into engineering, or go back to Ohio, so I ended up out in Phoenix, Arizona.

I had been a pilot for a number of years back in Ohio, and once I got to Phoenix, I kind of decided that I was going to make that my profession.

I acquired a couple of airman certificates. Then I moved up to Flagstaff, and worked for a while for a small organization that was just opening at that time—an outfit called Sky Aircraft. That would have been in the March-April time frame of 1971. And of course, a lot of what we did in aviation brought us close to the activities that were going on over at the USGS, Branch of Astrogeology in Flagstaff. You guys had people at the time like Jim Lovelace and Dave Roddy. Actually, Dave was one of the key people at Astrogeology that I got to know fairly well. He suggested that—just due to the timing of several things going on at Astrogeology—that I should put in an application over there.

Eventually, Jim Lovelace moved on from Astrogeology, and he left the flight program at Astro with Putty Mills. Putty was engaged in everything under the sun; and at the same time, there was a lot of what we would call politics around the aviation activities in the Department of the Interior. Somebody did a study. They looked at a lot of the accidents, etc. and they formed a new organization called the Office for Aircraft Services (OAS)... That happened about 1972 as I recall.

So in 1972 they came up with the new requirements for anyone who was going to fly. You had to have a commercial license; you had to have an instrument rating. A lot of our guys were just private pilots—like Jack McCauley, Don Elston, and Henry Holt. Actually Roddy was only a student pilot at that time.

So, as Dave had suggested, I find my way over to the USGS in Flagstaff. I had to go over and talk to Russ Wahmann. Russ got me hooked up with Jim Vandivier (Cartography Group); so I ended up going to work for Roger Carroll over in the drafting department. That is where I started out. It was kind of a placeholder for me I guess. I began increasingly picking up an awfully lot of the Branch flights. And every time that someone would call, or Hal Masursky needed to shoot over to LA or Phoenix in the middle of the night, I would get the call and do it.

We had the Comanche 210-c for probably two years while I was there. We then shifted to a little Cessna 210 from Northland Aviation for a while. But we weren’t really approved for any of these air operations it turns out!

When Jack McCauley was Branch Chief of Astrogeology, during the Cessna 210-C days, there was an awfully lot of pressure on him because the budget was dropping after Apollo. Gene Phillippi and I went over to visit Herb Skibitsky, Water Resource Division, over in Prescott,

Arizona, to figure out how he was working all of his aircraft deals. We found out that we could just go over to the military and pick up airplanes [from Vietnam], bring them on board, maintain them, operate them, and then—when they are worn out—just get rid of them and go get another one. So, I went down with Gene Phillippi (Branch of Astrogeology) to Davis Monthan AFB and we liberated three Cessna Skymasters [push-pull propeller type]. We took them over to Prescott where we basically took the best components out of each of them.

There was military gear in them. Some of them had these little smoke racks or bomb racks in them—jettisoning doors and things like that. They were also armored. We made one airplane out of the three Skymasters; we maintained it to a civilian standard, but we never went through the drill of getting air worthiness on it. By that point, we had kind of figured out how the public aircraft trick worked—where you could just simply fly it.

I remember the days when we had two Viking Landers on Mars at the same time—in the summer of 1976. I used to make a run over to Burbank, California on a 20-hour schedule. They were getting Viking data in; so every 20 hours I would go over and pick up the data on magnetic tapes and Ray Batson's group would work on the data.

We got an awfully lot of pressure to quit using the Skymaster for transportation of people. There was some obscure requirement [in the government] where you weren't supposed to be competing with local businesses. But in any case, we continued on just doing a lot of work with those airplanes. We used to support a lot of other organizations—Water Resources, Oil and Gas. We did a lot of that in the Skymaster with William Hemphill and Robert Watson of the USGS. We put a Fraunhofer line discriminator on that Skymaster. We did a lot of remote sensing mapping with that. We did a lot of support for Water Resources, EPA, and the Forest Service. All of this work was done while the plane was still under Astrogeology; but we got money in return; we just journal-vouchered the money across organizations in the government.

Eventually, the aircraft was moved out of Astrogeology and it went to Terry Donovan over in the Oil and Gas Branch. That was probably in 1977 or so when the Skymaster went to Terry, and then we turned it into an old military twin Bonanza so we could do better magnetics survey work with it. So it slowly emerged from hauling high priority passengers and freight to more or less a remote sensing platform.

*Schaber: I am sure that you have a few stories about Hal Masursky and/or Gene Shoemaker."*

Hal Masursky and Gene Shoemaker would always show up late on Friday; they would have a crisis brewing. Occasionally it involved a flight at the end of it. You know, on a Sunday, we would get done with these maps. I jammed them into a map tube and I'd run them over to LA. More typically, they would usually go with Hal or Shoemaker, and I would simply get them down to Phoenix. For a lot of these operations, we would use the Comanche and the 210.

There were a couple of times there in Flagstaff where Hal would have it kind of worked out. Hell, he did have it worked out, I would go to Betty Bachstein [Hal's secretary], and she would kind of shove him out the door, stick his tickets for American Airlines, or whatever, in his coat pocket—

and tell me here are his tickets. Make sure that he knows where they are when you get him down to Phoenix. So we get down to Phoenix, and you know, Hal was just a little late all the time.

A couple of times, I actually worked my way around Terminal 2 at Phoenix. Back in those days before security, in the early 1970s, nobody advertised it, but if you talked to the controllers like you knew what you were doing, they would let me actually get up and drop someone off at the airline gate [on the tarmac]. And of course, Hal would just run on board, and typically be writing a GTR (Government Transportation Requisition) or something at the same time.

But I remember the last time we tried to pull that off. I think it was an American Airlines flight. The controller I guess just hadn't gotten the word about security. But security met Hal and hauled him off. They told me don't ever do this again! I am sure that they got Hal through security and that he probably made his flight [Hal always did!]

I remember one time when Hal went off to Europe with a group, and broke his leg. Later, someone [Author's Note: I heard that it was George McGill of the U. of Massachusetts] found him at the airport in Frankfurt, Germany, sitting in diabetic shock [as usual] with his leg propped up on a chair. I found out that at the hospital where the accident happened [in Switzerland I believe] Hal asked them to put a walking cast on it—and he walked out of the hospital. Hal knew no personal limits.

When Hal arrived back after breaking his leg, Gene Phillippi helped get him into the hospital out in Scottsdale. I flew down with Dave Roddy to pick Hal up and bring him home. We had to take a seat out of the airplane because Hal had to sit in the back because his leg was in a cast and couldn't extend up forward.

When we arrived back up at Flagstaff, Hal said I've got to stop and get something for Annie [Hal's current wife] before I go home. So, we pulled into this flower shop and Dave Roddy asked--you just want some cut flowers? Hal screamed no. I want something that's alive! We took Hal to his house, and Hal just insisted that we come in. I think Annie just wanted to get rid of us so she could kill him.

I remember talking to Russ Wahmann one time about a trip with Gene from Flagstaff to Phoenix, and it was snowing to beat the band. Gene grabs hold of Russ and they climb into one of the Survey Travel-alls, and they launch for Phoenix airport. The way Russ described it; he is kind of sitting in the back with Gene at the wheel, and is hanging on for dear life. Russ described how, some place off the snow-covered highway (I-17) down past Cordes Junction, Arizona (about halfway to Phoenix), they went flying off the road backwards and got stuck in the snow. Gene tried to rock it back and forward, and he can't get the Travel All out of the snow. Gene was just pounding the steering wheel and saying G-- damn it! G-- damn it! Finally, Gene calms down and Russ says I guess we will just put the chains on. All of a sudden, Gene comes to life and says we've got chains? Russ said, all of a sudden Russ said Gene was back in the game! [Gene knew then that he might still have a chance to make his Phoenix flight and meeting, wherever it happened to be].

I picked Gene up once when he was with a group of people at the Grand Canyon. We had a rental airplane. I picked him up at the Grand Canyon and flew him over to the Four Corners where he was supposed to meet with another group of people, and then go on some kind of brief afternoon fieldtrip with them. Gordon Swann was there. It was perhaps about 1978.

Getting Gene moving was the problem. He always wanted to know exactly what the latest time needed to move the airplane from the little landing strip up there at Four Corners/Monument Valley area to Phoenix. It never really occurred to me until the Sun started down that I thought—wait a minute, this isn't a lighted airstrip; we've got to get the heck out of here. So, by the time we made our way back to the little dirt airstrip, it was just pitch dark. There was not enough detail on some of the charts of the area to even tell where some of these buttes where.

So what we did--I think it was Gordon Swann's idea--we moved one of the Travel Alls down to the far end of this very short dirt strip. We parked it there with just the tail lights shining back up the strip. Then, halfway down the strip we put just a flashlight. So we had three lights, two red and one white. I took the plane down to the far end of the strip. I just sort of figured well as long as we can keep the white light between the two red lights I would know that we are still on the strip as we were accelerating. Boy, Gene just seemed to be just as happy as a clam in the sand—and off we went charging to Phoenix.

Actually on that trip we were supposed meet his wife Carolyn in Flag. He was going to pick up a suitcase. We just got to be so damn late that we just bypassed Flag and just kept on going. I think Gene probably ended up buying new clothes when he got to Washington. It didn't seem to bother him any—so on we went.”

*Schaber: “You know Gene looked an awful lot like Howard Hughes, and he acted a lot like him. Someone said that Gene could sell ice to Eskimos. I think that was Carl Roach. So you left Astrogeology in what year?”*

Hallinan: “The end of December 1980. I think it was a pay period in 1981 when I reported into FAA up in the Great Lakes region at South Bend, Indiana. Basically I ended up as an air carrier inspector for FAA. Actually Flagstaff kind of spoiled me. Once you get east of the Mississippi I don't know what it is in the government, there are more policies, procedures, requirement--it just goes on ad nauseum. After I was back there I thought I have to get out of here.

I left South Bend in December of 1980, so I only spend maybe 10 or 11 months in South Bend. There is something about being in the East. In Flagstaff when you wake up every morning and you look out at the peaks while you're shaving, and you get used to that, and its part of your existence—and you don't even know its part of your existence. All of a sudden I'm back east and there is nothing but corn, and you never hear a coyote howl, and boy you miss that—you really and truly miss that.

Finally I put two bids in the hopper, one for Fairbanks and one for Anchorage, and the guy from Anchorage called me 45 minutes before the guy from Fairbanks did. We moved up here in December of 1980 and we've been her ever since.”

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**Rae (Schaul) Harvey** (Mathematics Aid; 1950-2005), a long-time mathematics teacher at Coconino High School in Flagstaff, was born and went to high school in the Phoenix area. While attending Northern Arizona University in Flagstaff, Rae was looking for additional work to help support her schooling. A friend, Jo Ann Jordan (wife of Ray Jordan of the Branch of Astrogeology), suggested that she look into the USGS, given her math and computer programming skills. Rae reported to work with the Branch as a Mathematics-Aid for the Branch of Astrogeology in Flagstaff in June 1970.

The following is from an interview with Rae (Schaul) Harvey by Gerald G. Schaber on 1 August 2002:

Harvey: “I started working down at the Arizona Bank Building in downtown Flagstaff—on the fifth floor. I can’t remember the name of my first supervisor but I want to say something like Dailey or something like that. I definitely worked for Hank Holt at a latter time. My first job there was looking at photographs of the lunar surface and circling the size of rocks. It was mostly working with the photographs and doing data collection. I wasn’t really doing anything mathematical or statistical at that time.

I went down to Cape Canaveral with Hank Holt [to help calibrate the Apollo lunar Hasselblad camera prior to flight]; and I also went to MSC/Houston where we were actually working on some of the returned lunar samples [from Apollo 15.] Some of the other people on that USGS team from Flagstaff were Jo Ann Jordan and Anthony “Tony” Hall.

I think we were doing some sort of photometry on the samples. I can’t remember all that we were doing. But a funny story was going through the museum where there was a lunar rock on display—and everybody was looking at it. All of us were sort of chuckling, and thought we have been working with the lunar samples all day long. Here, the people were ogling them!

I actually got to put on an astronaut suit [spacesuit]. That was because Jack Schmitt was a good friend of mine; so he made sure that I got a little TLC and preferential treatment.

I also helped Hank Holt calibrate the Apollo lunar cameras.

Following the final three Apollo missions, I went over to JPL a couple of times because our computer facility in Flagstaff was not able to handle a lot of things we needed. So, I was taking big magnetic tapes of Apollo photographic data over to JPL, and doing a lot of imaging processing on the photographs—both orbital and surface photographs.

When I think of the size of the room we had our computers in [in those days]-and the type of computer punch tapes we had—I crack up!

Henry Holt left Astrogeology for a while [to take over the Geology Program at NASA Headquarters I believe] and Larry Soderblom sort of took me under his wing-- given my computer skills. I know I did some type of image processing for the lunar landing site for Apollo 17 [Taurus-Littrow].



In my classroom at Coconino High School I still have a very large picture of the Moon that was nicely image processed. And I have Apollo 17 photographs of Gene Cernan and Jack Schmitt—and then a picture of me actually driving Grover [Astrogeology’s hand-built Lunar Rover Vehicle, or LRV, used for training crews for Apollo missions 15-17]. When my students see this picture they ask who is that on the Lunar Rover? I say, that is me! I know my hair was long and blond—and now it is short and gray!

You are probably aware of this, but I still have some students who believe that we never went to the Moon! And why do they say that? What has happen? That really irritates me because of all of the work and all of the time, and all of the camaraderie and team work we had [to make Apollo actually happen.]

I finished up my work on the final Apollo 17 report and one day I quit; but I ended up coming back to work later to work with Larry Soderblom on a part time basis. That was part time because I had already taken a job teaching math in high school. That would have been in 1973.

My work with Astrogeology during the Apollo program was very exciting. I still treasure the little mementos from that whole thing. I have the Apollo 17 mug and a silver necklace that Jack Schmitt gave me with a little charm of the Apollo 17 splashdown module [Command Module] on it. You know, I will never get rid of these things. They are things I will keep the rest of my life.

Rae Schaul Harvey participated in the final geologic exercise for the Apollo 17 crew (with the Grover vehicle) on 2-3 November 1972 at Sunset Crater National Monument and the Survey’s nearby Cinder Lake Crater Field just northeast of Flagstaff. She recalls that there was a big party at Red Bailey’s home in East Flagstaff following that exercise, and that she was astronaut Jack Schmitt’s date for the evening.

[Author’s Note: Sadly, Rae [Schaul] Harvey got breast cancer and died at the age of 54 from complications from that disease on 18 January 2005. Rae was a very intelligent, caring, and wonderful human being, and superb mathematics teacher who was loved by everyone who ever came in contact with her.]

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**John D. Hendricks** (b. 1941; physicist; B.S., 1965--math and physics--Northeast Oklahoma State; M.S.-math and physics, Northern Arizona University)

The following was taken from an interview with John Hendricks (Flagstaff, Arizona) by Gerald G. Schaber on 8 October 2002:

Hendricks: “I was born in Washington D.C. My dad was in the Geological Survey there. I graduated from high school in 1959. The summer after I graduated I started working out of Denver, Colorado for the USGS as a rod man on a survey crew—surveying dam sites in Colorado. When that ended I got on with what was then called the Special Projects Branch of the Survey at the Nevada Test Site. I did everything from being a rod man to driving a water truck. Mainly, the project was short seismic refractograph studies. I could see that that wasn’t going to go anywhere,

so I decided to go back to school. I went to a little school in Northeastern Oklahoma—Northeast Oklahoma State—and was there for four years. I got a BS in Mathematics and Physics.

As soon as I graduated, I turned in the standard form for Federal employment, and two job offers came back. One was at White Sands, New Mexico, and the other was here at Flagstaff with Astrogeology. This was 1965. Since I had worked for the Survey earlier, and my dad was with the Survey, I decided to take the job in Flagstaff. I started work on 4 July 1965. My first job was with Paula Ables doing time and motion studies [in support of developing geoscience activities to be carried out by NASA astronauts on the lunar surface during the upcoming Project Apollo]. That only lasted about a year and then [geophysicist] Bob Regan showed up. Since I had been doing some geophysical work, I got on with Bob. He was doing gravity and magnetic surveys of craters. I was with Bob until 1971. In the meantime, I got my M.S. Degree in Geology from NAU while I was working at the Survey.

At the end of the Apollo Program (when they were transferring people out) I went to Woods Hole Oceanographic Institute [Mass.] with the Marine Geology Branch. I was there about a year and a half. Marty Kane was the Office Chief there, and he talked me into coming back there. And, being how I get real seasick, I didn't last long at that job. Then, a cooperative with the state of Arkansas came up. This would have been in late 1972. This work involved doing a gravity survey of the whole state of Arkansas; so I went to work in Little Rock for five years. We completed that survey and I left Arkansas in 1978. I went back to Denver with Oil and Gas Branch. I didn't stay there long because I met up with Terry Donovan, who was doing geophysics, and was stationed out of Flagstaff. So, I moved back to Flagstaff and worked for Terry (Oil and Gas Branch) until probably 1987 or 1988. We were doing airborne magnetics over oil fields. Then, when Terry left Flagstaff, I was picked up by Ted McKee of the Mineral Resources Branch here in Flagstaff. There were three of us, Gordon Haxell, Clay Conway, and myself. That was fun for a while, but when the money ran out it got to be doing mineral resource assessments, and that really wasn't my cup of tea. But while I was doing that I did get to do some gravity surveys out in Western Arizona, and helped on some seismic crews. That was from 1988 until about 1993 I guess. That was when they [the USGS] offered that first early buyout. By then I had 31 years in, so I took it [and retired in 1993]. I am still a volunteer up at the Flagstaff Field Center.”

[Author's Note: John Hendricks still resides in Flagstaff, Arizona.]

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**Henry Edward Holt** (b. 1929; Ph.D., 1961, University of Colorado, Boulder) arrived on the scene with the Branch of Astrogeology in Flagstaff on 13 August 1964. He was born on 27 September 1929 in West Virginia and raised in Clarksburg and Charleston, West Virginia, where he graduated from high school. His undergraduate work was at the University of Idaho, and graduate work at the University of Colorado at Boulder--culminating in his Ph.D. in 1961. He went to work with the Geological Survey in the summer of 1961, working for the Military Geology Branch in Washington, D.C.

The following was taken from an interview with Henry Holt by Gerald G. Schaber on 28 December 2001:

Holt: "I was born on 27 September 1929 in West Virginia. I was raised in Clarksburg and Charleston, West Virginia, where I graduated from high school. Undergraduate work was at the University of Idaho, and graduate work was at the University of Colorado (Boulder)--culminating in a Ph.D. in 1961. I went to work with the Geological Survey in the summer of 1961, and worked in the Military Geology Unit in Washington D.C.

Prior to that, I was interested in the Moon. I was the only graduate student in geology at the University of Colorado that had keys to the Colorado Observatory. I used to take people out, and we'd look at the Moon, Saturn, Jupiter and Mars. When I went with the Survey I kept my interest up. A Moon map was being prepared at Military Geology (Bill Mason and Hackman) and I was looking over the shoulders of the people doing the trafficability studies on the lunar surface.

Then, John Glenn orbited the Earth, and I got together with some people out at Goddard Space Flight Center. We were talking about photography from orbit of the Earth. I heard Gene [Shoemaker] give talks; and of course, being in the Geological Survey, one heard a lot about the budding lunar geology studies of the people mostly in Menlo Park at that time. So I expressed an interest in going with Gene (by talking to him and also knowing what was going on--and my earlier interest in the Moon).

I came to Flagstaff in June of 1964. Military Geology released me, and allowed me to come there. I showed up in Flagstaff a few weeks after Harrison Schmitt did. Schmitt went with Don Elston's group (Astrogeology's newly formed Manned lunar Exploration Studies), and I was the next one coming along, so I was assigned to work with Gene Shoemaker and Elliot Morris (Astrogeology's newly formed Unmanned Lunar Exploration Group). Gene at that time was one of the PI's on the Ranger Project, so I was with Gene for Ranger target selection for Ranger 8 and 9. Of course, at the same time, I was involved with the Surveyor Project.

So I spent those early years lunar mapping at the telescope and preparing for the Surveyor landings on the Moon. The first Surveyor (Surveyor 1) landed in 1966, so I was at Pasadena for that during the 1966 through 1968 period when the Surveyors were landing on the Moon.

Following the Surveyor activities, I sort of gravitated toward the Apollo program. That is where we trying got utilize photometric studies--a carryover from Surveyor photometric studies to photometric studies in the Apollo effort. I was with the Apollo lunar geology group through Apollo 17. I was never in SPE, but I had a lunar-funded project under SPE--but all the time I was still carried under the unmanned part of the house."

*Schaber: "Who funded that work?"*

Holt: "That was Bob Bryson [NASA Headquarters]. I was down in Houston for all of the Apollo lunar missions. I was sort in the advisory room. Gene kept me in with him when he was there [at MSC Mission Control] on Apollo 11 and 12. Then he bailed. Seeing that it was photometry and photogrammetry, and other people didn't understand it, it kind of took a back seat after Gene left."

*Schaber: "Didn't you help calibrate the lunar surface cameras?"*

Holt: "Oh yes. Matter of fact, I calibrated all of the lunar cameras, down at the Cape before they went [to the Moon], so we got the transfer function that allowed us to get meaningful luminescence from film densities. And of course, we had the gnomon with the gray scale and the color chart on it so we could measure the color--what little there was. Most colors we saw turned out to be on the last mission--Apollo 17--when [Jack] Schmitt kicked some orange soil loose. Until then the only color on the gray lunar surface was the American Flag and the gold foil of the LM.

Then, I also measured the photometric properties of some returned lunar samples at the Lunar Receiving Laboratory at MSC. I started that on Apollo 12. Then, I brought some Apollo 12 and Apollo 14 samples back to Flagstaff. It was fascinating looking at the small fragments they gave me because you see these small glass-lined [micrometeorite impact] craters. I think the largest crater I saw was still smaller than a BB. However, we were measuring the spectral reflectance, etc. of these samples. Then, I started having some problems with the containers I was using, as they began leaking. That was changing things. We had lunar soil in Flagstaff too, so I did my photometric measurements on that, and it worked out. I applied it to the data reduction, to the black and white films when we had them--and then on the color films when we had those. Then, MSC said that we [in Flagstaff] weren't advanced enough [had insufficient lab facilities] to have lunar samples after that!"

*Schaber: "How did that correspond with what you had done looking at the Surveyor photometric properties from the lunar soil--or did you?"*

Holt: "Yes we did. Our problem there was that it was more refined with Apollo because we had more fine-scale data. Remember, the Surveyor camera was a line-scan camera, and analog line scan camera. So things weren't as fine--it was a pretty coarse measurement."

*Schaber: "But you still had a color and black and white bar for the Surveyor cameras--right?"*

Holt: "Right, we still had a colorimetric chart and photometric chart on Surveyor."

*Schaber: "Okay. So you helped write up reports after Apollo in the Preliminary Science Reports--on the photometric function, and that sort of thing."*

Holt: "Yes, and of course on the Surveyor reports too."

*Schaber: So what did you do after Apollo?"*

Holt: "After Apollo, I ended up submitting a research proposal to NSF, and went to Antarctica on some Mars-analog studies--in the Dry Valleys. I also made it down to the South Pole. I was kind of a geologic interpreter for the Secretary of the Navy and the Congressmen. That was in 1971-1972."

*Schaber: "So that was actually during Apollo?"*

Holt: "Yes, that was during a period when Apollo wasn't flying. That was right after Apollo 13 when I went down there [the hiatus between the launches of Apollo 13 and Apollo 14 was between 11 April 1970 and 31 January 1971]. I was just gone the months of November (1970), through most of February (1971). I was back in time for Apollo 15 [launch was 26 July 1971]."

*Schaber: "Did you work with Ray Batson on the photometry of the photos, or getting them processed properly. You inputted that into Karl [Zeller] and those guys or did you input that data to NASA to process the photos?"*

Holt: "Well yeah, I helped NASA decide how to process the photos after that [photography] mess on Apollo 12."

*Schaber: "Why, what happened there?"*

Holt: "Well, if you look at the photographs, you'll see that the processing was incorrect."

Then I came back [to Washington, D.C. in the summer of 1973]."

*Schaber: "That's right; you went back there to work with Steve Dwornik didn't you?"*

Holt: "Yes. So I was back at NASA Headquarters for a little over two years. I was a specialist for Dwornik for geologic applications--in his various science programs.

After me back there [at NASA HQ] it was Russ Wahmann's turn. Russ was at it for a couple of years. I guess it was originally Elliot Morris, then me, then Russ Wahmann came in. Then after Russ was someone else [Author's Note: Michael Joseph Boyce replaced Russ Wahmann as the Branch of Astrogeology's assignee at NASA in the summer of 1977; see Boyce interview below in this Appendix].

*Schaber; "Then, when Steve Dwornik left NASA Headquarters, Joe Boyce took over his job as Discipline Chief for geology/geophysics."*

Holt "Yeah, Steve left because of illness. I'll say; he had a form of MS. He didn't always know where his feet were. He couldn't even feel his legs. He would always say I didn't exactly know when to go to the bathroom because he didn't have any feeling.

I came back to Flagstaff the summer of 1976, and I was part time under [then] Branch Chief Mike Carr working on Mars studies, and part time on another USGS project being carried out on the Navajo Reservation. This was a mapping study being funded under Geologic Division; it primarily had to do with people crying about the Peabody Coal mines. I was working on that project under Thor Karlstrom [formerly with Astrogeology]. We were looking at the various surficial deposits, and trying to age date them--which we did--showing that they had periods of alluviation in the streams. They would fill up and then the water table would become higher, and then you would have more violent rainstorms. That was in the late 1970s."

*Schaber: "Okay, so after that?"*

Holt: "Then in 1980 I came back to NASA Headquarters again. Dwornik asked for me to come back and work with him on the geology program again. Then back to Flagstaff, and I sort of wrapped things up and retired on 1 September 1986.

I started working with Shoemaker on his asteroid and comet studies [with Carolyn] after I retired. I started working with him in the spring of 1987. That project was funded out of NASA Headquarters. I had become aware of the project when I was funding Gene out of Dwornik's money [when Holt was back at NASA that last time]. We were also getting money out of astronomy--we were sharing it--with Bill Brunk [head of NASA's astronomy programs at that time]. He didn't particularly like Gene, so he was dragging his feet."

[Author's Note: Henry Holt still maintains his residence in Flagstaff, Arizona.]

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**Keith Howard** (b.1939; geologist; Ph.D., 1966, Yale University), who was offered a job with the Branch of Astrogeology in late 1965, arrived for work in Menlo Park in April 1966.

The following was taken from an interview with Keith Howard (USGS Menlo Park, California) by Gerald G. Schaber on 1 Feb. 2002

Howard: "I was born in Utah in 1939; I moved around a lot and grew up in California. I went to college at UC Berkeley, and then went to graduate school at Yale in New England. When I was in the process of writing my thesis and looking for a job, I contacted the USGS because I had worked for them as a summer employee. I interviewed with the Survey in 1965 and received my Ph.D. in 1966. The Astrogeology Branch of the USGS offered me a job in late 1965. I had gone down to Washington and interviewed there, and I had some contacts in Menlo Park.

*Schaber: "Who did you interview from Astro?"*

Howard: "I don't think I interviewed anybody from Astrogeology in Washington. I think my first interview {with Astrogeology} must have been with Don Wilhelms. At some point I met Hal Masursky."

*Schaber: "So what background did you have that Hal was interested in?"*

Howard: "I was a structural geologist; I had worked in Nevada--not too far from where Hal had worked in Nevada. At that time there were no astrogeology programs in graduate school. As far as I can tell, he and the other recruiters in the Branch [Astrogeology] were just looking for people who were good geologists. I had been working on the structure of metamorphic and plutonic rocks in Nevada. So I had no experience with lunar or planetary work at all.

And I have to say that Astrogeology didn't sound to me like it was the most exciting thing; I was anxious to keep working on structural geology of metamorphic rocks somewhere. But this was the

job offer, and it was the area where my wife had already started medical school in the [San Francisco] Bay area--and I thought well, this sounds good. I showed up in Menlo Park in April of 1966.

*Schaber: "What did you start working on when you first got with Astrogeology in Menlo Park?"*

Howard: "I started working on the Ptolemaist Quadrangle of the Moon that Hal [Masursky] had begun, and then dropped for bigger and better things because he was so involved in administrative activities. The Branch Chief at that time was Gene Shoemaker, although I didn't meet him for a couple of years. I was doing observation at Lick Observatory for my lunar quadrangle, but frankly I was a little frustrated.

A few months after I got to Menlo Park, I applied for NASA's astronaut program. I thought this was fun! I later discovered that it was a lot of fun. I got into the finalist program there at NASA but didn't get selected.

*Schaber "You were in there with Jack Schmitt then?"*

Howard: "No, I was in the next go-around [after Schmitt's]. There was one other geologist chosen for that round--Tony England [of the USGS]--in the spring of 1967

Then I moved to Menlo Park about this time--a few months after I got there--from Denver. Gene said why don't you come out and do some fieldwork with us in West Texas? And I jumped at the chance--and that just really inspired me. This was at Sierra Madeira-- a cryptoexplosion structure--since clearly identified as an impact structure.

So I joined a group of Branch geologists who were working in West Texas: Terry Offield, Larry Rowan, Dave Cummings, and Howard Wilshire. This was just lots of fun; I really enjoyed it. It got me really inspired towards the lunar program. Then, about that time Lunar Orbiter started flying and Masursky got me involved me into going down to Pasadena (JPL) to sit in on analyzing the images as they came through--and that was pretty exciting.

I was also on Lunar Orbiter IV, and since that had lots of good geology it was pretty exciting. I must have worked some on Lunar Orbiter V--but I didn't remember it much.

After Lunar Orbiter, I continued to make lunar geologic maps of various places. I got involved in some of the regional geology for some of the landing site maps generated for Apollo. I did some Apollo site maps for Apollo 15--the regional site map (probably the 1:100,000 scale map.)

Then I got involved with the early Apollo mission 10 when they were orbiting the Moon and taking pictures--so we did some analyses of them.

In the meantime, I was doing more geologic fieldwork for Astrogeology as well. I got involved in the study of lava tubes. These activities were mostly through Howard Wilshire who was of coordinating what was called the lunar analog program for the Branch--which was mapping terrestrial features that were deemed to have some value for lunar studies. Howard and I took a

trip through the Northwest and looked at some in Northern California, Oregon and Idaho. Then I ended up working on additional lava tubes in Northern California and Idaho. About the same time, Larry Rowan was working on Diamond Crater in Oregon--which was a little Maar crater I believe--and there were some other analog volcanic features being worked on. This was in 1968.

Then, Keith Howard was invited to go to an eruption site in the Galapagos Islands by Tom Simka at the Smithsonian Institution. He couldn't go because of family reasons, so he suggested me. So I managed to get Bob Bryson--who was our funding agent for the lunar program at NASA--to say it was okay to spend some of his money to go down and look at this eruption site in the Galapagos. It sounded like it was a caldera collapse; and that would have been of serious interest to the lunar program at the time. That was about a year ahead of Apollo 11--in 1968.

So I went down there and got all involved in Galapagos geology. I am still a bit involved and publishing things from Galapagos up until a year or so ago.

I remember that when we got down there Tom Simka and I were asked by all of the local biologists and others asked us about all the volcanoes we had been to. I think neither one of us have ever been to a single one! Tom had worked on some volcanic rocks for his thesis. But anyway, we became volcanologists. I think neither one of us have ever been to a volcano! Tom had worked on some volcanic rocks for his thesis. We were the great guest volcanologists! However, Tom is now the expert of the catalogue of active volcanoes of the world. So that was another diversion from my normal work at Astrogeology.

Then I got involved in seeing some other analog as well. I got involved somewhat later--in the 1970s--in a study of caldera in Hawaii--the guts of caldera as potential Martian analogs."

*Schaber: "Okay, but let's go back and talk about what you did during the Apollo mission period."*

Howard: "The first Apollo mission I got involved in was Apollo 15. At that time I was invited to be part of what was called the Tiger Team because I had made a [regional scale] map of the [Apennine-Hadley] landing site region, and I knew something about the regional geology. That was pretty heady stuff as you well know. We got transcripts of the astronauts on the Moon describing things, and we were trying to figure out what it all meant in the "backroom" during the mission. No sleep. It was damn exciting.

There was a fallout from Apollo 15 for me as well because after I'd come back home, about a month later, and the astronauts had come back home--I got this excited call from Houston that part of the team had discovered these fantastic lineaments around the Apennine-Hadley site on the mountains. They were preparing a press release on this. This was Bill Muehlberger, Dale Jackson and others. I came down to Houston and talked them out of it--holding off just for a little bit. I went back home and did some experiments. It was really funny because I was suspicious about these lineaments because you could see them down to footprint size. Where I got really suspicious was when you could look down at pictures of an astronaut's feet--and right around them you could see these lineaments in the dust. You shouldn't be seeing them in the dust! The regolith is how many meters thick?



So I did these experiments where I tried to recreate lunar lighting on a lunar landscape. The way I did it was by shining a 35-mm projector in a dark room on a platform on which I had sprinkled a bunch of dust. The dust I had gotten from Hank Moore. It was designed to have something like the lunar photometric function--copper oxide or something. I sprinkled it on the table in some kind of random way; then shined this light at it where you get stark shadows and just bright white, black and grays-- and darn if you couldn't see these lineaments. And they were about 45 degrees from the light direction.

*Schaber: "Yes, then Ed Wolfe and Red Bailey later did a similar follow-up experiment at Flagstaff using cement."*

Howard: "Yes, my earlier work inspired Ed to take another look at this. We published this in a little note in the *Apollo 15 Preliminary Science Report*. It never got beyond that, but I always felt that that was one of the biggest contributions I made to the Apollo Program.

Every time I fly nowadays I look for these lineaments in the cloud tops and forest tops."

*Schaber: "That was a major contribution. Dave Scott [the astronaut from Apollo 15] was insistent that they [the lineaments] were real. Even after you all told him that they weren't, he was so determined--and he was a hell of a good observer-- he said they were so obvious and so distinct that both he and Jim Irwin refused at first to believe it. I think they may have finally come around--between you and Ed Wolfe's experiments."*

Howard: "After Apollo 15, I worked on the orbital photography from Apollo 16. In fact, I was the coordinator of the orbital science geology report. So I wrote several articles that I think Jack McCauley asked me to do this. I went to Flagstaff for Apollo 16 and worked on that.

On Apollo 17, I got involved in the mission again because I was really interested in avalanches. I published a paper in *Science* that came out about a month after the mission about avalanches, trying to compare them on Earth and the Moon and pointing out that avalanches like the one on the Apollo 17 site--that was going to be visited--have this extended run out. And this seemed to be similar on the Moon to some of those on the Earth. And if that was the case, that would be pretty important for understanding mechanisms because there is no air to lubricate them, or cushion them on the Moon.

Actually, I think that that triggered quite a bit of interest in avalanches. A lot of people quote other analyses other than mine-- but I think it did some good. Anyway [as a result of that] I got involved in one of the astronaut training missions to the Blackhawk Landslide. Howard Wilshire was there as well. That was for the Apollo 17 crew.

I did get to go to the Cape before Apollo 16 to do some lecturing to the about what they could see from orbit--craters in particular--lunar wide. I was talking to them particularly about the shapes of impact craters and how they vary with size; and how to look for the fresh ones, etc. I did go to Houston for Apollo 17; it seems like I was also on the Tiger Team for that mission."

*Schaber: "You probably wrote up some papers for the AP 17 Science Report?"*

Howard: "Yes I did. I wrote about lunar thrust faults and a new vision of the stratigraphy for Mare Serenitatis--which came out of that mission--regional kinds of things. They were both pretty interesting actually. People since then are recognizing that wrinkle ridges are thrust faults on all of the planets.

*Schaber: "Let's talk about what happened after Apollo, when you left Astrogeology, and what you have been doing since."*

Howard: "Between 1972 and 1975 I was in Astrogeology mostly working on continued geologic mapping--on the South Pole geologic map [of the Moon]. Don Wilhelms eventually finished that map. We did analyses of individual craters and a lot of other things. And I mentioned that I got involved in another analog study that was in Hawaii looking at the guts of caldera. That was in 1974. I also got involved in some non-Astrogeology things. I was asked to lead a group that were asked to make a map of young Quaternary faults in the United States--active faults. So I got together a group and did that--so that kind of diverted me.

Ron Greeley was at NASA Ames and he put together a course for High School teachers at NASA Ames in about 1974--and had me come down and participate in that. Then, he had another one in 1976--and that one happened to be back in Virginia so I could come and participate in that as we'll. This was to teach teachers all about lunar geology.

About 1974 I got involved in the planning for the Viking-Mars mission. I quit my involvement in the Viking programs partly because of family personal reasons at the time--just couldn't afford to be traveling that much basically. My involvement in Viking was basically pre-mission planning. I was the Branch representative in Menlo Park for Astrogeology about that time also.

In 1975 I got an offer to move to Reston to be an Administrator with the Survey--a Deputy Office Chief--for the Office that included Astrogeology and all the regional geology branches--Paleontology, stratigraphy, etc. My wife had been talking about moving to Washington, in fact, and I thought, well maybe this is the right time. So we moved back there in 1975 for two years.

When I came back to Menlo Park after a couple of years [from Washington] I went into the Branch of Western Environmental Geology--in 1977.

I was given a project to work on regional geological mapping in the Mojave Desert. It was called the Needles 2-Degree sheet. It was designed originally to bridge the gap between the Flagstaff group [from the same Branch] and the people in Southern California, where we didn't know very much about the intervening geology. Ivo Lucchitta was working in the same region at that time.

I was made head of a big geological-geophysical consortium specific to Arizona--a crustal experiment we called it-- in the 1980s. We did a lot of seismic profiling, heat flow, and every other darn thing you could imagine--trying essentially to look at a transect of the crust from somewhere near Flagstaff into California. We had our sights set on San Diego and beyond but we didn't get that far.

Schaber: "Was that the PACE Program?"

Howard: "Yes. Then in 1995 the Survey was reorganized and I got rified and I spent a year and a half trying to get my job back. Then, in that intervening year I got the opportunity to work with the Survey as a part time temporary employee--working on Astrogeology. That was with Mike Carr who was working on the Galileo mission to Jupiter. So, I worked on the volcanic features of Io. I was reinstated in the Survey after about a year--in 1996-- and have been working ever since.

I have been assigned to many different projects--none of them for very long. I have a project in the Minerals Group in Northeastern Nevada working on regional tectonics. I am working for the same Branch, but it now has a different name; it is now called Western Earth Surface Processes. Actually I approve of that because in the late 1980s I was on the Geologic Division Scientific Advisory Committee. I saw an opportunity to push the idea of doing more environmental geology--which I thought was really important--entirely influenced by my mentor Howard Wilshire in this.

So, I got a group of people together that included Carl Wentworth, Howard Wilshire, and John Costa--from Water Resources Division--and others, and we had a little working group that came up with a little white paper to the Chief Geologist that recommended very strongly that the Survey push hard into Earth-surface processes geology as one of the really relevant and exciting scientific topics. The Division was interested, and the Water Resources Divisions were equally interested, but nothing really serious happened for a while.

It was kind of ironic that about the time that Howard and I lost our job in the riff; we were the two first-authors of this report that was a beautiful, colored USGS Bulletin. This report came out on Earth Surface processes and recommendations for the Survey to get involved in it. And it turned out that this report had a very strong influence of the Associate Chief Geologist and he's pushed the whole Division in that direction now. That's why we have a new name in our Branch.

Another environmental thing that I got involved in early on was when I was a Deputy Office Chief in the mid-1970s in Reston. I thought up this idea that the Survey should get involved in Climate Change research, and I worked really hard at it but barely got the idea through. So we got a small little program started--and it grew--finally. The Survey is not the big player it ought to be--but at least it's a player now, and I will take credit for that.

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**Jay Inge** (b. 1943; cartographer/illustrator; B.S. 1967(?), Pre-Med with Art minor, University of California, Los Angeles) came to work in Flagstaff for Lowell Observatory as an airbrush illustrator in November 1965 to work on the LAC Lunar charts that were being compiled by Lowell and ACIC [on the grounds at Lowell at that time] in early support of Project Apollo (see Appendix A). Jay would eventually work part time for the USGS Branch of Astrogeology in Flagstaff in 1973 [while still working for Lowell]. Jay was offered a full time position with the Branch of Astrogeology in 1974, so Jay then quite Lowell Observatory to work solely for Astrogeology.

The following was taken from a taped phone interview with Jay Inge by Gerald G. Schaber on 25 August 2002:

Inge: “I was born in September 1943 and went to North Hollywood High School in southern California. My major was physics. I went to UCLA in 1961 where I started out in Astronomy and Physics. My integral Calculus and Boolean algebra were severe, so I ended up in biomedical illustration a year or so later; which was essentially a pre-Med major with an art minor. I had three credit hours for my degree when in late 1965 I accepted a job with Lowell Observatory in Flagstaff, Arizona to work on the Lunar LAC-Series of lunar maps for the upcoming Apollo program. I only needed three hours of language and history to graduate, but because of the Viet Nam war at the time, if I would have stayed in school I probably would have ended up in the draft - probably typing letters somewhere in Pensacola, Florida. This was 1965.

On the campus at Lowell Observatory [in Flagstaff], there were two contingents in the lunar chart mapping. There was the Air Force Aeronautical Chart and Information Center (ACIC) group and the Lowell Observatory Group. I went there in November 1965.

Pat Bridges left ACIC [on the Lowell campus] in 1966 to raise her children, and I was basically very comfortable with the whole process of what I was doing, so I moved up very rapidly in terms of competence; and by 1967 we were doing work for ACIC, and we were getting contract work from the Army Map Service (AMS) that they couldn't handle. We were doing AMS maps for Lowell for ACIC.

I graduated in absentia at that time by taking correspondence courses in Spanish and history over at NAU—and I got my degree from UCLA in the mail.

Basically, we did the LAC-mapping series for the ACIC, the 1:500,000-scale series working mostly with Earth-based [telescopic] imagery and observations. About late 1967 to 1968 we started getting the Lunar Orbiter images back from the Moon and began making refined maps of the Moon with the higher resolution images.

In early 1968, we began compilation of the lunar globe that NASA had wanted. ACIC literally faded away in late 1968 [as Lunar Orbiter ended]. What it boiled down to was that ACIC didn't want to maintain a real mapping staff there [at Lowell]—just an indirect oversight through a manager [Terry McCann]. So, it became almost exclusively a Lowell operation.”

[Author's Note: starting in 1963-64, the ACIC and Lowell mapping groups in Flagstaff were also engaged in producing the base maps in support of the lunar geologic mapping effort within the Survey's Branch of Astrogeologic Studies.]

*Schaber: “You were also doing some mapping for us lunar mappers at Astrogeology-right?”*

Inge: “Yes, those would come through, and it was sort of a shotgun thing. We didn't worry [at that time] about a coherent map sequence; It was sort of targets of opportunity provided by the USGS [as the mappers' quadrangles on the Moon became accessible to the telescope]. That was the working style at that time. And like I said, we were juggling a number of different obligations at one time. Apart from the LAC and ACIC work that we were doing, we were also doing the ORB series of large-scale lunar maps at 1:1 million-scale for probable landing sites on the Moon. We

were working right up to the limit of the Lunar Orbiter resolution. These landing site maps were typical in groups of ten at 1: 100,000 of selected areas that were supposed to be “smooth” and suitable for landing.

We would get these sets in, and basically, I worked out a method of mass production so that we could put out a set of ten of these maps in the space of about a month. And these projects were not ours—they were not Lowell’s either. They were the contractual obligations that AMS had made with NASA--and that AMS could not fulfill. So we ended up doing them, and AMS kept scratching their heads wondering how we could do it so quickly!

You are well aware of the Service rivalry that went on between the Air Force and the Army in this regard.

So we kept up with that, and the word came down that NASA wanted the lunar globe. That would have been late 1968 and 1969. Of course, the impetus at the time was the upcoming landing on the Moon.

Well, after that we became obsolete essentially. We didn’t need the maps that much anymore; the higher resolution images being return from the early Apollo spacecraft going around the Moon were beginning to supplant anything that we could provide at the equivalent resolution [using airbrush techniques].

So, when we basically completed the lunar globe project, I accepted a job with the National Geographic Society in Washington D.C. In 1968 I had trained one of their illustrators while I was at Lowell earlier, so I knew someone at NGS. I left Flagstaff in March or April 1969 for the National Geographic job in D.C. When I left, they basically closed down the Lowell lunar mapping operation.”

*Schaber: “I see your National Geographic Moon map on many TV programs and movies. Whenever there is a science fiction program on, they always seem to have that map hanging up in the background.”*

Inge: “Yeah, I noticed that when I watch some of the old movies like “Close Encounters of the Third Kind” I can see my “Atlas of the Heavens” map that I did for National Geographic which had the five planets in different corners, etc. I see that with my NGS Lunar map frequently. I get a bit of a chuckle out of it. I am amazed at the longevity of the lunar map, but I guess no one else was crazy enough to try doing it that way again.

I was in D.C. working for NGS when they landed on the Moon [Apollo 11] and there was no need for the mapping, and that project had basically been closed out. No one seemed to think that there was any need for it; and when Mariner 6 and 7 came in, a number of other groups turned out some pretty awful first-look maps of Mars as I recall.

I was at NGS for two years. I returned to Lowell in Flagstaff in 1971 because I wanted to. I was too frustrated with all of the Mariner-Mars 6 and 7 imagery that as coming in --and me just sitting on the sidelines in Washington. Lowell had called me up to work on some albedo maps of theirs—

from Mars—from their telescopic Planetary Patrol program. I came back out to Lowell and negotiated with John Hall and Bill Baum there, and I rejoined Lowell in mid 1971.

I was basically doing small astronomical research projects, making maps. I have one paper in the journal *Icarus* out of that work.

Then, Hal Masursky started floating around Lowell in 1972, as I remember; and by 1973 he got me into a situation where I was trying to do some part time work for the USGS at Astrogeology while I was still at Lowell. I was using some of the Mariner-Mars 6 and 7 images doing preliminary concept airbrush maps. Then, when Mariner 9 got to Mars [1971], and as the huge dust storm started to abate a bit, I started helping Pat Bridges to gather some small scale maps of Mars: the 1:15,000-scale map of Mars was one of the things we did using Mariner 9 data. We just played with mapping the Mariner 6 and 7 data until Mariner 9 started sending down images. After the huge dust storm started to abate the first thing we needed was to have a small-scale planet-wide map—and that's when we got sufficient resolution to be able to concentrating on specific quadrangles at 1:5 million-scale.

When I first joined the Branch of Astrogeology part time in 1973 [while still employed by Lowell], I was located down on the fifth floor of the Arizona Bank Building. I would come in there every night-work for three or four hours—and then go home and get up early to work at Lowell the next morning.

By 1974 I had been moved out of part time with the USGS into a full time position, so I came over there and left Lowell. My first organized lunar quad I did at Astrogeology was LAC-26; Pat Bridges' map I believe was LAC-19. From then on we were making maps until Viking got to Mars [summer of 1976]. Then, we went into making the high-resolution, large-scale, maps of possible landing sites [because the landing sites the planetary science community had picked earlier (based on the lower resolution Mariner 9 data) turned out to be too rough].

It was during that last minute landing site selection process during Viking that Pat, Bridges, Ray Batson and I came out to JPL and were making that huge sandbox at JPL. At the same time, I was helping Rich Tyner [Astrogeology] make mosaics of the newly proposed Viking landing sites.

After Viking, we got into Voyager to some degree. So that meant recommitting time between the 1:15 million series on Mars; and then we were beginning to do some of the mapping from the Jovian satellites that were starting to come in about 1980. So, it was kind of a mixed mapping activity.

With regard to the high-resolution refinements of our earlier LAC lunar map, we really got hold of those because ACIC, which later turned into DMAC-basically were throwing away everything. We found out about this from Bill Cannell [at Lowell] as I remember. This was in the early 1980. I think 1982 or 1983 rings a bell. We got the entire original LAC series from ACIC and put it in our warehouse at Astrogeology. Then later we had the inspiration that maybe we could start refining, redoing, and improving the original maps with all of the new information and data that we had collected.

It started turning into a situation where the control that that existed for the original LAC chart mapping series was just not adequate to use safely and there was still no new control. So, it became a question of why are you making higher resolution uncontrolled maps?”

*Schaber: “Until Clementine, we didn’t really have good control for the Moon, right?”*

Inge: “Right! So Mert Davies [geodesist with the Rand Corporation] hadn’t really put out any control, so it was really a situation of how do we make a controlled map? That continued on. I think it kind of died, and then it came back up and about by 1989-1990. Clementine was imminent and a number of ideas were kicking around as to how to integrate that data, or how to get preliminary bases ready to assimilate the data.

Again, there was a hiatus of basically two years where basically nothing was happening-1969 through 1971. I was in D.C. [at National Geographic] when they landed on the Moon and there was no need for the mapping that had basically been closed done. No one seemed to think there was any need for it and then the Marine-Mars 6 and 7 came in and a number of other groups turned in some pretty awful first-look maps as I recall. Then I got back there in 1971 [to Flagstaff] and that’s when the idea was [thrown around] to start making maps of Mars in a coherent series; and we used Mariner 67 images to try to make larger-scale maps. And you might say we were doing I guess you might say concept or approach maps at 1:5 million and 1:10 million-scales, etc. using Mariner 6 and 7 data. They were pretty limited. Again, we just played with that until Mariner 9 started sending images down. And when Mariner 9 started sending stuff down, the first thing was that we needed to have a small-scale planet-wide map--and that’s when the 1:15 million scale map was done. And then the data continued to come down and we got sufficient amounts to be able to start concentrating on specific quadrangle at 1:5 million scale. That’s when we begin with LAC Charts 26 and 19.

We stayed with that mapping program, and that’s about the time I started working on the National Geographic Lunar map on the side. The lunar map for National Geographic was done basically for free—for them.”

*Schaber: “Did you even go out for some of the Apollo missions, or JPL? In which missions did you participate?”*

Inge: “I never went to any of the Apollo missions, but I was at JPL for all of the Viking and Voyager missions. I was providing weird support for the Viking missions beyond simple airbrushing; such as mosaicking landing sites with Rich Tyner [Branch of Astrogeology] and making the great sand box at JPL for the Viking Lander.

I didn’t really get to know Hal Masursky intimately until 1972 or 1973, and then the interactions I had with Hal were primarily on his Pioneer-Venus mania.

I can think of a couple of sort of funny stories with Hal’s mapping of Venus [from the low resolution Pioneer-Venus radar data]. His mania was for getting these hand-made Venus globes he would need for a meeting. I would finish the globe for one of his many meetings, and I would

make a few little comments to him—please bring it back so I can finish it! Then the globe would disappear into parts unknown and I would never see it again.”

*Schaber: “He would give it to people at NASA headquarters to get money!”*

Inge: “Right. I would eventually see the globes appear on a desk somewhere at NASA. I think I saw one appear on the President’s desk at one point. It was always kind of annoying—how does he not understand that things had to be finished?”

*Schaber: “What about stories about Shoemaker?”*

Inge: “I do remember that I was making one of the first maps of Mimas. [a satellite of Saturn], and if you recall in those days we would get the images back in a hectic manner, and we would literally draw a hand coordinate grid over the image trying to make that work with the entire satellite.

So it was just eyeball grid-to-grid translation from that image to an overall preliminary map. I had started out with the zero degree longitude line at the center of the map, but because the data I was getting on Mimas had been screwed up; I was 180 degrees out of phase on the images. So basically, instead of drawing that one great big crater on Mimas the way it should be relative to latitude—I drew it off center. I knew this—I wasn’t worried about it—we will take care of it later.

Gene came in, saw it, and he made the assumption that rather than being on the expected leading hemisphere it was on the Saturn-facing hemisphere [which was not true].

Gene grabbed the map and went to a press conference, then went into this great circular logic argument about how this could happen! He was really winging it beautifully, and I was just sitting there shuttering!”

*Schaber: “On how the big crater impacted from the Saturn-facing hemisphere—Oh no!”*

Inge: “Yeah. He was leaping into the area of science fiction that people would no believe Gene and I told him. The map is 90 degrees out of phase! He said Oh my God! I said I hope that didn’t screw you up? He said it doesn’t matter-- they won’t care.”

*Schaber: “Only Gene could get away with that!”*

Inge: “Gene got away with it--the guy had an amazing ability to overcome reality sometimes. “

The reality distortion field of Gene Shoemaker—you know. His enthusiasm was the key to the reality of his distortion! No body could dispute such a passion—and I think that was part of the secret. It was good fun.”

*Schaber: “So when did you finally tie off things here at Astrogeology?”*

Inge: “Well, that was in 1994.”



*Schaber: “So what were you doing before you left-- after the Voyagers’ encounters with Jupiter, Saturn, Neptune and Uranus?”*

Inge: “I was doing all of the control of our mapping products. Aside from actually making the maps, I had to design all of the map collars, etc. I was overseeing the photomosaic production at that time because Rich Tyner had left. I was trying to keep the 1:500,000 scale and 2- million scale photo-mosaicking of Mars on track. I was going out to D.C. to do quality control on the printing of the maps so they wouldn’t screw it up to much—which was always exciting.

Then we began to drift into the 1:15 million mapping of Mars in the late 1980- early 1990s. Then, the whole apparent need for airbrush mapping basically came to an end because of the [rapid innovations in] computer image processing.”

*Schaber: “We got a lot of mileage out of your group and their products. The NASA and media PR people really loved those airbrush products.”*

Inge:”Well, ultimately I think that is the important point of the maps still. I think it is too easy to overlook your basic funder who is the taxpayer; and popularizing what you do and selling your results to them is great for more funding for science. It is sort of lowering ones self in a way, but nevertheless, I think it was good public relations. Certainly Lowell’s observatory’s use of our maps in their established visitor center is a big plus for them—in terms of pulling in the funding.

I think it is too dangerous sometimes to take all of those neat pictures and put them into a drawer somewhere and say--come back later. I think it is always better to get the stuff out to the average guy on the street.”

*Schaber: “So you finally left and retired in 1994. I always ask the people I interview to just comment about their work and career at Astrogeology, and to comment generally on the Apollo Era in general.”*

Inge: “Well I think it was the golden age of planetary exploration, and like most golden ages, it is unappreciated by the people who benefited by the results. It is misunderstood by people who are too lazy to look at their history, and that is the sad aspect for the people who actually did it! For us, it was a great moment; you got to unwrap and play with everything for the first time in history—and that will never come again!”

*Schaber: “The first exploration of the Solar System!”*

Inge: “Right!”

*Schaber: “But of course, everybody admits that beating the Russian was President Kennedy’s big goal in getting to the Moon. But it was really too early for us to go—computer wise—safety wise—we were probably all quite crazy I thing.”*

Inge: “Right! There wasn’t the preoccupation with the risk as there is today. I think that is where the idea of the “can do” mentality came through.”

*Schaber: "And everybody worked together, unlike the screwed up world we have today where all of the news is bad 24 hours a day."*

Inge: "Well, most of these people [back then] weren't worried about building empires. It hadn't devolved into that. You had people who were committed to the program; committed to the objectives; committed to the fact that this isn't going to happen again--and we better darn well make sure that it works right the first time!

There is no sense of adventure anymore. Again, that is the point; the Golden Age was the adventure. To me, the whole thing was like sitting in a room at JPL and watching the first download image from a spacecraft that landed on another planet—and that first image footprint came in. I mean, that moment is going to be mine forever, and I really don't know how to share it with a kid who had been growing up on Star Wars and instant gratification of ephemeral things."

*Schaber: "Amen brother!"*

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**Darline Johnson** (clerk-typist) entered on duty with the Branch of Surface Planetary Exploration in Flagstaff in September 1969. Darlene would work with the Apollo Lunar Geology Experiment Team down in Houston during Apollo missions 13 through 17 typing verbatim transcripts of the astronaut's verbal communications and inputting relevant geologic data into the team's primitive data base and word-processing software programs, SEARCH and WYLBUR.

The following was taken from an interview with Darline Johnson (Bullhead City, Arizona) by Gerald G. Schaber on 19 August 2002:

Johnson: "I was hired on by Astrogeology in September 1969 when we moved to Flagstaff from Phoenix."

*Schaber: "What were you doing down there?"*

Johnson: "We had moved from Gallup- where we both grew up—and Bill [her husband] started going to school at Glendale Community College [greater Phoenix area], and I was working for the Bureau of Indian Affairs (BIA) at Phoenix Indian School."

*Schaber: "So, you came up here to Flagstaff. Did you have a contact with Astrogeology-or did you just move up here?"*

Johnson: "Well, we just moved up there-no contacts or anything."

*Schaber: "So was Bill going to go to school up here?"*

Johnson: "Yes, because all of his credits transferred; he didn't lose any credits. Neither one of us had a job. It was sheer foolishness! He was working on the railroad that summer; and we bought the only house left in Flagstaff [on Patterson Blvd. in Lower Greenlaw]—but it had been left empty for five months. At that time Ruth Wolfe [who married Ed Wolfe with Astrogeology] was

on the welcoming Committee for Flagstaff. The day she came to see me she ask do you have a job? I said I was a secretary-administrative person for the Phoenix Indian School, and I worked on plant management. Ruth said her husband Ed is looking for a secretary. Ed was working [for Astrogeology] in the Arizona Bank Building. So that is how I got the job. So I went up there and was hired right away. Irene Gedney was Branch Administrative Officer at that time.”

*Schaber: “So who did you work for when you showed up?”*

Johnson: “Ed Wolfe, George Ulrich, Ivo Lucchitta, Bob Sutton, and you on the third floor of the Arizona Bank Building.”

I was participating also in the Apollo missions—it was arranging all of their [the Lunar Geology Experiment Team] travel to and from Houston etc. I started learning the WYLBUR program on a big old computer in the Bank building. Prior to that, we were key-punching the San Francisco Peaks geology data for Ulrich and Wolfe. They had this loud computer in a separate room in our office down at the bank Building. I was working with Red Bailey, Jack Fife, and Dave Dahlem. Dave was surprised that I picked up the computer stuff so fast”

*Schaber: “Then you were working on the IMB-360/30 down at Fourth Street when it was built. It was 1967 when they constructed that building.*

Johnson: “No, the only time I worked in the building on Fourth Street was during the Apollo missions—as backup.”

*Schaber: “So you became more involved as we started developing the SEARCH AND WYLBUR programs—and that’s what led you down to Houston with our Apollo Lunar Geology Team, right?”*

Johnson: “Yes, because they needed more people to do that--the inputting and all that. I did backup for Apollo 14 up in the data Center there [Apollo Data Facility, or ADF] on Fourth Street.

*Schaber: “So you started with Apollo 13 which failed—and then you did Apollo 14 through 17 I guess.”*

Johnson: “Yes.”

*Schaber: “You said that you were at Flagstaff, then left, and then came back later.”*

Johnson: “See, what happened was as follows: Astrogeology kept me on the payroll when Bill graduated in 1970 and got his first job in Grants, NM. Of course, I went with him in August 1970. Then, when Astrogeology needed me for the Apollo missions, I just went down to Houston from Grants because I was still on the payroll. I could make more money doing that than any jobs they had in Grants.”

*Schaber: “So you were there in Grants for how long—for all of the Apollo missions?”*

Johnson: “I went down to Houston for the rest of the Apollo missions from Grants [Apollo 14 through 17]. I didn’t go back to Flagstaff until 1978 when I went to Earth Geology and the Apollo thing was all over. I worked for the Geologic Division [Western Regional Geology] in Building-Three on the Mesa]. I wasn’t in Astrogeology then.

*Schaber: “So during the Apollo missions in Houston—what did you do?”*

Johnson: “We input verbatim everything that was said [by the astronauts] on the Moon. We had Fern Beeson, Lynda Sowers and me.

*Schaber: “Were you down at MSC/Houston a long time after each mission as we were-or did you come back when the missions were over?”*

Johnson: “No, sometimes I would be there two weeks—sometimes three—but that’s about it. But I know they did a lot work on the data after each mission.

Okay, I’ve got the right dates. I worked from September 1969 to 1978. I worked Apollo 16 and 17 [out of Grants, NM]—but I was on WAE with Astrogeology. After August 1971 through 1972 I worked out of Grants. I worked the last missions in December 1972.”

*Schaber: “So how did you make a contact from Grants about the job with Western Regional Geology in Flagstaff?”*

Johnson: “Well Bill had gotten teaching jobs in Grants—and then we moved to Gallup where he was coaching. Then we moved to Kansas for one year

Bill’s folks were living around Show low-Pine Top, Arizona, and they were elderly and had gotten sick. So, we decided to try Flagstaff. That’s when I went up to the USGS and George Ulrich and Ivo Lucchitta talked me into looking for a job up there. Bill also thought he could find something in Flagstaff--which he did. He worked for the Survey for a while.”

*Schaber: “So you worked up here on Landsat and other things. Who did you work for?”*

Johnson: “George Ulrich [who was also working for Western Regional Geology at that time]. I also worked for Oil and Gas--Terry Donovan, Randy Forgey and Mary Hendricks

*Schaber: “Did your notes say when you actually came back to Flagstaff in 1978?”*

Johnson: “Okay, the Branch of Western Regional Geology hired me back in August of 1978. I think that’s right. I retired in May of 1994. We moved all over the darn place. I moved out to the Hopi Reservation because Bill was working out there teaching and coaching. He was going to finish up there because it was BIA and he would have the Government work when he was in the service, and when he worked for the Survey for retirement. Then, they fired everybody and changed the BIA school to a contract school. Well, they offered him his job back, but he would

have to take a \$10,000 pay cut (along with everyone else); plus, you would not know what your benefits were. He did get a severance pay from the Government—so he took that and went back to school and got certified in special Ed. We went down to Phoenix College and got it actually. It didn't take but one summer. So he was right back at work. His first job was at Zuni; then a friend of his who he had coached and taught with at Grants—found out that he was at Zuni—called him and wanted him to start at Crown point [NM] on the Navajo the next year. So that's where we finished up--at Crown Point.

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**Raymond Jordan** (b. 1937; Cartographer/photogrammetrist; B.S. 1960, Sienna College) arrived in Flagstaff, Arizona to work for the Branch of Astrogeology in October 1966 from the U.S. Air Force Aeronautical Chart and Information Center (ACIC) in St. Louis, Missouri. He was hired by Ray Batson and would work with his cartography group during the Surveyor program. Jordan provided significant help in Surveyor camera calibration, mission operations at the Space Flight Operations Center at JPL, and contributed to application of innovative mapping techniques that produced detailed topographic maps of the Surveyor landing sites using images from the two onboard cameras. When Jordan later worked with Sherman Wu's photogrammetry group at Flagstaff, he used the Branch's new AP/C stereo-plotter and helped compile a topographic map of the Apollo belt of the Moon using the Metric Camera photographs. He was also successful in using the high-resolution Apollo Panoramic Camera photographs obtained by Apollo missions 15-17 to compile detailed topographic maps of selected areas of the Moon. Ray played a major role in compiling the first global topographic map of Mars using stereo images from Viking Orbiters I and II.

The following was taken from an interview with Ray Jordan by Gerald G. Schaber on 28 July 2002:

Jordan: "After receiving my B.S., the Army called and made me an offer I couldn't refuse. I went in the Army in October 1960 and got out in 1963. I had a difficult time when I got out of the Army because I had been out of school for over three years and the government had a rule that if you haven't had any math-qualifying courses or experience for three years, you were not qualified. So I did get a job in geophysical prospecting for oil. The headquarters of the company was in Houston, Texas. I reported there on a Monday morning at 9:00 AM, and the guy said do you have your bags unpacked yet—and I said no. Then he said I've got you scheduled for the 1:00 PM flight to Midland, Texas. We had a six-month training program there that they put me in. It covered all phases of their work, from surveying to going out in the field and laying geophones. My training there also included drafting, and seismic theory (reflection and refraction).

Anyway, they were getting ready to ship me overseas to North Africa—and I had been in the Army overseas for three years. I was single and the money would have been nice, but I just decided to quit then. About that same time, in 1965, the US Air Force gave me an offer I couldn't refuse too. They offered me another six-months training program in cartography--their way. It included a little bit of everything—a lot of aerial photography; surveying again, photogrammetry, all kinds of map making, computer programming. Anyway, I went to work for the Air Force at Air Force Aeronautical Chart and Information Center (ACIC) in St. Louis, Missouri in January 1965. My first wife (Jo Ann) I met when she sat across the table from me as I was filling out the forms.

I was at ACIC in St. Louis for about two years. Oh, by the way, after I graduated from their training course there, I had some suspicions about some of the things that the Air Force was doing then. I guess I can talk about it now. It was very highly classified at the time. They took me into this program where they were mapping the Soviet Union using satellite photography. This was fairly early on. This was the most sophisticated equipment and techniques that they had at the time.

I worked there for about a year. Then, there was a photogrammetry convention in St. Louis where I saw a lunar geologic map. By the way, I didn't like St. Louis—and they were doing this work around the clock. In order to spread the grief--because they figured people didn't like the night shift--you would change shifts every two weeks. So, in the course of six weeks, I would work all around the clock. I am a creature of habit and have very tight biological clocks and everything. It was just horrible, and I couldn't talk them out of it. I would say well, I don't care if you pay someone more just to work those hours. Anyway, I wasn't happy.

So I was at this photogrammetry convention and I saw this lunar geologic map produced by the U.S. Geological Survey in Flagstaff, Arizona. This was 1966. So, as soon as my last automatic promotion came up (right after I saw this lunar map) I fired off this letter (giving my qualifications) to the USGS in Flagstaff, and it was passed on to Ray Batson. He hired me immediately. I came out to Flagstaff in October of 1966 and went to work for Ray Batson.

We had to wear ties in St. Louis. So, I show up at the parking lot (Building One on Switzer Mesa) and here's this guy in boots and jeans, open shirt. I thought he was the gardener out doing something outside. So I got chatting with him, and he seemed to be a pretty nice guy. He said I will hand you over to my assistant; she can show you around town; show you all the different places where you might like to look to buy a house. Anyway, that was Gene Shoemaker!

Batson brought me to his office. Surveyor I was already on the Moon; and Ranger was already completed. I hadn't been there for Ranger. Anyway, Batson said we've got one camera on the Moon and we would like some maps—and how you get them is your problem. And I loved it. I thought--what a place!

I was at JPL at one of the Surveyor missions. By the way, I was also on the camera calibration Team [for Surveyor], so I had to go down to Cape Canaveral to help do the camera calibrations. Anyway, during one of the trips down there, we took a fifteen minute break and we went outside and watched one of the Surveyor's launched.

In one of these cases—it might have been a more advanced Surveyor camera—I had just finished that calibration and I had to get on the plane, go back to Flagstaff, do my laundry, and head out to JPL to be there for the landing.

Anyway, one of these pictures from the Moon taken by Surveyor showed a strange honeycomb pattern. And everybody is looking at this and nobody could figure out what it was. We were all scratching our heads. Then Shoemaker jumped up and runs out of the room. Apparently he went to the Van Karman auditorium [at JPL] where they had a backup spacecraft sitting there.

Somehow, he charmed someone into taking the footpad off; and he came back with it. Then in typical Shoemaker fashion—there were no trumpets but there would have been—he bangs the footpad on his head and in the process crushed it you know. In doing so, you could see the honeycomb pattern that we saw in the picture on the Moon. It crushed coming down. I think it freefell about twelve feet.

One of the mapping techniques I used was called focus ranging. They would take the camera for some given azimuth. I think there were about 3 degrees detents on the azimuth setting and 5 degrees on the focus setting on the elevation settings on the mirror. Anyway, they would take each one of those combinations of azimuth and elevation settings, and shoot through the entire focus range. They had preflight calibration settings for where the focus would be for that particular focus setting.

I analyzed this and kind of made a really neat thing out of it. It was all programmed. I just had to lay out the things and analyze the photos to see where the best focus was; then measured that and that gave me the azimuth and elevation to it. You put all of these points together and it gave you a whole bunch of spot elevations, okay, along a given azimuth. And from that you could make a topo map—you could draw lines.

Anyway, they didn't do anything [with my program] on Surveyor III, but I was all ready to go on Surveyor V the spacecraft landed on the side of a small crater and was leaning pretty significantly in towards the center of the crater. They had this nominal focus setting for landing on a flat surface. This thing was just programmed to have a certain focus setting at that elevation and azimuth. Obviously the thing probably wasn't working because they were on the side of a crater.

Gene Shoemaker came over to me and he said you know that thing you are working on—that range-focus thing—is that going to work? I said sure! I could only guess that it was going to work. So he gave the spacecraft controller's orders to go ahead along one azimuth and just shoot up a whole bunch of pictures. I analyzed them and ran them through my program and gave him a profile of the crater—and it was exactly what he had been anticipating. So he gave them the go-ahead to shoot up the entire scene using that so I could make a topo map of the site.

Anyway, I was going home after I had done a lot of work you know, and Gene came over and said, how are the pictures? Are they shooting them just the way you want them? How's the F-stop and all of that. I was feeling pretty good. You know, when you are a team member and you start contributing? You are pulling your share. I just wanted to contribute.

Oh, by the way, there was one more thing I did on Surveyor—that was to analyze the shadow of the solar panel as it marched out across the scene. In other words, the width of the shadow would be the same as the actual width of the solar panel—so by measuring, I could tell the distance to it, and also get additional topographic information that way.

Oh, I also tried to locate the Surveyor spacecraft. It was very difficult to locate by looking at features as far out on the horizon as you could see—and comparing them with lunar Orbiter photographs.

I later went to work with Sherman Wu [head of Astrogeology's photogrammetry group in Flagstaff]--sometime after Surveyor VII.

After I went to work for Sherman Wu, we got an analytical plotter here [in Flagstaff.] It was the only one in the entire USGS. It was an APC but that was a spin from the military AS-11's, and I used them while I was in the military to map satellite photography. It was pretty complicated stuff. They had a panoramic camera with a two-foot focal length lens. And, as they were going along in orbit, they panned the thing from horizon to horizon. It took a significant amount of time for that camera to swing from one side to another, meanwhile, this spacecraft is moving. The camera is also nodding to take out image smear. So you had a dynamic camera system here. This was a military satellite. But ITEK used almost the same system for the NASA Lunar Panoramic camera--okay. So you had a system here where you had to program in calculus because you had to know the rate of change of angles relative to time. So it was pretty neat. So the later Apollo missions were pretty neat with the Apollo Metric camera and the Apollo Panoramic camera. I mapped all of the Metric camera coverage around the Moon.

On one of the Apollo Lunar Missions, with the Panoramic Camera photographs [Apollo missions 15, 16 or 17]—which was the highest resolution [1.5- to- 2m as I recall--given a high sun—I could look down and see the LM spacecraft sitting on the Moon.. I could measure its height within a few feet. The neat thing was that I could see the foot paths as they walked around from experiment to experiment. They kicked up a lot of soil and they changed the albedo of the surface.

My last job was the topographic mapping of Mars from Viking topography. That was a nightmare. There was a lot of guessing—selective judgment!

Another thing I did when I first started laying out the contours on Mars, I started with the zero elevation line that was sort of arbitrarily chosen; it had something to do with the triple point of water. And darn if as this thing was going along you could see these very clear cut channels and all of a sudden, they would kind of peter out you know. And the zero elevation line was following along many of the main drainage lines. So I really think that there is a good chance that the water line came up to that level!

Also, another thing that I noticed-- and I talked to a lot of geologists about this and nobody really seemed to be on to it. The quality of the image of a lot of the old craters that were under [below] the zero elevation line (that is, possibly under water) looked subdued [degraded] like they had been eroded by something other than wind. Anyway, I thought that was very interesting-- but nobody at that time seemed to want to pick up on it.

I did a neat thing on the Viking Lander. There was an Elliott somebody who worked at Stanford. He had a colleague there, a Sid Leadman (sp?), who developed a system. There were two cameras on the Viking Lander so you could do stereo work or mapping in the immediate vicinity. So Sid built some hardware using an old Delft stereoscope—instead of looking down at a table you looked sideways at these two TV monitors—the output coming from each of the two Viking Lander cameras. I looked into that old Delft stereoscope—and they projected a cursor onto each scene. Like a floating mark in conventional photogrammetry. And I had a track ball to control with. He wrote the concept, and someone else at JPL wrote the software—but nobody could run it.



People would look at the pictures and they couldn't fuse the image [in stereo in their brain]. This was my specialty. I did all of the stuff from the Apollo surface photography—the crew's Apollo Hasselblad cameras. Anyway, I got called in to help. That system we used was called RANGER.

The soil sampler device—when it extended out—was like rolled up film. It was driven out by a motor, and if the scoop hit a rock, it would burn up before you could do it because of the time delay for the signals to go and come back.

So Sid and I were called up before all of the spacecraft managers who asked us if this thing that we were working on was going to work. We said yes, we think so. Here again, there was a mock-up of the Viking Lander in Van Karman auditorium [JPL] in the sand box. They went ahead and did a simulation and shot all of these pictures. We did our mapping from that simulation [using the RANGER system]--and it came out just exactly the way it should have."

[Author's Note: Ray Jordan retired from the U.S. Geological Survey, Branch of Astrogeologic Studies in December 1993 at age 55 with thirty years in the Government. He still resides in Flagstaff, Arizona.]

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**Martin F. "Marty" Kane** (b.1928; geophysicist; Ph.D.):

The following was taken from an interview with Marty Kane (Denver, Colorado) by Gerald G. Schaber on 23 January 2001:

Kane: "I was born in Portland, Main, in September 1928, and was educated locally in grammar school and high school. I did my undergraduate work at St. Francis-Xavier University in Nova Scotia. I had a certificate in engineering. The Canadian system is a little different. Engineering is five years but at the end of three years of engineering you can opt to take a science. I had enjoyed my geology more than anything else I had taken, but I also had Math and Physics. So, I took a degree in Geology and did very well; and as a result I went first to work as an engineer in Portsmouth, New Hampshire, at a naval shipyard. At the end of six months I found a notice to the USGS for a Geophysicist."

*Schaber: "When would this have been?"*

Kane: "This would be in 1951. I graduated in 1951. There were three of us who had taken both geology and geophysics--and the head of our Geology Department recommended that all of us to go into Geophysics. So, I applied for the Geophysics work and was accepted; I went to Washington and then reported to the Colorado Plateau."

*Schaber: "The Uranium Project?"*

Kane: "Yes, the Uranium Project--and I worked there for the summer."

*Schaber: "So, it was sometime after 1951 when you went there?"*

Kane: "I talked to Gene Shoemaker [on the Plateau]. I was telling him that I had kind of a checkered academic career until my junior year in college when I ran into this senior actually, who was a geologist professor who really turned me around.

I went to Grand Junction, Colorado in May of 1952. I worked out there through the fall, and then I went on a series of jobs. I traveled pretty much for the next five years--actually up to 1957. I did work on the Mohave project looking for the saline deposits and Boron. Then I went into exploring the desert valleys--like the Panamint Valley and Saline Valley. After that, I started to do some of my own work in Owens Valley."

It was about 1957 or 1958 when I moved out to [Menlo Park] California. I remember Gene Shoemaker and I started discussing the Moon during a conversation on day. He was interesting, of course, in talking about [Grove Karl] Gilbert's work and what he was thinking about with regard to the Moon.

The people that I knew were—"Mr. California Geology"--Tom Dibley. I also worked with Don Mabey who was Chief of the Project. And it was after his recommendation that I took over the Survey's Owens Valley Project. Then, I worked off and on back east coming out to field projects. Then, the summer of 1956 I went to Cuba and spent three months on a chromium exploration project using gravity methods. I came back, and I got married in 1957, and went out to Menlo Park. And there I worked on the Owens Valley report and related things. Two years later (1960) I went back east and was stationed out of Washington and worked on a New England gravity project, along with some aeromagnetic work in Mass.

In 1963, the Branch of Astrogeology was looking for geophysicists for the astronaut training."

*Schaber: "Did you see an announcement to that effect?"*

Kane: "No, let's see. I'm pretty sure that I heard about it from Dale Jackson. Dale was asking around for interested people. Dale was a Deputy Office Chief in Washington at that time."

*Schaber: "Then, shortly thereafter, Dale was asked by Gene Shoemaker to go to Houston to take over the USGS Astronaut Training program at MSC. Is that right?"*

Kane: "Well, he [Dale] really wanted to get out of Washington D.C. Anyway, I got contacted by Dale in February 1963. There were some problems with my work in New England--not on my part--but something happened that I found very disagreeable; so I decided--this [astronaut training] job looked very interesting. At the same time I had in mind going to graduate school, because I really needed a lot more graduate work. I had the basics to understand fundamental geophysics. In any case, we went to Houston in the fall [of 1963], and we stayed there a year. The Survey's astronaut training group at MSC in Houston was headed up by Dale Jackson, and included, Al Chidester, Don Wilhelms and me. I believe it was the most agreeable work circumstance--in terms of colleagues--that I ever had to work with.

*Schaber: "Were you training the astronauts in the field, or just lecturing to them or demonstrating geophysical techniques to them?"*

Kane: "Well, it was lectures at first, but at the same time--there were a fair amount of funds around at NASA--so I suggested that one of the techniques that could be used on the Moon for exploration was seismic refraction technique. In some ways it was simpler than the seismic reflection technique. So, NASA agreed to fund a project--oh--it started out very large--it ended up being about \$200,000."

NASA originally offered us \$750,000. Then, a few weeks later it went down to \$500,000, and eventually \$250,000. Then, I said I'm just going to be honest with you. If we are going to do this we need a first-class piece of equipment and that equipment cost \$350,000. We need \$250,000 field expenses. I said, the best thing you can do is take your \$250,000 and we'll help you find a first class exploration refraction group--and that way your money will be well spent. We simply couldn't spend your money wisely. Then I went back and told Dale Jackson and Dale was just [livid] "Oh, how could you do that....I said, Dale, it's my responsibility and I'm not going to take it. I wouldn't take the money. Well, they came back with \$450,000; so we took it and did a lot of nice work with it."

*Schaber: "So that is how the In situ Geophysics Project started in Flagstaff--from your work down there at MSC, and in getting NASA interested in doing that. And then Flagstaff [The Branch of Astrogeology] ended up with the project---right?"*

Kane: "Yes, it was an awful lot of money [especially in 1964]. You know there is a problem when you take money--if it's too large--you have a hard time spending it. We wanted to get state-of-the-art-recording equipment. Just about this time magnetic tapes were coming into the picture. I don't think digital processing was on the scene yet.

We secured the money while I was in Houston [in 1964]. And of course, it was not to be done in Houston--it was to go to Flagstaff where they would do some of the experimental work. And then I got back in touch with Joel [Watkins] and Joel agreed to take the program over. I was not really a seismic practitioner, but Joel had some experience. In any case, that's how Astrogeology's geophysics program got started and Joel basically built the operation out at Flagstaff

Now, at this time, I was supposed to be the chief geophysicist [on the in Situ Geophysics Project]. However, Dale Jackson had said that if I would come with the program [to Flagstaff], then he would help to arrange to go through the government training act for me to go back to graduate school. It was approved in the Survey. At the time you could go for a calendar year--not an academic year--so you had 12 months of school, but then it was bumped up to two years. So, I spent two academic years, including summer. I went to St. Louis University in the geophysics program. At the same time, in part of the summer and some of the holidays, I worked with the astronaut training exercises--and notable, there was the one such trip in the Grand Canyon.

I also attended one of the astronaut exercises in Alaska [Katmai].

The big geophysics operation I took part in [with the astronauts] was [on 3-6 June 1964; see Table I and 1964 in main text] at Philmont--the Boy Scout camp in New Mexico [near Cimarron] (see Fig. 15 in text). And there, we devised a whole series of geophysical field experiments [for the

astronauts]. We trained several of the astronauts with seismics, a gravity meter, and a magnetometer. One of the most successful exercises was the seismic refraction. For example, we traced a basaltic outcrop into the subsurface, and showed them that it was a sill-like feature dipping off at an angle of 5 to 10 degrees. There was quite a debate about all of this between the geologists and the geophysicists that night.

So now, I'll skip to the summer of 1966 when I had finished my basic courses toward my graduate scholastic studies; and so we moved to Flagstaff from St. Louis. I took charge of the geophysical group as a whole, because it had become quite large.

That fall [1967] the Branch was facing a real crisis, and at that point I got an offer to go back east to be the Branch representative in the Washington office; and our home was in the east. At the same time I had been looking at what was going on with the Moon [science wise], and I realized that it wasn't the scientific path I wanted to follow. I was, and really am, a terrestrial geophysicist.

So I went back to Washington. However, the summer before going back there I attended the NASA summer conference [August 1967] at Santa Cruz, California [Author's Note: see summer 1967 in main text for more details]. And that was a very interesting meeting. There was a lot of discussion about the nature of the exploration of the Moon. There was [also] a lot of urging by NASA was for manned exploration. I was part of a smaller group because I was not a prominent member of the lunar community at the time. However, our consensus was that a remotely operated vehicle would be the most logical [first] step to take because the testing that was needed for a manned landing was so predacious."

*Schaber: "I believe the Santa Cruz summer conference was a turning point in NASA's lunar thinking about Apollo science."*

Kane: "Yes, it was at the point that a manned lunar lander was what they were going to come up with. There were several earlier [NASA] summer conferences [Author's Note: Iowa City, Iowa, summer 1962; and Woods Hole and Falmouth, Massachusetts, summer 1965; see main text for more details]. Joel Watkins and I, and Jean Claude de Bremaeher at Rice University had put together a very neat seismic experiment to be done on the lunar surface. And in my mind, it was very well conceived. Joel and I were field people. I have been doing fieldwork since I first went to the Survey in 1952. And so, given the limitations on the Moon--and the reality you face when you are in a field situation--I thought it was put together very well. Then MIT came in with one [a lunar geophysics surface experiment proposal] under Frank Press--one of the big instrumentalists--and a grad student called Bob Kovachs. And what happened was that Frank Press was in charge of the selection committee--so, what they ended up doing was to decide that both ours and Caltech's proposals had great merit. However, there would be too many people with all of us who were involved--so they made Joel Watkins [from Astrogeology] and Bob Kovachs the Co-Investigators on the Experiment."

*Schaber; "Chances are, NASA wouldn't have given us both the Apollo Geophysics Experiment and the Apollo Lunar Geology Surface Experiment. We would have really heard a lot of static from everybody."*

Kane: "Well, after listening to both Joel and Jean Claude de Bremaeher --who was a theorist—all three of us felt they were going to use some type of explosives [for generating seismic energy on the lunar surface], and what we were going with was some type of spring-loaded mechanical source."

*Schaber: "They eventually did use a mortar-like devise for launching explosives on a later mission. It was manned--but it was remotely- triggered--but the astronauts were to lay it out."*

Kane: "The other guy who was involved a lot was Jack Schmitt. Jack and I just got along very well. He decided that my real strength was that I was a very experienced field person [in geophysics].

We went around to a number of graduate schools and talked with them on what might be done on the Moon [geophysics-wise]. But then, Jack went off to become an astronaut [June 1965].

*Schaber; "So who hired Dick Godson and Bob Regan, and the other geophysics types at the Branch of Astrogeology?"*

Kane: "That was Joel Watkins (see Appendix A) and Hans Ackermann. They were brought in as part of a seismic group--and Hans was an extremely experience field man. Hans was at world level as a refraction seismologist. He did some things in refraction seismology, such as his report on Meteor Crater that was very imaginative. And when it came to the Geophysics Branch, he developed an automated system for constructing cross-sections from seismograph information that was ultimately used by the people in Menlo Park in the Crustal Studies. I left the Branch in 1967 but Hans stayed on until 1969.

I enjoyed what I did very much [in Astrogeology], but my real bent is toward research--not research of the Moon-- but research basically in the application of geophysical methods.

I think that I brought in Bob Regan, because I had connections with Boston College and I was looking for someone [another geophysicist]. There were two people at Boston College who had Masters Degrees; and they had some seismic experience working with a small geophysical engineering Company there. So we hired Bob Regan from there (see Appendix A)."

*Schaber: "What did you do after you left Flagstaff?"*

Kane: "I was in the Washington area from 1967 to 1970, and I got into some marine geophysics work off the east coast and did a report that I used as a dissertation on the gravity and magnetics off the Gulf of Maine. Then, I went one step further into the Marine work, and in 1970 went up to Wood's Hole, Massachusetts, and worked doing marine geophysics and helping set up an east coast marine deep seismic program."

*Schaber: "Then sometime later, you transferred to Denver."*

Kane: "Then in 1971, I got asked if I would take over as Branch Chief [of the Survey's Geophysics Branch]. I was in Denver as Chief of the Branch of Geophysics for six years--came back and did

some continental-scale type gravity and magnetics and then I joined the Saudi Arabian program--stayed there for three years. I think we discovered about 35 billion barrels of potential oil reserves there.”

*Schaber: “Tell me Marty, how did you enjoy your brief career with the Branch of Astrogeology?”*

Kane: “Well, I enjoyed Flagstaff very much. My wife and family enjoyed it very much; they still talk about it. It was the only place that I ever saw my wife really shook up about when we left. I told her if I didn't get out of there in one year, they will bury me in Flagstaff. No, it was nice time; it was a very agreeable office to work out of. I really admired Gene Shoemaker--as a human being and as a scientist and over the years.

Gene not only had the ideas but he did the work that you have to do to make a good idea to become a reality. He was truly an inspiration. Of all the people I knew in the Survey, and over my career I got to know most of the people who had reputations of one kind or another, Gene was easily the most outstanding scientist.

I do remember a story I have to tell you. Dale Jackson, Don Wilhelms and I were in Washington, D.C visiting the headquarters of the National Geographic Society [date uncertain, probably 1963-64]. We were talking with Chairman Gilbert M. Grosvenor about the Moon. During the course of the conversation, Grosvenor pulled out a recent issue of their magazine, and he said have you seen this. In the magazine they had this gorgeous photo of a half Moon. And Don [Wilhelms] looked at it and said I would like to say something but I don't want to upset you. So Grosvenor said--go ahead--is it about the picture? Don said yes, he said unfortunately, you have it printed upside down. Actually, they had a very large image [of the moon] that we were actually looking at. And, as a result of the conversation --I don't know-- I think he said something that they would have to correct that. But whatever, he gave Don the large Moon photo that the magazine photo was shot from.”

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**T. N. V. “Thor” Karlstrom** (b. 1920; geologist/geomorphologist; University of Chicago) transferred to the Branch of Astrogeology (Flagstaff, Arizona) in August of 1965. Gene Shoemaker recruited Karlstrom at the time he was working with the Alaskan Terrain and Permafrost Section of the Survey’s Military Geology Branch. Thor had only recently completed compiling the surficial geology map of Alaska using aerial photographs, so he was well aware of photo-interpretation. He had also recently finished a job mapping the geomorphology and geology of the northern region of Greenland. Thor was assigned by the Branch of Astrogeology to work on Apollo lunar mission planning right from the start using Lunar Orbiter photographs of the Moon. He was assigned to design and put together the Apollo Lunar Surface Data Packages (the maps the astronauts would carry to the lunar surface) for missions Apollo 11-13.

The following was taken from an interview with Thor Karlstrom (Seattle, WA) by Gerald G. Schaber on December 17, 2003:

Karlstrom: “I was born in Seattle Washington, March 10, 1920. During my first year we moved out to a little neighborhood in Seattle called Fauntleroy. So, most of the early years were spent

beachcombing. I went to Faunteroy Elementary School and then Madison Jr. High and West Seattle High School. I started college in 1938.

I went for a little over a year to the University of Washington. The first year I made the Dean's list, and the second year I decided that I wasn't sure of my major, sociology, so I took the rest of the year off to work.

That would be in 1938-1939. I then went and finished up my undergraduate work at Augustana College. In the middle of my junior year I took a course from Dr. [Fritiof M.] Freyxell. He turned me onto geology. I changed my major then, and I graduated in science (geology, liberal arts degree). I had received the faculty scholarship to the University of Chicago for my graduate studies. So in 1943 I started at the University of Chicago?"

*Schaber: "To keep from getting drafted, right?"*

Karlstrom: "Well, actually, I was part of the group instructing soldiers coming back from Europe in photogrammetry. I completed all of my residence classes by the end of 1943; then I got my first professional job as head of the Geology Department that was just developing at a small college (a sister college to Augustana) in East Orange, New Jersey. It was called Uppsala. So I designed the buildings and the curricula. We had a three-man department that had a quite extensive student population. The after three years, I joined the USGS in 1949.

I took the Survey job because I thought I needed more field experience in geomorphology. So I took a job with the Alaska Terrain and Permafrost Section in the Survey, which was essentially military intelligence of Alaska."

*Schaber: "So you were in Military Geology?"*

Karlstrom: "Yes. The headquarters was in Washington, D.C., but the field area was in Alaska, 5200 miles away."

*Schaber: So you were downtown (in Washington) in the old GSA building then?*

Karlstrom: "Yes, well actually before that—the Washington auditorium."

*Schaber: "So you worked for them until what year, and what happened after that? I heard that Gene Shoemaker called you out of the blue, is that correct?"*

Karlstrom: Yes, indeed. At that time I had finished up mapping the surficial geology of Alaska. I was the principal compiler of the map. We had done this largely through aerial photography of areas where we had virtually no ground truth. So I was well aware of photographic interpretation.

*Schaber: "So Gene Shoemaker must have heard about your work then?"*

Karlstrom: "That's right, and my colleagues were quite impressed because I also did a photogeologic mapping job on Greenland; they enjoyed my interpretation of the geology there very much. The area I mapped was way up past the 80 degrees N. latitude---an ice-free zone."

*Schaber: "So did you interview with the Branch of Astrogeology?"*

Karlstrom: "Oh yes. I was brought out to Flagstaff. However, I'm not sure that Gene was there at my first visit, or not. It was August of 1965 when I actually moved down to Flagstaff."

*Schaber: "What assignments did you have when you arrived in Flagstaff?"*

Karlstrom: "Oh, I think I was assigned as a lunar mission planner-right from the beginning."

*Schaber: "You were probably doing lunar maps from Lunar Orbiter weren't you?"*

Karlstrom: "Oh yes, you are right, because I mapped the Schickard Quadrangle of the Moon (1:1,000,000-scale)."

*Schaber: "Were you out in the field with us during any of the Apollo training?"*

Karlstrom: "Indeed! I was directly involved in that."

*Schaber: "I don't recall that you actually down to mission control with the Apollo Geology Team during any of the Apollo mission, right?"*

Karlstrom: "I never went to Houston with the Mission team there. I was principally responsible for the Apollo Data Package and the briefing maps that we used in training the astronauts in the traverses."

*Schaber: "Oh yes; which maps or missions?"*

Karlstrom: "Oh, Apollo 11 and Apollo 12. I finished the Data Package maps for Apollo 13, but of course, it was a failed mission."

*Schaber: "So who was helping you put together the Apollo 11 Data Package?"*

Karlstrom: "Oh, you know, it was so hectic. Gordon Swann was certainly in the loop there, but I was essentially on my own on the Apollo Data Package compilations up through Apollo 13. Maury Brock and Phil Shafer helped on Apollo 13."

*Schaber: "Oh, tell me about your meeting with Walter Conkrite there in Flagstaff before Apollo 11. I was in Houston Mission Control during Apollo 11 with the Apollo Lunar Geology Experiment Team, and missed all of the fun up in Flagstaff with CBS News."*



Karlstrom: “Well, he came to Flagstaff about a month before the actual mission to check things out. All of you Apollo Experiment guy were in Houston. I said okay, I’ll stick around here. Then CBS sent their crew to Flagstaff, so we were the ones featured in the media,” [Author’s Note: Absolutely no media people were allowed in the science operation room in mission control during the actual lunar landing period.]

[Author’s Note: Walter Conkrite came out in June 1969, about a month before Apollo 11, when Neil Armstrong and Buzz Aldrin were supposed to practice out at the Branch of Surface Planetary Geology’s brand new crater field at Cinder Lake just northeast of Flagstaff by Sunset Crater. Conkrite was thinking about coming to Flagstaff during the actual Apollo 11 mission, but he ended up sending CBS’ George Herman and Mike Wallace’s son, Chris—now a major anchor at CBS].

Karlstrom: “Yes, and Wallace’s son. Anyway, it was quite an exciting deal. In fact, I had Conkrite out to our house there in Flagstaff when he came before the mission.”

*Schaber: “I heard there was a big party after Apollo 11 with the CBS people and the Astrogeology people at the Elk’s Club in Flagstaff.*

Karlstrom: “Yes. My last big job at Flagstaff was working on a plan for the advanced lunar colonization at Mare Orientale.”

*Schaber: “Okay, the Advanced (post-Apollo) Lunar Projects.”*

*Okay, so what did you do after Apollo 11 and 12? I have some pictures in this document (Fig.74) of you in Astrogeology’s ADF on Fourth Street during Apollo 12.*

*So what did you do after Apollo 13 failed?*

Karlstrom: “Well, after Apollo 13, I was working on Mare Orientale, and also with setting up the field areas for the astronaut training.”

*Schaber: You did the geologic map of our first crater field [astronaut training site] the Branch constructed at Cinder Lake just east of Flagstaff as I recall.”*

Karlstrom: “Yes, I was involved with that.”

Then--on the side, and at the same time, I was the geologic consultant to the Black Mesa archaeological salvage operation. So I worked with the archaeologists.”

*Schaber: “What did you do after the Apollo missions were over then?”*

Karlstrom: “I was released of any further obligations for Astrogeology, but I had an office of my own in Flagstaff working on the effects of the 1964 Alaskan earthquake.”

Schaber: "Oh, I see; but you were still working with the Branch of Astrogeology."

Karlstrom: "Yes, I was still with the Branch."

Schaber: "So you actually left Astrogeology and retired in 1983, but you were still in Flagstaff."

Karlstrom: "I was still in Flagstaff up until 1989. Then I moved up to Seattle in 1989."

Schaber: "So what have you been doing since arriving in Seattle?"

Karlstrom: "Well, I've been following through on paleoclimate. So I have been presenting a series of papers to Paclim (Pacific Climate). I have macular degeneration now, so I have bad vision which is very disturbing. I get along wonderfully with everything except reading, and that just frustrates me like heck."

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**Barbel "Barbara" Lucchitta** (b. 1938; geologist; Ph.D., 1966, Penn State University arrived in Flagstaff with her husband Ivo Lucchitta who had been hired by the Branch of Astrogeology in October 1966. Barbel herself joined the Branch in the fall of 1967. Barbel started working with Robert Wildey and Howard Pohn on Astrogeology's photogrammetry project at the Branch's recently constructed building at 2717 N. Fourth Street in East Flagstaff. She was assigned to do various geologic maps of the Moon, including the 1:50,000-scale geologic map of the Taurus-Littrow region of the Moon that would eventually include the landing site for Apollo 17 in December 1972. Barbel also worked on the Mariner 9 images of Mars, was an active team participant on the Viking Mars mission (summer 1976), as well as on the subsequent Voyager missions to Jupiter, Saturn, Uranus and Neptune. In the later phase of her career she was very active in geologic studies of Mars, and in monitoring the ice sheets in Antarctica. Barbel, who published extensively throughout her career with the USGS, retired along with her husband Ivo in 1995 and still resides in Flagstaff.

The following was taken from an interview with Barbara Lucchitta by Gerald G. Schaber on 11 February 2002:

Barbel Lucchitta: "I was born in Munster, Germany, in October 1938--at a time when WWII almost broke out. Chamberlain kind of rescued it, and he sold Czechoslovakia down the drain to save the peace. At that time my parents lived on the River Rhine, and my mother was scared to give birth there because my dad wasn't going to be around, and because she thought if the war starts they are going to overrun the River Rhine--the first thing they do. So she moved to my grandparents place in Munster to have me. So I am kind of a child of the war you know--right at the beginning, even though the war then didn't start for another year. Chamberlain had saved the start of the war for another year, but, of course, Ivo [her husband who was born in Czechoslovakia] was not very happy about that.

My family moved to Weimar in the early 1940s, maybe 1941 or 1942. Of course my father wasn't there; he was off in the war eventually. We were [still] in Weimar at the end of the war. The Americans occupied that part of Germany first, but then it was ceded to the Russians in the Yalta

Conference. So after about 6 weeks or so, the Americans moved out and the Russians moved in. I remember very well the bombing because at the time I was going on 7 years of age in 1945; so I do remember the bombing very well. Even though the city I grew up in--Weimar--wasn't bombed very heavily--it was bombed some. One bomb fell right across the street from my house. And of course we spent most of the time sitting in the basement, and it was very scary. When the bomb came down you remember from the movies the way it whistles down--and of course the whistle was coming straight for us. We thought we were done for. It was a huge bomb too; it was a major explosion. Of course all of the windows were out in the house, and the roof was damaged. But the house stood; so we survived. It was really something in those days!

I remember well the Armistice Day because it was a beautiful, sunny, spring day, and I was walking around looking at the sky thinking oh my God, I am going to live! It was quite something.

After one year, it was the Russians, who as I said earlier--came in. The Iron Curtain was supposed to come down. My father was a prisoner of war at the time in England. My mother decided at the time that she was going to move us west. They were threatening to put the Iron Curtain down, and then no one could escape. So, we picked up and partly took the train--and partly walked-- across the border [into West Germany] when it was still open. Then, we went to Munster where my father's parents lived.

So, I grew up in Munster in West Germany, and that's where I went to high school, and that's where I started out at the University--for one year there, and then one semester in Freiburg, which is in southern Germany.

At that time I had applied for a Fulbright Exchange Scholarship--and it came through. So, in the fall of 1959 I came to the United States as an exchange student on the Fulbright. I was placed at Kent State University. And there I got my undergraduate degree in 1961 in geology. Then I applied for all kinds of assistantships, and I received a bunch of offers. I decided to go to Penn State because professor Krynine (a renowned sedimentologist) was there, and I had heard of him. He had kind of impressed me in what he had written about sedimentology. So I went there [in the fall of 1961 [to Penn State]]; however, I switched away from sedimentary geology because there was an opportunity to get an assistantship with Robert Scholen, who was a Dutchman, although he had been in the States for quite some time. I met Ivo at Penn State during the fall semester of 1961-1962 [see Ivo Lucchitta in 1966].

I was at Penn State from 1961 to 1966. Because Prof. Scholen was a structural geologist and a field geologist--and he got an NSF Grant that supported some students--I went with him and did field work in the Rocky Mountains on a structural problem in the overthrust belt--right on the continental divide between Colorado and southwestern Montana.

I camped with another couple, who were also with the same Prof.'s assistantship, doing an adjacent area, because I was sort of scared to camp all by myself, except, every once and a while I did camp by myself--but by and large, I didn't. So I did field work out there mostly from 1963 through 1965.

I got my master's degree in 1963. I got masters, even though it was on the same problem, because I figured that I might get thrown out of the country because, as an exchange student, I had to renew my student visa year after year. So just in case I couldn't make it all the way through [to the Ph.D.], I got my masters degree. But then, I continued on the same problem and got my Ph.D. in the spring of 1966.

And on course, at the same time I had the baby [her daughter Mya] who arrived the day before my thesis defense. It was planned for two weeks later--but she arrived early--probably much to the relief of my thesis committee. Ivo had to go in and postpone it. I think it was postponed about ten days. I was really pooped having the baby and all that--I was really done for. But he [Ivo] rescheduled it, and in hindsight, it was really a good thing because I was all primed.

But that whole time was really something because of course I didn't know a thing about babies. At that time you didn't have all of these courses and get all of these instructions, and I didn't have any parents around to ask. So, I knew nothing, and I decided that I was going to get my thesis out of the way--and then I will have two weeks when I will study up on babies.

And of course, it was [the way it turned out] the other way around, so when I had the baby, I didn't have a clue what to do. So we brought the baby home and she started screaming, and Ivo and I looked at each other and said what do we do? So we called up a friend and said it screams, what do we do? He said how about changing the diapers? So I changed the diapers and all the screaming stopped. That's how little I knew. I was totally innocent.

I had one brother who was a year older and a sister that was three years younger, but I never really had anything to do with little kids. I never baby sat or anything.

Then, of course, I didn't work for a year because with the baby and everything I was kind of fed up. So, when Ivo got the job at Flagstaff [with Astrogeology in October 1966] I didn't work. He got the job and I just went along. But after about a year just sitting at home with the baby, I decided that was not what I had gone to school for. That was just not enough for me! So I checked in with the Survey--whether they had some sort of room for me there. That was in the fall of 1967. They were interested in getting me on. But there were several problems. For one thing, I was not a citizen at the time. Ivo was; he had become a citizen much earlier--but I was not. Being an exchange student, just marrying a citizen doesn't help. It is very difficult to get your visa changed--to get an immigration visa. And I only could do it because I had an American daughter--and I had prospects for a job. These two things helped. So the Survey was interested in getting me one, but there were these difficulties with me not being a citizen, etc.

So Jerry Harbour [who worked in Administration at the Branch of Astrogeology at the time] devised a scheme where he said well we need somebody who is a technician. He said if you come on as a technician for a year, then you can get some kind of experience that maybe eventually we could use to write out a job description that would fit so we could get you on as a geologist. So I came on and I worked WAE only about four hours a day because I had the baby.

I worked over at Fourth Street [Astrogeology's recently completed building in East Flagstaff] and I worked on photoclinoetry. What I did was run the photoclinoetry machine, together with Art

Dial. The whole thing was to get photoclinometry profiles; and the people who were really interested in that I guess were Bob Wildey, who had just completed the albedo map of the Moon, and, I guess, Howard ["Howie"] Pohn who was into this kind of stuff also. So that was what I pretty much did for a year. Then, at some point Richard Hereford even worked there for a little bit. He actually started out working for me--but just a few weeks--if that much--because he didn't like it at all. He was a [geology] student at the time.

The Branch Chief for Astro when I arrived was Hal Masursky, and my supervisor was probably either Bob Wildey or Howie Pohn--one of those guys.

After I did that for about a year I was hired as a geologist, still WAE, but as a Geologist GS-11. That was probably late 1968.

Then, I was assigned to do the Aristoteles quadrangle on the Moon. Of course, I was in Astrogeology and I talked with Don Wilhelms and at that time they needed these 1:1,000,000-scale geologic maps of the Moon. So I did Aristoteles which is sort of off to the northeast of Mare Imbrium. At that time I moved to the Arizona Bank Building. I had an office there on the fourth floor. There was Mareta West [hired by the Branch at Menlo Park in November 1964]; she had an office with a window and there was kind of a cubbyhole on the outside between her office and the hallway--the stairwell. That is where the IRS is now--or was. After I completed the Aristoteles quadrangle, I did the geologic map of the north side of the Moon--the whole north part.

Then somewhere during that time, you know, the Apollo stuff was really becoming the thing. That became pretty much foremost--especially where they were going to land. I wasn't really in on much of that until Apollo 17. However, prior to Apollo 16 I was assigned to make possible landing site maps. Like, for instance, I did one that was called the Proclus area as a possible landing site because there were some sort of dark-mantled craters there. And of course, I realized looking at them that they were all impact craters. I was kind of against Farouk El Baz [then at Bellcom, Inc. in D.C.] who wanted to make them all volcanic. Of course they never went to the Proclus site.

But when Apollo 17 came up, they assigned me another kind of site to do. I attended one of the site meetings. Jack Schmitt was there, and I think probably Lee Silver and all kinds of people discussing where to go [i.e., land Apollo]. That meeting was in Flagstaff, and as a junior scientist I was sitting along the back wall just sort of listening in on it. But by listening in I realized that Jack Schmitt wanted to go to Taurus-Littrow. So when the meeting was over and everybody was gone, I switched my science site to Taurus-Littrow. I said that's the one I wanted to do! That was the 1:50,000-scale geologic map of the Taurus-Littrow site."

[Author's Note: The larger-scale ones, 1:25,000 and 1:12,500-scale maps were only open-filed and were not formally published. They were used for traverse planning and by the LGE team for astronaut training and mission operations at MSC.]

When asked by the author whether she participated in any of the astronaut geologic field exercises, Barbara responded as follows:

Barbel Lucchitta: "At some point the Apollo 17 crew (Eugene Cernan, Jack Schmitt and the other prime and backup crew members) were in town [Flagstaff area] doing their final field exercises. [Author's Note: This would be the final geologic training exercise for any Apollo crew--and one of the most extensive. It took place at Sunset Crater and the Cinder Lake Crater Field just northeast of Flagstaff on 2-3 November 1972--just about a month before the Apollo 17 crew left for the Moon and the spectacular Taurus-Littrow Valley.]

I can't remember whether they were on the Grover or not. I remember that Rae Schaul [later a math teacher at Flagstaff High School was there [see Rae (Schaul) Harvey above]. She and I were out there watching them. We were kind of standing on the side, you know sort of lining the path where they were supposed to be coming through [on their traverse]--and [Gene] Cernan showed up. He was talking in his microphone all about the geology here and there, etc., and then he came around the corner and nonstop he talked into his microphone and said now the scenery around here is getting much better (laughter), and then he kept on talking about the geology! As he said it, he kind of looked at us.

There was a party that night [at Red Bailey's home in Upper Greenlaw in East Flagstaff], and Rae and I were there at that party.

Rae Schaul actually went to quite a few of the missions; she was involved more than I was at the time. Of course, for Apollo 17 I got pretty heavily involved because I was doing the 1:50,000-scale regional geologic map. So I went to the Cape twice to lecture the crew. The funny thing was that I was not lecturing the prime crew--I was lecturing the backup crew.

I never did go to any of the Apollo launches, but I had dinners with the astronauts in their quarters at the Cape. That was a lot of fun also. And of course I was pretty young--and that was big hero stuff! I did that twice. You know how it was, you are invited for dinner and afterwards you give a talk.

Ron Evans [CMP for Apollo 17] was late for one of those dinners at the Cape. He came in and he kind of looked around the table and said I thought we were supposed to get a lecture from Dr. Lucchitta tonight! Where is Dr. Lucchitta? Of course, he didn't have a clue it was a young woman you know instead of a heavyweight professor type.

Well, that was a lot of fun because we all went out afterwards to hit some of the bars with those guys [the astronauts]. It was fun!

Then during that Apollo Era, the German TV came around three times. A guy named Schieman, who did their science programming on the second German channel, came around and interviewed me because, of course, I spoke German. That was not for Apollo 17, but for the three missions before that [Apollo missions 14-16]. By the time Apollo 17 came round I guess he got tired of me. Even though I didn't really have anything to do with missions 14-16, I was just repeating [on German TV] what other people said; but it was kind of fun because he sent me out on the Little Colorado River overlook for Apollo 15 because there was a little gulch there like Hadley Rille at the Apollo 15 site. Then he set me up out at Meteor Crater for Apollo 16 I believe.

After Apollo 17 I published a paper with Jack Schmitt on the orange glass [that Schmitt found at the Taurus-Littrow landing site]. Had looked at the pictures very carefully--first the Pan Camera pictures and the Metric Camera pictures from earlier missions of the Taurus-Littrow site. Some of the Metric Camera pictures were in color. I realized that there was orange color over in the Sulpicius Gallus area. It was associated there with some sort of oval-shaped depressions [possibly volcanic]. So Jack and I published a paper together with some of the new images he had obtained [in situ] at Taurus-Littrow, along with the [orbital] images that showed the orange stuff in Sulpicius Gallus area. Of course the upshot was that it was all volcanic. At that time there was a lot of people who didn't believe that the glass [beads] were volcanic. There was a big school of thought that they were the result of impact as opposed to volcanic.

I moved into Building One on the Mesa after the Bank Building but I can't remember exactly when. I may have moved into Building One before a lot of the people [in the Bank Building] did. All I know is that I moved into Ray Batson's old office in Building One. I was in that same office for 30 years until spring 2001 when they kicked me out because Building One was unsafe and was to be demolished to make room for the new Shoemaker Building completed in September 2002.]

After the Apollo days I worked on the Moon until Mariner [9] came around and we got decent pictures from Mars. What I finished up on the Moon was my northern hemisphere map; then I published a lot of papers on grabens and wrinkle ridges--and all kinds of stuff on elevations. I played with elevations using the topographic maps; especially wrinkle ridges in a major way for a while.

Tycho was a major paper also. I recognized at the Apollo 17 site that the secondary craters there were from Tycho. So I published a paper that mapped out the trace of Tycho secondary craters, and one of these secondary crater streaks went right across the Apollo 17 site. So since they had dates on some of the secondary craters at the Apollo 17 site--they then could date Tycho. So that was a major paper.

I published on the landslide in that area [Taurus-Littrow landing site] saying that it was probably triggered by the impact from the secondary ray from Tycho--that is it was not a common landslide.

So I did a whole bunch of little things until Mars came around and I was so intrigued by something that was more like real geology--which is what I liked better than craters! I never really got into craters. This began with Mariner 9 in 1971.

During the Mariner 9 days I actually stayed in Pasadena [JPL] quite a bit because I was working for Henry Moore on his Viking landing sites project [the Viking Orbiter/Lander missions would take place in the summer of 1976]. Henry was in a trailer at JPL as an observer during Mariner 9 time with the idea that he was going to help plan the Viking landings. I worked for Henry making photomaps off Mariner-9, etc.

However, I was a guest Investigator on Viking. So I went back out to Pasadena/JPL for that mission also. I was there when they landed, etc. It was very exciting. Then, of course, after Viking we had so much data I did a lot of stuff on Mars. I started out with wrinkle ridges again because I was still kind of into that. But then I got very intrigued with the channels and with

Valles Marineris. I published a big paper on the landslides in the Valles Marineris. The cold climate features on Mars were something else I was really into. I got real intrigued with the channels--that they looked a lot like glacial features [on earth], and I published a lot of papers saying maybe what we are seeing were not made by big floods, but by ice.

Then I went with Larry Soderblom out to JPL again during the Voyager missions [1980-1989]. I got really into that. I published the first big paper that came out on Europa--together with Larry. I also did some work on Ganymede. Shoemaker had the Ganymede map so I became a junior author on that one. However, I published my own papers afterwards saying that there were all kinds of strike-slip faults there--which at the time nobody believed either--except it turned out to be correct! You know, --photogeology has a lot going for it!

Then, of course, I ran the Galilean Satellite geologic mapping program [for NASA out of Flagstaff.]

The other thing I got into was Antarctica. I got into this because of Larry Soderblom. The Antarctic Treaty was coming up for renewal I think sometime in the mid 1980s. Larry thought that there was going to be a major interest in Antarctic work. He realized that people had been using just the old paper prints of aerial photographs where you couldn't see anything. There were some people who thought the Landsat images could be useful--like Swizenbank in England, but they never used image processing. So Larry proposed to the Survey to do some decent image processing on Landsat images of Antarctica. So the Survey gave him some money to do that. Then Larry bugged out because he was too busy, and he threw the whole project into my lap. I have been running the Antarctica project ever since.

[Author's Note: Barbara Lucchitta, a producer of numerous, high-quality, and well-received scientific papers and maps while at the Branch of Astrogeology, formally retired from the Survey on January 1, 1995. At this writing, she is still in Emeritus/volunteer status with the USGS. She is still actively continuing her research on Antarctica and Mars. She and her husband Ivo Lucchitta (also retired from the USGS) still reside in Flagstaff, Arizona.]

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**Ivo Lucchitta** (b. 1937; Ph. D., 1966, Penn State University), an articulate European-and American-educated intellectual with proven talents in field geology and geomorphologic evolution of landforms, in addition to impressive oral and written communication skills, arrived in Flagstaff with his equally-talented geologist wife Barbel (or Barbara) (and their young daughter Mya) in October 1966 to work for the Branch of Astrogeology. Barbel Lucchitta would formally join the Branch of Astrogeology in 1967 (see 1967) (see above).

Ivo would be instrumental in the early research of the Branch's Manned Lunar Exploration Studies (MLES) group. During the Branch's early field testing of manned lunar geologic procedures in nearby Hopi Buttes, Arizona (and elsewhere), he would help develop entirely new procedures for how the astronauts could most efficiently and effectively communicate what they are observing on the Moon to geologists and other geoscientists gathered together in what as envisioned as a Science Backroom in Houston. Ivo left Astrogeology in the mid-1970s to work for the Survey's Branch of Central Region Geology, and then the Branch of Western Regional Geology—all while



remaining in Flagstaff. Ivo and Barbel Lucchitta both retired from the USGS in 1995 and still reside in Flagstaff.

The following was taken from an interview with Ivo Lucchitta by Gerald G. Schaber on 8 February 2002:

Ivo Lucchitta: "My story is a complicated one. I was born in Czechoslovakia--or what was then Czechoslovakia--in 1937. This was a very bad time to be born, at this particular time and place. When I was very small, my father was the manager of an airport of what was then Anglo-Egyptian Sudan at Wadi Halfa. So my very first years were there. When WWII broke out in 1939 my mother and I managed to fly back to Europe on the very last plane that left--and that was shot at, etc.--but we managed to get back into Czechoslovakia where we stayed. I don't remember how long, but until my mother noted that people who owned businesses and factories and things had a way of disappearing because of the Nazis. So, she did not think this was a very healthy place to stay. So she and I, by quite an adventurous route, went to North Italy, and specifically to the Venice area--to Lido, where we stayed for the rest of the war. It was relatively quiet, but just as bad as anyplace else really in terms of food or anything else.

After that I went to England. We are now in 1947. I spent three years there going to college--what they call school here. That's where I learned English. Then I went to Italy and spent about a year and a half up in Northwest Italy in the Turin country. That would have been about 1950. Around 1951-1952, I rejoined my mother in Rome, and that's where I finished my secondary education earning a Baccalaureate in Classics.

My mother's mother was Czech; my mother's father was Russian. She wasn't really doing much of anything [as far as a profession is concerned] except trying to survive. You know during the war and in the post-war years--especially with a kid-- it was very difficult. My father was an aeronautical engineer, and he disappeared from the scene on the very first day of the War in 1939 because he was an Italian citizen. My father came from the northeastern-most corner of Italy that was Austro-Hungary until WWI, and then he and everyone else became Italian citizens. As such, the British interned him on the first day of WWII. He basically disappeared into the Sahara or someplace for about nine years and nobody knew anything about whether he was alive or dead. He was not part of my growing up at all, so essentially I did not have a father.

Well, he reappeared, but he was, to say the least, strange. Most people died; it didn't matter whether they were prisoners or guards. But he was a tough bird, and he survived--but he was strange. Then of course, he and my mother didn't get along at all anymore because they each had had such different life experiences. So they split up. So, no father!

I graduated from high school, as I said earlier, with a Baccalaureate in Classics--in Rome. I then went to England to study aeronautical engineering as what was called a student apprentice. In other words, you work in the airplane factory during the day, and in the evening you went to school at the University. The idea was to get both a theoretical and practical education at the same time. I was there at the Bristol [England] Airplane Company and Bristol University for a little over a year when it turns out mine and my mother's immigration visas finally show up after eight years from the time of application. So, at that time I went back to Rome for a few months, and then my

mother and I traveled together to the United States--which in those days you did by ship. We then crossed the country and went to Pasadena, California. This was April of 1956.

While still back in Italy, I had taken the U.S. college entrance examinations, which were kind of interesting because the European educational system is totally different from the American one. So I did not really know what was going on. But I did well enough to get into Caltech.

So there I was and I didn't know beans about what was happening, and the advisors at Caltech didn't do any advising. So, I had not only to support myself, but also had to find ways of paying tuition. So basically, I was working 8 hours a day and going to Caltech. This was 1956 through 1961. I survived. This is the best I can say. I am not a fan of Caltech; I'll say that, except for Professor Bob Sharpe [also one of Gene Shoemaker's favorites at Caltech] who got me into geology. I was a physics major, and of course you had to take all of these courses in other sciences. They were required. One of them was geology, and Bob Sharpe was the instructor, and I just sort of said wow! He was amazing, and I am still in touch with him. So Bob Sharpe got me into geology. Then when I graduated, Dick Jahns, who was also the sort of human being who really cared about students, at that point went to Penn State to become Dean of the earth Sciences Department there. So I basically followed him there [as did David Schleicher who also received his BS at Caltech and went to Penn State.]

I was very pleased with Penn State. I finally had the feeling of really learning something that I understood. D. Paul Krynine really taught us how to think. There were a whole bunch of really amazing people there. And I got my Ph.D. there in 1966.

My dissertation [the geology of the Lake Mead country at the mouth of the Colorado River] was something that was kind of cooked up by Eddie McKee--the spirit of the Grand Canyon. He persuaded the personnel at the Museum of Northern Arizona [Flagstaff] to have a little stipend for a couple of us--Dick Young and me-- and we both worked on problems that McKee thought were important. Then we presented our results in what is now the almost legendary McKee Symposium in 1964 here in Flagstaff.

Then, some of my colleagues like Dave Schleicher and John MGonigle, who were at Penn State--had come to Flagstaff and had jobs with the USGS [Branch of Astrogeology]. They said, well why don't you apply? I had always dreamed of coming back to Flagstaff; so I applied and I got a job here.

I showed up in October 1966 with Barbara [who Ivo had met while at Penn State] and our little daughter, Mya and started out in the Arizona Bank Building in downtown Flagstaff. I think it was on the 3<sup>rd</sup> floor--and then we all moved up to the 5<sup>th</sup> floor. I was assigned to the Manned Lunar Exploration Studies [MLES] group. The Branch Chief at first was Al Chidester, and then we had Arnold Brokaw--and then Jack Strobell.

I was involved with a lot of the Hopi Butte field tests, you know, just learning how to figure out how to communicate [geology as an astronaut on the lunar surface]. That's the time when Dick Jahns was out there being the field guy [senior geologist field advisor]. He was just astonishing! I'll never forget one time when we were looking at him on the TV monitor [in the science

backroom trailer], and he sort of bent down for a minute, and never stopped talking. Later, we found out that there was a rattlesnake at his feet. He kind of pushed it off or something and never stopped talking to us.

At that time, Dave Schleicher, John McGonigle, Red Bailey and I [as test subjects in the field] were all quite involved in trying to communicate from the "Moon" to the science [back] room in a way that the scientists can understand. We were kind of trying to systematize description and things like that.

Then I was involved in the Black Canyon crater field that Astrogeology built down in the Verde Valley [called at various times the Black Canyon Crater Field, and the Cottonwood Crater Field]--both during the construction phases [February, 1970], and then in the geologic mapping of it [see Norman "Red" Bailey in this Appendix.] The geologic mapping was me and Phil Shafer--and boy did I get into trouble on that one with people like Gordon Swann and Bill Muehlberger because I did my part, but I could never get Shafer to finish his part. They kind of figured it was my fault.

I was quite involved in the lunar hand tools as well--the gnomon, Apollo staff, etc., but especially the description techniques. I was also involved in astronaut training. The only place I went was to Sudbury, Canada [7-9 July 1971]. It was always a matter of trying to get those guys to tell us what they see and did in a way that we could understand. When it comes to the astronauts that I got to know at least--my favorites were Neil Armstrong and Fred Haise. I thought Haise was such a human being. He was a really neat guy.

As a field geologist I was happy to do fieldwork related to the space program, but then that ceased. Then, I felt I'm losing my hand at this thing, and you know you can't afford to get out of it too long because you're going to be obsolete. So the opportunity came up when Gene Shoemaker cooked up this project using ERTS [the first commercial geoscience-oriented, space-borne photography in the early 1970s]. Nobody knew how to use this new stuff. So the project was about how can we use this, and can we use it for terrestrial [geoscience] applications? It was then the early 1970s, maybe 1973. I know the report was published in 1975. At that time, I kind of disappeared into the wilderness in the Shivowitz Plateau north of the Grand Canyon and mapped some gigantic piece of country up there.

Then Mike Carr became the Branch Chief [in May 1974] and the Astro funding was way down [after Apollo] and he was trying to get rid of as many people as he could that were not mainline Astrogeology types. So, at that time, or just before that, when this project came up with Gene Shoemaker [the ERTS project mentioned above], I said great. And once I was in that, I had obviously branched off from Astrogeology.

When Arnold Brokaw arrived in Flagstaff he realized that some of us "field geologist" were sort of down in the dumps because we weren't doing any field geology. He did something good for McGonigle, Schleicher and I. He sent us for a couple of months up to the Yellowstone Project. I worked with Hal Proenca (sp?) and Schleicher worked with Jerry Richmond I believe.

While I was still in Astrogeology I went to Nunivik Island with Joe Hoire and Tom McGetchin. That is an island in the Bering Sea that had big explosion craters--maars.

The idea was to go there and study large explosion craters as one of the Branch's lunar analog sites. This would have been in the early 1970s--before 1974. Tom McGetchin was interested in volcanism. He passed away from stomach cancer quite young. Joe Hoire was the field geologist from the Alaskan Branch who had been mapping in Nunivik. He was also interested in studying the distribution of ejecta and stuff like that. So that was while I was still in Astrogeology.

I then ended up in Central Regional Geology. This would have been in the mid-1970s. Then we all ended up in Western Regional Geology Branch--and then back in Central or something.

Later, when I joined Western Regional Geology, they asked me to work on the highly extended terrains of the Colorado River corridor. I think I started this in 1976 and continued on until 1980. There, they wanted background geology for possible siting of nuclear power plants and reactors. In 1991 I started on the Glen Canyon Environmental Project. That started in 1991 and went on until I retired.

After 1980 I was involved in environmental impact statements with Dave Schleicher [who had already left Astrogeology; see 1965], and who was the Chief, of the Branch of Technical Reports in Denver.”

[Author's Note: Ivo Lucchitta retired from the USGS in 1995 and still resides in Flagstaff with his wife Barbara. In 2001, Ivo published a book entitled *Hiking Arizona's Geology*.

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**John F. “Jack” McCauley** (geologist; b. 1931; Ph.D., 1959, Columbia University):

The biographical narrative on John F. McCauley that follows was abstracted from commentary he gave to Gerald G. Schaber during their interview on 19 February 2001; and, in part, from McCauley's own memoirs (unpublished):

Jack McCauley:

“I have been asked to give some background on myself as to how I wound up in the U.S. Geological Survey Branch of Astrogeology. It's a long story partly determined by my hometown of Millburn, New Jersey—a town dating back to Colonial time, and a very pleasant place in which to grow up.

My dad commuted to the New York Telephone Company building on West Street in Manhattan by Lackawanna Railroad and Hudson River Ferry. Millburn, before WW II, was not yet filled in as a New York City bedroom town. Millburn Township also included a community called Short Hills where I went to grade school through the fifth grade. The growing up process on the edge of a relatively unspoiled small town inspired a lifelong fascination with the outdoors. These forays into the fields and woods also kindled an interest in the Earth. I noticed, for instance that there were outcrops nearby of red sandstone, which I later found out was Newark Formation of Triassic age. There were all kinds of other rocks and large boulders- some eight to ten-feet across in a nearby valley. Well, these features were the outwash of the Wisconsin Glacier. Another notable local feature was one of the segments of the Wachung Mountains made of blackish igneous rocks-called

basalts. I had become interested in things geologic before I even knew anything about the profession itself. I went through the middle of fifth grade in Millburn when all this came to an end on December 7, 1941. My brother and I were listening to the radio about six or seven PM when the news of the Japanese attack on Pearl Harbor was announced.

The war came and my father who had a managerial job with the New York Telephone Company was asked to move to the city to work better with the many new customers in the military demanding new phone lines and equipment. The move from Millburn was early in February 1942, only two months after Pearl Harbor. We settled in Jackson Heights, then a comfortable upscale apartment area in the borough of Queens that still had greenery and also open spaces for tennis courts, a small golf course and many empty lots that served for “pickup” baseball and football games. The school I attended was across the street from our apartment. There was a problem being a new kid on the block, especially one who came from the other side of the Hudson River. I became very withdrawn and spent much time learning about astronomy and geography. That was a tough time that was softened a bit by the Saturday movies in one of the two theatres within easy walking. There were no summer vacations to the Jersey shore or to Vermont as before the war. I completed grade school coincident with the end of the war in Europe and began high school just before the end of the Pacific War in September 1945. I took part with my father and mother in the tumultuous victory celebration in Times Square.

I went on to an urban high school in lower Manhattan, La Salle Academy. It took about an hour each way on the subways. Classes started at 8:00AM, so I had to leave the house about 6:45AM everyday. It was a real grind. During the middle of my freshman year, my dad died from a heart condition aggravated from the stress of his wartime job, and from smoking too much. I think he was up to about three packs a day toward the end. My mother had to go out to work after about a year. Even though he did leave a sizeable stock portfolio, it was not quite enough to raise two young boys.

The Christian Brothers who ran LaSalle were very helpful with my studies, and I developed an interest in astronomy following an earlier exposure to visual studies of the night skies during family vacations to Ocean City, New Jersey. I acquired a small reflecting telescope that I used on clear nights on the roof of my apartment house. I did well in math and the sciences up through my junior year. I worked like a demon during the senior year with encouragement of the Brothers. This effort paid off with an array of five scholastic college scholarships and as the Valedictorian of the class of 1949. I originally wanted to get way from New York City to a campus with open space and lots of greenery. I also wanted to avoid the smelly, grimy subways that had carried me back and forth to high school. I discovered that three of the college awards were in cash that could be used in any New York State school. . Altogether, these scholarships provided enough for tuition and board with some left over every semester. I also knew that I couldn’t go too far away because of my mother and younger brother. I wound up going to Fordham University that offered a program in geophysics. It also was a green island of trees and grass within the urban middle Bronx only about ten miles from where I had lived in Jackson Heights, Queens. I would be able to live on the campus to get the benefits of college life with no more commuting except now and then to visit home.

I had a great time the first year during which I was bunked in with two senior year varsity football players. They were indifferent students and somewhat amused by my studious habits. The nicknamed me “Rocky” as a result of my interest in geology. I was in part of the Freshman Class designated as the Mathematics-Physics section that was thought to consist only of nerds. I was true to form until they discovered that I played sandlot football on the weekends, so they gave me some very helpful pointers. The books paid off again, and I was fortunate enough to win the class medal for best overall grade average in the Math-Physics section.

The Korean War began during the summer vacation and shortly after the new semester I was called for the draft physical and after going through all the physical tests we emerged into a room where a Marine Sergeant in full dress uniform, congratulated us as we were the first men to be drafted in the entire history of the Marine Corps. We would receive orders to report to Camp Lejune, North Carolina within 10 days. After checking with my local draft board, the orders were cancelled because of my student classification. My grades slipped quite a bit during that academic year. Since I had four years of French, and some money saved up from the many part time and summer jobs I had since I was 16, I decided to go to Europe during the summer of 1951. Student ships were converted WWII auxiliary aircraft carriers were cheap, bunk bed, cafeteria affairs that made the crossing in 10 days. They were the way for college kids of limited means to go abroad without the expense of the luxury liners. I traveled through France, England, Ireland, Denmark and Germany partly by overnight ferries, hitchhiking, and second and third class train rides. These were the days when you could get along nicely on five dollars a day or less. I even had the chance to go by boat up the Rhine from Cologne--that was still mostly rubble from the WW II bombings—to relatively unscarred Heidelberg. Other highlights of the journey were Paris and London and Denmark, where I was invited to spend several days with a Copenhagen family by a fellow I met on the ferry from England to Denmark.

When I returned to Fordham, I found that the Geophysics Program had been cancelled due to the death of its only geology professor. I had no wish to continue in the pure Physics Program where I would be laboratory bound probably for the rest of my working days. I yearned to spend as much time as possible outdoors—the writer Stegner, a student of the American West, aptly stated “what one spontaneously responds to...is what one grew up in.” For me it was the open spaces of my childhood.

I decided that I would glide the rest of the way through college with liberal arts and a French Language major since I was already reasonably fluent. I would get my Geology training after finishing the required military duty at the time. Shortly after graduation in May 1953, I was accepted in the Officer Training Unit at the U.S. Coast Guard Academy in New London, CT. It was a grueling four months—the instructors were tough but fair and ones overall demeanor, i.e., ones “cool” under pressure was as important as the academics. It was here that I was immersed in the various skills needed to navigate on the open ocean as well as along the coasts. These would serve me well during the later part of my geology career in the Sahara. Despite my previous lack of experience in nautical matters and engineering, I finished in the middle of my class that “washed out” along the way one quarter of its total. After graduation I was assigned to the 165-ft CGC cutter Dione WPC-107 built in 1932 (same year I was born). It had seen considerable action during WW II as a sub-chaser in the North Atlantic. During the time aboard we sailed mostly off

the coast of the Yucatan with special trips to Pensacola, New Orleans, Key West, and Matamoros, as well as to Galveston and Houston.

Toward the end of my service commitment I was bold enough to apply to the highly touted Graduate Geology Department at Columbia, despite my discomfort with New York City. To my surprise, I was accepted as a conditional student. Perhaps, I was lucky because the Department wanted a national cross section of students. I was allowed to take a full load of courses but was told that I had to take a qualifying exam at the beginning of the second year in order to continue. Hard work and knack for the subjects brought me through and I found myself measuring up to fellow students who had the benefit of undergraduate majors from various prime geology departments. I became interested in the Economic Geology Program under Professor Charles Behre whose beginning course I found interesting. At the time, this facet of Geology appealed because of its practical aspects. He set me up during the first summer as a Cooperating Geologist with the Pennsylvania Geological Survey to begin work on a statewide survey of known and as yet to be found uranium occurrences. This was to begin after completion of the required University of Wyoming field course held in the Medicine Bow area west of Laramie. This type of working arrangement was still very common at the time because the NSF was just getting in gear so few professors had any monetary surplus in their own funds for grad students. The remainder of the summer in Pennsylvania was a delight. I was assigned a car, a very modest salary and the then adequate per diem of \$6.00 per day. The work was mostly along the Allegheny Front north of the town of Bloomberg in Montour and Columbia counties where the majority of the known uranium occurrences were located. It was a great learning experience, along with the field work for me.

By the time that the second year began at Columbia, I had gained confidence. The examination to become a degree candidate was a breeze, so I was on my way to a long and exciting career in many different aspects of geology. A lab teaching assistantship came along to supplement my finances. I also had the benefit of the G.I. Bill. I concentrated on the mineralogy of the summers' samples and worked over the field sketches and notes. Igneous and metamorphic petrology became an interest that year to the point that it became an unannounced minor under Professor Arie Poldervart.

During the midyear break I was married to Camilla McCarthy from Syracuse, New York and we moved into Columbia grad housing in apartments on the edge of the compact university campus. Cam was a non-degree student with a Bachelors Degree from Syracuse who took several courses with me that year in addition to the earlier Wyoming Field Course. I put together two papers after the field season along with a Masters Degree thesis that marked the beginning of my professional career. During the third year at Columbia I moved up from a teaching assistant to Lecturer that made me a faculty member of low rank. I taught Professor Art Strahler's Physical Geography course by virtue of having taken his course and seminar in geomorphology and my various teaching experience in the Coast Guard. I was in the active reserve and spent time teaching general seamanship one night to enlisted personnel throughout the Columbia years. I worked earnestly on my Ph.D. thesis and began to think about General Exploration Geology as a career that would keep me out of the dark, damp, dirty and dangerous mines that were visited during numerous Economic Geology field trips. I took more petrology courses and was leaning toward "hard rock" geology as my main interest. Prof. Behre became aware of this, and late in the third year, 1958, he told me of an opening at the University of South Carolina that I should look into. I

was not wild about a teaching career but did go down to look the situation over in Columbia, SC, and was pleasantly pleased. It appeared that the Department was in the early stages of rebuilding and offered a nice salary that after deliberation with my wife was accepted. However, I had not yet defended my dissertation, but had met all other Ph.D. requirements. I agreed to do that during the first year at South Carolina. The thesis was successfully defended during the mid-semester break at South Carolina in early 1959. The review panel was pleased with my laboratory work on exsolution features in the sulfides associated with the uranium minerals, along with my field studies. This effort indicated that the uranium features were low temperature ground water deposits and not hydrothermal as some workers claimed.

During the time at South Carolina I taught General Geology, Mineralogy, Petrology and Structural Geology on a regular basis, and later Microscopy and Geochemistry as the need arose. An effort to upgrade the Masters Program met with a good degree of success. I did geologic mapping of 7.5 minute quadrangles with a couple of Graduate students and County mapping by myself under the auspices of the South Carolina Development Board, part of which was a defacto State Geologic Survey. A number of consulting jobs came my way during vacation time. Several took me up to the Blue Ridge Mountains and the upper Piedmont-all hard rock areas. A different but particularly interesting effort was Forensic Geology and involved working with a county sheriff that led to the conviction of two safe robbers using mineralogical evidence. Unfortunately, as the number of students increased more of the weekends were taken up with visits to various field areas being studied. I spent almost five years there and got tenure and an Associate Professorship in my fourth year. I was also involved with a time-consuming major faculty committee. However, I was becoming restless in the small world of South Carolina.

It was now 1962 and there was much buzz about going to the Moon and my interest in things celestial was reawakened. That year as part of the NSF Visiting Science Program we had Ed Chao of the U.S. Geological Survey, Branch of Astrogeology as a Guest Speaker. He was the co-discoverer along with Gene Shoemaker of Coesite, a very high pressure mineral found at Meteor Crater (Arizona) that clinched its impact origin. One of my graduate students was already working on a modern classification of the very good collection of meteorites in the Geology Department. President Kennedy had already set the goal to send a man to the Moon and this goal had stimulated my drift toward lunar studies.

To my surprise in early November 1962, I received a late afternoon call from Gene Shoemaker, Chief of the Branch of Astrogeologic Studies saying he was at the local airport and would I be interested in my coming out for a meeting. I was elated but was not sure what this was all about. After some familiarization with each other he told about his plan to move the Headquarters of his Branch from Menlo Park, California, to Flagstaff Arizona. He already had one geologist there and he wanted to have one more to help set the stage for a move there the following year. He asked if I would be interested in this job that would also include lunar geologic mapping. The answer was a delighted yes, but was contingent upon my obtaining a release from the upcoming second academic semester at the university. He gave me a U.S. Government SF 57 application form and left shortly for New Orleans where the GSA was holding its annual national meeting.

Gene was giving a paper there at the meeting [the 1962 GSA Annual Meetings in New Orleans, Louisiana], thus facilitating his stopover [from Washington D.C.] in Columbia, South Carolina. I



went back home to tell my wife that I had just accepted a job with the USGS and that we were to leave as soon as possible. The only down side was that it entailed a considerable loss of income, and we learned that Flagstaff was a well watered, small mountain town. Sight unseen it appealed to me due to my Milburn experiences. Getting loose from the University was not all that easy. The way was smoothed by the Dean of Science Davis who had been helpful before when difficulties arose with L. L. Smith who had been unduly stingy with the Department funds. A suitable fill-in was found for the second semester and it was then possible to leave without ill feelings in the University Administration.

It was mid February 1963 when we left Columbia, South Carolina for the new world of Arizona. The Southwest was a complete unknown and was to be a new experience tempered only by many Cowboy movies of the time. When we reached Amarillo passing by way of Oklahoma City we were beyond the 100th meridian and began to get the feel of the West. The trees were gone and you could see over the scrub and sage as far as the dry air allowed. We both experienced a refreshing feeling of openness and enjoyment in the distant horizons. The Interstate System was far from being done, so we rolled along in our 1963 Rambler station wagon on the old mostly two-lane Route 66 at an average speed between 45 and 55 mph. The weather was cool and clear despite the late January date. Albuquerque was a visual delight with its' Mexican overtones. The Uranium Country near Grants, about which I had read much during my thesis work, provided new geologic vistas including our first fresh volcanic flows. We were pleased with all the naked outcrops uncovered by layers of Green Stuff. By the time we came to Holbrook with its tan and brownish somewhat ragged buildings and the bleak appearing surroundings we began to worry about what Flagstaff was really going to be like. Our addiction to greenery of some sort was still very strong. As now displaced East Coasters we had been imprinted with an instinct that lots of chlorophyll was a normal part of ones surroundings. As we climbed from dusty Winslow toward Flagstaff with the San Francisco volcanic peaks dead ahead, the pinions and junipers began to appear at the 6000 ft. level and the stately ponderosas on the outskirts of our goal at 7,000 ft. The main street through town along side the railroad tracks was, however, somewhat disappointing. A drive the next day in the northern part of the town convinced us that it was a livable place. The comforting array of high altitude greenery-mostly ponderosa pine squelched our doubts about the move. I had given up a tenured Associate Professorship at a major State University and about \$1,500 cut in pay to join the fledging program in Flagstaff. I was worried about the new environment. We got lucky and acquired a typical suburban ranch style home on the northern edge of the older part of town. It was comparable to our South Carolina abode but with a smaller lot and no trees (no complaints since I was to have little time for yard work.

The spring [of 1963] was marked by travels to Caltech to deliver the telescope that had been used by Marshall in his "Seeing Survey". It was to be modified for use in the Southern Hemisphere (Peru) to study the reported clouds [of dust] in the gravitational null points between the Earth and Moon. These clouds might contain material ejected from the Moon by meteorite impacts and could be sampled before any of the Apollo landings using unmanned spacecraft. A major explosive volcanic eruption in the South Pacific ruined the transparency of the upper atmosphere and the project came to naught. During the Caltech visit, I met with Howard Pohn, who would join the Branch in Flagstaff the following year, and Bruce Murray who would figure later in the Mars exploration program. During this visit I learned that Apollo was not the only NASA program at the time. A program to explore Mars by means of unmanned Mariner and Voyager spacecraft

was evolving at the Jet Propulsion Lab and I followed it closely because Mars promised to be more geologically interesting than the Moon.

I visited the Menlo Office about this time and was pleased to meet the group there-Masursky, Moore, Wilhelms, Eggleton, and Carr. During this trip Wilhelms took me on a tour of nearby San Francisco that led to a lasting affinity for the City by the Bay. I was assigned the Hevelius Quad as my area to map on the Moon. It was logistically a tough area because of its position on the western limb. Because there was no suitable telescope in Flagstaff at the time, I began a bimonthly commute to Menlo to use the Lick Observatory 36" refractor on Mt Hamilton east of San Jose. These two to three day sojourns proved fruitful and enjoyable-they included a one or two night stay at the Lick Dormitory and at least an overnight at the Menlo Park Office. Later we would build under the guidance of Elliot Morris a very good 30" reflecting telescope at Marshall's Anderson Mesa site in Flagstaff. It would be used only for a short time after which the Unmanned Lunar Orbiter pictures made it obsolete for visual work (Author's Note: This site is now crowded with about a half dozen astronomical instruments that surround the USGS telescope that was transferred to Lowell Observatory after the Moon landings). Trips to Tucson to look into the availability of the new telescopes being built at Kitt Peak were made in the company of Bill Cannell and [Chuck] Marshall. Wilhelms was off to France to learn how to use the Lyot Polarimeter so as to make our lunar mapping a little more quantitative; another of Shoemakers many attempts to make our work credible to the skeptical engineer types in NASA.

I think we moved into our wing on the Museum building in the early fall 1963. It was speedily built from a graph paper sketch that I gave to Ned Danson, the Director some three or four months prior. Quartered there was Gene Shoemaker in the front office, Don Elston, Ray Batson and Russ Wahmann (both were from the Topographic Division), Ken Watson (a recent Cal Tech Grad), Elliot Morris (a Menlo Assistant with a new Ph.D. from Stanford), and Henry Holt along with me and several technical people who were in the dark room at the back of the building. Marshall was also there but was increasingly uncomfortable with the lively, industrious group that had disrupted his slow-paced lifestyle. Al Chidester and Gordon Swann who Gene recruited elsewhere in the Survey were there for awhile and then were sent to help staff the crew at MSC [the Manned Spacecraft Center in Houston- later the Johnson Space Center]. Dan Milton was also at the Museum Office from time to time. The Menlo people dug in their heels and refused to move for various reasons.

Because of the limitations of air transport from Flagstaff and the increasingly heavy travel demands of new NASA projects we began to look into the use of a light airplane. We needed to get our people either to Phoenix for connections to Washington, D.C., the Manned Spacecraft Center, Houston, Texas, directly to the Jet Propulsion Laboratory (Pasadena, California), or to Menlo Park, California. The commercial connections to and from Flagstaff were very time consuming. The branch decided to utilize light aircraft that could be scheduled at our convenience.

I received my Private Pilot License and joined Don Elston, Ray Batson and Henry Holt as incidental USGS Pilots who would ferry themselves and other Branch personnel to the increasing load of required meetings associated with our expanding array of projects. Early AM departures and return trips in the PM were common to JPL in Pasadena. The only other connections were by the Santa Fe Chief that required a night on the train each way for a one day meeting. So, light

airplanes became practical and economical. The only other connection to NASA meetings prior to the completion of the Interstate-17 to Phoenix was a Frontier Airlines noon flight. The midday turbulence was often so severe that it was “fondly” called the “vomit comet”. There were rumors that the stewardesses were sometimes overcome by air sickness on that run.

The Second Meeting of the Working group on Extraterrestrial Resources was held at Holloman Air Force Base in New Mexico late in October and attended by several of the Flagstaff crew-Elston, Morris and McCauley--a chance to rub elbows with some of what was then the Lunar Establishment.

It was at about this time that Gene Shoemaker formally ended his tour of duty with NASA where he had set up the machinery to build the Branch. Chuck Marshall resigned shortly after Gene took up residence in the Front Office. A lunar geologic mapping conference was held in the Conference Building at the Museum during November. It included all of the then current Flagstaff and Menlo lunar mappers. Several major refinements of the original stratigraphy were agreed upon in a departure from the original works of Hackman and Shoemaker. The changes were the result of the collective efforts of the lunar mappers in Menlo as well as Flagstaff. The “Photoclinometry” work begins to go into high gear. Masursky recruits a dozen main-line survey people from Menlo to begin visual roughness estimates at the 36” Lick telescope of an array of areas in the lunar equatorial belt. Some did this as a lark; others because their regular projects were broke. One of these study sites turned out to be the place where the first Surveyor successfully landed almost two and one half years later. I began to discuss my preliminary visual studies of the Orientale ejecta blanket as part of the Hevelius mapping work. Several nights of intermittent very good seeing at various telescopes revealed that it was fresher looking, and therefore younger than that of the Imbrium Basin. The multiple rings around Orientale were described by Bill Hartmann in one of publications of the Lunar Lab at the University of Arizona. During this time I flew in a Flagstaff Aero Club Cessna 150 to Tucson to discuss matters with Hartmann. I found him to be pleasant, amenable and very capable. He was part of a group of 4 or 5 grad students trained in Astronomy then taking geology courses from Professor Spence Titley. Spence had come to Astro during the preceding summer as a WAE and was part of the group that spent the summer of ’63 in the Hopi Houses along with other USGS people. Many of his students went on later to commendable careers in lunar and planetary work. The Imbrian blanket was the key regional stratigraphic unit on the lunar nearside and had a certain aura attached. My visual observations were met with some doubt among the original group of mappers who may have thought that Orientale was some kind of threat to the Imbrian based stratigraphy we were using as a key mapping tool. (I have found in Geology that it is too easy to criticize that which you have not seen personally). The character and relative youth of the Orientale blanket was dramatically confirmed later during the Lunar Orbiter IV mission.

Shoemaker obtained sufficient funds to fully test the Surveyor’s imaging system. This work absorbs him, Ray Batson and Elliot Morris, along with several support people for much of 1964.

In March 1964 the second Astronaut training session was held in the Grand Canyon area under the guidance of Eddie McKee, the leading USGS expert on the area. Gene had already run a test trip at Meteor Crater and around the San Francisco volcanic field in November, 1963 with mixed reviews. I had been down the Canyon the preceding year as Assistant Scoutmaster with a troop of

Boy Scouts led by Scoutmaster Bill Cannel, of the Lowell ACIC group. We made two trips, the first being a trial run to get our geologic stories straight, and the second that was divided into two Astronaut groups for scheduling reasons. The checkout trip included Dale Jackson, Al Chidester, Dan Milton, Ted Foss [MSC] and several other people from the Manned Spacecraft Center, Houston. Each geologist was assigned two astronauts to guide them through the observations at key outcrops. These were selected to demonstrate how the physical history of the Earth can be read. My students were astronauts John Young and Tom Stafford. They proved to be excellent pupils-observant, astute, interested and personable.

During the prior months I did a lot of flying around the array of volcanic craters in the San Francisco volcanic field to show visitors some of the reasons the Branch was located in Flagstaff. Meteor Crater was only 10 minutes flight time to the East. In the space of less than an hour of flying it was possible to demonstrate the distinctive visual features of each type of volcanic crater and how they differed from impact craters. Marshall and I along with Bill Cannell in our search for observing time had discovered the utility of the McMath solar telescope at Kitt Peak in southern Arizona [Tucson.] It had not yet been put into service, and to our delight we found that it projected a thirty inch image on to a white platform. By using an inclined mirror with a magnifying lens one could view the Moon quite effectively without moving the telescope. You simply moved the viewer from place to place within the projected image. This made group viewing possible-just perfect for exposing the Astronauts to live tours of the lunar surface. I proposed a two stage training trip to Dale Jackson and Gene Shoemaker and they encouraged the effort. It took place in May 1964. The fly-over was done with two Cessna 182's flying in tandem. I was the guide in one plane; and Shoemaker in the other plane. After that exercise we went on to Kitt Peak in the evening where we were joined by Elbert King from MSC and Spence Titley of the Univ. of Arizona, Hal Masursky, Don Wilhelms and Elliot Morris the fabricator of the viewers along with others I don't recall. A fruitful evening was spent observing "live" images of various features previously seen by the students only in gray photographs.

The last of the early field trips was held in New Mexico at the Philmont Ranch during the first week of June. The geology was messy and fortunately I had too many other things to do and did not participate. The trip was not well received by the busy Astronauts who were very jealous of their technical time. Relations with the MSC geology team had become extremely difficult so that the USGS people withdrew completely from MSC. Al Chidester came to Flagstaff and the others went back to their home bases. Chidester eventually took over from Don Elston the Apollo-related work of the Branch that later would persist as the Branch of Manned Lunar Exploration, as opposed to the precursor projects-Ranger, Surveyor, Lunar Orbiter, Terrain Analysis that were combined to formed the Unmanned Lunar Exploration Group headed by McCauley. The group in Menlo Park assumed the original name of Astrogeologic Studies with Hal Masursky leading as the senior USGS geologist.

Although the work since early 1963 had been exciting, mental fatigue began to set in. I decided on a trip to Yellowstone Park with the family plus a mother in law hauling a small trailer. There were a number of pleasant sights and experiences including encounters with bears that were then becoming pests. Ranger 7, following a series of embarrassing failures was a success taking pictures all the way in to its selected crash site on the 31st of July while I was in Yellowstone. The last TV pictures before impact were at resolutions never before seen. They showed that the

lunar surface was indeed in a “steady state” i.e. it was so saturated with small craters that if more were added they would destroy those already there. This was confirmation of the impact model for the vast majority of the Moons’ craters as held by the Astrogeology group- however, die hard volcanic holdouts such as Tommy Gold persisted up to the Apollo landing. I was annoyed to be so out of touch at this exciting time. Shortly after returning to Flagstaff, I flew with Ken Watson in a rented Cessna 182 for a meeting with MSC people concerned with the Apollo implications of the Ranger data. This would be the first of numerous gatherings at MSC as the Unmanned Exploration Program began to pile up an impressive array of lunar terrain data as well as good science advances. Another event of particular interest to me was a successful flyby of Mars by Mariner 4. This first spacecraft look at Mars was disappointing because it revealed a cratered surface looking like a degraded Moonscape.

Ground breaking for a permanent building on our McMillan/Switzer Mesa tract occurred in August 1964. Jack Schmitt came aboard with a brand new Ph.D. from Harvard. Gordon Swann and Joe O’Connor also settled in Flagstaff at this time. Gordon would become a leader in the Apollo part of our house and O’Connor went on after several years to study Law. As projects blossomed and new ones emerged from the hierarchy at NASA HQ, arrangements were made to acquire a large part of the fifth floor of the Arizona Bank building downtown. I believe that Don Elston was a key figure in this arrangement. Howard Pohn came from Cal Tech to the Unmanned Group about this time. He began to develop a three dimensional model of the last Ranger VII frame to give an idea of what the local visibility might be for a man on the surface. Gene was so delighted with it despite its limitations that he persuaded the US Army Corps of Engineers to publish it. The Lunar Orbiter Program began to rise on the horizon, and I was elected by Shoemaker to go to Langley Space Flight Center to tell the people there what we were doing so as to break the ice with this important new segment of the Unmanned Lunar Program.

I went to Langley Spacecraft Center about this time to give a briefing to the people there who were cranking up the Lunar Orbiter as an effective means of searching for suitable Apollo landing sites. Jim Alderman joined me there; he came to Astro from the Topographic Division of the USGS was very outspoken about its weakness as a mapping tool. I remember Izzy Tabeck, Norm Crabill and Tom Young among the people we met at Langley. I was asked by Tabeck to give briefing to the Director as to what we were doing in terrain analysis. Izzy gave me a tip that I carried through every talk I gave through the rest of my career. It was to limit myself to one minute for each slide used. Quick surgery [on my presentation] on the stairs up to the Directors office assured a successful briefing that was to give the Branch the inside track in this key program for Apollo and for the regional Lunar Geologic Mapping Program and the mapping of candidate Apollo landing sites. It was clear that we would need additional people to work on this potential bonanza.

Maurice Grolier turned up with a new Ph.D. at our motel there [at Langley] and after a short talk together I decided to sponsor his joining the unmanned part of Astrogeologic Studies. He was approaching middle age but had the look of a serious, capable scholar. Our group lacked mature talent as Maurice was to show. The Lunar Orbiter was to be an Apollo support effort without any Principal Investigator. The Flagstaff Unmanned Group was to provide the bulk the geologic work as well as participation in the Mission Operations. The pace of our involvement in Apollo was accelerating. Meetings with various engineering people were more and more common and our flying here and there more frequent. This work included almost semi-daily flights around the local

area to orient various engineers and other NASA people to our trade. I personally logged almost 100 hours of flight time during this gearing up period. Examples include trips to Denver to examine the topographic potential of the Ranger pictures and to Santa Barbara to have a look at the GM version of the SLRV [the Surveyor Lunar Roving Vehicle.]

In November there was a long 182 flight with Don Elston to the Annual GSA Meeting in Miami, Florida. I presented a poorly attended last day session on the use of slope frequency distributions as a means of classifying lunar terrain. In my own mind this was to satisfy the engineers rather than being a useful geologic tool. Larry Rowan also attended the meeting with his parents and introduced me to Gerald G. "Jerry" Schaber, a fellow grad student with Larry at Cincinnati. I talked with Jerry at that meeting, and he became an enthusiastic Astro candidate; and soon found himself working full steam in the manned part of the house in Flagstaff.

In December, 1964, most of the science and engineering people in Flagstaff had settled into the fifth floor of the Arizona Bank building downtown. These proved to be comfortable quarters. They served the Astro Group up through the completion of most of the work preparatory to the Apollo landings-Ranger, Surveyor, and Lunar Orbiter. Both the Manned and Unmanned Groups toiled here and in parts of the fourth floor for almost five years. A number of smaller specialized units were scattered around Flagstaff such as the Rock Lab and the Photogrammetry unit.

January 1965 was a time of settling in and reorganizing. Ranger VIII was programmed to hit the Moon in Mare Tranquillitatis, and suddenly there was interest in whether the impact could be seen by telescope. Lowell, Lick and Kitt Peak telescopes were focused on the target area, Feb 20 to no avail. The flash and dust cloud expected was too small to be seen despite good viewing conditions. The mission was a success. The same array of subdued overlapping craters was seen in the late frames. The Apollo people breathed a tentative sigh of relief. The surface appeared landable so that no design changes were necessary if it had sufficient bearing strength. This would be tested thoroughly by the Surveyor Landers shortly. It still amazes me how much utter nonsense was being bantered about by certain so-called lunar scientists. The most outrageous among them was a maverick astronomer Tommy Gold who was a master of horror stories that always were bruited about by the press. We are all aware of the need to vet new scientific ideas, but we take for granted that objections are serious alternatives. Gold's objections to the existence of a firm impact gardened lunar surface were more like nightmares conjured up in the dark of night. They included a soft non-cohesive surface into which both men and machines would be swallowed. Another maverick by the name of Jack Green would hold on much too long to a volcanic origin for the majority on the Moons' craters. But another kind of battle was brewing between the NASA engineers and the scientists appointed by NASA to guide the originally science missions. I experienced the early stages of this fight when Gene Shoemaker asked me to backstop him at the Ranger IX mission in March 1965.

We all understood that the Museum facility would be a temporary workplace but we were able to tackle two new projects. One that required a good deal of travel was an evaluation of the merits of two competing small prototype lunar roving vehicles. The winning design would move around in a prescribed pattern measuring the strength of the Moons' soil and also examine its' visual characteristics. It was called the Surveyor Lunar Roving Vehicle and its' acronym was the SLRV to be carried aboard the later Surveyor Missions. Ron Scott, Roy Breteton, Ray Batson and Noel

Hinners and me carried out the work (we were referred to as the “Rover Boys”). We later wrote a negative report regarding its ability to certify an Apollo landing site that did not matter in the long run because the later [Surveyor] missions were canceled.

[Author’s Note: Toward the end of 1965, McCauley was over committed with the Terrain Analysis project, Surveyor Lunar Roving Vehicle (SLRV), and Lunar Mapping program. Gene Shoemaker suggested that he head up ongoing Lunar Orbiter Groups at Langley and drop the Surveyor (SLRV) work. This was not a problem because the SLRV died in NASA after the report of our field tests. Larry Rowan was induced to join the new project and worked planning sessions mostly at JPL throughout the first half of 1966. Rowan was helpful in organizing the data analysis team to put together the incoming photo strips into large photo sheets. Then, after a successful launch and insertion into lunar orbit, the high resolution camera was not working. It was still in a parking orbit far above the height needed to satisfy the Apollo site-resolution needed. McCauley, backed by Rowan, recommended keeping the spacecraft in its high orbit from which large swaths of 20 to 30 meter resolution pictures could be taken. These, in turn, could be far more effective in finding suitable Apollo landing sites than the nested Ranger pictures or the spot checks of the Surveyors. They also would greatly enhance the quality of our ongoing geologic maps. There was agreement among McCauley, Rowan and Bellcom reps that this was the most reasonable task for the wounded spacecraft. Upon their return to the Space Flight Operations Facility (SFOF) at JPL, they found the spacecraft down at 58 km based on the forlorn hope that the maneuver could jar the high resolution camera into action. It did not work, and most all the pictures returned were useless.”

After that disappointment, McCauley lost his trust in the NASA engineers and resigned from the Lunar Orbiter team. He went back to terrain analysis and the near-side mapping program, and to the effort to utilize the nested Ranger pictures--which turned out to be a useful effort that enhanced our lunar knowledge.

McCauley continues:

“I burrowed into a special facility on the fourth floor of the Arizona Bank Building with Dick Eggleton. As its focus, this facility had a large glass-topped table beneath which was an excellent high-resolution photograph of the near-side of the Moon about 5 feet across. We had there several stratigraphic conferences and many casual meetings on lunar matters.

By early 1968 almost the whole focus of the Flagstaff Center of Astrogeology was on the upcoming Apollo landing. Training, simulations, and practice with space suits at crater fields that were full-scale models of parts of the Moon. Lunar Orbiters 4 and 5 [May and August, 1967] provided a wealth of high to medium resolution data that could not be properly utilized for lack of lunar mappers. Early in the year, Masursky became Branch Chief of Astrogeologic Studies and moved to Flagstaff to be closer to the Apollo action, and the cameras—as called upon. The Astro group was severely trimmed as Rowan, Watson, Pohn, and Offield were transferred to Denver to form a Remote Sensing Unit. Pressures were building up from Houston to complete the 1:5,000,000 Near-Side of the Moon map. My isolation and the need to work more closely with Wilhelms, led to a request for a transfer to Menlo Park where most of the lunar science was then centered.

Sometime during the summer [1968] and after the Mariners' successes, we were notified that our proposal for Mariner 8 and 9 had been approved. A Branch proposal from November 1965 entitled "*An Investigation of the Geology of Mars by Means of a Two-camera Television System for the 1971 Voyager*" by J.F. McCauley, Harold Masursky, W.T. Borgeson, and E.C. Phillippi had been resubmitted with Masursky as the Principal Investigator (PI) sometime earlier without my knowledge while Hal was still in Menlo Park. Naturally I was upset, but assured by Hal that he would seek the leadership of the whole group including the biologists, astronomers, atmosphere specialists and those looking for signs of life. I was to head up the geology group where my lunar orbiter work would be best utilized. I was mollified and enthused. Additional people were added from Caltech, Stanford, and the University of Arizona. The USGS proposal was patterned after the Lunar Orbiter, but with vidicon cameras instead of bulky photographic systems."

[Aside from Jack McCauley: "Our first overture to NASA for participation in the Mars exploration program had been an informal effort dated August 1965. This was followed in November 1965 by a more elaborate two camera orbital mission (see above) taking into account the changes in NASA's planning, and the Branches considerable involvement in the planning stages of the Lunar Program, as well as the prior successes of the vidicon cameras of the Ranger Missions."]

McCauley continues:

"In September 1968, I moved to the Menlo Park office to work more closely with Wilhelms [to finish the 1:5 million-scale Geology Map of the Near-side of the Moon] and to have easier access to JPL where almost weekly planning meetings were to be held. The move to Menlo was with mixed emotions. The family had begun to put down roots and we were comfortably established in the old Wilson house [in Flagstaff] that we moved into after our son Bill was born. On the other hand, the pace of the work in support for the first Apollo landing was picking up and practical applications such as mission simulations in the field and a mock up SFOF were paramount. The lunar mapping being done was almost exclusively at large scales for candidate landing sites. The 1:5,000,000 scale map of the near side of the Moon, split between Don Wilhelms and me, was lagging and badly needed if only in preliminary form. Working together and using the Lunar Orbiter IV panels assembled in the Menlo Office were also a drawing card. I decided to live in San Francisco to give the children a taste of urban living. I was followed to the City several months later by Don Wilhelms. We commuted to the USGS via a private bus used mostly by the Stanford Research Institute (SRI) next door to the Survey [in Menlo Park.]

After transferring to the Menlo Park and getting settled I divided my time mostly to finishing up the Near Side of the Moon synoptic map, or compilation, as we called it, and doing mission planning for the up coming Mariner orbital missions to Mars."

[Aside from McCauley: "My own interest in Mars began as a seven year old youngster vacationing in Ocean City, NJ during the close approach (conjunction) of 1939. The bright red orb hanging over the ocean was an attraction to the point that some telescopes were set up on the boardwalk and I was lucky enough to get a brief peak at this planet whose surface markings had been studied and speculated about for centuries. I think I saw some of these and the polar cap, or at least I imagined so. The then popular Buck Rogers comics and the famous "War of the Worlds" radio program by Orson Welles the year before were the key stimulants that would produce a strong interest in Mars. This broadcast of an imaginary invasion of the U.S. by technologically advanced



Martians by the British author H.G. Wells was so realistically done that it frightened many in my hometown of Millburn, and all over northern New Jersey. People were frantically searching maps for the fictitious town of Glovers Mill, the landing site from which the Martians were devastating the puny Earthlings. I don't remember the original broadcast but later accounts and replays well into my teens had their lasting effects.”]

McCauley: “It was the successful experience Astrogeologic Studies people had with the five Lunar Orbiters that assured their role in the Mars program. This was a key turning point for the Branch because the follow-on Apollo missions were to be cancelled, leaving an excess of people for the USGS to cover. Being in the Mariner program brought funds-not only for mission planning and operations, but also for desert studies to help understand the pictures of Mars yet to come. Fears of closing the Flagstaff Center ebbed away.

My main incentive for assembling this Mars orbiter effort was our experience with the Lunar Orbiter and the deceptive results of the Mariner 4 that revealed a moon-like surface--but it covered only of a tiny part of the planet. What about the polar caps and the wave of darkening that moved southward each Martian Spring to say nothing of the non-lunar appearing markings and the famous canals? Having spent some time on Percival Lowell’s beautifully crafted 24 inch refractor, this flyby glimpse would not do. To see Mars properly and to do justice to this superficially-active sister planet we had to have at least an orbiter or better two. With the success of all five Lunar Orbiters and the experience gained by the mission operations teams made up in great part by people from the USGS Unmanned Lunar Exploration Group, orbital missions began being planned.

The JPL plan for a two camera, two spacecraft mission to Mars was approved in 1968 and my original plan was refurbished a bit and submitted to NASA without my prior knowledge by Hal Masursky who was then heading up the Astrogeologic Studies Program while still operating from the Menlo Park office. It was approved along with other proposals from Cal Tech and the Lunar and Planetary Lab at the University of Arizona. A television science team was formed and divided into six different discipline groups: 1) Atmospheric Phenomena, 2) Geodesy/Cartography, 3) Geology, 4) Physics of Polar Phenomena, 5) Satellite Astronomy and 6) Variable Surface Features. Earlier, I had tried my hand at making experimental geologic maps from the Mariner 6 and 7 which were put into one of our interagency reports, and felt that Mars was variegated enough to make maps of the sort we had done for the Moon using telescopic and lunar orbiter pictures.

I wound up as the head of the Mariner 8 and 9 Geology Group with Masursky as the Chief of the Television Experiment and member of all of the discipline groups that suited his political instincts. The Geology Group consisted of a powerful group of imposing individuals, all of whom had impressive lunar and planetary credentials. Although I had considerable managerial experience, I was nervous about keeping such a group focused on what was our prime mission, and that was to make a complete map of another planet that I knew--as well as they--was far more complicated than the Moon. The USGS bunch felt that they had a good handle on that for comparative purposes. The roster of the Geology Group on the Team was as follows: Mike Carr (USGS), Bill Hartmann (Lunar and Planetary Lab, Univ. of Arizona, Tucson), Don Wilhelms (USGS), Dan Milton (USGS), Bob Sharp (Caltech), Bruce Murray (Caltech), Larry Soderblom (from Caltech on his way the USGS), Jim Cutts (JPL), and Carl Sagan (Cornell University).

By what Martian optimists would call bad luck, the two spacecraft took pictures of more lunar like terrain. But there were subtle hints of things more interesting. The Martian craters seemed more worn down than those on the Moon as would be expected on a planet with an atmosphere, however thin. Extensive plains areas were seen along with patches of jumbled, blocky ground and given the name Chaotic Terrain.”

The McCauley’s spent 1968-1970 in Menlo Park; then, I was then asked by Gene Shoemaker to return to Flagstaff as Branch Chief. During his Menlo assignment, my focus was on the geology of the near-side of the Moon using the sometimes disparate data from the forty-four 1:1,000,000-scale quads. It required re-mapping, not just compilation; so it became a new piece of work that has since seen very wide distribution as one of the most popular USGS products—having gone through several printings.

The preflight work for the Mariner Program took at least one trip a week to JPL that was easy from San Francisco. During a good part of this commuting I would be met at the ramp of the incoming plane by a JPL helicopter and dropped off on the roof of Building 9, inside the JPL compound. I had then only to walk down several flights of stairs to the meeting rooms. In the afternoon, I generally went back on commercial flights because of the unpredictable length of the meetings.

Another fortunate event took place late in 1970 when, as told by Gordon Swann, he was talking to Hal James in his office about being a substitute for a NASA astronaut to give a major address on the Apollo Program to the first Latin American Geological Congress in Lima Peru. It was only a few days from Thanksgiving and Gordon was not inclined to go. I was walking in the hall when Gordon spotted me and said there’s your man! I accepted James’ offer and to compensate he told me to hook up with George Erickson who was working on the 1970 results of the disastrous earthquake in the high Andes of Peru. I gave my lecture that was simultaneously translated into Spanish, and then took off for Huaraz and Yungay, high Andean towns hard hit by the quake. Coming back to Lima along the coast I realized that I was looking at hyperarid terrain sculptured mostly by the wind. This area should be studied with a view toward Mars terrain—the Southwest U.S. being only semi-arid and not suitable for useful analog data. Upon my return, I asked Steve Dwornik if the NASA Planetology Office whether he could fund a pre-flight Mariner support expedition that would be useful in the analysis of the Mars data. He agreed, and George Erickson rounded up a group of his friends in Peru, and we were shortly on our way.

We traveled through almost all of coastal Peru in a period of about six weeks by 4X4 vehicles and a light airplane taking hundreds of pictures before Mariner arrived at Mars in November 1971. This work established a group in Astrogeology as being leaders in desert work that would carry on through other regions—Iran, Australia, North Africa, up through the mid 1990s.

Mariner 9 was inserted into orbit and began playback to earth on November 13, 1971, and the orbit was trimmed slightly on the 16<sup>th</sup>. Our worst fears were soon realized as we had arrived in the midst of a great planet-wide dust storm. The preplanned Fixed Feature (geology) mapping program had to be shelved and a search for holes in the dust and high places above the pall. This was a time of considerable anguish and frustration. There was an interlude when the targeting computers went out and Bill Hartmann and I were reduced to using slide rules to aim at places that looked like they were high and clearing. This may have been one of the last utilizations of analog

computers in the space program. In mid December, the summits of the spots we saw earlier proved to be the craters (calderas) atop high volcanoes.

By January 1972, the storm was clearly on the wane—and a second orbital trip took place to maximize the overall science return. The Menlo Co-Is came in turn to JPL for first looks and to set up a rotation program so that at least one geologist would be at the Lab during the entire mission. Their objective was to evaluate the incoming data, alert the engineers if anomalies occurred, or some spectacularly noteworthy was spotted. Included, were Don Wilhelms, Mike Carr and Danny Milton. Masursky was in and out as he floated from team to team and took care of most of the press conferences.

As the mapping strips were put together, we went from anguish to exhilaration; the air was clearing all over by early January, 1972. There was, however, a fear that all the early mission maneuvering might cause the gas to run out before we could finish mapping. Another scare came along about this time (Jan. 11) when the filter wheel stuck. I remember running with Brad Smith to the SFOF to see what the damage was. By luck, the wheel stuck in the clear window. This loss did not affect the geology mission greatly, but filter-wheel color or polarization studies could not be done.

The overall television experiment included a number of colorful and prestigious people. Included, were Joshua Lederberg, a Nobel Prize winner. Carl Sagan, a great science author and sometimes nuisance who too frequently wanted to point the spacecraft away from nadir to look for this or that anomaly. He didn't realize what damage the resulting holes or "gores" would be in the overall coverage and the proposed topographic maps to be prepared after the mission. Fortunately, he was retold time and again. Other team members who made a special impression were Bruce Murray of Caltech (later to be Director of JPL) and Jim Cutts, the nemesis of Sagan. I remember one of his jabs at Carl's proposed erosional rate for the Martian surface. Given Carl's rate, Mars would have to have been as big as Jupiter at the birth of the Solar System.

Highlights of the mission were the great volcanoes, the 3,000-m long canyon (Valles Marineris), the numerous channels and gullies, and the layered material in the floor of the canyon that looks like layered sediments. The data merited intensive study, including both topographic and geologic, of the type made for the Moon. A follow-up mission was planned even before the end of its useful life of 515 days (27 October 1972). The Viking follow-up [summer of 1976] would land on Mars and conduct numerous experiments, many of which would search for signs of life. Thus, the longevity of the Flagstaff Center of Astrogeology had been assured even though the massive Apollo program was phased out—the Center would remain although smaller up through the Millennium and beyond.

I did not wish to continue with Viking but rather get back to field geology, particularly in the deserts of the world, starting with the Southwest United States and the relative role of wind and running water in shaping landforms. We did go back to Peru in 1973 to provide additional analogs for the Viking people

My work, and that of my colleague and later wife (geologist Carol Breed), led us to about all the major deserts of the world. These trips and expeditions included more than a dozen studies in

Egypt, work in Saudi Arabia, Australia, Morocco, Tunisia, northwest India and China. Numerous papers awards and commendations ensued until our retirements about 30 years after the Mariner 9 mission.”

[Author’s Note: There are numerous quotes from Jack McCauley throughout the main text regarding early Branch history in Flagstaff, as well as the participation of Branch personnel in the Mariner 9 mission to Mars in 1971, among other topics. Jack McCauley and Carol Breed currently reside in Bonita Springs, Florida. They also maintain a house in Flagstaff, Arizona.]

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**John M’Gonigle** (b.1934; geologist; Ph.D., 1965, Penn State University) arrived at Astrogeology in Flagstaff on the Fourth of July 1965. He grew up in Binghamton, New York, took his freshman year at the University of Rochester, and then went into the Army for a couple of years. John then went to the University of New Mexico where he received both the Bachelor’s (1958) and Master’s Degrees (1960) in geology. He worked for the Kansas Highway Department for about a year and a half in Manhattan, Kansas, and then went back to graduate school at Penn State and received his Doctorate in 1965. John joined the Branch of Astrogeology in 1965.

The following was taken from an interview with John M’Gonigle by Gerald G. Schaber on 10 April 2001).

"Before I graduated from Penn State, I was interviewed by Harrison “Jack” Schmitt who was there interviewing several people. So I applied for the USGS and came to Flagstaff. I was supposed to work with Jack Schmitt, but at that time he had taken on working as a NASA astronaut. So, I inherited this project of his on planning early Apollo manned missions. Don Elston was the Acting Branch Chief when I arrived in Flagstaff. Jack had some things going, and we did some various tests to see what astronauts should be looking for and describing. Shoemaker had many of the ideas of the type of equipment that should be used [on the Moon]; a number of us worked with the machine shop people deciding what prototype mockups [of the lunar tools] to build. However, about that time, I think they decided that we should help with the work geologically mapping the Hopi Buttes. So we went out there that summer [1965] to develop these test sites and lunar surface procedures for astronaut training.

I remember once being on an airplane with Gene Shoemaker when he was writing a paper or something. In between airplanes when he'd get to an airport, he'd run into a phone booth and phone what he had written back to his secretary Jody Swann. There was this family and their kids behind Gene and me on one of the plane we had to take on that trip. I kind of felt them under the seat and stuff, and I heard oh Mommy Frank threw up. Shoemaker looks around a little bit and kept on writing you know. Then, oh no, he did it again. Gene just kind of looked irritated. Wow, three times! By then, I'm a wreck. Shoemaker was oblivious to it. He just kept writing away.

I left the Branch of Astrogeology in 1970 and went to Puerto Rico, and I was there until 1973. Then I got drafted to go up to the Publications Unit in Washington D.C. Then I went to Denver--the Coal Branch-- during the so-called “midnight massacre” [massive lay off] there-back when they put a lot of people into the Energy Projects--back during the energy crisis (1973-74 or so).

After I left the Publications Unit I joined Eastern Environmental--so I was still there in Reston (Virginia). Then, they put me back in the Coal Branch and I went out to Denver in 1977. I was there the rest of the time doing coal mapping (along with Dick Eggleton who was also earlier at Astrogeology). I subsequently was acting Chief for the Coal Branch in Denver. There were about eighty people there at that time. I was mapping in Wyoming and various other states. I retired from the Survey in 1995.”

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**Harold “Hal” Masursky** (1923-1990; geologist, B.S. in 1943 and M.S. in 1951 from Yale University) arrived in September 1962 to work for the Branch of Astrogeology in Menlo Park. Hal had attended Yale University on a full scholarship and earned his B.S. degree in Geology and Civil Engineering in 1943. After two years with the Army Engineer Corp., where he reached the rank of first lieutenant, Hal returned to Yale and earned his M.S. degree in geology in 1951. He also worked toward his Ph.D. in geology at Yale, but never received that degree. He joined the USGS in 1947. Hal arrived in Menlo Park, California in 1962 to begin work with Gene Shoemaker’s new Astrogeologic Studies Unit. Hal would then later move to Flagstaff, Arizona in March 1968 and lived there for the Branch of Astrogeology until his death on 24 August 1990.

[Author’s Note: Much of the commentary below was taken directly or paraphrased from a personal biography of Hal that appeared in the program for the Memorial Service held in his honor on 30 August 1990 at the Elk's Lodge in Flagstaff, Arizona:]

Hal Masursky was born in Fort Wayne, Indiana on 23 December 1923. He was the youngest child and only son of Celia (Ochstein) and Louis Masursky, Polish-Russian immigrants from Poland. At Central High School in Fort Wayne, Hal edited the student newspaper and played intramural basketball. He earned a full scholarship to Yale University, where he was captain of the fencing team and editor of a scientific magazine. He received Yale’s B.S. degree in Geology and Civil Engineering in 1943. After two years with the Army Engineer Corps, where he reached the rank of first Lieutenant, he returned to Yale and earned his M.S. degree in Geology in 1951. Hal joined the USGS in 1947.

During Hal Masursky’s scientific career, he successfully demonstrated an imaginative approach to difficult problems that was the hallmark of his scientific style.

In the 1960's, Hal played a major role in the choice of Apollo landing sites. In the 1970's he headed the scientific team that first mapped the planet Mars (Mariner 9), and he was actively involved in selection of the Viking landing sites on Mars in the summer of 1976. Through the 1980's, Hal was a key figure in the Voyager Project, and champion of geologic exploration of Jupiter's Moons. He was also a major participant during the historic encounters of Voyagers I and II with Jupiter, Saturn, Neptune and Uranus, and their many moons. Another of Hal's significant contributions was the role he played in helping to gain NASA approval for the Magellan mission to radar-map the surface of Venus. At the time of Hal Masursky's untimely death in the summer of 1990, Magellan was just beginning its systematic orbital mapping operation around that body.

Hal's work resulted in many abstracts, formal publications--and his strong suit—well-received oral presentations. He was an officer or member of many scientific societies and innumerable NASA

committees involved in planetary research. His close friendship and cooperation with Soviet scientists during the waning years of the Cold War resulted in joint efforts that would not have been otherwise possible. Hal Masursky received medals for Exceptional Scientific Achievement from the National Aeronautics and Space Administration in 1972, 1973, 1977, and 1980, and has been honored by the USGS and many other scientific bodies.

The role that Hal Masursky played in admirably representing the Branch of Astrogeology on a large number of important NASA committees, meetings, and mission teams, has been credited by virtually all of his scientific colleagues as an important reason for the success of the Branch, and its world-wide recognition. Hal's constant travel to participate in these NASA meeting and mission team activities, or give one of his many hundreds of talks, was made even more remarkable given that Hal suffered from diabetes, a disease that for him became progressively more debilitating over the years. Nearly all of Hal's many planetary science colleagues around the world knew of his problem with diabetes, and knew what to do whenever they saw him slumped over in a chair at an airport somewhere or starting to behave in a strange manner at a meeting--suggesting that Hal needed sugar or insulin.

Hal Masursky was a remarkable, kind, and soft-spoken person. He was exceptionally dedicated to his work at the Branch of Astrogeology. Hal was liked by everyone who met him; and he always gave a presentation that kept everyone's attention--perhaps even equal to the master, Gene Shoemaker himself. NASA loved him and admired him. As a result, Hal was one of their main enthusiasts, spokesmen and scientific advisers for virtually all of the manned and unmanned missions NASA wanted to pursue, including the Magellan Radar-Mapper mission to Venus that Hal fought especially hard to see flown. [Magellan's systematic radar-mapping of the surface of the cloud-covered Venus had just begun at the time of Hal's death in August 1990].

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**Rutledge "Putty" Mills** (b. 1923; mechanical engineer/pilot), who along with associates Bill Tinnin and Dick Wisner would design and hand-build the Branch of Astrogeology's two widely-heralded simulated "lunar vehicles"(Explorer and Grover), for geologic training of NASA's Apollo, 15, 16 and 17 astronauts arrived in Flagstaff in the spring of 1965.

The following was taken from an interview with Rutledge "Putty" Mills by Gerald G. Schaber on 22 January 2001:

Mills: "I was born in Germantown, a suburb of Philadelphia, Pennsylvania on 25 November 1923. We lived there until March of 1930. At that time the family moved out to Santa Barbara, California. I had an interest in mechanical things starting at about age six. I built my first little sort of a go-cart at six years of age. Actually, it was powered by a wind-up motor from an old 78 RPM Victrola that were popular in the 1920s. So I got some help from a neighbor across the street--an electrician--because I was using the motor, and put a fan on it to drive it. It was going to be an air-driven little cart. He helped me out because I needed something with a better 1/4-inch shafting to connect it with the fan blade. So the first thing that happened is that--being a Victrola motor--it had a centrifugal governor that kept it at 78 RPM. So, again, the electrician tells me that there's a governor in that and you're going to take that out and get rid of the flyweight. So, I did,

and then it would really spin up. Actually, it wouldn't carry me sitting on it; it would run by itself down the sidewalk.

Then from that on, I built one with a gasoline-powered, old, 1906 Holt motorcycle engine. So, my interest in the field [mechanical engineering] started very early. Also at about six years of age, I got interested in airplanes. So I was interested in nearly everything in school. I took machine shop, radio, and printing; just about everything because I had a really diversified interest. My early electronics started in junior- high and high school. I continuously followed machine shop, drafting and mechanical drawing, and all of that. Then from high school, I got hired by a local shop that did modified engine work on motorcycles and automobiles. They built these retro engine outfits that you know--converted flat-head engines into overhead valves for racing engines. So I learned quite a bit in that because we did our own head castings and machined the parts. I worked for Herman Kline.

After that, he opened a machine shop in North Hollywood when the war was on in 1941-1942. We were building small parts for the different aircraft factories--North American, Douglas and a little bit of work for Boeing. Herman did a lot of outside work so I was the machine shop manager that was producing these aircraft parts. We did motorcycle work in that shop on the side, and sold used motorcycles and serviced them. Some of the customers I had were Roy Rogers, particularly, and Andy Devine, and Clark Gable. Andy and Clark hung around together. It was a quite time for Clark Gable, because he had just lost Carol Lombard in that aircraft accident when she was on a U.S. Bond tour. Oh, then there was John Carradine who also came into the motorcycle shop.

Then, I went into the service, and of course applied for pilot duties. I qualified for the Cadet program. I had just been assigned to Kent State in Ohio when they had a major cutback for aircrews--pilots, bombardiers and the rest. So I went back into a ground unit--an artillery outfit--and we ended up with General Patton's Third Army in Europe. We had previously been trained for the assault on Japan, so we had a lot of amphibious landing training. Thus, we were the first boat load home after VE day in Europe. We got a month off and we were shipped out for Japan. Of course, on the way over there, they dropped the atomic bomb and that ended that. I stayed over there for about another nine months.

During the off time, in order to keep busy, I built small aircraft and I built a powerboat using an aircraft belly tank. I also built a midget auto. This was in Manila in the Philippines. So I kept busy doing things like that. I was an aircraft mechanic. That's what I ended up doing in the military occupation service (MOS). But I was an aircraft mechanic on the light planes. That is what I did for the Third Army, both in Europe and in the Philippines. After discharge in 1946, I came back and went to work for a Jeep agency. They were also building equipment to go with jeeps. They were into plowing, orchard support, drilling holes for planting trees, and all of that. So I got I to some designing and building--you know, fabricating--pressure systems for water tanks, drilling devices for drilling holes for planting trees from jeeps.

Then, the owner wanted to sell out and I had an offer to go to work for American Motors as a rep on the road. Then he offered to sell me the agency. So the Sales Manager and I (I was running the shop) went together and bought the agency. So, we were Jeep dealers up until about the time I got involved with General Motors. The dealership was at 318 State Street in Santa Barbara.

Then, I got involved in a glider club that started up in Santa Barbara. Most of the people in that club were engineers from the space group--missile guidance [at GM]. Some of them were from the German group that came over here after the war. So, we ended up with a lot of rocket engineer types. So I kind of hung out with that bunch for quite a while. And this led to my friends out at General Motors.”

[Author’s Note: The following block of text from Putty Mills describing the Mobile Geologic Laboratory (MOLAB) has been modified for accuracy.]

Mills: “About that time NASA Headquarters and the Marshall Space Flight Center (MSFC) proposed this Mobile Geologic Laboratory (MOLAB) concept for advanced lunar missions, and the Branch of Astrogeology proposed to work with it. NASA funded the study, and General Motors (GM) was to build it. NASA asked GM to do a selection for who eventually would be Chief of the field test systems--where I ended up. So they had a list of candidates and were down to six finalists. I was number two on the list. Mike O'Reilly was a really talented fabricator and designer/engineer who worked for GM--and actually did a lot of work on the MOLAB. He was asked to take it, and both he and his wife didn't want to move to Flagstaff--so they turned it down. So, me being second on the list, I was in a position where I could move to Flagstaff. I was single then.

I turned the job down at first, and wasn't quite sure. Then Don and Shirley Elston, and Gene and Carolyn Shoemaker flew out here [to Santa Barbara, California] in the Comanche aircraft [the Branch of Astrogeology had just leased]. They talked to me. They actually came out twice I believe. And finally they said, "Hey, you'll get to fly this new Comanche (250 model). So I said, hey you've got me! So, I signed up and went with it. That's how I got involved with the Branch of Astrogeology in Flagstaff.” (See Fig.23 in main text).”

*Schaber: "It was a good thing you did!"*

Mills: "Yeah, oh, it turned out to be so rewarding. You know I go back-- because my earliest adventures [as a kid] were Admiral Byrd and Lindbergh, and all that bunch, so here I got to join the program, you don't even have to explain that now. It was a tremendous experience that you and I--and all the rest--got to enjoy.”

*Schaber: "So, when you got to Flagstaff, you looked after the MOLAB, and what else?"*

Mills: "Okay, so the first thing that happened was the first field test with the MGL [MOLAB] with Bob Reagan. We went out to Amboy Crater in California. He had just gotten hold of a Rubidium vapor magnetometer. First of all we had to have it isolated from any ferrous material in the vehicle, so we made an aluminum boom. This would have been in 1965.”



*Schaber: "So that was a test at Amboy?"*

Mills: "yes, we went out there around that Amboy lava field--and also we did a demonstration for a local school--a tiny grade school there near to Amboy. So Bob and I put on a little demo of the vehicle and told them what were up to and what we hoped it might all lead up to."

*Schaber: "Describe the inside of the MOLAB for us; I remember it had a little kitchen and a bathroom!"*

Mills: "Yes, it had a bathroom and a kitchen facility. It had a step-down with a center unit. It was made up of three units--it was an articulated vehicle like some of those front loaders working on a highway. Both the front and rear would swing. And you had stand-up room in the center section. It had a little over six foot of clearance. Oh, also out there we did some radioactivity work with Carl Roach."

*Schaber: "So you had a truck big enough to transport the MOLAB?"*

Mills: "Yeah, we had a flatbed--a large trailer--originally designed for retrieving tanks during WWII. I picked this thing up in the Navy Station at Coronado Island down in San Diego. So, that's what we used to haul it around."

*Schaber: "Gosh--We did more things in those days--it's amazing!" So after that test, I know it [MOLAB] was used out in Hopi Buttes one or two times."*

Mills: "Yeah, it had a couple excursions out in the Navajo Reservations at the Hopi Buttes."

*Schaber: "What about Meteor crater?"*

Mills: "I think we had it out there once."

*Schaber; "Then, all of a sudden it seems like we started using that full-size plywood LM ascent stage simulator that NASA [Marshall Spaceflight Center] built for us. And I did some of the analytical work in there."*

Mills: "I remember you using that portable thin section machine that came out of Grand Junction, Colorado."

*Schaber: "Yes, and we used the miniature Surveyor X-ray diffractometer built by Phillips Electronics in that plywood LM once or twice during field exercises in Hopi Buttes."*

Mills: "Going back to the plywood LM. When I arrived there someone had already--maybe it was Gordon Swann --had a face plate with the two windows in it. That's all that existed in 1965. Then, from there on we built up the complete unit to enclose it [the entire plywood LM simulator]. In order to have it realistic we had to have it parked on a built-up mound of dirt because the eye level on the actual the lunar lander was about 16 feet. That thing then became too bulky and once we started doing tests out of state, and all that, I talked to Al Chidester and I said why don't we

construct a light, collapsible one that we could just throw in the back of the truck? So we came up with the canvas ones. They were just made out of 1-inch square lightweight tubing with a [canvas] cover and a zipper. And that was designed for three people--you know, like two astronauts and a geologic instructor or observer during field exercises.”

*Schaber: "About the time you arrived in Flagstaff, the Branch had purchased a commercial, multi-wheeled dune-buggy-type vehicle--called Trespasser--to be used as a Lunar Roving vehicle. Tell me about that."*

Mills: "Yeah, Don Elston had ordered one of the little four-wheeled vehicle cars with a Briggs and Stratton engine. They had a devil of a time with it. This was while I was still out at Santa Barbara at GM. It would hardly get across the sand. So then we knew about this other one called the Trespasser--and I talked to Don Elston about that. This thing had ten very low-pressure tires, and it had a Corvair, six-cylinder, engine--so it had more than enough power (see Fig. 40a).”

*Schaber: "I recall that this was about 1965."*

Mills: "Yeah, late 1965. Don and I agreed on that; so with Jim Lovelace's help, we sent out a purchase order and were able to pick up the Trespasser. Of course, it was good for sand, but if you remember we had nothing but trouble with it. It just punctured tires because it had no suspension, so it'd end up having most of its load on a single tire [going over rugged terrain]—a tire that only had three pounds of pressure it—so, the rims would rim-cut the tires.”

I'd say we used the Trespasser in the field for no more than maybe three field tests. We had some of the astronauts out driving it. Joe Engle drove it early on; as did one of the eventual Skylab astronauts, Paul Weitz. That was maybe the third event with it out there--and then we put it to rest.”

*Schaber: "So, from the time you got here, you were--you were managing the MGL and then helping out with some of the construction of these things, and then it came up for the need for our first hand built, simulated, Lunar Roving Vehicle to be used for astronaut training called "Explorer"--that we built at Flagstaff."*

Mills: "I kind of worked with Maurice Brock on that. Bendix Aviation had proposed a separate vehicle that would go on an unmanned rocket, land on that lunar surface and would be deployed--and it would be radio controlled--and if everything checked out--then two Apollo crew would land as close as possible to it. If they were within walking distance--they would walk over to it. If it were a little beyond walking with the portable life support system, then they would command it by radio and bring it over to where they were. [Author's Note: The design plans for the eventual Boeing Lunar Roving Vehicle, or LRV, had not been determined at that time; see "Grover" construction in 1970 in main text].

So, we built Explorer using just steel tubing (see Fig. 50a-c). It was kind of following race car construction that I was familiar with. We needed a power plant, but Jim Lovelace said we could get a Jeep pickup without the body, and just take what we wanted off of it. So, Jim order that material and we ended up with all of the power plant, transmission, and differential--all from this

basic government Jeep truck. We built that up—actually; I had that one radio controlled. But I used a control from an old model airplane set that I had. It was a ten-channel peanut tube type with a reed frequency control.

We had some trouble with remote control for Explorer however; everyone was into this Citizen-band radio at that time, and of course we were on the same frequency. Everybody was chattering away on their CB radios and it worked the reeds and started sending signals into the vehicle. So, I had to build a one-second delay system into it which eliminated all the people on the Citizen's band. We could start it up, drive it--and we had a television camera on it--one that I borrowed from Johnny Nuttall [Head of Astrogeology's Electronics Unit]. Thus, we could look at the television remotely. I guess it was the television that actually controlled the engine controls as I could watch the speedometer and tachometer with that TV camera."

*Schaber: "And you used big old tractor tires as I recall."*

Mills: "Yeah--to come to scale. The Bendix design had 48-inch wheels. I kind of looked around and I thought that the little International Cub tractor has about the right size--and they turned out to be within an inch of that size--so we were able to keep the scale of the Bendix design."

*Schaber: "The Explorer was probably built in May 1967. So, you built the Explorer—then Bendix and Grumman people came out to consult because they were bidding on prototypes for the Lunar Roving Vehicle for NASA-right?"*

Mills: "Yes, some very nice engineers from Grumman came out to take a look at our situation here in flagstaff; they spent a couple of days with us. One of them actually did the fabrication of that Grumman prototype LRV for Grumman."

*Schaber: "We actually tested both the Bendix and Grumman prototype versions of the LRV out in our Cinder Lake Crater Field [29-30, September 1969]."*

Mills: "The tires on the Grumman vehicle looked like big fiberglass funnels (see Fig.67c, d). Then we had the Bendix Rover at the same time in the Crater Field at Flagstaff (see Fig. 67 a, b). Dan Hayden brought that out, and his partner—an Italian fellow. We also brought our Explorer vehicle out there for that demonstration (that was before we built our actual Boeing simulator lunar Rover we called Grover (see 1970 in main text). So we had this big shootout in the Cinder Lake Crater Field."

*Schaber: "In fact, Bendix was very upset when they lost that [NASA contract for the Lunar Rover] to Boeing Aerospace Corporation([Seattle, Washington)—and they didn't even participate in making a prototype design to show NASA."*

Mills: "I set in on a couple of meetings and what NASA really leaned on everyone about was to make straight-forward, uncomplicated vehicles; and the Boeing proposal was very simple and straightforward. And it did very well even though we had a wild man, Commander Dave Scott, driving it on the first mission on which it was used--Apollo 15.

Let's go back on how we got involved in building the Rover in-house. We were supposed to get one from Boeing--they made six vehicles, four for the Moon, a vibration test unit, and then a training one, similar to the one that we were supposed to get.

However, they were having trouble with the terrestrial training version of their LRV. They apparently used some exotic materials for the chassis, and they kept breaking next to welds. And so, they weren't going to deliver it to us, and NASA wanted a dependable LRV for Dave Scott and Jim Irwin to train in; that was about the time that the crew starting serious training--about July 1970."

[**Author's Note:** Some of the following dialogue has also been included in the main text, but is repeated here for continuity; see 1970 in main text:]

Mills: "So Don Beatty [NASA Headquarters] came to me and said Putty, we're in a bind with our Boeing training LRV breaking down--we've got 90 days. Could you put something together for us? So, I said Don I already looked into it, and we were going to end up where we need an electric vehicle. So I said have the astronauts put on their calendar--90 days from now to come to Flagstaff to test-drive their simulator [Rover]. He said do you really mean that--I said Yeah. So he said, meet me Monday down at Huntsville Alabama-- they had a whole Boeing setup down there a lot of people were designing the rover--and he said bring your Polaroid camera. So, I met Don down there [in Huntsville] the following Monday and he told everyone that we [the USGS] were going to build this thing ourselves. So, I got all the drawings from Boeing--and a lot of pictures. Of course, I had to make a simpler version--there was no way I was going to make a complex vehicle in 90 days."

*Schaber: "So the training version of the Boeing's LRV was having problems, but it seems to me that one was eventually delivered to the Kennedy Space Center; but did they end up using it?"*

Mills: "They used it mostly for PR--Walter Conkrite, etc."

*Schaber: "So, it was mostly for PR, and it had problems all along. NASA gave Boeing over a million bucks for that I believe".*

Mills: "It was actually \$1,250,000. Anyhow, that's how we ended up getting asked to build it."

*Schaber: "Don didn't call you directly did he?" He probably went through the Branch Chief of Astrogeology--right?"*

Mills: "I think it was probably Al Chidester, and then Al just turned everything over to Don Beatty. I also worked with Don Beatty's second in command; I can't recall his name. Then, anytime there was the least change in the actual Lunar Rover design, they would send out the changes to me because they wanted everything to be exact. Any little nut or bolt change in the camera or something on the instrument panel--they would send me the update on it--and we would respond to keep our design up-to-date."

*Schaber "Yes, they were getting our handmade version [called Grover] of the Boeing-designed LRV trainer for a song!"*

Mills: "Yeah, they [NASA] were a little embarrassed about the price. They said how much did this thing cost? And I said something like \$1,900 in parts and materials. They said, oh, we couldn't say that--So they multiplied it by ten and they claimed that we built it for \$19,000 or \$20,000."

*Schaber: "So, tell me how you built it? I understand you used some kind of old B-29 flap motors to power the wheels."*

Mills: "So it [Grover] had a motor in each wheel which included a gear reduction in that same motor case. So, the actual lunar rover on the Moon had that 80-to-1 reduction. I was hoping to find about a 60-to-1. I had to settle for a B-26 landing gear motor that had a reduction of 43-to-1. That little motor had about 6 ft/lbs of torque, and you multiple that by that 43-to-1 and you come up with 250 some pounds that were adequate for the vehicle."

*Schaber: "How on Earth did you find a B-26 landing gear motor?"*

Mills: "A Baltimore-built B-26. I got those out of surplus in of Los Angeles for \$12.50 apiece. So, everything was cheap on that. Then, it was a torsion bar suspension. I know I can find some already built up because you know we can't do a custom build--do all of this in 90 days. So I searched around the wrecking yards in Phoenix-- a German [something] had torsion bars, and then a Morris Minor from the 60s also had torsion bars--and I settled on those and it worked out great. So, there again, those things were really cheap. The rest of it was built out of stock--you know--metal racks we had in the shop there at Flagstaff."

*Schaber: "We had a great machine shop at Astrogeology in Flagstaff; I suppose that they helped a lot with building the Grover."*

Mills: "Okay, some of the parts are made by Walt Fahey and Robert Blecha--Blecha was just about ready to retire at that time. So, we shared that warehouse with Byrd--the sheet metal company on the lower side of the railroad tracks [at 1980 Huntington Road in Flagstaff]. That is actually where we built the Explorer. We rented half the building from Byrd."

Schaber: "So, what kind of navigation system did the Grover have on it?"

Mills: "Okay, it had a regular speedometer that was wheel driven; it cost \$12.00. And we had the Branch Surveyors Jim Crossan and Bud Dahl chain out and mark a surveyor mark--we put into concrete. So, we had a very accurate 1/4 mile course that was listed both in miles and kilometers. And then, to calibrate it, we'd run back and forward on that measured course; in that way we get the exact mileage down.

Oh, a funny thing that happened when we had the Apollo 16 crew--John Young and Charley Duke--over checking out the Grover (see below). We were talking about the navigation equipment on the actual Boeing Lunar Rover—that fact that it costs 3 1/2 million dollars. What it did is--it was an

electronic device--and it took the average speed of the three closest wheel speeds--to consider that one wheel was up in the air and was spinning--wasn't driving. That kind of system costs a lot of money. John Young was joking with us because the system we had on the Grover was very accurate and only cost \$12.00.

Also, we had a gyro on the panel. I had a friend Rob Williamson at Santa Barbara; I told him what I needed and he came up with an electric-driven gyro out of one of the Frontier airline airplanes. So we modified that to meet our requirements on the Grover. That was supplied by Williamson Aircraft. I recall that we might have had maybe \$800 bucks in it.”

*Schaber: So, you get the Grover completed in the fall of 1970 and we—as you promised Don Beattie three months earlier—invite the astronauts to check it out at your shop on [1720] East Street in East Flagstaff; is that right?”*

Mills: “Yes, for that first test drive [September 1970] we had astronauts John Young, Charley Duke, Tony England, Fred Haise, and Gerald Carr, and we had geologist Bill Phinney (MSC/Houston) and some of our own people. This test was in front of our shop on East Street. We test drove it but we didn't have the S-band antenna, cameras, or any of that on it--just the basic vehicle. All that other stuff was added later (See Fig. 78).”

*Schaber: "So John Young, Charley Duke, Fred Haise and Gerald Carr got to drive the Grover at that time. What was there general impression of it?"*

Mills: "Very good. They were real happy with it and were looking forward to using it.”

*Schaber; "Yeah, It was pretty amazing to build it in that short of time--90 days.*

Mills: "This meant using a lot of off the shelf items because you couldn't have started from scratch in that time.”

*Schaber: "Then you probably modified it from then on over the next few years.”*

Mills: "One interesting thing did happen. Right after Dave Scott and Irwin came back from the Moon on Apollo 15 [July 1971] they came down to our vehicle shop in Flag and said hey, the first thing we noticed is when we got on this thing in 1/6 gravity on the Moon, is that we set up much higher than on the training vehicle; the view from the Rover while you were sitting in it was totally different because we were sitting up like a foot higher. So then Dave Scott suggested that I make some thicker seats for it to more-or less simulate the lunar surface. So, we made that a lot thicker--the seats--to raise the astronauts in the seated position.”

*Schaber; "But I know the astronauts really loved that thing. They especially loved using Grover out in our Cinder Lake Crater Field at Flagstaff."*

Putty: "We ended up doing around 18 to 20 field trips with the Grover vehicle; the first few were just in-house-checking out cameras. Let's see, I think Hal Masursky wanted to look at the Hycon

lunar camera. They were thinking about mounting a Hycon on the real LRV but they ended up doing something else (see Fig. 79).”

*Schaber: "Tell me something though, what is the farthest you ever took the Rover away from Flagstaff?" the Boulder City test, I think you used it out there with the AP 15 crew--the 17 crew."*

Putty: “We had it out at Boulder [City], Nevada and we had it up at Craters-of-the-Moon National Monument; we also took it to Fort, Stocken Texas with Gene Cernan and Jack Schmitt. Then we had it out at Blackhawk landslide near Victorville in California.”

*Schaber: "As far as the Hawaiian trips, we never really had it [the Rover there]. Tell me about that. On one of the Hawaiian astronaut training trips I was on, you modified some Jeeps to drive using an airplane-type stick."*

Mills: “First, we were turned down on the Rover; even Dave Scott said oh, I know the Navy; I'll get a ship--everybody had connections, but none of them came through. Then, we had a Jeep at the Observatory--the Volcano Observatory. So I went down at Babbitt Motors the local Ford dealer [in Flagstaff], and I found a CJ-5 on their lot. They said well, you can borrow that and make some measurements. So, I put electric motors for the steering--took the steering wheel off because you needed that 13 inches of clearance with the [astronaut's] Hasselblad [cameras] hanging off your chest. Then, we had to have the electric steering, so we took the seats out and made room for all of that. So, I tried it out on Babbitt's Jeep and then I carried it with me on the airlines when we carried it over to Hawaii.”

*Schaber: "I remember. I was on that test."*

Mills: "Oh were you. Okay. Then we bolted it onto the Survey's [Volcano Observatory] Jeep at Hilo there and we had a good jeep for the guys too use.

*Schaber: "Pretty amazing, I know they really liked it because they told me so while we were there."*

Mills: "They used it for seven days over there.

*Schaber: "Putty, what do you know about the truth of this story, or rumor, I heard. As we all know, the black and white TV pictures from the surface of the Moon during Apollo 11, 12 and 14 were so horrible. Then all of a sudden on Apollo 15, the TV pictures not only were in color—they were beautiful and clear. Now, I heard a rumor about a guy in Pasadena who called NASA after Apollo 14 and said--look, your pictures are real lousy--and I can make them better... Apparently, this guy came up with an entirely new scheme to process the TV signals or some such, and produce a much higher quality picture from the Moon?"*

Mills: "He enhanced them somehow and it caused us a slight delay, like 1.4 seconds or something. They did go ahead with it but the only penalty was that slight delay.”

*Schaber: "The TV images were had down there at Mission Control in Houston during Apollo 15 were really spectacular!"*

Mills: "Yeah, they had much better pictures. I forget who that guy was."

*Schaber: "But apparently he was some real whiz guy--so it's true then?"*

Mills: "Yeah, so that's what we ended up with--and I think it started with Apollo 15. I think this guy's TV link somehow started with the Goldstone Receiver [at Goldstone, California]. The guy was from California; it may have been Pasadena. I remember there was a delay, and if my memory serves me right--it was about 1.4 seconds."

*Schaber: "what did you and you shop associates do after Apollo 17; the program basically was over, as for as using the Lunar Rovers in training was concerned?"*

Mills: "We had to decide what to do with the vehicles because we're closing the shop down. We had earlier shipped the MGL [MOLAB] to the Marshall Spaceflight Center in Huntsville, Alabama. Billy Tinnin and Dick Wiser took it down there. Then, in 1973, Billy Tinnin and Dick Wiser transported the Grover to the Survey's National Headquarters in Reston, Virginia, to be put on display. We tried to get rid of our other equipment. We took a lot of it out to the Navajo Army Depot [at Belmont west of Flagstaff], and we were trying to clean up. I was in a rush because I was supposed to join the same moving van with my household furnishings as Tim Hait to move to Denver. So there were long nights to 3 O'clock in the morning, and long days trying to clean everything up. My memory was that I got to Denver to start working with Marty Kane about June 1973."

*Schaber: "So you leave Flagstaff and transfer to Denver, then what did you do up there?"*

Mills: "I took Jack Meisky's (sp?) place. He was a geophysicist who was running the airborne group (airborne geophysical Survey group). So, we had sort of a small squadron of airplanes, and Marty Kane was Chief of the Group. We had two Dehaviland Beavers, and we had a big Dehaviland Otter with a large single engine. They were just acquiring a twin engine Beech, model A-88. We were getting rid of the older airplanes and flying Beechcraft for these geophysical surveys with Marty Kane's group. And I was called in--they were having some trouble with the pilots. They were kind of pulling the wool over their eyes. I guess that's the way they put it. One of them was not qualified to fly and had falsified some of his qualifications. So, Marty asked me to come in basically as a troubleshooter. So, that's where we started. So, I ended up spending about 14 years at Denver.

*Schaber, So, the final thing then is you retired from the Survey?"*

Mills: "From the Survey in April of 1986.

*Schaber: "And what have you been doing since then?"*

Mills: "Well if you want to look up something--I'll show you what I've been doing. , Dial up Delphion.com. (on the Internet); that will bring up Patents from the U.S. Patent Office. I have a patent (5782323) on an improved hydraulic brake that eliminates brake fade from overheating.



Originally IBM started this thing, put about \$20 million of IBM's money and time and then it was changed to this Delphion. I have been working some people trying to promote it and sell it.

The rest of my time has been with my group "The Quiet Birdmen." It's an old group. It started after WWI. Well-known People who have been in this group include Jimmy Doolittle and Colonel Lindbergh. I've got three Air Force Generals with me right now in it. We have 144 members at the present time."

[Author's Note: Rutledge "Putty" Mills still resides in Santa Barbara, California.]

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**Daniel J. "Danny" Milton** (b. 1934; Geologist; Ph.D., 1962, Harvard University)

The following is taken from an interview by the author with Danny Milton on 15 February 2001:

Milton: "I was born on July 28, 1934 in Washington, D.C. At that time my father was working for the Geological Survey as a geologist and chemist--so, I sort of grew up with the Survey all along. I actually started studying geology when I went to Harvard--started there in my freshman year in 1950."

*Schaber: "So you grew up in Washington D.C.?"*

Milton: "Yes, in the suburbs. I was only seventeen during the second summer after my sophomore year when I got a job [with the USGS] as a Field Assistant on the Colorado Plateau. Actually, I spent that summer and the following summer as a Field Assistant to a man named Don Johnson, who was collecting minerals in the Monument No. 2 mine in Monument Valley.

But when I first arrived in Grand Junction in June of 1952, Don Johnson hadn't arrived yet, so I was assigned to a guy named Gene Shoemaker who put me to work in a warehouse sieving and splitting mill pulp--uranium mill pulp. It was the most God-awful warehouse; just clouds of dust--you know. I was offered a facemask for the dust, but it was just so hot in there so we didn't bother with the facemask. At that time, the fact, it didn't bother me that I was breathing high-grade uranium ore. That is the way it was at that time."

*Schaber: "Was that in Grand Junction, Colorado?"*

Milton: "Yes, that was in Grand Junction."

As I remember, Gene was going to get them to analyze the mill pulp for europium. He realized that europium is the only rare earth that commonly occurs in two valance states. So, he planned to work out the redox conditions of the uranium in the uranium ore by looking at the valance states of the europium. It didn't work out. It turned out that Gene was out of the range for the europium anomaly; it was much too oxidizing. I think the europium anomaly was first discovered in the lunar samples [Author's Note: brought back by Apollo 11--a strange twist of fate].

So, that's how I started working for Shoemaker."

*Schaber: "So Gene Shoemaker wasn't much older than you were?"*

Milton: "Not much older. I was almost eighteen, and Gene was twenty-three, or something like that."

*Schaber: "So, you went to school Harvard at an early age, and all that?"*

Milton: "Yeah, I was real young."

*Schaber: "I think he [Gene] got out of Caltech when he was nineteen-or something like that--with his Masters degree."*

Milton: "You know, I was very impressed with him on the Colorado Plateau--full of ideas."

*Schaber: "So, did Gene talk to you about the Moon at that time?"*

Milton: "I remember we talked about Meteor Crater in Arizona. He hadn't actually been there at the time; and he was still kind of dubious about the impact origin--but he hadn't actually seen it. I know he was interested in meteorites. He was discussing the journal *Geochimica et Cosmochimica Acta*. And I said, oh, there's too much on meteorites in it. And he said, oh, meteorites are very interesting. I'm sure--but I can't document it--but I am sure that he was very skeptical about the impact origin of Meteor Crater [Author's Note: This was before he actually first visited the crater in the summer of 1952].

I never really worked with Gene [while on the Colorado Plateau], except for those first two or three days [sieving and splitting uranium mill pulp] in the warehouse. I never worked directly for him. Two seasons on the Plateau was a pretty hectic time. Everybody saw everybody else. Particularly, Don Johnson liked to travel around and talk to people in other canyons. So, I really never really had close contact with Shoemaker for many years thereafter--until I got my Ph.D. -- which must have been 1962 from Harvard.

I went to Caltech for a Masters because that's where Gene came from, as well as my structural geology instructor Dallas Peck and another instructor, Larry Nobles. Then, I went back and worked in the northeast, including Greenland. Then, when I got my Ph.D., I was offered a job by the Survey. G. I. Smith wanted me to come to work on the isotope geology of Searles Lake, California--a salt lake with Boron and all kinds of interesting things. G. I. Smith had spent essentially his whole career working on the geochemistry and geology of Searles Lake.

First, I had to map a quadrangle in Kentucky. Every new hire in the USGS at that time had to map a Quadrangle in Kentucky. [Author's Note: The USGS at that time had a large project to map the entire state of Kentucky-geologically].

Then, Gene Shoemaker wanted me to come and work for his newly organized Astrogeologic Studies Unit in Menlo Park, California. He said that he could get me out of going to Kentucky--

and I really wanted to get out of going to Kentucky... So I didn't express an opinion [with regard to going to Isotope Geology or Astrogeology]. So, I just sat back and ended up in Astrogeologic Studies. So, that was it."

*Schaber: "When was it when he called you?"*

Milton: "This must have been about 1962. He had a little group in Menlo. There were people in Denver also I believe--Carl Roach. I was actually assigned to the small astrogeology group in Washington, which consisted of Ed Chao, myself, and Janet Littler.

The only thing I did there [in Washington] was read a paper in a Russian journal [1961] on a new synthetic phase of high-pressure silica by [S.M.] Stihov and [S.V.] Popova. We had a whole stack of X-ray film of unidentified phases that showed up during the hydrofluoric treatment of Meteor Crater material. We'd get all kinds of weird silicon-fluorides etc. I checked the lines. I said that I just wanted to see if the lines the guys report [from the Russian paper] showed up. So, the lines showed up in one of the residues, and it was Stihovite [a high-pressure phase of silica formed during impacts]. That was really the first thing I accomplished.

I really didn't get along with Ed Chao too well, so Gene said why don't you go out to Menlo Park?" So, I went out to Menlo Park.

Gene was there at the time, as well as Henry Moore, Elliott Morris, and Dick Eggleton. Maxine Burgess was the Unit secretary [in Menlo Park]."

*Schaber: "When did Mike Carr show up?"*

Milton; "No, I met Mike Carr later at a GSA meeting. Okay--one of the things that Gene Shoemaker wanted me to do was to measure the Huguenots of rocks. You know, setting up explosives. They were doing that at SRI [Science Research Institute in Menlo Park, very near the USGS Center]. The way they were doing it at SRI, is a very precise and expensive experiment. You have to machine the explosives and the substance until they're perfect shapes. Then blow them up--and they're all gone. Anyway, I went to a GSA meeting --I can't remember where or when it was [Author's Note: Most likely at the GSA Annual Meetings in Cincinnati, fall of 1962]. Anyway, somebody--who's name--I can't remember who it was--introduced me to--somebody who was a "Yaley" I knew--introduced me to an Englishman Michael Carr--who was working at the University of Western Ontario measuring Huguenots of rocks--on the cheap--by a method using pin contacts instead of using rotating mirrors. He was looking for a job. Boy, if you want to measure Huguenots--and I want to measure Huguenots--then I've got a job for you. So, I got in touch with Shoemaker, and he [Mike Carr] came out and started to measure Huguenots until one of his explosives blew up in his hand. Then, we found that Mike could do [many] other things besides that.

So, that's really the biggest contribution I made to Astrogeology was finding Mike Carr."

*Schaber: "So, you're out in Menlo Park, and then somehow you started working on terrestrial impact craters as Gene moved rapidly forward with his lunar mapping program.*

Milton: "Actually, Gene left pretty soon after I arrived in Menlo to accept a temporary duty assignment at NASA Headquarters in Washington, D.C."

*Schaber: "Yes that was in the fall of 1962, and he stayed there one year."*

Milton: "I think it was [Don] Elston who took over the [Menlo Park Astro] office, as Acting chief.

*Schaber: "The story goes that Gene was looking to move the Branch elsewhere than Menlo Park in early 1962, and you were with him on a trip to Flagstaff when that was decided. Is that right?"*

Milton: "Actually, it was on a field trip to Meteor Crater [27 March 1962] looking at a telescope at Lowell [Observatory in Flagstaff]. I'm the one that's credited with saying at a dinner at the Gables [a local restaurant in Flagstaff at that time on Milton Road in Flagstaff]--this is really where you ought to move the headquarters of the Branch [and Gene agreed]. I just suggested that we move there--and I wasn't too serious. I guess it was about the time that I went to Argentina (see below). When I came back, I found out that they had moved the Branch there [to Flagstaff]. But, actually, Gene--you know--he did things way in advance. I remember at one point--way back in our Colorado Plateau days-- Gene saying--two places he would like to live is either Flagstaff, Arizona or Carson City Nevada."

*Schaber: "So, you hired Mike Carr, and then you got into lunar mapping and other activities."*

Milton: "I started my lunar geologic mapping up at the telescope at Lick Observatory. Then we got Ranger photos--and then Lunar Orbiter. I wasn't on any of the remote sensing missions. The only lunar quad I did was Theophilis.

Well everybody in the Branch was doing lunar mapping. Bill Cassidy who was at the Palisades-Lamont Observatory was getting up an expedition to go look at the Compo del Cielo craters (S. 27degrees 38 minutes; W. 61 degrees, 42 minutes) in Argentina. Bill asked me to go to Argentina with them. I stopped off on the way to Argentina in Bolivia where Elliott Morris [and Hal Stephens] was building an observatory on top of Mt. Chapachaya to look [at Shoemaker's request] for Kordylewski's clouds [clouds of lunar dust proposed to be at the LaGrange points of equal gravity between the Earth and Moon] (see 1962 in main text).

So, I went to the Compo del Cielo field. I was a pretty annoying guy at the time. People didn't like me, and now I realized that they were well justified--so I wasn't invited back the next year. So Gene said, well you really aren't going back for the second season, how would you like to go to Australia instead to look at the Henbury Craters [proposed impact craters]?" So I went to Henbury. I was there for four months and actually--for the first part of it--I had working with me a physicist who wanted to come and get some geology and became an astronaut candidate. He became an astronaut but never flew. I can't think of his name right now. He was there for a while, and I spent the rest of the time by myself. While I was there at Henbury, a geologist with the Bureau of Mineral Resources came by who was doing the regional mapping on the two-degree sheet on which Henbury was located. I spent one day with him and his associates. They said you know that curious circular ring of hills out west at Gosses Bluff? We think we found shatter cones

there. Well, sometime later I got together a project to go map that [Grosses Bluff; 22 km-diameter; S, 23 degrees 49 minutes; E. 132 degrees, 19 minutes; 142.8 +/-0.8 m.y.]--and that's the main thing I've done in my career at Henbury and Gosses Bluff--which got to be a really big project with all the geophysics surveys and the cooperation with the Bureau of Land Resources."

*Schaber: "What year did you first go to Henbury crater?"*

Milton: "We must be up to 1964 about now.

*Schaber: "So, you came back from Henbury--back to Menlo Park."*

Milton: "I was always officially headquartered in Menlo.

After Gosses Bluff the Branch staff was talking about lunar landings-and Apollo missions. I guess sometime in there I was selected by NASA for Scientist/Astronaut. I think there were 16 of us that went to Brooks AFB in San Antonio for a physical. Four of us were geologists.

There was Jack Schmitt, Mike Duke, and myself from Astro--and a guy by the name of Dave Atkinson.

*Schaber: "Dave Atkinson. Where was he from?"*

Milton: "He was an Englishman. I had known him earlier. He was a geology-1 instructor at Harvard as post-Doc."

*Schaber: "When did you first meet Jack Schmitt?"*

Milton: "I first met Schmitt after Harvard when I went to Caltech to get my masters. I spent two years at Caltech. I was a Teaching Assistant for Jerry Wasserberg [renowned isotopic chemist]. Jerry had only recently arrived at Caltech and he had been given elementary mineralogy to teach. He didn't believe in this business of just looking at minerals as how to tell one from another. He wasn't going into that pet theory. But some of the students wanted to learn something about minerals. Anyway, one of my students was Jack Schmitt--obviously a very promising guy. Mike Duke [another geochemist who would join the Branch] was also in my mineralogy class.

After Jack Schmitt graduated from Caltech he received a Fulbright Scholarship to the University of Oslo [Norway]. Then, he went to Harvard for the Ph.D. and he credited me with the fact that he wanted to go where I came from. I really appreciate that if it's true.

Well, four of us showed up as geologists among the sixteen people testing for the available scientist/astronaut positions. We were all thrown out for medical reasons. I had one eye that was not up to class one standards. Mike Duke--he impressed me on the way there. He read a sign that I could barely see was a sign. It turned out he had 20/20 vision. He had real eagle eyes--so far sighted. During one of the test he had to accommodate, and he was a little slow accommodating -- so they threw him out on that grounds.

Dave Atkinson had had osteomyolitus and Jack Schmitt had an extra loop of intestine. So, they threw out all of the geologists. Actually, in my case, I think my eye was just an excuse--and they didn't want me for other reasons. I was either an obnoxious brat, or my father was politically left, or the idea of me finding my way to the Moon and flying back without getting lost was just ridiculous! They were probably right. I wasn't up to the standards. Anyway, they threw out all of the geologists. Somehow the powers to be, which included Gene, decided they had to have one geologist in the group. So they somehow managed to resurrect the candidacy of Jack Schmitt. Jack was obviously the right one. He was so much better than the other three.

I guess before that, one other assignment I had was in Apollo mission planning. This was a group that was set up near Houston, Texas, with Eggleston [not Dick Eggleton]--at Ellington AFB. Dale Jackson was in charge of the USGS astronaut-training group there [see main text; 1963-1964.]

Al Chidester was there--and I guess Gordon Swann and I must have been sent there at the same time on temporary assignment because we shared an apartment.

All the intrigues and fighting between Dale's group and the NASA group was really something amazing. I don't know what was in it but--you know-- it was personality clashes and territorial disputes. I guess Ted Foss was the guy in charge [from MSC]. He was a very ambitious guy, and I think fairly competent. But there were a lot of people he had that weren't so competent. You know, each side would get together--then we'd separate and call a huddle and work on strategies and--I didn't know what was going on.

And there were some aspects of the geologic training courses that were being run from there. I went on a few of those. My first trip I think was down the Grand Canyon (see Table I). I figured if we wanted to teach them stratigraphy--we take them down the Grand Canyon. That worked."

*Schaber: "The first trip down the Grand Canyon was in 1963. Then, there were like three in 1964."*

Milton: "I remember my students were Buzz Aldrin [Apollo 11 LM pilot] and Charley Bassett. We each had two astronauts under our wing."

*Schaber: "Aldrin is a pretty sharp guy. I didn't know Charley Bassett."*

Milton: "He was killed in a plane crash."

My second trip as I recall was Philmont Ranch [near Cimarron, New Mexico] (see Table I). Then, I kind of dropped out. I'm not sure if there were any other trips. I was along on one trip to Hawaii. I wasn't officially on it--I just happened to be there at the time working with Dale [Jackson] on a lava flow.

*Schaber: "So, you weren't down in Houston too long--when Gene eventually pulled the Astrogeology astronaut training group out of Houston/MSC because the USGS and MSC geologists you just couldn't get along. Then, you went from there back to Menlo. What happened after that?"*

Milton: "I guess I was mostly involved in the mapping at Gosses Bluff then. Much later--the only real mission I was on was the Mariner 9 mission to Mars--prior to the time of the Apollo landings.

I guess Gene was the Branch Chief in Flagstaff and Hal [Masursky] was just the Office Chief out in Menlo.

I am still impressed by Hal. That is the main criticism I have with Wilhelm's book--Don just didn't appreciate the fine points of Masursky. You know, any committee, working group, commission, panel that Hal was on--it ran more smoothly than it would [have without him]."

*Schaber: "So, you were working on Mariner 9. What did you do on that mission?"*

Milton: "Well, there was the usual mission planning activities. Then we had two Mariner-Mars spacecraft. One of which died, fortunately, because we couldn't have handled traffic to two at one time. One of them worked [Mariner 9]. Then, when we got to Mars--there was a great dust storm."

*Schaber: "For about six months or something."*

Milton: "You just couldn't see a thing. We drew an absolute blank. Except for three peaks sticking up through the haze--then referred to as North Spot, Middle Spot, and South Spot."

*Schaber: "Some people wanted to call them impact craters--other people didn't."*

Milton: "You could see that they looked like, they stuck up [out of the general haze on the planet].

*Schaber: "That was a good indication that they were high-right?"*

Milton: "The astronomers said we expect dust storms in this season of the year. It will clear up in a few months--and it did. So we all picked out something to work on--and I grabbed onto the channels. I claimed to be the first person who claimed to really say that there was [or had been] flowing water on Mars."

*Schaber: "So, then Mike [Carr] probably took off from that and got interested in channels."*

Milton: "Yeah, when I dropped out, he kind of took over. Vic Baker [U. of Arizona] also got interested in it. I wrote one paper by myself, and then one paper together with Vic Baker. Then Vic Baker continued and Mike continued [working on the Mars channels], and I dropped out."

*Schaber: "So after mariner 9, did you work at all on the Viking-Mars mission [summer of 1976]?"*

Milton: "No, I didn't. I did some mapping for the Apollo 16 landing site, which was in my Theopholis quad. I worked with different people doing geologic maps of the Apollo 16 site at different scales.

*Schaber: "Yes, tell me about the Branch's misinterpretation of the smooth-flat-lying facies at the Descartes landing site as volcanic instead of a fluidized ejecta facies. Tell me your side of that--what happened there?"*

Milton: "I called it volcanic. We did not recognize the flat-lying ejecta from--Imbrium ejecta-- the Fra Mauro Formation--we did not recognize that as impact ejecta. I don't know how the site was picked. I wasn't involved in that.

It was just announced to me as the landing site. I think I came back from Australia or something and found out that Apollo 16 was going to land in Descartes because there were volcanics there. And that was okay with me. I think I had mapped them as volcanics. The error was that they looked smooth and we just didn't recognize that impact ejecta could smooth itself out when it landed."

*Schaber: "Yes, it was probably fluidized ejecta [like we later saw on Venus in spades!]"*

Milton: "So I mapped at one scale--and Carol Ann Hodges (Branch of Astrogeology, Menlo Park) did another scale. Then there was a partnership of Don Elston and Gene Boudette (Branch of Astrogeology, Flagstaff] I think. They were mapping at an even finer scale. They were seeing things that were just beyond visibility. It was what they called filigree--or something like that."

Well, I was in the backroom for the Apollo 16 mission. I didn't enjoy that at all.

I later applied for the Voyager mission but I didn't get on the team. I kind of dropped out of astrogeology at the time. Besides, my parents were getting very old back in the East--so I came back east and decided to resume being an Appalachian geologist. I wasn't too successful at that, but I had another ten years or so of geologic field mapping."

*Schaber: "So you mapped in the Appalachians out of Reston [Virginia]; and when did you retire?"*

Milton: "About 1992-1993."

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**Henry John Moore II** (1928-1998; geologist; Ph.D., 1965, Stanford University, Palo Alto, California) was born in Albuquerque, New Mexico on 2 September 1928 and grew up in Salt Lake City, Utah. He attended elementary and high school there. In 1951 Henry received a bachelor's degree in mineralogy from the University of Utah. He received his master's degree in 1959 and Ph.D. in 1965, both from Stanford University.

Some of the following biographical information on Henry Moore II was taken from an obituary prepared by Pat Jorgenson, U.S. Geological Survey, Menlo Park, California, on 28 September 1998:

He became a U.S. Geological Survey summer employee as an undergraduate student, doing field mapping in Fallon, Nevada. Following graduation and service in the U.S. Navy, he returned to the USGS, doing geologic mapping in Grand Junction, Colorado.



In 1960, while attending Stanford University in California, Moore ran into Eugene Shoemaker, whom he had first met when both were working for the USGS in Grand Junction, Colorado (on the Survey's Colorado Plateau Uranium Project). Shoemaker convinced Moore to rejoin the USGS and become part of its fledgling astrogeology program. Henry showed up for duty with Shoemaker's Astrogeologic Studies Unit at Menlo Park, California in September 1960

Henry Moore studied experimental craters produced by hypervelocity impacts, in cooperation with Ames Research Center, and craters produced by missile impacts at White Sands Missile Range, New Mexico. He led and participated in Apollo astronaut training exercises utilizing craters produced by chemical and nuclear explosives at the Nevada Test Site and simulated crater fields constructed by the Branch of Astrogeology near Flagstaff, Arizona. Early studies include the preparation of geologic maps of the Moon, numerous internal reports for NASA, development of design criteria for lunar roving vehicles, analysis of hazards produced by ejecta from the active seismic experiment performed during Apollo 17, and significant contributions to the Viking Mars Engineering Model.

Henry Moore served as the Principal Investigator on NASA Experiment S-222 (Photogeology Apollo 15-17). As part of this effort, he contributed to the Apollo 16 and 17 Preliminary Science Reports as well as guided contributions by participants in Experiment S-222. He participated in cooperative studies of Earth-based remote sensing data and Apollo orbital remote sensing data with other groups.

Henry was a member of the Physical Properties Investigation Team of the Mars Viking Lander. Member of Viking Mars Site Selection staff; a member of Mars Viking Lander Sample Site Working Group; Principal Investigator for the study of Physical Properties of the surface materials at the Viking Landing sites; and Principal Investigator of study of Martian volcanic processes and lava flow rheology.

Moore studied the physical properties of Martian surface materials using Viking lander, Viking Orbiter thermal, Earth-based radar, and Viking Orbiter imaging data.

Henry received the Apollo Medallion for Contributions to Lunar Roving Vehicle Studies in 1971. He also received a Group Achievement Award for his work on the Viking Data Analysis Team in 1972. Henry received an award from the Geological Society of America for astronaut training in 1973. In 1986, Henry Moore received the Department of Interior Meritorious Service Award as a leading authority on the physical character of planetary surfaces, with special expertise in the physics of surface properties and processes.

Moore officially retired in 1994 from the USGS, but continued to work for the Survey in a Scientist Emeritus position. He also served as a consultant to the Jet Propulsion Laboratory (JPL), and on 4 July 1997, the Mars Pathfinder landed on a site that Moore had helped to select. Following the pathfinder landing, Moore spent the next six weeks at JPL, working with other scientists on the JPL/NASA team to interpret the images and other data that were being sent back by the Mars rover, "Sojourner," as it slowly moved across the boulder-strewn plain at the mouth of Ares Vallis.

He contributed to the Mars Pathfinder and other Mars programs by furnishing estimates of the physical properties of the surface materials of Mars. At the time of his death (September 1998), Henry was the Rover Scientist, and Leader of the Surface Material Properties Science Operation Group for the Mars Pathfinder Mission. He was also still engaged in geologic mapping of Venus and Mars.

Following the Mars Pathfinder Rover mission to Mars in 1997, Henry received the NASA Award for Public Service. Moore's sudden death in September 1998 came only three days after being elected a Fellow in the American Association for the Advancement of Science (AAAS).

[Author's Note: Another obituary of Henry J. Moore by Thomas W. Thompson (Jet Propulsion Laboratory, Pasadena, California) can be found in *EOS, Transactions, American Geophysical Union*; 80, No. 25, June 22, 1999; pp283-284.]

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**Elliott Cobia Newberry Morris** (b. June 24, 1926, d. Sept. 20, 2005, geologist; Ph.D., 1962, Stanford, University):

Morris was born in Ely, Nevada. A World War II veteran, he served in the U.S. Army in the campaign to liberate the Philippines and in the occupation of Japan. He was married to Margaret Derbyshire on January 6, 1950.

The following was abstracted from a filmed interview (Flagstaff, Arizona) with Elliot Morris by Harry Ryan in 1996):

Elliot's family moved to Nevada when he was three or four and he went through elementary and high school there. His dad was in mine prospecting. Elliot later attended the University of Utah, majoring in geology. Subsequently, Elliot attended Stanford University (1960-1962). He received a scholarship for tuition and books only--so he went to work for Phillips Petroleum Company for a couple of years to help support himself while attending Stanford. While at Stanford [1960 or 1961], Elliot saw a notice on a Stanford Bulletin board for a part-time job with the USGS [not Astrogeology] in Menlo Park. They needed someone to pick microfossils out of samples. He applied and got the job (WAE Physical Science Aid) working with Don Miller from the Survey's Alaskan Geology branch who, amazingly, had acquired the very same samples that Elliot had collected while working for Phillips Petroleum several years earlier.

While at Menlo Park, Elliot would also work with Hal Stephens [who would join the Branch of Astrogeology in Flagstaff in 1964] performing mineral separations, in addition to learning photolaboratory work. Elliot was introduced in 1960 to Gene Shoemaker, who he recalls was very excited about mapping the Moon at that time. Gene soon desired to collect the best lunar photographs available at that time to use as bases for the proposed lunar geologic mapping.

While still working WAE for Alaskan Geology Branch in 1961-1962, Elliot was also doing things for Gene Shoemaker, like going with Hal Stephens down to Mt. Wilson Observatory and copying

the best of the extremely high-resolution telescopic plates of the Moon taken early in the Twentieth Century.

This was about the same time as Gene was getting the Astrogeologic Studies Unit organized at Menlo Park. Gene had recently recruited Chuck Marshall and Dick Eggleton (see 1961-1962 in main text above), who together started doing the original lunar mapping. Another Survey Group in Washington, D.C. (Hackmann, Mason and Olsen) was simultaneously engaged in lunar terrain mapping.

When Elliot Morris finished his Ph.D. at Stanford in 1962 (dissertation-"*Mineralogy of the Eocene Strata of Southern California*"), he came on with the Astrogeologic Studies Unit full time at Menlo Park, California. About that time, Shoemaker wanted to set up a center for lunar mapping. Gene, needing a telescope to proceed with such lunar mapping, contacted the Lick Observatory in California to use their 36-inch scope for the early lunar mapping while still in Menlo Park (see 1961-1962 in main text above).

The first big project assigned by Gene to Elliot after he officially came on with the Branch was a challenging one indeed--to design and build a telescope. This was of course something that Elliot had never done before.

Elliot was not only involved in designing Astrogeology's 30-in telescope in 1962, but he and Hal Stephens were asked by Shoemaker to go to Bolivia and set up a small observatory on Mt. Chapachaya (at 17,600 feet elevation) (see main text, 1962 for additional details). The objective here was to observe the so-called libration points where cosmic and lunar dust had been proposed to exist in a paper by Polish astronomer Kazimierz Kordylewski. He had reported brightening in the direction of the points that might indicate particle accumulation where the pull of gravity is more or less neutralized by the pull of the Earth vs. the pull of the Moon--and any masses of cosmic dust [including lunar materials] might float into that area and more or less oscillate within a specific region there.

Shoemaker felt that photographing it with a Leica camera [like Kordylewski had done] was not a very definitive way to determine whether or not these "Kordylewski clouds" really existed or not. So he commissioned Elliot Morris and Hal Stephens to go down to Bolivia and build a small telescope on Mt. Chapachaya. They used an 8 X 10-inch aerial camera and spectrographic plates that were suitable for celestial photography. Unfortunately, at the same time as Hal and Elliot were taking the photographs of the libration points [looking for the Kordylewski cloud]; there was a large volcanic eruption on the island of Bali. The volcanic dust was coming through the atmosphere in such amounts that it obscured their view--even from 17,600 feet elevation. Thus the experiment was a failure, and the clouds were not photographed--if they exist at all" (Paraphrased from a filmed interview with Elliot Morris by Harry Ryan-1996).

Elliot was a team member and major participant on the Surveyor Television Experiment with Gene Shoemaker, Ray Batson, Henry Holt, and other personnel from the Branch of Astrogeology in Flagstaff (see main text for more details on Surveyor).

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**William R. Muehlberger** (b. 1925; Structural geologist; Ph.D., 1954, California Institute of Technology)

The following is taken from the biographic data sheet of the NASA Johnson Space Center Oral History Project [[http://www.jsc.nasa.gov/history/oral\\_histories/MuehlbergerWR/WRM\\_11-9-99.pdf](http://www.jsc.nasa.gov/history/oral_histories/MuehlbergerWR/WRM_11-9-99.pdf)]. An extensive oral history interview with Professor Muehlberger by Carol Butler (Austin, Texas) on 9 November 1999 is also available at the above web address]:

Bill Muehlberger attended the University of California at Berkeley, California with a Civil Engineering Major from 1943-1944. He then attended Caltech and received his B.S. and M.S. in Geology in 1949. Bill remained at Caltech and received his Ph.D. in Geology in 1954. Bill was a U.S. Marine in 1942-1946 and again in 1950-1952 [during the Korean War.]

Bill Muehlberger's pre-NASA involvements include the USGS (Reston, Virginia) as a Geologic Field Assistant (1948-1949), and as a Geologist (1949). He then became affiliated with the University of Texas at Austin, Texas as an Associate Professor of Geological Science (1954-1958), an Associate Professor of Geological Science (1958-1962), as a Professor of Geological Science (1962-date unknown), and as Chairman, Department of Geologic Science (1966-1970).

During the summers of 1954-1961, Bill Muehlberger worked as a geologist at the New Mexico Bureau of Mines and Mineral Resources, Socorro, New Mexico; and was Director, Crustal Studies there from 1961 to 1966.

Bill first joined the staff of the U.S. Geological Survey, Branch of Astrogeology in July 1966 (at Austin, Texas) and again in June 1970 when he became Principal Investigator of the Apollo Field Geology Experiment for Apollo 16 and 17 (June 1970-1973).

Bill Muehlberger was a Co-Investigator on the Visual Observations Experiment, Skylab (May 1973) and Apollo-Soyuz (1975) missions, and has been a geology instructor for NASA's Astronaut Training Program from 1973 to the present time.

Following his involvement with NASA's Apollo Lunar Geology Experiment Team, Bill Muehlberger has been a Professor of Geological Science at the University of Texas, (Austin) (1973-1985). He was then the Fred M. Bullard Professor (1980-1982), the Charles E. Yeager Professor (1982-1983), the John E. "Brick" Elliot Centennial Endowed Professor in Geological Sciences (1983-1985), the William Stamps Farish Chair, Geology Department (1985-1989), and the Peter T. Flawn Centennial Chair, Geology Department (1989-1992). From 1980 to 1988 Bill was the Director, Tectonic Map of North America Project for the U.S. Geological Survey.

Additional Awards and Citations received by Professor Muehlberger include:

Award Series "Principles of Geology"-Local Classification for College Instructional Program; the Institute for Education by Radio-Television, Ohio State University 1961.

American Association of Petroleum Geologists Matson Award, "Best Paper" presented at the 1964 Annual Meeting

NASA Medal for Exceptional Scientific Achievement, 1973

Houston Oil and Mineral Corporation Faculty Excellence Award, 1978

Knebel Distinguished Teaching Award, 1992

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**John C. "Johnny" Nuttall** (1927-1986; Electronics Engineer; 1955, Electronics Instrument-Maker Certification, NACA [later NASA] Electronics School, Langley, Virginia) arrived in Flagstaff in February 1967 on what was supposed to be a one-year loan from NASA/MSC in Houston, Texas, to the Branch of Surface Planetary Geology. Johnny had since 1965 been participating with the Branch as a spacesuit biomedical technician from NASA/MSC in support of the Branch of Planetary Geology's suited field exercises [for the development of astronaut lunar geologic activities] in Hopi Buttes, north of Winslow, Arizona. Johnny Nuttall built up, and was the head of, the Branch's Electronics Support Group until his retirement in 1977. Nuttall participated with the Apollo Lunar Geology Experiment Team at Mission Control in Houston, Texas, during all of the Apollo lunar missions.

The following was taken from an interview (in Flagstaff, Arizona) with Betty Nuttall (Johnny's wife) by Gerald G. Schaber on 30 March 2001:

Betty Nuttall: "Johnny was born on July 19, 1927 in Kewaunee, Illinois. His family moved to Brooklyn, New York, and he went to a Catholic school there. The Nuns refused to let anyone talk like they were from Brooklyn, so that's why no one could ever guess where Johnny was from. They moved down to Virginia in his senior year in high school to Gloucester County, which is right by the York River. When he got to his school there, they told him that he had all the credits that he would ever need. So he went to work on boilers, and other things there.

When WW II broke out, Johnny went into the [then] U.S. Army Air Force flight school. In his last week of school, they told him that since the war was winding down they were going to be overridden with pilots, they were going to break up the school. So, he became a radioman for the Air Force flights, and worked with the navigator.

Then, about 1946, he got out of the Air Force and went to school at Langley NACA (National Association Committee on Aeronautics; later NASA). They had done lots of flights with jets, and things like that, testing them out. He was there for a year, and then in 1949/50 he was recalled into the Korean War in the then U.S. Air Force. He was in for only for a year. He was stationed in the states most of the time. He did some training and other things. By that time he had become a Staff Sergeant.

*Schaber: "In electronics? Was he doing other things besides radio?"*

Betty Nuttall: "Yes, he was doing various instrumentation-type things. When he got out in 1951 he went back to the Apprentice Electronics School at what was by now NASA (no longer NACA) at Langley, Virginia. They had a four and a five-year course. I met him in my senior year in nurse's training in 1952 and we were married that same year. Our son John Michael was born in 1955. Johnny graduated from the four-year Apprentice Electronics School there at NASA in 1955 with a Certificate in Electronic Instrument Making. But the year before that, they offered him a fifth year. That was offered to some students because of their grades. I think there were only five out of the class that went to the fifth year. They were promised a thousand-dollar raise when they completed the course--which was very rough.

But to back track just a little bit. One of the first communications satellites that went up was called Echo [the large balloon satellite Echo I was launched 12 August 1960]. Well, Johnny did all of the instrumentation on Echo-I. They launched it from Wallops Island in Virginia. We went up in our field (we had two acres right off the York River) and we could see it when it went up--and it was so exciting. It kind of went up and then came down, but it did everything that it was supposed to do.

Johnny also worked on the Mercury, Gemini and Apollo Programs--all three of them after we went to Houston from Langley. We went to Houston in 1962 because the space Center Manned Spacecraft Center was being built down there. [The Manned Spacecraft Center, or MSC, formally opened in September 1963]. That was the time when a lot of the people at Langley were concerned about lives being lost in the manned Program--and Johnny just felt like that he wanted to be a part of that.

We were in the first group that went down to Houston from Langley; there were 400 of us. They didn't have the [Manned] Space Center completed at that time. People worked all over Houston in different groups and places you know.

We stayed in Houston for five years. It was very exciting there because they did a lot of the [g-force testing] with the racecar drivers. They went to Austin, Texas, and the families got to go as well. Johnny at that time was in biomedicine electronics. A.J. Foyt really stands out in my mind because he was one of the really great racecar drivers at the time. He was really anxious to help in any way he could with the NASA program. So they connected him up and they took blood-pressure readings, temperatures, g-forces, and all that when they were running the race cars at high speed. I believe that they were driving the Ford Cobra at the time. That was really exciting; it was about 1963 when they were doing that down there in Texas.

*Schaber: "Amazing, I had never before heard about NASA using the famous racecar drivers to evaluate body functions at high g-forces!"*

Betty Nuttall: "Yes, the whole thing was amazing. We were so proud to be a part of that!"

*Schaber: "In the meantime, Shoemaker was just then moving his Branch of Astrogeology from Menlo Park, California, to Flagstaff--in 1963.*

*Let's go on then. Johnny's down in Houston and you were working with the race car drivers. He was in the Biomedical Division of NASA at that time--right?"*

Betty Nuttall: "Yes."

*Schaber; "So what happened after that?"*

Betty Nuttall: "I might add too that those racecar drivers were just absolutely fantastic. A.J. Foyt is the only one that comes to mind--but there were others.

*Schaber: He was a legend."*

Betty Nuttall: "Yes he was--and just a wonderful person. As a matter of fact, A.J. Foyt was on the plane one time when Johnny, Gordon Swann, George Ulrich, Tim Hait, and perhaps others from Flagstaff, were coming back to Phoenix. Johnny went over to talk to A.J., and brought him over to meet the USGS group. [Author's Note: this event probably occurred somewhat later, just after the Apollo 12 mission in the fall of 1969].

They were just really surprised because Johnny had talked about A.J. Foyt, and they thought oh yeah—sure. But when they met A.J., they found out all about that g-force and biomedical testing work that had been done at that time in Houston. Johnny was working out of Baylor University then. He was working with NASA but he went to Baylor to help out with that the biomedical program because he had to learn something about the medical part of it as well.

*Schaber: "So what happened to get Johnny to Flagstaff?"*

Betty Nuttall: "Johnny was then on loan to USGS-Flagstaff for a year [Author's Note: that turned into three years, 1967-1969]. MSC told him he could go up to Flagstaff for a year. That was in February of 1967--the year that the big snow [7-foot snowfall] came in December of 1967. Johnny had an office up there at the Arizona Bank Building.

[Author's Note: While on loan to the USGS from NASA, Nuttall started designing and building up the communications links (audio and video) between the Branch's Command Data and Reception and Analysis (CDRA) Facility on the fifth floor of the Arizona Bank Building to field sites as much as 90 or more miles away in Hopi Buttes, Meteor Crater, etc. Starting in about late 1967 to early 1968 Johnny Nuttall set up the remote communications links to the Branch's new Apollo Data Facility (ADF) (Apollo prototype, science control center) down at on Fourth Street in East Flagstaff.]

The year of Johnny's original loan to Flagstaff was up, and he still had things to do here--so it turned into two years, and then it was going on three years. He was coming home for lunch one day and he said well, they want to know if I want to stay here permanently with them [Astrogeology]. I said, you had to ask--of course we do? The kids didn't want to go back to Houston, and neither did I.

I know that he was always calling down to MSC in Houston to get this, that and the other thing, and they were sending equipment up. One day they said my God Johnny look at all this stuff you're getting from us; what are you building up there? Johnny said well, just keep it coming!"

*Schaber: "What did Johnny think about his role during the Apollo Days, do you recall?"*

Betty Nuttall: "You know he was on every one of those Apollo [missions] through Apollo 17."

*Schaber; "Was Johnny at Houston during all of the Apollo missions?"*

Betty Nuttall: "Yes, he went down for every mission."

*Schaber; "I recall during the early missions at least, he was making the kinescope video tapes of the lunar surface activities from the mission control video uplink from the lunar surface."*

Betty Nuttall: "He said that all of his work [in Houston] took place at nighttime after all of the tapes and everything had come in--that is when he had to work."

*Schaber: "Okay, so he taping the TV coverage of the kinescopes off the old black and white TV on those big 4-inch wide, Sony, video tapes--which was brand new gee-wiz technology in those days. When did Johnny actually retire?"*

Betty Nuttall: "Let's see, he turned fifty and he retired. He was born in 1927, so it would have been in 1977."

Nuttall: "He was going to finish the house [he was building for his family in Flagstaff] which he never did--because he was such a people person Jerry that he couldn't stand being here working, and not having anyone to talk to. Anyway, he went and got a job with the state."

*Schaber: "He did?"*

Nuttall: "Yes, he went to work with the state in traffic lighting control. He worked there for almost five years before he had to quit because of his health [cancer]."

*Schaber: "Was he working out of Flagstaff?"*

Betty Nuttall; "He was working at the state office out by the Flagstaff Mall. He was over in Kingman, Arizona for a year when they were putting up all of those traffic lights over there."

*Schaber: "That would have been the Department of Transportation, lighting Division."*

Betty Nuttall: "Exactly, it was ADOT. He worked for ADOT. He had to put the wiring in those big boxes by the railroad tracks, etc."

*Schaber: "After working for both NASA and the U.S. Geological Survey during the pre-Apollo and Apollo Era, ADOT couldn't have been all that exciting for him."*



Betty Nuttall: "Of course, but he said that he loved the people he worked with."

*Schaber: "Of course, Johnny was a wonderful person to work with."*

Nuttall: "You know he was so proud of being part of NASA, and then with Astro. This was another great big step for him coming over here with Astro--and he just loved it. When we were in Houston everybody was working overtime, but they would not let them work but one day of the weekend and every other night; and then just one day on the weekend to keep the quality of their work up. Then, when he came here to Flagstaff, he just got so involved with this."

*Schaber: "Nobody cared about getting paid for overtime. We were working all the time—and loving it."*

Betty Nuttall: "And you know, Johnny said that he didn't know a single slouch in this whole division [Branch of Astrogeology]. He said that it was just so wonderful to work with people who were so dedicated."

Johnny said that it was just wonderful after coming from Houston, and then coming here. He said Houston people also had a lot of get up and go--but this place was just something special."

*Schaber: "It was Gene Shoemaker's influence!"*

Betty Nuttall: "Oh yes indeed."

You know, Johnny used to say that Gene [Shoemaker] was a man ahead of his time."

*Schaber: "He had that right!"*

[Author's Note: Sadly, Johnny Nuttall passed away in 1986 in Flagstaff, where his wife Betty still resides.]

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**Joseph "Joe" O'Connor** (b. 1934; geologist; Ph.D., 1961, University of Colorado, Boulder)

The following was taken from an interview with Joe O'Connor by Gerald G. Schaber on 9 July 2001:

O'Connor: I was born 11 January 1934 in the Bronx, New York and spent my earliest years in NYC; then went through early grade school (through third grade) out on Long Island (Jackson Heights and Baldwin areas).

That was just about the start of WW II, and my father got drafted. I was an only child, so my mother and I moved down to Washington, D.C. and lived for a couple of years with my uncle who was a doctor in D.C. I went through 4<sup>th</sup> and 5<sup>th</sup> grades down there. My mom moved back to Long

Island and I moved up to Fort Montgomery, New York, which is just a little bit outside of West Point.

I had moved back to Baldwin and lived down there with my mom for a while. This was like the end of 7<sup>th</sup> grade. Then my father came back from the war and we continued to live in Baldwin through my high school. I graduated from high school in 1951 without an idea in the world what I wanted to do.

Long Island is a sand bar--so I never even saw a rock you know. My basic interest at that time was fly fishing. However, I had a very good friend at school whose father was an airline pilot and lived in Denver. They still maintained a house in Denver and he was going to get in-state tuition and go to the University of Colorado. He said Joe, you ought to go to the University of Colorado (CU); there is a trout stream that runs through the middle of town. I said, oh, that sounds good. I was [had planned] to go to Hofstra University Hofstra is in Hempstead, New York--and was a bus ride from my house at that time. It turned out that it cost me \$10 a year more to go to the U. of Colorado-live in the Dorm--and even get a train trip back to New York at Christmas time--than to go to Hofstra. CU was actually a pretty good school.

I started out in mechanical engineering. God, I am "not" one of them. There was another group of guys there [in engineering] who spent twenty hours a day bent over slide rules. Remember slide rules? You could hear them in their rooms with that slide rule-clicking sound going on all the time working their buns off to get a B or a C+ average, and never met a girl the whole time they were in school. I didn't want to be one of them, and everybody else flunked out. So I said this is probably the wrong school for me; so I think I will go over to what we generally referred to as Arts and Parties [Sciences] at that time.

So, the second year out, I switched from Engineering to Arts and Sciences. It turned out that Geology had a major's course at 8:00 AM--and I was a morning person.

By this time, one of the things that I really liked to do was play basketball, and you couldn't get into the gym early. But I could get up, go the 8:00 AM geology class, and then cut the rest of my classes that day and play basketball! It took me maybe four weeks before I realized that hey, this geology stuff is fun! I was sort of hooked from then on, but I was no brilliant student. The New York schools were so good that I had had everything in high school that they gave us in engineering school, but that wasn't true when I got over to Arts-and-Parties. I realized, okay, this is going to take a little bit of work. But I really wasn't up to work; I really wanted to play basketball. So I wasn't doing very well in school. I went through my sophomore year--started my junior year--and was really doing terribly--and decided to drop out of school.

I went back to New York and got engaged by that time to a girl who was a year behind me in high school, and I got a job in construction. We got married, and I did all of those sorts of neat things--and I volunteered for the draft because I knew I wanted to go back to school, but I couldn't afford it. I just had this good idea of getting the GI Bill; but they took me into the Army I think ten days before the GI Bill ran out. That was December 1954. I spent half the time in Ft. Monmouth learning electronics and half the time in Germany doing public information. But the whole Army experience scared the snot out of me. I very soon discovered that there were some very dumb

people out there. And if you didn't work with what you had, you were going to end up with them as your bosses. So, I got motivated! I must have been awful to be around.

Here I am in Germany; I lived over there, and my wife came over. We were living in a German house. I learned how to speak German. I sent off for the Army's course in Calculus. I taught myself Calculus in about six months; and evidently did a reasonably good job, because when I went back to Calculus in college, I said hey, I already know this!

When I got back in school [after the Army] I went from a 0.4 average to a 4.0 average. That attracted enough notice that they gave me a degree when I got out. It also got me an NSF (National Science Foundation) Fellowship. I graduated from the University of Colorado in 1958.

I had met Norma Neff [later Norma Swann, Gordon Swann's first wife] when I was in school [at CU] the first time. We shared the major's course in geology, and then when I went back, we were both living in "Vetsville"--in separate areas--and I began to run into this guy [Gordon Swann]. You know, Gordon is a nice guy and very friendly, and so forth. We sort of became distant buddies, but, as I said, we were both doing the real study thing at that time. Gordon mentioned me to Norma, and I used to wear these red and black-checked wool shirts as an outer jacket. Norma said a Joe that wears a red and black-checked wool shirt? I used to know someone like that. So, Gordon brought me over, and sure enough that was Norma.

So Gordon was going to get his degree and go out into the oil patch, but we both got interested in metamorphic geology. He worked with Larry Warner and I worked with Bill Braddock, but we picked areas that were adjacent to one another. That work--not to blow one's own horn--was a real revelation to the geology of Colorado. It was an area that wasn't the continental Divide/Idaho Springs, high-degree metamorphic stuff. We went all the way down to the greenschist facies.

What I wanted to do was get a teaching job somewhere. Jobs were getting very hard to find about that time, but there was a big flush of jobs just about when I graduated--and then they had all the trouble in the Middle East; and all the geologists came back from there. The place was saturated with geologists.

So, the USGS had the Nevada Test Site Project going. So I signed on then with them and I worked down there with Bob Christensen and Pete Lipmann. I actually joined the Survey before I got my Ph.D. degree from CU in 1961."

*Schaber: "When the whole Colorado Plateau Uranium Project folded up in 1958, didn't some of the geologists get sent to the Nevada Test Site?"*

O'Connor: ""Yes, but very few of them, because the NTS didn't want them up there. The people who NTS did not want are the ones who finally got sent to Kentucky [to work on the Kentucky geologic mapping project]. They didn't want the guys who did layer-cake stratigraphy; and the reason they didn't want them was that the test site was all of these welded tuffs--and they weren't layer cakes. It was really pretty complicated stuff--and man it was fun!

About that time, I ran into Carl Roach. Carl was working on this esoteric--let's find the buried [nuclear] explosion project for ARPA. I'm not sure how I actually met him. I think that someone suggested that I go and talk to him because I was getting very interested in X-ray crystallography--especially in defect studies [of minerals]. After we talked for a while, I thought these things [defects] in quartz structures ought to be susceptible to [shock] effects if there is enough energy around.

And of course, we talked about some of the things that Gene Shoemaker had worked on with the appearance of Coesite and Stihovite [high temperature and pressure phases of quartz]. I said well, that's a maximum rearrangement [of crystal structure]--but I wonder whether we could see down lower [i.e., at lower energies] when you are getting anomalies in defects. So, I started to research that; then Carl got interested in it, and then Special Projects got "disinterested" in it. So I said to heck with it, I will just switch over and join Carl's project--which just happened to be in the Branch of Astrogeology. At the time, I was just not interested in the early things that were going on in Astrogeology. The extraterrestrial [lunar] geology itself wasn't particularly intriguing to me, although I got interested in it later.

I would later join the Branch of Astrogeology without even meeting Gene Shoemaker. What I had heard were Carl Roach's stories about Gene. Carl was a really good friend of Genes, but all of his stories were really negative. He used to say you've got to stay away from Shoemaker, because if you get next to Shoemaker, he is so magnetic that he'll just turn you on--and before you know it, you're going to have five times as much as you know how to deal with!

Anyhow, Carl got me going, and I think he hired Howard Wilshire. I told him that there was this guy up in Boulder who is making thin sections, and is just finishing up his Ph.D. in metamorphic geology--and this was Gordon Swann (see Appendix A). Yeah, we got Gordon on; although I think I had to hire him first as a lab assistant--or maybe a GS-7 Geologist. He was just finishing up his Ph.D. So we took Gordon on and we embarked on a project of mapping a granite body out here around Twin Lakes, Colorado. Essentially, the idea was that this body had great big feldspar phenocrysts in it. We were going to introduce these things to shock waves by subjecting them to Carl Roach's 30-06 rifle, and by blowing them up with Mike Carr's explosions. We were going to determine how you could figure out where the pirated atomic explosions were [as part of the Nuclear Test Ban Treaty]. It was pie in the sky; I don't think it would have ever worked--but it was going to be a lot of fun.

And then Jack Kennedy was assassinated--and in comes Lyndon Johnson. And one of Lyndon Johnson's first items of business was to cut the ARPA funds--and Carl Roach was completely ARPA funded. So, here we were, sitting in Denver looking at each other. Howie [Howard] Wilshire was the only one with the smarts to really do anything about it. He immediately fell back on his work on ultra-mafic rocks and got a whole lot of things published. We couldn't go anywhere, we couldn't do anything; we couldn't buy anything. Gordon and I just kind of stared at one another and said what are we going to do?

About that time, Don Elston began to bug us to come to Flagstaff. He started talking how wonderful it was going to Flagstaff. He actually flew us down there and--you know-- Flagstaff [Astrogeology] wasn't even in the Arizona Bank Building yet; it was in the Museum of Northern

Arizona. It [the Branch of Astrogeology] was wild--going a hundred miles an hour--but everybody was bouncing off one another, and nobody knew what anybody else was doing. And Gene was never in town; his office was commonly said to be on the Super Chief (Santa Fe) going back and forth from Flagstaff to Caltech. He'd come into town, and he'd be there for like 18 hours. He'd give out maybe two or three new lunar quads to people to get mapped. He designs maybe a whole bunch of other things to do--and boom--he was gone. If you wanted to talk to him, you had to go to the train station [in Flagstaff] and get him while you were standing on the ramp waiting for the train to come in--hoping it was late so you had a little more time to talk to him!

But I was really interested in doing X-ray crystallography at that time. I had taken--after I got my Ph.D.--all the necessary courses in advanced Calculus and crystal structure analysis--and there was a guy in the Chemistry Department at CU who was one of the top people in the world in this. He was doing three-dimensional analysis. Boy, I really wanted to do that, and I did not want to go to Flagstaff and start cranking out lunar maps. I didn't want to do it!

I started looking for a school to get a job at; and I got one offer at Iowa State, but I wasn't going to go to Iowa; so finally I decided I've got to go to Flagstaff. But during that time, which was like six months, Don [Elston] probably called up to Boulder about twice a week [to get Gordon and him to move to Flagstaff].

The Branch of Astrogeology was an amazing group of people. I think that one of the things that helped in that--is that they got a whole bunch of bright new Ph.D.s.

We eventually arrived in Flagstaff in June 1965. This was just before the tripartite division of the Branch took place [into Unmanned Lunar Exploration (Leader-Jack McCauley), Manned Lunar Exploration (Leader-Don Elston) and Astrogeologic Studies (Leader-Hal Masursky)]. I had the Advanced Lunar Studies Project [under the manned group].

Upon my arrival at Flagstaff, some Branch personnel had just recently moved into the Arizona Bank Building--maybe just that summer. I stayed at the Ski Lift Lodge [across Route 180 from the entrance road to the Arizona Snow Bowl, northeast of town] for a month or two while we looked for a house.

Gordy [Gordon Swann] got off on the astronaut training pretty big, and most of what I did down there was buying equipment. Since I had the Advanced Apollo Lunar project, we had all the telemetry, the X-ray machines [and other analytical lab equipment], etc. You know the way we spent all our time down there was in planes flying to JPL and to Houston, and to Huntsville and to Washington to re-train the next bunch of NASA Administrators."

*Schaber: "So the first thing you get involved in is the Advanced Lunar Study Project buying equipment. Now I remember, we had the Rock Lab on Mike's Pike that I ran for a while after you left."*

O'Conner: "That was also part of my job was to get that thing started--just to get it started. What I wanted to do- when we got that started was to do what you ended up doing--and what Dave Dahlem did for a while."

*Schaber: "Doing the x-ray, spectroscopy, and doing the thin sections, etc."*

O'Conner: "Absolutely!"

*Schaber: "As I recall, you were involved in the Advanced Lunar Studies Project, and also heavily involved in the Early Apollo Project--trying to show NASA that you can actually do some geology on the moon in suits. Is that correct?"*

O'Conner: "Yes, I was mostly just a suit subject on the Early Apollo stuff. You know we were also working with that monster machine [the GM built Mobile Geologic Laboratory, MGL, or MOLAB) that appeared in Flagstaff about the same time as Putty Mills [June 1965; see 1965 in main text.]

I left Flagstaff at the end of 1966, the beginning of 1967. I went out to Berkeley and took a job at the Space Sciences Lab out there."

*Schaber: "Why did you leave?"*

O'Conner: "I wanted to do lab work, and I was really into thermodynamics. I thought that we were going to work on the geology of the Martian surface out at the Lab at Berkeley--because they had an ongoing project that was all ready to go on Mariner--and that was interesting to me.

And one of the things that drove me out [of Flagstaff] was that Gene [Shoemaker] kept trying to get all of us to map a lunar quadrangle, and that type of thing. If you were in Flagstaff, particularly us earlier guys--all you got to do was to re-educate NASA administrators. I was getting really frustrated by got being able do what I considered [real] geology."

*Schaber: "Tell me, what if anything before you left [Flagstaff] did you have to do with building up our computing capability [at Astrogeology]?"*

O'Conner: "Well, Ken Watson and I "were" the computer committee [at first]. We were responsible for getting that IBM 360/30 computer--but that came in just about the time I was leaving, and I never got a chance to use it."

*Schaber: "Had our Apollo Data Facility (ADF) at Fourth Street [in East Flagstaff] been completed before you left?"*

O'Conner: "The Fourth Street Building was just being completed.

But I think actually we were still in the Arizona Bank Building when we ordered that computer [the IBM 360/30]. We had just ordered it, or it had only recently been installed. I moved to Berkeley in January of 1967."

*Schaber: "So you went to Berkeley, and then you went to Law School."*

O'Conner: "Yeah, I worked for several years at the Space Sciences Lab [at Berkeley]; the things we were working with out there had a lot to do with infrared. We were charged with trying to find a site for a terrestrial infrared telescope--the one that I think ended up out there in Hawaii [atop Mauna Kea].. But at that time, they were considering several different places, and each school had its own favorite site. Our site was in the White Mountains down in South California. Kitt Peak [near Tucson, Arizona] was being considered, as well as a Chilean site; and of course the Hawaiian site.

At the same time, there was a student-faculty joint seminar on Science and Society going on at Berkeley. And that sounded like a neat thing; this was the 1960s and all of that stuff was really neat. So I joined in with that and noticed that there were two kinds of people attending--one kind was lawyers and one kind was scientists--and they could not talk to one another. They did not have a clue what the other person was saying--even when the words were the same.

So I had a mid-life crisis and got this idea that wouldn't it be neat if a scientists--namely me--went to Law School and learned what it was that these [scientifically] ignorant lawyers were talking about. Then, I could sit in the middle and talk to both sides. So, I decided, hell-why don't I do that?

I had just gotten divorced, so I got into Law School at Berkeley. However, when I got out, I found out that now neither side [the geologists or the lawyers] would talk to me."

*Schaber: "What years were you in Law School?"*

O'Conner: "I started in 1972 and got out in 1975. About the time I got out of Law School I was talking to Gordy [Swann] and I was telling him what I wanted to get into was environmental policy. I wanted to get into the place where the rules were being made that controlled how you dug up resources or pumped oil--or did any of these things. I wanted to make sure that there was both a scientific and a reasonable legal format in it.

And so I was talking to Gordy about this on the phone one night, and he said let me call you back next week. He went into Reston (VA), and he and Dick Sheldon were old buddies. Gordy told him I have a friend who has a Ph.D. in geology and a law degree, and he wants to do blah de blah. And Dick said we'd better hire him before somebody else does. So, I got a job. They didn't even know what to do with me. They put me back in the Survey in what was called the RALI (Resource and Land-Use Investigations) run by a guy named Ron Jones."

*Schaber: "Where were you stationed?"*

O'Conner: "In Reston [Virginia]. One of their big things was how do you mine coal and leave the place useful for something else; because nobody was thinking about it at the time. We did a whole bunch of things there, but they were far away from the Astrogeology stuff. I worked in that for a while; then I moved into the Office of Surface Mining--right about the end of the 1970s. It was the last two years of the Carter Administration. They got the office of surface mining going, and I

moved down there and worked with them for a couple of years until I got crosswise with the Reagan Administration--and they managed to get me fired.”

*Schaber: "Tell me about that. The rumors were that you were testifying against the Survey."*

O'Connor: "Not against the Survey. Well, the Survey was mad at us when we got down to OSM because we managed to take their function--the old conservation had the function of licensing coal mines on Federal lands--and we took that away from them. And it was largely on my testimony that the Secretary decided to give this to OSM rather than the Survey. So I lost a few friends over at the Survey after that. But, then Reagan came in--and James Watt [as Director of the Department of the Interior].”

[Author's Note: After a prolonged period of--to put it mildly--strained relations between Joe O'Connor, USGS management, and the Department of Interior Headquarters [led by James Watt], he applied for an opening for Branch Chief of the Survey's Coal Branch, and got the job without the knowledge of his Department of Interior detractors who had previously had him fired. O'Connor was in that position for a year and a half.]

O'Connor: “They tried [to get me fired] again a year later, and they got the papers cut to have me fired, but I hired a lawyer and made a bit of a fuss in the Survey--and they retracted it. So, after that I have just been doing science --mostly mineralogical stuff.”

[Author's Note: Joe O'Connor officially retired from the USGS in February 2001.]

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**Terry W. Offield** (1933-1999; geologist; Ph.D., 1962, Yale University) joined the U.S. Geological Survey in 1961 where he worked on mineral resources of the outer Himalayas and on mineral surveys in Brazil for the Survey's Branch of Foreign Geology. Offield transferred from the Branch of Foreign Geology to the Branch of Astrogeology and arrived in Flagstaff in February 1966. He began serving as a photo-mission advisor for Lunar Orbiter missions, and participated in lunar geologic mapping, the selection and geologic mapping of potential Apollo landing sites using Lunar Orbiter photography, astronaut training trips, and the development of the Branch's Lunar Science Operations room concept during field exercises in and around Flagstaff. Terry (along with Dick Eggleton) mapped the geology of the Fra Mauro landing site that was selected for Apollo 13 [then Apollo 14 after Apollo 13 failed to land]. Terry was appointed a Co-Investigator on the Apollo Geologic Field Team for Apollo 13. Offield contributed to, and was a co-author of the Apollo 14-*Preliminary Science Report* that was submitted to NASA by the Branch of Astrogeology in May 1977. In 1977 Terry became chief of the U.S. Geological Survey's Branch of Uranium and Thorium Resources. He was selected as chief of the Office of Energy and Marine Geology in 1982. Soon after his retirement from the USGS, Terry passed away on 5 February 1999 of complications following heart surgery in Fairfax, Virginia.

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**Richard Joseph "Dick" Pike** (b 1937; geologist/ terrain analyst; Ph.D., 1968, University of Michigan), whose Ph.D. Committee at the University of Michigan had included Ralph Baldwin (one of the earliest of the modern lunar geology pioneers-even prior to Gene Shoemaker), spent the



summer of 1965 in Flagstaff with the Branch of Astrogeologic Studies as a guest scientist. Pike was hired in 1967 by Hal Masursky to work for the Branch of Astrogeologic Studies doing lunar terrain analysis (Lunar trafficability) and photoclinometry in preparation for the upcoming landings of the Surveyor spacecraft. Dick, who thought he would be going to Menlo Park (not Flagstaff), arrived in Flagstaff just following the great 87-inch snowfall during the period 13-19 December 1967. Dick would officially enter on duty at Flagstaff with the Branch [after the snow was removed] in January 1968. Dick would later transfer to the Menlo Park office of the Branch of Astrogeology in June 1970.

The following was taken from an interview with Dick Pike by Gerald G. Schaber on 18 May 2001:

Pike: "I was born on 28 June 28 1937 on Nantucket Island in the State of Mass. We moved a lot during WW II. My father had a job for the Department of Agriculture. So, we ended up going all down the eastern seaboard, down to Florida, and then out to Ohio and then Illinois and stuff. We finally settled down in New York City in 1943 or 1944. We were there in Manhattan for two years, and then we went to Forest Hills in Long Island for two years; and then back to Mass., where my father came from.

Anyhow, I ended up growing through my teen years in a town called Melrose, a suburb North of Boston. When it came time to go to college, I didn't have much direction. But I felt that it would be fun to be in the Foreign Service. Tufts College, which was right near Melrose, had a Foreign Service school; so I applied and thought that perhaps I would major in history and maybe go to the school of Foreign Diplomacy and I would join the Foreign Service.

Well, it turns out when I showed up at Tufts my freshman advisor said well you know Dick you have an awfully heavy humanities load for your freshman year, so why don't you perhaps get your science requirement out of the way--why don't you take Geology?

It turned out that my geology-1 class was taught by the charismatic teacher, Prof. Robert L. Nichols. I just fell in love with geology. What really nailed it for me were the field trips."

*Schaber: "When did you start at Tufts?"*

Pike: "This was September, 1955. I did graduate; but believe me I did not graduate at the top of my class. Anyhow, here I was at the end of my senior year and I didn't know what the hell I was going to do. So I said well I'd join the Air Force; I'll do something military because I always thought that military was-okay. However, they wouldn't take me because I had one bad eye. So I had an operation and they tried to fix the eye--but that didn't work. So I thought that I would just go into Boston and get a job-- doing something--anything--just so I can just take a moratorium and think about what I'm going to do with my future. So I went to a couple of bookstores, and they were hiring. I ended up working as a clerk at a school supply shop in Boston; I worked there for a year."

*Schaber: "So this was like 1960?"*

Pike: "Yes, this would be 1959-1960! I began to think what the heck am I going to do? So I began thinking about geography. Now I don't know how I got thinking about geography, except that I had just fallen in love with topographic maps while I was at Tuffs--to backtrack a little.

I had a little fellowship during my last year at Tuffs working in the map room, storing all of the USGS topo sheets. And as I put them away and filed them I would look at them. I became fascinated with topography.

Anyway, a high school kid that used to come into the book store told me that he was going to go to Clark University because they had a great Geography Department. I thought that's not very far away. So I trotted over to Clark University (Wooster, Mass.) and interviewed there with Dr. von Volkenberg--an old Dutchman. He said that I had an awfully good background in geology, but you don't know anything about geography to speak of, except what you have picked up on your own. So he said read a couple of books on geography and we will let you in as a special student. I found out that meant that I had to do well or they wouldn't keep me. That's where I really began to work.

Professor Walter E. Wood, at Clark University changed my life. Wood was working with the U.S. Army Laboratories there at Natick, Massachusetts, a suburb of Boston. He was the terrain analyst of the Environmental Protection Unit there at Natick. Wood's charge there at Natick was to look at the environment and try to decide how the environment could be used to protect the soldier and make his personal and fighting environment more efficient. So, there were a lot of geographers there at Natick

What Walt and the others would do at Natick was, among other things, analyze topographic maps. They measured parameters from these maps and got these statistics that could say interesting things about topography.

During my second year as a graduate student at Clark, I needed to look for a thesis topic. Well, that coincided with Walt needing someone to work over at the Natick Army Laboratories. Walt hired me to work there during the summer of 1962. Using profiles from the topographic maps, they applied statistics to show that it was possible by looking at data at coarse scale to predict what you could see from smaller scale topographic maps. The military was very interested in this [Author's Note: as would the Branch of Astrogeology Branch be in the near future].

What Walt Wood and Joan Snell had done earlier at Natick Labs was to take a whole bunch of terrain samples all over central Europe. At each of these map points they measured on contour maps six parameters--slope, relief, grain, and that kind of stuff.

I was at Clark from fall 1960 to winter 1962. I finished my thesis and did the last bit of typing when I was at Michigan. My Master's thesis was "Landform Regions of Southern New England.

Then, through the influence of his old Mentor at Tuffs University, Bob Nichols, I enrolled at the University of Michigan in June 1963. Walt Wood had left the Natick Labs in mid-1962 and he

went to work for a think-tank--Cornell Aeronautical Laboratories (Buffalo, New York). They had lots of fat Army and Air Force contracts.

The summer of 1963 I went back to the Natick Army labs after his first year at Michigan about the same time as Cornell Aeronautical Laboratories had gotten a contract to do terrain roughness studies for the lunar rover vehicle from the Air Force (perhaps Wright Patterson). What they proposed to do was to take the brand new 1:1,000,000-scale lunar maps that had been turned out by ACIC (the Air Force's Aeronautical Chart and Information Center) in Flagstaff and St. Louis and do terrain analysis on them.

In 1964 Walt Wood asks me to come to work for him at the Buffalo Cornell Labs. Walt Wood proposed to divide the new Lunar LAC Charts into terrain regions and do terrain analysis on them--like they had done using the terrestrial topographic maps.

Well, the first thing I see on my desk when I get there in is this map of the Ptolemeus Quadrangle of the Moon. I said look at all of those "round" things. I thought these craters are the most interesting landforms.

I didn't have a clue as to whether they were volcanic craters or impact craters. That wasn't even on my radar screen as yet. What we were thinking about was the nature of random topography, continuous topography, etc., because that is what we wanted to quantify. At that point we were interested in roughness--not in how some landform (like craters) originated. That was something those geomorphologists did-right? We were terrain analysts.

Then in the spring of 1964, I began looking at the craters on the Moon. Coincidentally, I went to the library to try to learn a little bit about craters. I knew nothing at all about craters at that point. I read Gilbert Fielder's books and Ralph Baldwin's second "*The Measure of the Moon*" that came out in 1964. It was only then that I had become aware of Baldwin's earlier [pioneering] 1949 book "*The Face of the Moon*", which I subsequently got and read.

Ralph [Baldwin] had all of this old German data that goes back a couple of hundred years, and he plotted this other data up that he got from Gordon McDonald in the 1920s. I've had these brand new lunar maps with fresh measurements of depth and rim heights from shadow measurements, in addition to all the data that the Air Force had compiled. So I began to analyze these datasets. Although still quite immature about craters at this point, in 1964 I wrote "Morphometric Properties of the Lunar Surface--a Preliminary Investigation from Aeronautical Charts." Anyway, I just spent the rest of the summer plotting up this and that for various samples of craters, and looking at the various types of craters. This is before I discovered Baldwin's earlier book, which would have told me a lot more. I was very excited all excited about these craters. I had bowl-shaped craters, pan-shaped craters, and some broad, flat-floored craters. I could classify these things and come up with some new information. I thought this make a great thesis?

So I go back to the University of Michigan and begin my third year, and began thinking about the thesis. They said gee, this looks interesting but maybe you shouldn't just focus on classification of [lunar] craters. How are they formed anyway? I said some people think they are impacts and some people think they are volcanoes.

[Author's Note: Dean McGloflin died about the same time that Dick passed his Doctoral prelims; they replace him on Dick's thesis committee with Ralph Baldwin --known as the father of modern lunar science studies.]

Pike: "That was interesting because Ralph Baldwin was running the Oliver Machinery Company up in Grand Rapids, Michigan. I get this invitation to go up and meet Ralph at his company. I didn't even have a decent pair of shoes--poor graduate student! I borrowed a pair of shoes from a friend, drove my VW up there and met Ralph at his company. He takes me to lunch at his club, and then we go to his really lovely home.

So we sit on the sofa, we leaf through books and he shows me a lot of stuff. He gives me a lot of fatherly advice. He said from an established man to a younger man on the way up. He told me to watch out for certain people in the field who he thought were snakes; one of them he said was Z. Kopal. He thought Gene Shoemaker was wonderful. He said that Gilbert Fielder is a good man, but with a lot of bad ideas.

Now we are in the spring of 1965, and I sat down with Dr. Eschmann, my thesis chairman. He said well I like this latest thesis proposal but you know you really just can't do it here in isolation. We've got to immerse you in things lunar. There are three things you can do. One is to go back to Cornell Labs. The other two alternatives are you go to the Lunar and Planetary Laboratory at the University of Arizona in Tucson--where Gerard Kuiper is--or the USGS had a facility in Flagstaff. What's more, he said, I know Jack McCauley there.

That was the spring of 1965. I told Don that the Survey thing sounds good; so we can try for that. So Eschmann made the contact, and McCauley agreed to accept me at Flagstaff as sort of a guest scientist and observer in the summer of 1965.

So in May 1965 I get into my VW and head west for the first time ever. I hadn't even as yet met anyone from the Branch of Astrogeology. This was a totally new thing for me. I had filled out no forms; officially, as far as the government goes, I didn't exist.

So I drive to Denver on the way to Arizona to see somebody I knew there; then I drive down to Flagstaff. I got as far as Meteor Crater that first night. It was about 11:00PM at night when I arrived at the crater. I just throw my sleeping bag down on the ground somewhere near Route 66. I wake up in the morning with about an inch of snow. At night the stars were incredibly close. So then, the next day I show up in Flagstaff and am welcomed with open arms by Jack McCauley, and he introduced me to Larry Rowan. This was at the Arizona Bank Building. That's where everybody was. So I rented a room in the Mount Elden Apartments. I realized that this didn't cost much, but I didn't have much money. Oh, I had a little stipend from Sigma Xi. I got \$400-\$500 bucks from them.

I wanted a place to stay in Flagstaff that wasn't the Mount Elden Apartments; so the University--Arizona State College then-- in Flagstaff agreed to put me up in one of their rooms for the summer for just about free--and I could use the cafeteria there to get cheap food. So I was set money-wise.

Larry Rowan and Jack McCauley had me give them a journal-club talk--to just the two of them--about this 1964 report I had written [Morphometric Properties of the Lunar Surface]. I had sent copies of this paper to about fifty people, and McCauley was on the list. They were very interested in the terrain analysis; they didn't care too much about my crater stuff. You see, by then I was interested in my crater stuff, and not so much in the terrain analysis; but the Branch of Astrogeology people were interested in the terrain analysis [for lunar terrain roughness work related to the Surveyor landings as it turns out].

So I spent the first few weeks in Flagstaff with log-log paper plotting up depth-diameter and rim diameter ratios for different classes of craters. This is when I discovered that the Baldwin depth-to-diameter curve was not smooth, log, or hyperbolic--it was a dogleg thing with two straight legs that broke at a critical diameter--the so-called crater threshold diameter. So that's when I began to get together the idea for my dissertation---the origin and modification of lunar craters.

That summer of 1965 in Flagstaff was probably the most wonderful summer in my life up to then. While I was in Flagstaff that summer I attended the farewell party for Jack Schmitt. [Jack left in June 1965 for Houston to begin training as an Apollo scientist-astronaut]. Toward the end of the summer I drove down to see Bob Strom at the University of Arizona (Tucson); then I drove over to Menlo Park to see the Astrogeology people there. I had a car wreck. My VW was hit at an intersection not too far from the Survey [in Menlo Park]. In the meantime, Don Wilhelms put me up at his apartment there in Menlo Park. Don showed me around the city (San Francisco), which was really great. Then I had this meeting where I talked about my crater work I am doing at Flagstaff. I talked to Hal Masursky, Don Wilhelms, and Newell Trask--all the guys who were there at the time. They were very impressed and liked the report I had turned out in 1964.

At Flagstaff I interacted mostly with Larry Rowan and Jack McCauley; Jack was an old terrain analyst because he was at Columbia-and he knew Straylor's terrain statistics data. Jack and Larry had this [NASA] contract that involved using a computer card punch and reader; they were getting data down at the Arizona Bank Building. I recall that Joe-Ann Higley was there. I think she was sort of the curator of photographs up there on the fifth floor of the Bank Building. I met John Running up there, and John and I became very good friends. Don Hart had a younger brother [James A. Hart] who worked there with us.

McCauley was urging me to hold off on my thesis until they got the data from Lunar Orbiter. This was the best piece of advice I ever ignored!

Very late in the summer, I head back to Michigan, and on the way back I do something that is another one of the best things that I ever did. On a late Friday afternoon, I get to ACIC in St. Louis, which Hal Masursky had suggested strongly that I do on my way back. I tell them that I would very much like to get some raw topographic data that ACIC used in creating the LAC charts. They say sure, write a letter. I get back to Michigan and I draw up this list of craters for the LAC charts form which I would like data. They send it to me all of these LAC Charts with little dots on them and numbers, and a long list of two or three hundred craters.

Things began to happen quickly that spring--I think it was in St. Louis. Anyhow, there is a GSA meeting that I go to. I had sent Masursky copies of my depth-to-diameter graphs. He wanted to

hear more about them. I ran into Hal at the GSA, and he essentially told me that Astrogeology was going to hire me. I think this was spring 1966.

I decided on the crater depth-to-diameter ratio and "Schroeter's Rule" for my thesis. I needed an extra year in Ann Arbor, so I got a small grant for sustenance from the University while I write a paper. Danny Milton and Don Wilhelms reviewed it; it gets accepted in 1966 and is published the next year, in 1967. I come back to Ann Arbor in the fall of 1966, and I meet this graduate student Jane Nelson; we start dating.

When Hal Masursky had hired me in 1967, he was hoping that I would show up a little earlier. There was a phone call when he said we need you here as soon as you can. He said you would be doing terrain analysis on the lunar surface in Flagstaff for Project Apollo. I thought I was going to Menlo Park. I put the phone down, and I was almost in tears.

I had sort of fallen in love with Menlo Park, and when I knew I was going to Flagstaff, I said oh God, "I'm being sent to Siberia." Even though I enjoyed my summer there, we were literally dragged kicking and screaming to Flagstaff. What you need to know is, when we left two and a half years later, we left again in tears because we didn't want to leave!

So Jane and I get married in the fall of 1967. Jane and I drive out to Flagstaff at Christmas time of 1967--just after Flagstaff had a 7-foot snowfall [over a three day period]. We pulled into town and right away get a second-floor apartment in the Fort Valley Apartments [on route. 180--just northwest of downtown Flagstaff].

I show up at the Branch and Larry Rowan says we have these two projects. We've got lunar terrain analysis, and then there was Lunar Trafficability. They were both well funded, but there was no staff. I was it!

Hal Masursky said that we need all of this stuff for Apollo, and we've got this thing called photogrammetry, which we can turn lunar shadows into topographic data, with which you can work your magic. I said okay. To work your magic, Hal said, we will give you this assistant who is a mathematician, who can write programs for you. So, this young guy Steve Wilson [a recent graduate from NAU with a degree in mathematics] shows up in my office one day. Steve he is incredibly self-confident, but very delightful person to be with. He is teaching himself FORTRAN while he is writing code for me. Other people who worked for me--along with Steve Wilson included Louie Middlestob (Geology major at NAU) and Paul Bateman Jr. [who had arrived in March 1970].

To make a long story short, Steve was writing [FORTRAN] code at that time [1968] that was so advanced that had we published it at the time we [both] would have been very famous. It was that good. I didn't know what I had.

I received my Ph.D. from Michigan in the spring of 1968

The first program I had Steve do was an elevation analysis. I had him compute Straylor's Hypsometric Integral. Then, we computed the Walt Wood parameter that was called the elevation-

relief ratio, and we kept getting very similar numbers. I asked Steve if he could write a proof to demonstrate that these parameters are identical. He wrote the proof. That was fabulous! I submitted a paper to GSA Bulletin with Steve--Pike and Wilson--and it was eventually published in 1971.

Our real job was, of course, to analyze the Moon--not the Earth. And to do that we would get these photogrammetry profiles from the Photogrammetry Unit [at Astrogeology--which Barbel Lucchitta was running that at the time. Neil Gambell [arrived October 1967] worked for her. Barbel was properly skeptical of photogrammetry!

What would happen was that Hal Masursky would come into my office--or get with me on the phone--and say we have a landing site we want you to look at--or a whole bunch of landing sites--2-P-3, 3-P-8, etc. My job was to deliver roughness statistics so they could compare them.

[Author's Note (also in main text): Mathematician Wesley A. Rozema (Professor, Northern Arizona University, Flagstaff, Arizona), who started work at the Branch of Astrogeology back in July 1964, also played a very important role in the terrain analysis work that Dick Pike accomplished during the two and a half years he was at Flagstaff, and in subsequent years. Wes was a professor of Mathematics at Northern Arizona University (NAU) in Flagstaff at that time. The Survey had hired him to do some work even before Dick's original summer of research in Flagstaff. Rozema applied the power spectrum, or Power Spectral Density (PSD) to Pike and Wilson's (already complex) terrain analysis program. PSD is the descriptive technique that comes out of signal processing. It essentially breaks down features along a topographic profile into sine waves of different amplitude and frequency. Wes wrote a program for Dick that takes a lunar terrain, or any profile-- and produces this power spectrum. Dick and Wes did some real topography out east of Flagstaff to help calibrate the technique. They had the Surveyors from Astrogeology gathering topographic data out near Roden Crater [currently a local work of art/sculpture] and other places] (from an interview with Dick Pike by Gerald G. Schaber on 18 May 2001).]

Pike: "The Power Spectral Density, or PSD, as we called it, was a really important program. Like I said, the Branch of Astrogeology had two contracts when I first arrived at Flagstaff, one was lunar terrain analysis, and the second was lunar trafficability. The second turned out to be very important and it had to do with the Lunar Roving Vehicle (LRV). NASA needed design criteria so that they could build a vehicle that wouldn't turn over or bog down, or do other dumb things when it got up on the Moon in that 1/6-gravity; and they were particularly concerned with the nature of the wheels and the suspension. Our PSD data was used to calibrate the suspension system-- the spring rates and bounce--and all the other things that engineers have known about for quite some time.

Larry Rowan, Jack McCauley and Ester Holm also published a paper called "Lunar Terrain Analysis." They were doing lunar terrain analysis of the lunar equatorial belt based from telescopic data.

At the time my wife Jane went to get her Ph.D. at Stanford University, so we moved to Menlo Park, California in June 1970. With Hal Masursky's blessing, I simply transferred to the nearby

Astrogeology offices there in Menlo Park. I was initially assigned a 1:1,000,000-scale lunar quadrangle--the Hyginus Rille Quadrangle. I also continued his statistical work on lunar craters and greatly expanded my work on statistical comparison of terrestrial and lunar geologic features--and especially--showing statistically-- the morphometric differences between impact craters and volcanoes. I introduced into this effort the use of principal-component analysis and cluster algorithms to group the various samples. I gathered depth/diameter, rim width, rim height for lunar craters, along with a lot of terrestrial analogs (i.e., volcanoes, caldera, etc.). I eventually published this work in 1972 in the *Proceedings of the International Geologic Congress*.

I also accomplished some important work on comparing impact craters on Earth, Moon and Mercury, and the [still mysterious] square root of two spacing of impact basin rings. The study comparing craters on the Earth, Moon and Mercury I carried on even after I made the break with Astrogeology. The last thing I did [for Astrogeology] was to write a big paper on the craters of Mercury where I summarized a lot of his crater data. That was--about 1988 or so--after I left Astrogeology.

After departing the Branch of Astrogeology, I began to get a lot of positive support from Earl Brabb, Karl Wentworth and Steve Ellen of the USGS to do terrestrial terrain modeling. That period was between 1985 and 1988. I got in on the bottom floor of the Survey's big Geographic Information Systems (GIS) and Digital Elevation Model (DEM) work. Between 1987 and 1992, I worked in collaboration with William Acevedo and Gale Thelan. This study culminated in the U.S. "Black" map that Gale Thelan and I did--the terrain relief, and image map of the United States--which turned out to be a significant and highly-visible product for the USGS..

[Author's Note: At this writing, Dick Pike still works for the USGS (but no longer for the Astrogeology Team) in Menlo Park, California.]

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**Howard "Howie" Pohn** (geologist; b. 1935; B.S., University of Illinois; also attended Purdue University, University of Chicago, and Caltech)

The following was taken from an interview with Howard Pohn by Gerald Schaber on 6 March 2001:

Pohn: "I was born in Chicago, Illinois, on 17 August 1935 and went to grammar school at National College of Education in Evanston, Illinois until the fifth grade. My parents moved to Highland Park in 1945, and I went to high school there. I graduated there in 1953 and enrolled in Purdue University. I started in metallurgical engineering, and then took a course in ore minerals. I fell in love with that during my sophomore year, and switched to geology.

Purdue at the time--being a land grant school--had a deal with Indiana University that whatever major was being taught at one school could not be taught at the other school. Indiana University was teaching geology--and so Purdue couldn't. They could teach it, but they couldn't give a degree in it. So, after my sophomore year, I switched to the University of Illinois and got my B.S. two years later. I also went to graduate school there, and had what they call a personality conflict with the department chairman. I had already been accepted at the University of Southern California, for



my Ph.D.--so I left Illinois. I was married in 1957 (to my wife Sheila). We moved to California in 1958 or 1959 and I was in graduate school there (USC) working on my Ph.D. when my old department chairman from the University of Illinois came out to visit the head of the Department on Climates at USC. And when Tom said we have one of your former students here, Howard Pohn. He said, get rid of him. So, he screwed me up in Illinois and USC as well! So, I ended up with three and a half years of graduate studies--with only a B.Sc. Degree. Anyway, I worked for a soils-engineering company for a while. Sheila was pregnant at the time with our oldest daughter Maurine.

I had gotten interested in astronomy and had bought the [Gerald] Kuiper Lunar Atlas; I had my little 4-inch telescope that I used to put out on the balcony to look at the Moon, and make sketches and stuff while in Los Angeles. I had submitted a job application with an employment agency--and they sent me up to Caltech who was looking for an astronomer--not a geologist. But when I brought up the work that I had done on the Moon--it was Bruce Murray's lunar lab [at Caltech] that hired me. Gene Shoemaker had started his [USGS] lunar lab at Menlo Park at the same time as Bruce Murray did at Caltech. Very quickly, I became the liaison between Caltech and the USGS (in Menlo Park), and had to arrange with Elliot Morris and Hal Stephens to copy (for Gene) the whole Mt. Wilson and Palomar photographic plate collection of the Moon at Caltech [see 1962-63 in main text].

I was pretty much dead-ended at the end of the three years that I was at Caltech because I didn't have my Ph.D. Bruce [Murray] came to me and basically told me that. So, I called Gene Shoemaker and told him my situation. He said do you mind coming to Flagstaff? And I said "heck no." He said okay, you're on at the USGS in Flagstaff! I moved to Flagstaff in 1964 [into the Astrogeology wing at the Museum of Northern Arizona]. When I got there, I believe there were twelve people at the USGS. Let's see there was Gene Shoemaker, Don Elston, Elliot Morris, Ken Watson, Hal Stephens (who arrived in June 1964), Dick Eggleton, Russ Wahmann, Ray Batson, Jody Lowman (later Jody Swann) was Gene's secretary. Kay Edwards may have been there.

Gene was my supervisor. My very first assignment within weeks of the time I arrived in Flagstaff was the Ranger VII impact on the Moon [28 July 1964]. Don Lamb [on temporary from Bendix] and I got sent back to Heightstown, New Jersey, to pick up the film, and Gene was so particular about this. This was the first successful hard lander. He was so worried about what might happen to the film that he had Don and I come back on separate planes; he had half [of the film] and I had the other half. I guess at the time, NASA hadn't released the images yet. We had been given one of the three secondary sets made from the original nine. We were in charge of printing the whole thing. We made books and books of these damn prints--most of which didn't show any differences until you got to the last hundred miles out from the lunar surface.

But very shortly thereafter, the photolab [at Flagstaff] grew. Actually, by the time I had been there a year; Astrogeology in Flagstaff had grown from about twelve professionals to 125 of us. There were all kinds of support people. Walt Fahey [machinist] came onboard in the instrument shop about two months after I arrived.

After Ranger VII, the first thing I got involved in was lunar mapping with Don Wilhelms; I worked on the Macrobius Quadrangle, and later the Tycho Quadrangle. We moved into Building One on McMillan Mesa, and then, in 1966 or 1967, moved down to the Arizona Bank building.

And so I mainly worked on lunar mapping--and then [Lunar] Orbiter came along. I was peripherally involved in Lunar Orbiters I, II and III, but I was closely involved in Lunar Orbiters IV and V [Author's Note; see 1967 in main text for the story of how Howard Pohn actually help save the Lunar Orbiter IV photographic mission]. About that time--right at Orbiter IV--was when Terry Offield came onboard [with the Branch]. Terry and I got teamed up on the fourth floor of the Arizona Bank Building.

Oh, before Lunar Orbiter I was working on the albedo map of the Moon; Bob Wildey, Gale New (who later married Bob Sutton), and me. That map is what we used to set up the exposures for Lunar Orbiter photography.

I was also doing the lunar quadrangle mapping; and later on, worked on the Apollo 12 landing site. Tim Hait and I were doing the Apollo 12 landing site work together."

*Schaber: "So, I recall that you were using the Lunar Orbiter survey photomosaics trying to do a terrain age-dating thing. What was that about?"*

Pohn: "What occurred to me--it was kind of weird. We were assembling the pictures. I forgot what his first name was--but his last name was Graham. He was the one who was in charge of processing the images [at JPL]. He was a photo specialist. He'd bring out the Lunar Orbiter [LO] strips and assemble them on the plastic sheets; that's when we would have a chance to look at them [at JPL]. It was very interesting because we were photographing the back side of the Moon and somebody would come in and say how big of an area are we looking at? I would answer about 400 km on a side, or something like that. Then people would ask how do you know?" And I was thinking--how do I know? Then it finally occurred to me after studying it very carefully that craters that were perfectly circular without any major central peaks or slopes got up to about 20 km in diameter, and for bigger craters, things changed. I then started to compare the craters on the backside with the craters on the front side. Sure enough, it turned it to be right--any crater below 20 km was essentially circular, and any crater larger than about 20 km started to become hexagonal or octagonal with [wall] terraces and stuff like that. So I realize that you could date stuff --relative date surfaces by the freshness of the craters, and by their size. So, I was able to make up essentially a paleontological chart--you know how the paleontological charts are when species bloom and then they die out. We went to check out this idea on crater rays, crater walls and terraces, and central peaks---depending on size and degradation.

About that time, Terry [Offield] got involved with the crater work. Terry and I looked at very small craters that were on ejecta blankets; for instance, on the Imbrium ejecta blanket. We determined that the smallest-size [crater] that we could see was down to about a kilometer on this surveying of the whole Moon [LO IV images]. You could see that the very oldest stuff looked far older than we knew that Imbrium was. So, we showed that as you got to very small craters, the processes of degradation [mostly micrometeorite gardening on the Moon] occurred much more

quickly, but you could relatively date an entire surface; in this particular case, one could date everything on the Imbrium surface based on the oldest-appearing craters.”

*Schaber: “So, we were using this degradation model in the Branch’s lunar geology mapping program while you were still developing the model--as I recall.”*

Pohn: "Yeah, indeed so. We were able to use the model for mapping surfaces of ejecta blankets, as well as [relative] dating of large craters and small craters.”

*Schaber: "So, there were some people coming through [Flagstaff] looking at these things and you would describe what you were doing.”*

Pohn: "Oh Yeah, very regularly. I remember one meeting--I think it was at the Arizona Bank Building, but it could have been up on McMillan mesa [Building-One]--when astronaut Walt Cunningham came by. We there discussing all the information we found out about the [geology of the] Moon-- and what they should be trying to do [observe] when they were circling the Moon. Cunningham essentially said I am going to do what I'm going to do--I don't care what you guys want to know. [Author’s Note: But that was Walt Cunningham’s opinion of most anything “scientific” from what I understand].”

*Schaber: "So what do you do after Lunar Orbiter--when the Branch started getting into gear on training and planning the traverses for Apollo?”*

Pohn: "Mainly, the stuff I got involved in was because of the close relationship that I had with Terry [Offield] that had started during Lunar Orbiter. Terry had just finished mapping the Sierra Madeira structure [in West Texas] with Howard Wilshire and Keith Howard, and he then got assigned to work on Decaturville [impact structure] in Tennessee.”

*Schaber: “What year was this about--right before Apollo?”*

Pohn: "It was 1968, and I got assigned to work with Terry on Decaturville; here, I really found myself structurally [in a geologic sense]. I started becoming a structural geologist in 1968, although it had interested me in college. I really found myself doing the structure at Decaturville because it was really complex, and I just took to it like a duck to water.

We started Decaturville in 1968; we finished the field work in 1971 and finished the paper in 1972.”

*Schaber: "And after that, what did you do?”*

Pohn: "Well, I think in 1970, several of the Branch professionals [Author’s Note: Larry Rowan, Ken Watson, Bob Watson, and Terry Offield left to go to Denver [to be part of a new terrestrial remote sensing group under Ken Watson]. I stayed in Flagstaff a year longer, and then left in 1971 to join the remote sensing group in Denver.

We were mostly doing infrared studies, including overflights and trying to determine if we could tell the difference between rocks like limestones and dolomites--depending on their thermal heating-and-cooling curves. We were doing that study in the area around Sulfur, Oklahoma. We had overflights there and up near Red Lodge, Montana.

Well, after about six years there [in Denver], I was just in a dead period. I wasn't being productive of anything else, and the things we were doing were just deadly dull. Ken Watson came to me one day and said, you have the choice of two things--you can either join another Branch, or you can go to [the National Survey headquarters] Reston, Virginia. I scurried around Denver for about three weeks trying to find another job with another Branch--and nothing was open at the time. So, I opted to go to Reston. It turned out to be the best decision that ever got made for me, because I really found myself doing structural geology in the Appalachians.

Well, Larry [Rowan] had also transferred from Denver to Reston [in 1972]. Larry, a few other people, and I were doing remote sensing research there. One of the things we got assigned to do was looking for a repository for spent nuclear materials. One of the sites that were being looked at was the Salina beds [a salt-bearing rock] in New York State. One of the things we were looking for on the plateau there was seeing if strike measurements could be used as an indication of what was going on in the subsurface. So we started up in the Finger Lakes area doing joint measurements. There was a team composed of Melvin Podwysocki, Larry Rowan, Dennis Kron, and myself-- and probably one or two others who I can't think of off hand. We got into joint mapping in the plateau, and gradually me and my various field Assistants got down into the Valley and Ridge. Here, I started to find these structures (which later I called disturbed zones) which are surface indicators of deep-seated faulting. I went and talked to Len Harris, who was "Mr. Appalachian" for the Survey; he said go with it, I'll arrange to get you funding to work on it. So, I started on these fault systems in the Valley-and-Ridge of southern New York, and ended up just outside of Knoxville Tennessee. I was doing that right up until the day that I retired.

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**Robert D. Regan** (geophysicist; Ph.D. in geophysics/electrical engineering, Michigan State University) arrived in Flagstaff to work with the Branch of Astrogeology in April 1965. Between 1965 and 1978 (when he left Flagstaff), Bob was primarily involved in magnetic surveys of impact and volcanic craters for use in astronaut training. He participated in numerous geology/geophysics training trips for NASA astronauts while he was with the In situ Geophysics Project within the Branch of Astrogeology and its sister Branch of Surface Planetary Exploration (in existence only between 1967 and 1974).

The following was taken from a post-Flagstaff biography of Robert D. Regan on the web at (<http://www.sis.pitt.edu/~rregan/reganbio.html>)

Bob, after leaving Flagstaff following Project Apollo, served in several staff positions such as chief of the satellite magnetometer studies project. His research in this area demonstrated the application of satellite magnetometer data to solid earth studies and provided the basis for NASA's MagSat program. One of the best known results of Bob's work is the discovery of the Bangui anomaly. This satellite magnetic anomaly is situated over Bangui, Central African Republic. A

global magnetic anomaly map produced by Regan and his co-workers and the analysis of the Bangui anomaly provided the initial impetus for a new satellite program by NASA.

From 1978 to 1980, Bob Regan was Director of the Earth Sciences Division and Vice President of Phoenix Corporation in McLean, Virginia. Regan basically established the Earth Sciences Division and brought it to an international position in a few years with research contracts with leading U.S. Government agencies and application contracts for foreign government agencies such as the Kingdom of Jordan and Saudi Arabia. Bob was responsible for the scientific, technical, and management aspects of such projects, and was singularly involved in other projects such as the review of the use of magnetics in the Department of Defense.

Bob Regan joined Aero Service (the largest geophysical service company in the world) in 1981 as Vice President and Chief Scientists. As such, he supervised geophysical, remote sensing, and synthetic aperture radar programs. Prior to joining Aero Service, he was Senior Vice President of Barringer Resources where his attention was focuses on management and administration areas.

Bob later founded a company of scientists, programmers, and mathematicians that consulted with companies on various earth-science issues. The company developed and marketed a library of several hundred subroutines of theoretical and practical mathematical techniques for analysis and interpretation of geophysical data.

In the early 1990s, Regan served as Director of the GIS Center at Boston College wherein he designed and developed a geographic information system (GIS) and program for the earth sciences. Before joining Boston College he was a private consultant whose principal focus was in geographic information systems, remote sensing, and the geosciences. Actively involved in proposal and report writing and in training programs, he designed and developed geographic information systems for geotechnical, agricultural, and environmental applications.

Bob has since relocated to Pittsburgh, Pennsylvania. He has worked there as GIS Program Manager, establishing a GIS presence in several companies, and has been responsible for GIS projects for state agencies, tool road authorities, and municipalities. Regan has also serves as an adjunct professor in GIS at the University of Pittsburgh where he has conducted several GIS short courses.

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**Carl Houston Roach** (geological engineer/geophysicist) (b.1926, Ph.D., 1950, MS University of Oklahoma):

The following was taken from an interview with Carl Roach (Grand Junction, Colorado) by Gerald G. Schaber on 26 March 2001:

Roach: "I was born on July 1, 1926. I went through all of the school system in Oklahoma City and then attended the University of Oklahoma where I graduated in 1950 with an MS degree in geological engineering. Actually, I started out in Electrical Engineering but switched to Geological Engineering in my second year. I graduated in 1950, but I stayed on [at the University] for another year because jobs were pretty hard to find at that time. Even in the petroleum industry

there was a temporary down. So I stayed on for another year and did the class work for a Master of Science degree in Geological engineering; I finished everything but the thesis; and I left and took a job with the Geological Survey. I took the exam, the Geological Survey exam, and I took a job with the Geological Survey in Lincoln, Nebraska. My first assignment was to determine the recent rates of erosion in a river basin in Wyoming. I stayed there one year and then transferred to the Colorado Plateau Uranium Project Grand Junction, Colorado.]

When we showed up in Grand Junction in the summer of 1952, we had to have a place to live. As luck would have it, Gene Shoemaker was out in Nevada in the summer working for Jim Gilluly, and we rented his place. That's when I first got acquainted with him [Shoemaker]--when he came back from that field season. So, we were on the Colorado Plateau. I went on to uranium exploration--the drilling exploration--and was down at Uravan, and then later at Naturita in southwestern Colorado.

The Uravan Mineral Belt was what we were working in, all along the western border of Colorado. During that time, of course, Gene Shoemaker was the big gun of the whole thing you know, and I got acquainted with him. When my drilling project came to an end in--let's see, at that time it was Monogram Mesa (I drilled some of the first deep holes in the overlying rocks that they hadn't drilled before). When that project came to an end, I was ready for reassignment. That must have been 1957. So, in 1957, I was ready for a new assignment.

Our boss, USGS geologist Arnold Brokaw, assigned me to join Gene Shoemaker in Hopi Buttes, Arizona. At that time, Gene had a project going mapping the Hopi Buttes down there, especially the diatremes.”

*Schaber: "Still looking for uranium-correct?"*

Roach: "Yes; my assignment down there was kind of odd. The assignment that was made by the manager of the office here was to study the uranium of the diatremes; but when I got down there Gene said well, in addition to that, I wanted you to map six quadrangles!"

*Schaber: "Is that all?"*

Roach: "Well, that led eventually to a big field trip of all the management of the Survey coming down there. There were about twenty big shots from the Survey coming down, and of course the program came to an end in 1958.

When Arnold Brokaw left, he said Carl I want you to remember that your assignment is not quadrangle mapping, but to study the uranium in the diatremes. So then, as soon as I left, Gene started banging on me again about the [quadrangle] mapping. That was kind of odd. But at the end of that season, one of the funny things I remember about that was when we got back in the office after that field season--that would be the early winter of 1957. My office was right across from Gene's. I remember him picking up my maps. I knew he had my air photos. He was looking at them; and he walked up to the window to the light you know. I had about three or four square inches mapped--that was all. So, when we went to lunch Gene said well I guess I didn't make a quadrangle mapper out of you.

Then the Survey's part of the uranium exploration program closed down in 1958, so I was transferred to Denver to the Nevada test Site Project. First, I should say that while I was on the Plateau I was working on physical properties of the rocks adjacent to uranium deposits. My effort was to try to find a guide to ore. So I got pretty heavily involved in the physical properties: porosity, permeability, and a number of the other physical properties of rock. I guess that was responsible for me being assigned when they had to find assignments for people when the Survey was closing its office here in Grand Junction. Brokaw assigned me to the Nevada Testing Project--the underground nuclear weapons testing project that was going on. My assignment was to develop a physical property laboratory in Denver to study the physical properties around the underground nuclear explosions."

*Schaber: "Was this the MICE Project?"*

Roach: "Well, that was the same time that Gene was on the Project MICE (Megaton Ice-Contained Explosion); however, Gene's project was a separate one. So this work was on a separate project that the AEC developed with the Survey to study the underground nuclear chambers on the test site. But, of course, Gene was very heavily involved in those projects too; so I began to get my first real association with Gene--in a professional sense--there after one year on the diatremes down in Hope Buttes, Arizona [northeast of Flagstaff].

During the progress on that report on the physical properties, I got very interested in shock in rocks, and what shock might do to the physical characteristics of the rocks that we might use for [nuclear] detection purposes. So, I just kind of bootlegged some experiments. One of the experiments that led to my future close relationship with Gene was when I decided that shock in some rock types ought to modify the electronic energy of some of the molecules of certain minerals.

So, as one of my experiments, I took a block of marble from Marble Colorado. I went up to Marble, Colorado, and got some blocks of marble that they had mined there from the old quarry. The only way I had at that time to shock--all of this was being bootlegged and I didn't have a budget for it--was to use my deer-hunting rifle. One of the fellows working in my lab there went to the guard service there at the Denver Federal Center and got me some armor-piercing bullets. I shot one of those bullets into the flat face of the marble and took samples before and after the shot. I saw a significant difference in the thermal luminescence of the marble that I related to the shock. So things went on for a little while. The next thing that happened was I got a visit from Frank Senfle. Frank Senfle was a solid-state physicist with the Geological Survey. He had his office at the National Bureau of Standards. He did a lot of work with them. Well, he happened to be visiting, and he came in to see me.

Frank asked me what I have been up to, and I showed him a number of things I had been working on. However, when I showed him the results of the shock-induced affects on the marble, he got really interested in it. At the time, I didn't really know what his interest was based on."

*Schaber: "What year was that?"*

Roach: "That would have been around 1959, or early 1960."

So he left and he said well you'll hear from me. I thought well, I may hear and I may not. But, not long after that, along about 4:15 PM, Gene Shoemaker called and said I want you to meet me on the midnight express. That was the plane that left from midnight from Denver for Washington D.C. That was before jets, so they flew all night long. I said what's the deal? He said Lorin Steiff called. At that time Lorin Steiff was the Chief Scientist at the Air Force. He had previously been with the Geological Survey and, in fact, had been out on the Colorado Plateau. You might possibly remember him through the age determination on the uranium ores--Steiff and Stern did the original work on that.

Anyway, I guess Frank Senfle had told Lorin [Steiff] about my work--and of course I didn't know what Lorin was doing at the time--but Gene said I've got to meet you--it's real important--I got to meet you at midnight on the plane. We are going to Washington and we are going to the AFTAC--that was the Air Force Technical Application Center, and I want you to give a talk on the work that you did on the shock in rocks, and particularly the shock in the marble. I said I don't have money to travel. Gene said well use this number on your TR (travel request). I said oh, you have the money? Gene said no I don't have the money but we'll get it later.

*Schaber: "That was Gene!"*

Roach: "Yes, that was Gene--always on the cuff!"

Anyway, we flew in there. We flew all night long. I found myself kind of in a ridiculous position that I learned later was not unusual with Gene. We walk into the AFTAC, both of us needing a shave; both of us tired and red-eyed you know. We got in there; and after the introductions and all the niceties and everything, Lorin said Carl I would like to hear about the work that you did on the shock in the Uel Marble. Fortunately, before Gene called, I had made some slides of that because I was going to give a talk to the local Survey group there in the Federal Center. So I gave about a fifteen or twenty minute talk on that, and showed the dramatic change in thermal luminescence that was created by the shock of the 30-06 bullet impacting on the marble. I showed the before and the after photographs.

Well, the General that was sitting there started asking me a number of questions like--would larger shock cause similar effects--or maybe even larger? I said all I can do at this time is guess that it probably would. It would show the effects of the crater further out than the little four-inch crater that I got from the impact of the projectile that I fired. That seemed to interest him greatly."

*Schaber: "I'll bet it did!"*

Roach: "All of this time I didn't really know what they were up to. But then, at the end of the talk after he had questioned me more, he said well we've got this [Nuclear] Test Ban Treaty thing hanging fire and we don't want to sign that thing until we know we can detect any clandestine explosion. Would you be willing to take on a project of near-in effects? The seismologists have



gotten the technique down now where they can get within a thousand yards or so of a nuclear explosion, but we need some technique that will recover the radio-nuclides created by the explosion. That would be the only sure, definitive test.

I said as far as I'm concerned I would. So then Lorin Steiff looked at Gene, and Gene said yeah we'll take it on! So then the General said well okay, then we're settled. Lorin Steiff had talked to Gene before we had gotten in there, but he knew that there was a financial problem [from the USGS point of view]. So Lorin said well General, there is a problem. There is a financial problem here. He didn't tell him that we didn't have the money to even pay for the trip we were on! The General said well that's no problem. He said Lorin call (somebody) (the comptroller there in their office) --I talked to him just about an hour ago and he had about a hundred thousand dollars that was uncommitted. See if he's still got that. Lorin called the guy and then said [to the general] yes he still has it. He said well assigned that to--what's your full name son? I said Carl Houston Roach. He said okay--that's it--so you're on the program.

I asked when we might start on the program, and when those funds would be available. The General said they are available right now. Then Lorin said, well General its not quite that simply, Carl and Gene work for the Geological Survey and we've got to get the Director's approval to start this program.

But as luck would have it, the Director at that time was Vince McKelvey, an old friend and great supporter of Gene, so that was taken care of right away. So when we left there we had enough money to pay for the trip for both of us, and to get started on the work. That's how I got started.

At that time I was a Geophysicist with the Theoretical Geophysics Branch of the Geological Survey. So then Gene started an immediate campaign to transfer me to Astrogeology. Astrogeology at time was just Gene and two or three people out at Menlo Park [as the Astrogeologic Studies Unit]. I said well, the only thing is Gene I would like to join the Branch but I'm not going to leave Denver. He said well I'll promise you that you will never have to leave Denver; but let's transfer you to Astrogeology and get this [project] in house because I want you to work on some other things.”

*Schaber: “What year was it when you joined the Branch?”*

Roach: "I went over to Denver in 1958--then 1959, I must have gone over to Astrogeology in 1961.

So I agreed to join the Branch but I got a firm commitment out of Gene that I would never be asked to transfer out of Denver where I had all of my analytical support and everything that I thought was important to the kind of work I was doing. To make a long story short--Gene never gave up trying to transfer me to Flagstaff. That ultimately led to our separation.

I went on with that work and started working on the impact craters [applying his thermoluminescence work]. Lorin Steiff, in the meanwhile, transferred from AFTAC Headquarters over to an agency created at that time called ACDA, the Arms Control Disarmament Agency.”

*Schaber: "So how did Ed Chao fit in? Did Ed Chao work with him [Lorin]?"*

Roach: "Oh yes Ed Chao was real close with him and I, of course, visited with Ed Chao in Washington on the work that I was doing. So Lorin transferred to ACDA, and then the sponsorship of my project transferred from AFTAC to ARPA (Advanced Research Projects Agency). I was assigned project 7.9 that was near-in effects on underground nuclear explosions.

Then I did a number of things down at Sierra Madeira, Texas [near Ft. Stockton at N. 30 degrees, 36 minutes; W. 102 degrees, 55 minutes; 13 km diameter; <100 m.y.] and I did some drilling down at the meteorite craters in Texas [N. 31 degrees 45 minutes; W 102 degrees, 29 minutes; 0.168 km, < 0.05 m.y.]. So I was working on meteorite craters while I was working on the nuclear explosions. ARPA wanted to know if there were any geologic examples of shock on rocks that might be misinterpreted as being the effects of underground nuclear explosions. So one of the things I got involved in was looking at the San Andreas Fault--for one thing. Years before, Tom Dibblee discovered the intersection of the San Andreas and the Garlock Faults wrote a GSA Bulletin on that. Anyway, he surmised that since those lateral faults were moving in different directions at right angles, that alternate quadrants ought to be in tension and the other alternate quadrants ought to be in compression.

Well, at the time I was expanding my studies from thermal luminescence to other physical characteristics. One of the things I got interested in was *in situ* stress, and how it could be measured by over-coring strain gauges where you'd smooth a piece of rock in place and cement strain gauges on it. Then, I would make it thermally stable by putting an umbrella over it and wait until the thermal equilibrium was arrived. Then, you'd measure the resistivity of the strain gauges. Then I designed a core, a four-inch diameter-core that would over-core the strain gauge and then would measure the stress relief. I was able to verify Dibblee's conclusions on the San Andreas, and extend the study into some of the plutons in Colorado. One of the professors at the Colorado School of Mines had surmised from geologic fieldwork that those three plutons (the Silver Plume, Pikes' Peak and the Indian Hills plutons) had crystallized at different depths. So, I thought to test this theory (i.e., that the plutons did crystallize at different depths, I should be able to tell that with strain gauges. So, I was able to do that."

*Schaber: "So you were doing this research while still working for Astrogeology?"*

Roach: "Oh yeah! You know, one of the things about working with Gene is that he never established any limits."

*Schaber: "He was interested in everything!"*

Roach: "He never had any limits at all; in fact, he always pushed you to do things."

*Schaber: "Of course, he [Gene] had self-serving interests in all of this shock work, including Sierra Madeira and the work at Hopi Buttes. It wasn't the uranium thing, but the impact-shock thing. So somehow, you all got into shock in quartz and shatter cones, and that type of thing."*

Roach: "Yes, that's right. It is my understanding that he [Gene] made a fundamental contribution to the initial underground nuclear testing there at the Nevada Test Site. The first test they had, I think it actually was an explosive test, not a nuclear test. The first question was if we dig a tunnel there into the Oak Springs Tuff cliff there on the Test Site, can we just go in there and block that thing off with cement, with grouting and block that tunnel off and set off a nuclear explosion. In some of those diatremes that we were mapping down in the Hopi Buttes Volcanic Field, Arizona, they had rocks in the throats of those [diatremes] exposed at the surface. They had rocks there that were all the way down through the section; and things that we couldn't identify. We didn't know where they came from."

*Schaber: "From down in the Mantle!"*

Roach: "From the mantle, yes. So Gene was saying that's where he developed his concepts of the fluidized system and how powerful it could be. It is my understanding that Gene was the first one to realize that. The first shot that they had, it blew the shot doors across the canyon. It would have vented if it had been a nuclear explosion.

It is my understanding that Gene suggested that they make a hook in these tunnels. When they get down to where they are going to shoot it, they make a big loop and a hook, and come back in such a way that the shock wave will reach the main tunnel before anything from the main chamber could get there--going around the hook. So that's how they did all the shoots after that--so the whole thing was tied together.

At the time we were at Hopi Buttes, we'd work part time on the diatremes, and then we'd run over and work a few days on Meteor Crater."

*Schaber: "Now tell me about Meteor Crater and how Gene first got involved in that?"*

Roach: "I was with Gene down there in Hopi Buttes in the summer, fall and winter of 1957, and up to the spring of 1958. Then, the program came to an end in the summer of 1958. At that time, Gene was tying--conceptually--his work at Hopi Buttes with Meteor Crater. But Gene's main interest in Meteor Crater was first to prove that it was an impact, and secondarily to work out the dynamics of the [impact] explosion and to calculate what the velocity might have been and the mass or size of a meteorite that might have created the crater."

*Schaber: "But he really thought that it was an impact all along?"*

Roach: "Yeah, yeah, he did!"

*Schaber: "Because Danny Milton tells me that when he first started working with Gene, he wasn't sure, he wasn't so sure it was."*

Roach: "Well when he first saw it, of course, it was a big debate you know. I think it was [Walter] Bucher who classed it as a cryptovolcanic structure. There were a number of them [cryptovolcanic structures] around the country; Gene finally followed up working on these with a number of graduate students.

When I was working at Meteor Crater with Gene in the fall of 1967, we would map there all day until just before sunset, and he would say now is the time to look for impactites. I would say, what are impactites? He said that's fused Coconino Sandstone. He says if you wait until the light gets low right before sunset, you can just look across the terrain there and you can see them or spot them--just one right after another. So of course, that turned out to be true.

So I think that's when he first became convinced that something had fused those pellets of Coconino Sandstone, and that eventually led into his working with Ed Chao and the others."

*Schaber: "So you collected a lot of this stuff, and that's what they used to find the Coesite."*

Roach: "Yes, that's right. When the sun would get low just before setting, you could see those things that you couldn't [usually] see. It was something about the texture and the way the light hit them. Gene had figured out that there is no sense looking for them until about ten to fifteen minutes before sunset; and you could then go out and pick them up a hand full. So that got him [Gene] involved with Ed Chao, and the resultant discovery of Coesite, and Stihovite."

*Schaber: "So from what I understand, they sent some of samples up to Ed Chao [in D.C.] to see if there was some evidence of fusion in the quartz or fused rocks. And then the whole business of a 'funny' line showed up in the quartz [X-ray pattern]. So how did that happen?"*

Roach: "I did some work on Laue patterns, the X-ray Laue patterns where, around the severe shock, that some of the atomic positions were dislocated; they'd show up as fuzzy dots. So I was doing working on that. But of course, Ed was doing work on the Coesite and Stihovite, so it kind of all tied together. Gene was essentially integrating all this information. I was working it from one angle and Ed was working from another. So that's how that developed."

*Schaber: "So did Ed or Gene tell you to send the samples up there to have him look at them--to Ed Chao?"*

Roach: "Well Gene was sending samples to Ed Chao, of course, that he was collecting around the [Meteor Crater?]. In the meantime, he had gone off to study other cryptovolcanic structures. He had become really interested in them. Once he became convinced that impact was a really important geological process, then the next question that he got really interested in was what's been the geologic rate of impacts. That's when he started branching out and went around looking at all of the known cryptovolcanic [or cryptoexplosion feature] structures; and then it became a question of how long these things have been impacting the Earth? So that got him interested in Pre-Cambrian terranes, and that's when he first became interested in Australia--the Henbury Craters first--and then all the others that were found."

*Schaber: "So Gordon Swann originally worked up there in your lab Denver. Who worked in your lab at that time?"*

Roach: "Let's see, I had Gordon Swann and Joe O Conner and Howard Wilshire."

*Schaber: "How did you acquire those guys?"*

Roach: "Well, I hired them to come into my physical properties lab. Gordon was going to be my petrographer. I hired him out of the University of Colorado. That is kind of an interesting story. I had to hire him as a technician. Washington wouldn't approve him as a geologist, and Charley Anderson was Chief Geologist at the time. I made several requests and finally received a letter that said something to the effect of: we here in the Survey don't consider the University of Colorado as turning out the kind of geologists that we need in the Geological Survey.

I showed that to old Ed Engle, head of the Geological Engineering Branch in Denver. Ed was one of the old boys, and he said Charley doesn't know what he's talking about; actually the University of Colorado has a higher percentage of passage on the USGS exam than any other school. So, I just kept on, and finally I kind of pressed Gene on him [Charley Anderson]. As a result, we finally we got Gordon, Howard, and Joe their geological rating. It was an interesting experience.

Anyway, as I look back across my career, working with Gene was the most significant thing that happened to me during my career. It was unfortunate that we came to a little bit of a sad ending in a way. You know, I mentioned earlier that Gene never stopped asking me to go to Flagstaff. Well, one time he asked me to go to Menlo Park in the early days of Astrogeology. I even got as far as taking the family out there and looking at houses. I said no Gene; I'm not coming. Then, the next time I called out there he had moved to Flagstaff and set up the office there!

Joel Watkins was really responsible for me leaving Astrogeology. At that time, Gene had Joel as Coordinator of Astrogeology's Geophysics Group, and I was coordinator for Cratering. So then Joel got to the place where he wanted to leave. He told me that he was going to leave, and the next thing Gene appointed me--this letter came out saying that I was coordinator for Geophysics and Cratering, and that I would have to move to Flagstaff. So I wrote a letter down to Flagstaff and said I appreciate your confidence in me but I've got my work here and you agreed to leave me in Denver, so I have to decline the Geophysics assignment that you need to have headquartered there. I really still don't understand how Joel was in a position to write my efficiency rating for the year. However, he wrote me an unsatisfactory efficiency rating, and it was based on the fact that I refused to come to Flagstaff. So then I quit. I transferred over to the [Survey's] Gold Program. That must have been close to 1970. I transferred over to the Bureau of Mines to be the Project manager of the Gold Exploration Program. That's when I transferred all of my lab stuff down to Flagstaff. [Author's Note: Yes, to me as then leader of the "Rock Lab" in Flagstaff.]"

*Schaber: "Now there is another story that Gordon told me about--about the famous poker game in Grand Junction."*

Roach: "Oh yeah. We had a poker club here in Grand Junction [in the early days]. Everybody in the poker club, except Gene, wanted to play a sixteen-cent limit. That was a red, white, and a blue chip. The red chip was worth a nickel, the white chip a penny, and the blue chip a dime. And the rules we had were a raise was limited to sixteen cents, and you had a three-raise limit---you couldn't have more than three raises. But Gene just kept insisting that he wouldn't play unless it

was table stakes—i.e., how much money he had on the table. So, when we'd meet over at Gene's place, then we'd play table stakes. We played a lot of wild games--very few straight poker games. It was just everyone thinking up a wild game. Gene liked the game "Red Dog." It's a real wild one, and can get into a lot of money, particularly when you have the table stakes that he did. When he dealt he said, okay we are going to play Red Dog. I'd never heard of it you know. He explained it real carefully, and everybody understood it, and said okay we'll play.

Then we started playing, and he [Gene] put out quite a bit of money. I thought well, knowing Gene, I'd better put out about just as much as he's got there if I'm going to play, or he'll force me out.

Of course, with Gene, the idea was to "win." That's the way Gene was in everything. You've got to be number-one and you've got to win. So anyway, we got into this game, and a couple guys dropped out. Gene bet a dollar--which we thought was pretty high--and then he bet two dollars and I called him. All this time, we were drawing cards. We would bet and then draw a card. So it got down to the place where Gene said, well Carl, I don't know if you know what you're doing or not, but I'm going to raise you three dollars. I looked at my cards and say well, I'm going to call your three and raise you three. Gene's mustache twitched and he looked. Gene had his big mechanical calculator there, and he calculated the odds on the calculator. He said do you know what you're doing? I said probably not. He said well okay, I'm going to call you; but I hate to do this to you. I just calculated it and there's only one card in the deck that'll beat me, so I will call you. Gene said what have you got? I said I've GOT THE CARD!"

*Schaber: "I'll bet that was a bad day for Gene."*

Roach: "I told him I've got the card, or I would have been out of here a long time ago! Well, Gene was always a great one on odds you know, and I said odds are great if you play a million games but on one game you've got to be sure that the guy's not sitting there with the card."

*Schaber: "What year was this poker game?"*

Roach: "Well let's see. That game would have been before 1957 because that's when I was down in Hopi Buttes; so it would have been around 1955 or 1956, yeah."

I've got one other quick story about Gene for you. This would have been about 1953 or 54, somewhere in there on the Uranium Project here in Grand Junction. Gene had a project called Distribution of elements program. That's where he was having spectrographic analyses made of all of the mill pulps from all of the ore trucks that came into all of the mills that from here on the Plateau. What he was doing was trying to study the geochemistry of all of the uranium deposits across the Plateau, everywhere they were coming from. And of course, they turned into a lot of data.

And to make a long story short, Gene came up with a novel idea. The problem came up on how to interpret the data at the lower end of the detection range of the Survey's spectrographic equipment. Gene came up with this report where he extended the curve--the log curve--down below the detection limit. There was some geochemical problem that he wanted to work on that required that

information. The director of the Survey at the time--I think it was Bill Pecora. First of all, Bill Pecora at that time was head of Geochemistry and Petrology, or P&G), and he later became Director. Gene challenged the G&P Branch-- the labs that were doing all of the analysis-- when they complained that he couldn't interpret that data below their detection limits--"there was no way you could do that!"

Gene wrote a real extensive reply (that I later saw) that Dick Fischer, the Chief Geologist out here at the time, showed me. Gene wrote this long report justifying how statistically, you were allowed to make some projections down below that detection limit--that that detection limit was artificial, but that the distribution curve was natural. They got into a big fight over that. They went back and forth. The head of P&G wouldn't cave in; they tried to put Gene down you know. In fact, I heard that it got so bad that they tried to hold up Gene's promotion at the time. Anyway, they tossed it over to the Bureau of Standards to Dr. Eisenhardt, who was the chief statistician of the Bureau of Standards. They sent Gene's report over there and the G&P Branch's objections to it. They made a study of it, and Bill Fischer showed me the letter that he got back from the Bureau of Standards Chief Statistician, Dr. Eisenhardt. I still remember the closing line of that [letter]. He said, not only is Shoemaker correct, but he's made a fundamental contribution to geo-statistics.

*Schaber: "He always fell over and came up facing up."*

Roach: "Yes; they tried to stymie Gene several times, but nobody could hold Gene back--nobody."

*Schaber: "But you know something Carl; that same thing of pushing the statistics of "one", and so forth, ended up years later in Gene's asteroid research. Calculating the rates of impacts on the earth and other planets based on statistics of a few earth-crossers asteroids, or whatever. He was pushing this stuff to the limits; but you his impact-cratering rates came out darn close, as later verified after many more asteroids were discovered in the subsequent decades."*

Roach: "That's right. Every time he came to Grand Junction he would stay at our place, and I remember one time we were standing here looking out the front window at the Book Cliffs that he had always admired here in Grand Junction; and we were talking. He said I've got this idea where we may be able to find a fairly good number of asteroids that aren't known at this time. He went through the discussion of it you know, and said I'm going to try to get something started on that. Carolyn's [Carolyn Shoemaker] the one he got started working on that. I don't remember enough about it, but it seemed like to me that he thought that there should be a zone of these things, and that over time were strung out--you know. He was a great dreamer and forward-looking guy."

*Schaber; "Carolyn wasn't really involved with him in the astronomical stuff with him until much later--right?"*

Roach: "Yeah. In fact, Carolyn was telling me later that she talked to Gene one day and said, Gene all of our kids are gone and I need something to do to kind of get my interest up. Gene said I've got this little project that needs a volunteer. That was looking for asteroids."

*Schaber: "Now she is the world's leading comet discoverer!"*

Roach: "One thing I'll leave you with is--one of the geologists that I grew up with all my life--from boyhood up--ran into Gene out in Menlo Park one time. He was in Menlo when I was in Colorado here. He talked to Gene, and he was very impressed with him. He asked me what kind of guy is Gene? I explained him several ways, but I also said that he's the kind of a guy that--if his project all of a sudden needs to have a nuclear physicist and you're the next guy hired who walks in the door--you're it!"

Gene knew that most people didn't realize anywhere near their full potential, and he would push them into it! It was one of the greatest things that ever happened to me, personally.

Oh, I remember another Shoemaker story related to our work at Sierra Madeira in Texas that just came back to me. I was down there [in April 1961] when we were looking at some possible impact structures. We finished our day's fieldwork there at Sierra Madeira, and were coming back. I was down there with one of my field assistants Tom Sterett. Tom killed a rattlesnake--a big one--on the way back to the jeep after the day's work. Gene said if you kill that snake we're going to have to eat it! Tom said well, I'm going to kill it! The next morning it turned out, we did eat it. Carolyn fixed the snake and we ate it. But that night, Tom and I had a jeep and there was only room for one of us to sleep in the jeep, and the other one was going to have to sleep on the ground. And when we brought that huge rattlesnake into camp that day; Carolyn said I saw one in camp here the other night like that! So we had to flip to see who was going to sleep outside the jeep on the ground--and I lost. So I was out there trying to stay awake you know, and worrying about the snake --and I saw this light moving real fast across the sky. I told Gene the next morning, I said it wasn't a plane, it was moving too fast. So, we left early that morning and when we got into Ft. Stockton for breakfast, it turned out that that was Uri Gargarin [the Soviet Cosmonaut]--the first man to orbit the earth!"

*Schaber: "Oh, that's a great story!"*

[Author's Note: Carolyn Shoemaker told this author a somewhat different story about that snake at Sierra Madeira, described above. She says that Gene had asked her to cook up the snake, but she flatly refused. Gene instructed her how to simply drop the snake in boiling water to loosen its skin. Carolyn finally agreed to go as far as placing the snake in the hot water, but when she saw it immediately curl up--as if still alive. Carolyn then told Gene, if you want to eat that snake--cook it yourself! They did; the guys ate the snake while Carolyn walked off alone away from the camp (and the snake meal) until they were finished. Branch of Astrogeology geologist Don Elston (see 1962 in main text) tells a similar story about rattlesnakes at when he went to Sierra Madeira with Gene Shoemaker, Carolyn, and their children the following summer, 1962].

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**David J. Roddy** (1932-2002) (geologist/cratering expert; Ph.D. Caltech, 1967) first joined the Astrogeologic Studies Unit of the USGS in early 1961, only about a few months after Shoemaker started the Unit in Menlo Park on 25 August 1960. Dave arrived in Flagstaff to work full time for the Branch of Astrogeology in September 1967.



Dave graduated from Miami University in Ohio in 1955 with a major in geology and math. He received his master's degree from Miami in 1960. While at Miami, he became part of the Air Force ROTC, where Dave was a Distinguished Graduate of the Air Force ROTC program; thus began his lifelong love of both science and flying. Dave attended the U.S. Air Force Navigation Flight School (Harlingen, Texas) in 1957-1958 and the Air Force Instructor Navigator Flight School (Harlingen, Texas) in 1958. Dave then went to Caltech, Pasadena, California from 1960 to 1966 and earned his Doctorate in geology (Impact-cratering mechanics of Flynn Creek, Tennessee) under the guidance of his assigned dissertation advisor Lee Silver, as well as Gene Shoemaker, in 1967. Dave was Associate Branch Chief from 1983-1984 (G.G. Schaber, Branch Chief). He received a Unit Award for Excellence of Service in 1986 from the U.S. Department of the Interior for his contributions to the Defense Nuclear Agency's Pacific Eniwetok Atoll Crater Explosion Program (PEACE). Dave received three Letters of Appreciation from various DOD agencies (General's level); in addition to two letters of Commendation from Secretary of the Air Force, D. Rice, for his work at the Pentagon during "Desert Storm" (1990-91). In 1994, Dave received both the Barringer Medal (International Award) and the U.S. Department of the Interior's Meritorious Service Award for excellence in impact and explosion cratering, and planetary research.

David J. Roddy served as Project Officer in explosion cratering, ejecta processes, and shock effects, as well as in intelligence studies, for the Defense Threat Reduction Agency (DTRA) from 1965 until his death in 2002. He also served as a Principal Investigator in the U.S. Geological Survey for NASA to investigate impact cratering processes and ejecta formation in the field, in experiments, and in theory and numerical simulations since 1961. Dave officially retired from the U.S. Geological Survey in 1992, but remained with the Astrogeology Team in Flagstaff in Emeritus status, and was extremely active in science until his untimely death in 2002.

The following was taken from an interview with David Roddy by Gerald G. Schaber Between May and June 2001:

Roddy: "I was born 27 May 1932 in Springfield, Ohio. My folks were farmers during the depression. I grew up in an agrarian or farming environment, but I always had two things I wanted to do. One was to fly, and the other was to do science. I had no idea what doing science was but I did know what flying meant. So I have been incredibly lucky. I went to school there up through 12th grade and graduated in 1950. In 1950 I entered Miami University for a five-year program in the sciences, and I also became a part of the Air Force ROTC so I had--at the college level--a start in flying, and science in-progress.

Theoretical math was my undergraduate major, along with a major in English and a major in geology--and minors in physics and chemistry. I graduated in 1955 with a specific major in math and geology. And then, because there was a backlog in the Air Force Flight School, I had an extra period of time. A professor Dave Scotford, a P-52 fighter pilot that used to escort B-29's from Okinawa to Japan for bombing and things like that was my academic advisor. He said you are the only one who came through this department who has this odd technical background--why don't you put together this differential thermal analysis (DTA) unit that this petroleum company gave us. We will make a master thesis out of it. I said, heck yes, this is great; it would give me something to do between now and the flight school with the Air Force starts. So I did such--did a Masters with a major in thermodynamics and a minor in geology.

I went off to flight school in 1957. Thirty-six months later--the day I finished my active duty tour with the Air Force-- I became a flight navigator instructor. This was at Harlingen AF Base in Texas, as far south as you can go without going over the border. Harlingen had a T-29 wing--Air Training Command--and I was part of the ATC Training Command as a senior flight instructor there for two years. I graduated not only first in my class, but the highest that anybody had graduated there in several years, with their academic and flight scores combined.

I married Andrea "Andie" Leader in 1955; we went to the Air Force; we had our first child David Michael Roddy just at the end of graduate school. Then we went to Lackland AFB and did the introduction kinds of things with the AF, and then went down to Harlingen AFB to become a navigator.

My wings were for a navigator in the Air Force. You could get either pilot's wings or navigator wings, and I got navigator because my eyes had changed. I still wanted to fly--so I did it. I graduated at the top of my class, and so I was able to pick my assignment. I stayed as a flight instructor and did my thing. About halfway through the formal contract period, Andie and I looked at one another one evening and said do we want to do this the rest of our lives? I said I would like to go back to graduate school; and she said I would like us to go back to graduate school. So we did.

I was skeptical that I could get into a good school, so I applied to about 17. I was accepted to all of them with full scholarships. I was so amazed and flabbergasted that I thought somebody made a mistake. The first letter we got back was I think from Indiana. I thought, wow man this was out of sight. Andie and I were just elated. We didn't know quite what to do. Then the other acceptance letters began to flow in. To make a long story short, I got a full scholarship to MIT offered.

Shortly thereafter, a very odd thing occurred. I got a postcard from Caltech's Geology Department. The postcard said we do not normally admit students during the middle of the academic year, due to difficulty in course work in transitions, etc. I thought oh shucks, there goes Caltech. Then I thought what the heck; I'm not going to let a secretary's postcard defer my decision--so I will just send one back to Professor Dick Jahns (at Caltech then). Jahns sent a letter back, almost by return mail--and said you're right David, the secretarial staff does not determine the future of our graduate students. You are hereby admitted with full scholarship.

Dick Jahns was the main reason I wanted to go to Caltech, because I wanted to study solid state physics things--and big crystals--and Dick Jahns was a pegmatite man. This was hot stuff--at least to me--the inside of crystal lattices and stuff--and I think you share that interest yourself with me. In any event, I was numb as all get out.

I had heard of Caltech, and I knew it as a good school, but I just didn't know how good it was. I called MIT's Chairman--I think it was Dr. [S.J.] Schropp at that time who was a Paleontologist-Sedimentologist. We talked for quite a long time, and he finally said David, do you like cold weather or hot weather? I said well, I kind of like them both. He said you will get a great education if you came here to MIT, but I think you'll be slightly happier on the west coast with

your family and so on, and you'll get ALMOST as good an education at Caltech as you would at MIT. He said well take your choice--you're going to do okay.

So Andie and I went to Caltech; and if I'd known what I was getting into I probably would never had the audacity to give it a try at Caltech. Miami was a pretty intermediate level college. So, my beginnings at Caltech were kind of interesting. I came in at the middle of a term and went into some middle-of-term courses like thermo-dynamics and so on. I'll tell you, I didn't get much sleep for the next four months to finish that. In fact, that came closest to killing me as much as anything I had ever done. It was a real sudden death; but, I passed the courses and kept on flying in the Air Force--in the Reserve--and by then we were raising a family of two boys. Mark Roberts Roddy was the second boy. I was just having one hell of a good time. It was just exciting to get up and go to class.

I was still floundering however, in the sense that there are just so many interesting things to do in classes and talking to professors who were on the cutting edge of this that and the other. I was like a kid in a candy store, just grabbing a little bit of everything as I went by.

Then in the fall of my first year, 1960, one Dr. Eugene M. Shoemaker gave a visiting class at Caltech on the Geology of the Moon and Solar System, and I just fell in love with planetary science. My interest over in solid-state physics (where I was minoring) remained extremely high; so I kept taking these odd courses outside the department (of geology) that led to the odd background I had in physics and solid state stuff.

So Gene proposed toward the end of that that he'd like to have some people who really liked fieldwork--that were really field oriented--to step forward and look at one or two of these proposed impact structures. He proposed a new view of geology and I think I was probably the first--along with the other students in that class-- to be exposed to Gene's view on catastrophic effects of geologic processes--i.e. impact cratering. It was truly an enlightening offering that he gave because he had traveled the U.S. with Dick Eggleton and looked at these other impact sites--sites that were misnamed [as it turns out] crypto-volcanic or crypto-explosion structures.

Gene turned my entire view about peaceful uniformitarianism process to violent catastrophic occurrences that were periodic, and shook the Earth literally. At least it shook my attitude about the basic principles, and Gene and I had common views about how to collect the information that's of value.

It turns out that I was a couple years older because of the military experience, so Gene said why don't you consider a place called Flynn Creek in Tennessee? Gene said I am convinced that it is an impact structure.

Walter Bucher had listed it as a cryptovolcanic structure. So Gene said of all the "crypto" structures, this one holds the most promise of being the most complete--and we can get the most information from it. So why don't you attack the following problem--solve the origin of this structure completely, and to everyone's satisfaction, as a Doctoral thesis In addition, you can put to rest Walter Bucher's and others' erroneous designation of crypt-volcanic for these structures [until

Canadian Bob Dietz later found shatter cones and renamed them "crypto-explosion" structures].

In any event, Gene said if you can marshal enough evidence, you'll be able to solve all the rest of the similar structure's origins too, and no one will be misled calling them crypt-this and that. The other is--let's see if you can figure out something about this crazy Vredefort structure, or dome [Southwest of Johannesburg, South Africa] and its central uplift and so on. So that's what I went to do.

[At that time] I sure wanted to pursue the solid state things. Shock metamorphism wasn't a term used at that time, but, in effect, I was lobbying for moving into the field of shock metamorphism to try to look at solid state/internal crystal kinds of effects, and deformations in the micro-sense--were just religious to me. So Gene said, why don't you go up [to Denver] and see a fellow that's been working with us for a few months--his name is Carl Roach. So I went up to Denver and met a couple or three days with Carl, and I still remember the enormous excitement that I got when he drove me out along the Front Range. It is just the kind of things that effects a young student's mind in ways that were powerful pushers to get out and do field research.

So I came back and got started with that, the summer of 1961 or 1962 I believe [probably 1962]; I got started in Flynn Creek, and mapped all summer long. I was immensely happy, but tremendously disappointed in the amount of exposures that you have in Tennessee in the summer. And I think you know exactly what I am talking about Jerry! [Author's Note: I am originally from Northern Kentucky]. I ended up literally throwing all of my mapping away that I had done during the first two or three months because you just couldn't tell hardly weeds from the rocks.

I talked it over with Andie and said I'm going to have to leave the family because the kids were going to school etc., and I'm going to have to come back here and map in the winter. That is about all we can do. So I spent the next four or five years mapping, and doing lab work when I was back at Caltech for eight or nine months--and then mapping for three or four months.

So I spent well over a year of mapping at Flynn Creek--a 10 mile by 10-mile section at 1:6,000-scale--which is fairly detailed. It became clear after a while that this was not a crypto-volcanic kind of feature in any sense of the word. It became clearer and clearer that this was consistent with the deformation of a very highly centralized energy source.

Gene had finished his work there but never had written it up in detail, except for that landmark paper where he basically changed the thoughts of every geologist, geophysicist and planetary scientist that looked at his 1960 paper which was "*The Impact Mechanics at Meteor Crater*". That was published in 1960, but he had actually written it two or three years earlier. I was really hot on his heels, in terms of time, because it just worked out in time that way--if you see what I mean.

I came out of the Air force in March 1960, went directly to graduate school at Caltech in March 1960. In fact, I think I've got 15 or 16 years of going to school after I left high school, so I really sucked up that academic junk. My mother wondered if I was ever going to graduate and stay out of school. I still remember mom asking me a question once when I was somewhere about half way through at Miami. She said David, are you sure you can make a living in--what do you call it--

geology? I said I sure hope so mom, I really like it! I never dreamed of how much fun it was going to be--that we were going to go the Moon and things like that. Gene Shoemaker thought we were all along. He would say when I get to the Moon and other such statements but he wasn't very verbose about it.

When I went to Caltech I was clearly a fish out of water. I didn't come with my military uniform but I was still thinking in that direction, or at least I was trained to think in that direction. My first meeting was with Dr. Harrison Brown, who was an internationally respected man known for his work on the geochemistry of the Solar System. He had been at the University of Chicago when they had that enormous breakup and everybody flowed out in different directions.

In the late 1950s, I guess that was a trying time back there, but Brown was so well established in science that he didn't suffer--obviously he didn't suffer at all because he ended up at Caltech. Harrison Brown was a prime mover in getting mega funds to outfit the geology department with all of those black boxes like mass spectrometers, etc. Brown was a big-time mover. He was not a small man, but he was a very gracious and very gentlemanly-oriented fellow dedicated to science. He was secretary of the National Academy of Sciences at that time. When he would write a paper, he would dictate it. The secretaries used to talk about the [airplane] engine roar being so intense on the recording tapes that when he would start a text they often had to have him repeat parts of it or at least clarify it. He would get onto an airplane, sit down on a commercial flight--and just start dictating a science paper just like that right off the bat. He was that powerful; a clear thinker.

He wrote one paper that was of real interest that I remembered. It was a statistical paper on how many planets existed statistically speaking--that might have life on them just like Earth. That was done way back then. So Brown was truly a big-picture thinker. One night he was standing at his desk in his office and he was leaning back and sort of looking at me--we had just met- and he said well David I am in geochemistry and I don't do field geology.

At that time Lee Silver walked in the door, and Dr. Brown said well I'm not really quite the right person for you academic advisor--this is the man you need--Lee Silver. And that was the beginning of my long career duty with Lee.

Lee taught mostly igneous geology. He was responsible for some of the field courses. At that time there was an enormous combative situation going on in Caltech. Lee Silver (Igneous geology) was there, Barkley Kamb (smartest than all the others put together) was there, Jerry Wasserberg (geochemistry) was there; and Clarence Allen (structure). Barkley Kamb had been an undergraduate at Caltech, and he got the highest grade in physics that anyone had ever gotten there. When he would stand up at the blackboard and answer questions on relativity or something, he wouldn't bother going back with Einstein's development, he would start on an original on his own--and be correct.

In fact he married Linus Pauling's daughter, and eventually went on to become Chairman of the department etc. He was a very withdrawn but very handsome guy; he was almost totally indifferent to human interactions unless he chooses to be. His mother wrote a book called "*I had genius twins.*" His twin brother apparently was a brilliant biochemist or whatever in his own field. Anyway, that is the kind of people we had to contend with.

At any rate, Lee Silver became my academic advisor, in reality, but Harrison Brown was still my academic advisor on paper. Eventually, I worked my way through the thesis, spending a lot of time in the field, and then came over here to Flagstaff to compile my map.

I would come over to Flagstaff to compile my [Flynn Creek] map in the winter. I was living in Altadena, California, and then I would go back and do the fieldwork each summer through 1966. In fact I remember walking into Clarence Allen's office one day with new data on Flynn Creek. He looked at it and said that's good, --that's good. Then he looked at me and said David you come in here with "one more piece" of data on Flynn Creek and I am going to see to it that you are terminated. I want you to get finished David; I want you to write this up David. It's all okay David; you don't have to solve every problem before you give your dissertation defense and get out of here. He really laid it on the line. He said Gene's not going to do it; he'll have you out there until you an old man with a beard or something. He said Harrison Brown has delegated your field work to Lee Silver. I want you to get done and we don't care what Shoemaker wants.

I compiled my map back at Flagstaff, mainly because of [photogrammetrist] Jim Alderman. Gene assigned him to produce a special topographic, high quality, 1:6,000 map of the Flynn Creek area for me. A new map, so that we had a high-quality base to work on because I knew we were going to do volumetrics on the crater in the long run. Man, to have a topographic made for you of your field area. Well, you know what a terrific thing that would be for anybody.

I went up to Denver and met Jim Alderman, and once he settled into it he worked literally day and night to produce this very high quality topo map. It was over ten miles by ten miles. There were zillions of valleys and ridges and what not. He just did a terrific job. When I field checked it, I never found an error. The thing was we had this high quality map, and I got copies made to work on, and then I found that the aerial photography was what I actually had to work on to get all the detail. And I had zillions of detail on that map.

The place to actually manipulate this [mapping] was to come over to Flagstaff to Astrogeology's Kelsh Plotter, and actually sit down and trace the lines so that they superimposed onto the topographic map. So I used just simple hand-transfer techniques, and then eventually the more sophisticated Kelsh techniques to transfer the data--super accurately if possible--to the very high quality map he made for me.

I didn't start coming over here [to Flagstaff to compile my map] until about 1963-64. Gene had sent his mapping guys over to Flagstaff to get started--Chuck Marshall and Jack McCauley. I started in the Branch just about 4 to 6 months after it got started in Menlo Park.

Well, I started WAE with the Branch of Astrogeologic Studies Unit [in Menlo Park] back in the beginning of 1961. The first thing I did was to go up to Ubehebe Crater, California. My wife and kids and I drove up in our car to meet Howard "Howie" Wilshire. It was the worse sand-and-dust storm in the middle of the night that I ever saw. Anyway, you couldn't see your hand in front of your face. We got to someplace along Ubehebe Crater and we parked; and I was worried about the car getting sand blasted and all of this that and the other. Andie was worried about the kids, and I had my giant German Shepard dog. There we were in the middle of the night in a whirling

swirling dust storm and Howard Wilshire arrived with a bottle of champagne. It was just one of those kinds of things you never forget. It was a neat experience.

Howard came along because he was an expert in volcanism. That was his field. I was looking at Ubehebe as a possible analog to Meteor Crater. In fact, I wrote an abstract on it later on about the energy equivalence between here and the Moon--and why it [the crater] would be bigger on the Moon, etc. In any event, we fooled around there the next day or two and came away convinced 100% that volcanic structures are totally different than Meteor Crater structures--structural deformation, etc. So we were happy with that and then we went on about our own business.

I stayed WAE with the Branch for quite a long time. I was physically out of Caltech, but up until about the middle of 1964 I carried a full NASA scholarship---or rather a Caltech scholarship that had NASA money in it. However, the Survey was supporting me at such a level that I was able to do field work and live at Caltech. Andie was working part time too, so we were doing all we could do to keep on going. But somewhere about that time, it was decided that the Survey would support me doing what Gene wanted me to do--and at that point I was given three thesis advisors--which was wonderful--I say sarcastically. I had Clarence Allen, I had Lee Silver; indirectly I had Harrison Brown, and then, Gene Shoemaker. So I had a pretty impressive group of people but all of them had their own demands on me. Gene was too academic.

One day we were at Flynn Creek, I'd say in 1964-65, and Danny Milton, Gene and I were reviewing my field mapping. Everything was going okay, and then Gene sat down and said David I think you ought to change everything here. I think this should be an outcrop map! Yes, that's it--an outcrop map. That's the only way to do it---raw data, raw data. Prove it in the field; go out and get those outcrops, draw a line around them, tell us what they are, and go on to the next outcrop.

Danny said you are full of bull Gene! Dave is doing the right thing. He's not only doing an outcrop map in effect, but he's connecting the lines in the field where his common sense can come into play--and his visual perceptions etc. Gene said no, no. Danny said, David, just forget him, just do what you are doing. It is the right thing. I hadn't seen that happen quite that way before, but I continued in the path I was following, and it was the right thing to do. But Gene just hadn't seen enough of the area to know if you really knew the rock. Unless you plug in the microfossils or what not--the character of the rock--you had no idea which section it came from. But it turned out that I had been back there long enough to pick up a chunk of limestone and just about know which bed it came out of.

So Flynn Creek not only played enormous dividends in those days, but it had continued to be of prime use for the planetary world and the DOD world with regard to large-scale cratering ever since Gene got me started. He sure picked the right site for me.

I finished and defended-thesis in 1966, and graduated that same year. I stayed at Caltech until 1967, and came over to Flagstaff in September 1967. I do know that I came in the fall of 1967 with the whole family, because two months later we had that monstrous snowfall (in December) when all of us were up on the roof shoveling the snow off--jumping off onto the ground--82 inches or something. I got an office on the third floor (north side) in the Arizona Bank Building. I went

looking for a house for myself, Andie, three children under the age of 11, a big German Shepard, and excitement to do a hell of a lot of work.

One of the guys up in the Bank building suggested that I talk to a fellow geologist, Dave Cummings. He was leaving Flagstaff that day and wanted to sell this property. So I went over and we chatted; drove out here to Fort Valley (Baderville)--took one look at it and said you've got a deal-- I'll rent it. So, I rented it for several years, and then we bought it.

There was kind of a hodge-podge of things going on at the Branch at that time because the astronaut training [at Flagstaff] was just getting started and I was also deep into the DOD work; so I was living dual lives. It was almost like I was two different people. It was also three jobs because I was also doing NASA cratering, and I was doing big time the work for the Defense Nuclear Agency that was then enjoying cooperation with Canada, Great Britain and other allied countries."

[Author's Note: Dave Roddy recalled during our interview how he got started working with DOD; see below]:

Roddy: "D. Gareth H. S. Jones was a geophysicist of considerable repute and mental capabilities at the Defense Research Establishment, Suffield Alberta, Canada. He was in charge of the large-scale explosion experiment. As a geophysicist, he was the program overseer. That involved joint work with the U.S. back when the cold war was going very strong, and they were trying to learn a number of defense-oriented explosion cratering phenomenon etc. and understand the processes and the effects better. And Jones was the program manager for the Snowball explosion experiment in 1964. That was a 500-ton hemisphere on alluvium at the Defense Research Establishment in Suffield, Alberta, Canada. It produced a large, flat-floored, relatively shallow, crater that was very large in diameter over a hundred meters. They were quite surprised at how flat the floor was and how big the crater was, but they were really surprised at the miniature mountain in the middle of it--a central uplift--which they didn't understand was a central uplift [or central peak] at that time. It was just a mound of clay and goop, and the crater filled with water right away, so they had all kinds of trouble collecting field data from it.

But Jones was smart enough to get hold of Gene Shoemaker. He met him as I recall in a motel room someplace back on the East Coast during some conferences Gene was going to back there. I think it may have been about 1965 (in the fall-winter). Jones brought all of these cross-sections and laid them out--and Gene got all hyped up about it--and said I've got just the guy you ought to see. He's just in the middle of his Doctoral thesis at Caltech and I will send him up there because he will work fine. I see what you guys make of it after Dave has looked at it with you.

So I went up (to Canada) in 1965 and they had the excavations partly done--and the crater partly drained; and it was a real complicated mess in the middle. They had tons of markers (marker cans) with numbers on them buried-pre-shot--and they were excavating those slowly. And I said hey, why don't you take a can of spray paint and spray the contacts so we can follow this and photograph--and do a decent job. They mumbled and said oh well... The next morning--what did I find but they had tried it. It was a beautiful display of contorted thrusts, uplifted clays, and silts and sands. That took us about a year to sort out, and we kept on collecting the data.



Well, this was major to my thesis because what Gene wanted done on the Flynn Creek thesis was a once and for all--put to rest--the crypto-explosion, crypto-explosion theory--because Flynn Creek was one of those craters so-classed by Walter Bucher.

The field support at that time was provided by the Branch, and the formal Caltech graduate work was just graduate work. Before too long the Survey took over all support. I had a full NASA fellowship, and at that point Astro needed all the money they could get. They ask if I could get support elsewhere--which I usually could-- so I got Defense support, USGS support and NASA support. It just worked out; back in those days a lot of things worked out.

As far as the Defense work went; after that first year, they sort of enjoyed the geological-geophysical approach to collecting field data because it made more sense and gave a better picture than the things they had been trying. So, by default, I was the one who was showing them how to do it, and I was the only one there, so they just kept me around as their pet geologist--or something like that. Actually, I became a project officer for cratering and cratering effects. At that time it was the Defense Atomic Support Agency

Well, we had a major breakthrough with the Snowball [TNT] crater (in Canada) and because I was working on Flynn Creek at the same time, I knew in some detail what the deformation at Flynn Creek looked like, and I could see great similarities in the Snowball 500-ton crater. So I began to draw analogs wherever I could on the central uplift in terms of analogs at the impact site--at Flynn Creek-- and then I began to extend out underneath the crater floor. Then I was about to get drilling out there [when Arnold Brokaw was Branch Chief at Flagstaff].

Anyway, we eventually got a complete package of cross-sectional and structural-deformational information both for Flynn Creek and for Snowball; and they matched so extremely well that I was compelled to speculate some kinds of initial explosion shock-wave generation activity or configurations--and went from there. I eventually concluded, for the sake of general argument, that the Flynn Creek structure was clearly produced by a shock-wave process, and it had to have been from above because there was nothing coming up from below--because I drilled through those areas. In fact, I drilled six holes (I think) in 1967, and we drilled the next batch in 1977-78. It was something like that.

There was just a beautiful timing of those two databases becoming available to the guy who was doing the fieldwork--me, and all blessing and accolades go to Gareth Jones for having the good sense to recognize what this thing looks like. In fact, Jones told Gene this thing looks just like Copernicus on the Moon. Gene said show me. Then Gene said David--go do something. So the foresight of those guys was a remarkable step forward.

Gene was working on the Colorado Plateau in terms of trying to find uranium deposits. In doing so, he was exposed to certain areas of classified information. And somehow, he was on the Nevada test site near Sedan Crater, and he saw the two smaller craters Jangle-U and Teapot. Gene being curious eventually mapped those craters. That was the turning point in his acquiring data in the field of shock wave-produced craters at the Nuclear Test site--nuclear shock in this case--and how similar they were to Meteor Crater. I did exactly the same as he did but I hadn't known that

he had done it--in terms of these analogs. So he took Teapot-S and Jangle-U and compared them to Meteor Crater. And the bottom line is that he looked at the overturned flap and he looked at the bowl-shape and he was able to speculate on the depth of burial.

Well Gene did that classic turning the corner study of Meteor Crater, the one that proved we've got an impact crater for sure. It came from those two nuclear craters that he mapped. In fact, I have the original data of his mapping. I've got it in digital format on computer disk at this point.

Gene followed path A to B, and I followed path A to B and we came up with the same conclusions--shock-wave-produced impact sites do exist and it is not a crypto-volcanic or crypto-explosion feature. So I was successful in the first part, but I have never solved the problem of exactly what the reasons are for producing the central uplift--that is the physics involved.

Anyway, that took me down that path and opened the door, because we met with considerable success in translating the field data into cross-sectional data that could be published by Gareth Jones and company (and-eventually myself).

Snowball then opened the door permanently to the Defense Nuclear Agency, and I served as a consultant and project officer for them until the present time; and with NASA it was pretty much the same.

Then, when Gene did Apollo 11 and 12, he basically formulated in his own mind what the geologic field exercises and training should incorporate [with regard to study of impact craters] in a general sense. That is going from bowl-shape to larger craters--but not too much emphasis on the larger ones because they [NASA] wouldn't let them on the larger ones. So Gene thought that most of the stress should be put on the bowl-shapers and how they reposition material and how far they throw material [ejecta.]

None of us considered at that time how incredibly complex, and intensely gardened, the lunar surface was going to be. So you couldn't just pick a rock up and say well it came from that crater. So it was more complex on the Moon, but the basics that Gene laid out were the ones I think that any intelligent field person would lay out in terms of trying to teach astronauts what was going on in the cratering process from simple bowl-shaped craters.

The first Apollo crew that I got exposed to was Apollo 13 (Lovell, Haise). When Prairie Flats, the next 500-ton sphere [of TNT] was detonated in Canada, all of the astronauts attended. Well, I told the appropriate people about the experiment that was coming up, and I said I am sure that I could get authorization to get these guys in if they wanted to a live shot and a modeling of lunar cratering. They took it to heart. I can't remember if it was Jack Schmitt that latched onto this, but I think so. He, in turn began to rattle the cage of getting a Gulf Stream [aircraft] and they took the largest contingent of astronauts up to the Canada site that had ever gone in the field together at the same time. Tony England flew himself and his wife up there in their own plane.

The Canadians just pulled out every red carpet they could. It was a really big deal. It was an exciting time. All of the astronauts, except Tony England, came up on the same plane.

Frankly, I was kind of amazed that NASA let them do it that way--but they did [i.e., allowed all of their astronauts to be on the same plane. This was actually quite a risk].

They had a great experiment and a great time with the Canadians. They did it up proper and they sent them out a day or two later back to the States.

[Author's Note: I asked Dave Roddy how he first started his longtime association with the people at the California Research and Technology, CRT, on computer modeling of nuclear and impact cratering]:

Roddy: "Well, they (CRT) were tied to the Defense work, and they were independent of my work long before I got involved. They were big time! The whole defense world was built around developing computer codes that would simulate accurately the affects of nuclear blasts in different geology and under other different conditions. They were doing multi-million dollar calculations by the hour literally. I just ate that stuff up because it was quantizing the kinds of things they knew that NASA would never pay for in code calculations.

The calculational community at CRT (Creighagen, Schuster and the others) were involved in the most powerful computational efforts being done in the world. I was associated with them starting about 1970.

For a long period of time I had been trying to make comparisons between the detailed numerical computer simulations with explosion cross-sectional deformational data and impact cross-sectional deformational data. When I would compare surface morphologies with those we found at the Canadian explosion test sites, I found enormous similarities all the way from bowl-shape-to flat-floored-to flat floor craters with a central peak, up to multi-ring to basins.

That progression in surface morphologies as you get larger and larger yields was recognizable on planets, satellites and impact cratering environments, and I could recognize it in the explosion cratering environments. And so, it suggested very strongly that we had a very unique and important key to the deformational-cratering processes. So it was natural to try pick up the numerical simulation world which was the ultimate in explaining processes, because it was numerical and quantitative.

The computer world began to expand at a very fast rate, and it allowed us to do some things here that we couldn't have done before, but we continued a close relationship with Creighagen's group - California Research and Technology (CRT). I continued to work with them on both non-classified and much more advanced levels (classified). They were great in letting us publish a great deal of the information that they could de-classify.

To make a long story short, in 1976, it was suggested that we have an international symposium, and I tried to liven the effort by bringing in the Defense cratering community with the academic, NASA and USGS world of impact cratering. So we got about 50-60 authors and held a symposium in 1976, which we turned into a book that I edited and put together.

In order to use CRT's complex computer code to simulate 10-km bolide impacts into a 5 km deep ocean and a terrestrial crust impact, we modified it to take a kinetic energy source as opposed to a static source--but that was not a terrible difficult thing to do. Our initial conditions were movement of the bolide. We had to turn it into a moving object as opposed to a static nuclear device. But that just involved changing the original conditions. There were dozens of codes; we just used the most tried and true and most powerful ones for what we wanted.

All of this work eventually led to using the tested defense codes (both air blast, as well as ground shock), to examine a large body calculation. Even today, that is the most detailed code that had the correct geology, structure and correct physical properties in it. We ended up looking at a ten-km impacting body, both into a 5-km deep ocean and into a continental mass--and ran the results, as well as compared them.

The result was in the 5-km deep ocean calculation, we got initially an ocean wave that was formed as part of the transient crater rim (so to speak) that lifted a body of water adjacent to the forming crater about 40-km high. This ran out at about 5-km per second dissipating velocity in??, and increasing volume rapidly. It actually had rock underneath. The rock itself lifted up about 25 km on the rim. The floor was penetrated and excavated in transient cavity form down to about 40 km in 29 seconds. You were through the crust and into the mantle, but only a minimum amount of mantle was ejected--a very small amount.

We had a crust-1 and a crust-2. I think we had the crust thickness going down to the order of 10 km. The ocean actually didn't make a difference to the cratering, it was just one more layer; but because it was in the ocean of course you don't have the permanent crater left. But the crater itself continued to collapse for another hour--or days actually- forming those giant terraces as well as a central uplift.

After 30 seconds, the floor was starting to come back up. That was due to collapse of the enormous overburden surrounding the transient cavity. As that material flowed back in, it met in the center at the axis of symmetry. Man, it was coming up like a freight train--three or four kilometers per second. When you visualize that amount of depth--it was an awesome looking object with ocean waves 40 km above you and crater floor 40 km below you, and the floor starting to come back up like a monster.

In this impact simulation, almost 10 thousand cubic miles of rock was placed into the atmosphere. It was equivalent to 6.7 times  $10^7$  mega-mega tons [of TNT], and would have eliminated over seventy-five percent of all living species. What it did was put all of that debris into the atmosphere. That appears to be sufficient to totally overload our atmosphere long enough to disrupt our food chain. It formed a crater of about 250 km diameter. In an earth gravity environment, craters of 200 to 300 km-diameter will apparently be enough to wipe out all life on earth.

The large Cretaceous impact down in the Yucatan Peninsula that is credited with killing off the Dinosaurs got the shore dwellers [directly]. What it basically did biologically, is that it put so much debris into the atmosphere that the opacity was such that you just didn't keep the temperature up at the earth's surface. The stuff growing down there and the animals living down there just

couldn't make it in that severe of an environment; so the big dinosaurs, which needed to eat all the time, just ran out of food.

After the Impact cratering book and the impact computer simulations with CRT, I really began to look at the Australian sites, and I went over there and worked with Gene [and Carolyn] for a couple of years. I actually got a commercial pilot's license over there, believe it or not--in three days. We went out and photographed impact sites in the most remote areas. We got the job done. Gene was able to publish some of that and it was continuing on; and I am now archiving it. We can get it on the web site--all the Australian impact crater photography. Anyway, it paid some dividends and I got to spend some time working with Gene on Mount Toondina which is flat-floored central uplift feature and got to watch Dingoes in the wild because I used to sleep out on a cot every night.

After that I started looking at very large body cratering on the Earth--up to potential disruption of the planet. And we have now completed an animated series that does exactly that. In fact I hope to finalize it as a major game so to speak.

It's actually operational. You pick something like eight or nine parameters--like density, velocity, etc.--and it will produce a crater. It is really quite interactive and sophisticated, and you can get it up to the point where it actually disrupts the planet by having a large bolide crash into it--like a small moon.

Most of my time the last few years has been concentrated on capitalizing on all the years of looking at impact craters; i.e., capitalizing in the sense that I have been looking at larger and larger impact events. Once you get more mature in your thinking with age and experience, that is when you ought to turn yourself to the big picture--and that's what I've been doing.”

[Author's Note: Unfortunately, our colleague and good personal friend David Roddy passed away of a ruptured aorta while undergoing a heart scan on 21 March 2002 while in St. Louis, Missouri. Dave was 69 years old. Dave married his first wife, Andrea Leader in 1955. They had three sons: David Michael, Mark Robert and Andrew John. Dave married Jeanie Swan in 1974, and they had two sons: Glenn and Kevin Swan. Dave is survived by his mother, Nellie Aldrich Roddy; sons Michael Roddy and Matthew Roddy (both of Riverside, California); son Glenn Swan of Toronto, Canada; son Kevin Swan of Chicago, Illinois; and four grandchildren. The overflowing attendance at the memorial service held 18 May at Northern Arizona University in Flagstaff, Arizona) to celebrate Dave Roddy's life and achievements was a testament to this kind and gentle man, and dedicated scientist. This author was honored to speak about Dave--the person, the scientist, the friend—at his memorial service in Flagstaff.]

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**Lawrence C. “Larry” Rowan** (b 1933; geologist; Ph.D., 1964, University of Cincinnati) arrived on duty with the Branch of Astrogeology in Flagstaff on 26 August 1964.

The following was taken from an interview with Larry Rowan by Gerald G. Schaber on 5 February 2001:

Rowan: "My first contact with the USGS, and the Branch of Astrogeology, was through a fellow called Dick Wilmarth, who had been the Branch Chief of Engineering Geology for the USGS--but I think at that time he was an Office Chief. I met him at a GSA (Geological Society of America) Meeting--I believe it was in Pittsburgh, Pennsylvania. This would have been in 1963. My background at the time was hard-rock geology--metamorphic structure and petrology--having done my Doctorate thesis in the Beartooth Mountains under Dr. Leonard Larsen at the University of Cincinnati [Author's Note: Len Larsen was also my professor, as I was only a year behind Rowan at the University of Cincinnati.]

Quite frankly, I was quite ignorant of the lunar program. I had essentially two job opportunities when I came out of school. One was with Shell Oil, so I went to Houston to interview with them. The other, as I mentioned, was the interview with Dick Wilmarth regarding a job with Astrogeology--not that I knew what Astrogeology was of course!

I had spent four years before going to the University of Cincinnati working for the State of Virginia doing essentially engineering geology and [sitting on] a whole lot of drill rigs. Shell Oil offered an opportunity of two years, essentially working on a rig and doing well logging, and then two years in the research lab in Houston. I told them I wasn't interested in coming to work at the research lab. They didn't go for that--so I went to work for Astrogeology. I think I made--by far--the right choice.

I was offered a job (with Astrogeology)--as I recall as a GS-11 Geologist. That was the standard entry with a Ph.D. at that time. As I recall, the salary was \$8,500 per year starting at a GS-11 step 1. So we moved from Cincinnati to Flagstaff, Arizona--I believe in a 57 Ford that I later sold to you [this author] for six hundred dollars."

[Author's Note: I drove my wife Sandy and our one and a half-year old daughter Jennifer out to Flagstaff [from Covington, Kentucky] in that very same 1957 Ford in early July 1965. I remember that you could see the road through a large hole in the rusted back floorboards of that car; and that Sandy and I were afraid we were going to lose our daughter through that hole during the trip west to Flagstaff. Fortunately, Jennifer was fascinated and quiet during the 1,800-mile drive across the country on Route 66-- just watching the road speed by through the back floor of the car.]

Rowan: "Upon arrival in Flagstaff I was assigned to work with one Jack McCauley--in the Burris Building in downtown Flagstaff 119 E. Aspen)--which was very small quarters. We soon moved to --I believe--the fifth floor of the Arizona Bank Building [just to the north across the street from the Burris Building]. My assignment was to work on lunar terrain analysis under Jack McCauley. My main assistant was Ann Kelly.

We had great fortune, in obtaining one John Running [see interview with John Running below in this Appendix]. I'm not quite sure how John Running's name came to me--whether it was through Jack McCauley, or through someone else. John is now, however, a very successful photographer [in Flagstaff.] John was an ex-Marine who did his own knitting. He's kind of an off-the-wall kind of guy you know. At that time we were doing a lot of work using lunar photometry to map lunar surface roughness and turning out various reports along those lines [using the Isodensitracer, or IDT]. This was all in support of eventually landing Surveyor on the Moon. So the [lunar terrain

analysis] staff back then was essentially Jack McCauley, John Running, Ann Kelly, and I. Jim Alderman and Warren Borgeson were also sort of associated with our program, although indirectly. This would have been in the Arizona Bank Building in 1965-1966"

The next stage was Lunar Orbiter. I think probably Jack [McCauley] and Don Wilhelms were originally in charge of Lunar Orbiter exercises in terms of lunar mapping. But there came a need for someone to analyze Lunar Orbiter photography as it came in and to participate in the mission planning [selection of F-stop settings for each exposure, etc.] So, there were a number of people on the Lunar Orbiter missions at JPL. I think at least a dozen people did, because, it was a twenty-four hour exercise.

A bunch of us [from Astrogeology] went to Langley AFB in Virginia to analyze the Lunar Orbiter images as they arrived; there were about a half a dozen of us USGS people. The participants were changed from time to time, but Maurice Grolier and Terry Offield was there briefly, among others. And most of us lived in a house near Langley that we had rented from a NASA engineer whose family had moved to Pasadena (JPL) to work down there."

*Schaber: "So what the purpose of being at Langley when they [the images] would come in versus at JPL?"*

Rowan: "The mission was actually handled or managed at Langley—not at JPL. So all of the chief engineers and people working on various aspects of the mission were either there [at Langley] or--a few--were of course at JPL; but these were mostly the operational/navigational people. Kodak was a major player in this too because they made the old bi-matte film process that was used. They supplied the film.

Cliff Nelson, an ex-Admiral, was the leading figure at Langley; and the people that we worked with were Tom Young and Norm Crabill. And Tom Young went on from there to Martin [Aerospace], where I believe he became President of the Company. He has since retired. Tom had two degrees--one in Electrical Engineering and one Mechanical Engineering--and an enormous skill in getting along with people--which is worth more than all the degrees in the world.

There was also a group of geologists and analysts--I say analysts because I'm not sure that all of them were geologists--from the Manned Spaceflight Center [Houston, Texas] who helped with this analysis. See, the analysis was basically doing crater counts. It wasn't terribly sophisticated. It was very hard work and we worked very long hours."

*Schaber: "Mainly selecting landing sites for Apollo!"*

Rowan: That's right!"

*Schaber: "I did one of those maps"*

Rowan: "Yeah, exactly. In fact, at one time I had a whole stack of those pale green, Open-File [USGS] documents that resulted from that. I think now that most of them are now gone. I've had

to move two or three times and each time when I moved, some of those kinds of things jus go by the wayside. I tried to give them to the library--and they don't want them.

Anyway, we were at Langley I suppose, for at least a couple of months. Then the Lunar Orbiter photo analysis got more sophisticated by the likes of one Donald Wilhelms and McCauley [Jack]. I did some of that work as well. Well, let's see, I guess I gave a couple of different presentations to the public while at Langley in the auditorium. Actually, the attendance was mostly the people who worked at Langley itself--a rather large facility--both Air Force and NASA.

Then, I guess I was on national TV once when Surveyor I landed.”

*Schaber: "Yeah, I saw a videotape out of the Planetary Data Facility at the USGS in Flagstaff of you talking to some network anchor back about then in the mid 1960s."*

Rowan: "A guy named Fields."

*Schaber: "Yes, 1966."*

Rowan: "A long time ago man!"

*Schaber: "You looked like a real young kid then."*

Rowan: "A young skinny kid with big ears-right--who was scared to death.”

*Schaber: "You were pointing to various things--I'm not sure that anyone knew what the hell you were talking about."*

Rowan: "Neither did I!"

*Schaber: "So after Lunar Orbiter, what did you do?"*

Rowan: "I guess it was in about 1968 that the terrestrial Remote Sensing Group was formed out of Astrogeology. We moved I believe in 1970; so we operated there in Flagstaff for about a year and a half-to two years. Do you remember Ester Holmes?"

*Schaber: "Oh Yes."*

Rowan: "Ester worked for me a little bit on the lunar Orbiter Project, and a professional paper resulted that had an awfully lot of really nice photographs it--of Lunar Orbiter material. I don't even know if I have a copy of that. Ester was one of the people we got from Military Geology.

*Schaber: "Tell me about the Remote Sensing Group here-and what you were doing with them."*

Rowan: "As you probably recall, the group was established within Astrogeology, and then it was moved into Regional Geophysics [while we were still in Flagstaff]. The Branch Chief at the time was Don Maybe. The leader of the project was Ken Watson.



This was in the Arizona Bank Building [downtown Flagstaff]. The other participants on the project were Terry Offield, Howie Pohn, Jan Cannon, Maurice Grolier, and I. And then another guy who was hired later named Bob Watson.

Maurice Grolier and Jan Cannon did not go to Denver with us. Jan went back to graduate school. He went to the University of Arizona, then to Alaska, and then he went back to Oklahoma.

At any rate, the main thing we were working on at that stage was trying to figure what the hell it was we were supposed to be doing in terrestrial remote sensing. And of course, as you might imagine, Ken [Watson] had his own ideas, and Terry and Howie and I had other ideas.

I think we got a little better oriented when we moved to Denver. One of the problems in those days was that there wasn't any data.

Well, I mean, there was no remote sensing data worth working on. It was all un-calibrated data. It wasn't until we started working with the University of Michigan Willow Run Laboratory (ERIM) that we began to learn a little bit about instruments and how to calibrate data. And then in 1971, the opportunity to write a proposal for Landsat-1 or ERTS (as it was called then) came along; we wrote a broad-sweeping joint proposal from our whole group--but it was not successful."

*Schaber: " So that takes us past when you up to Denver. Since then, what have you been doing? You went to Denver and ended up in Reston."*

Rowan: "Put very briefly-- I only stayed in Denver for two years. I was then asked to come back [to Reston] to work as the staff Geologist for Remote Sensing. At this time, the Geologic Division didn't have anyone on their staff that specialized in remote sensing. Because of the success of the first Landsat, ERTS-1, and the fact that the EROS Program Office was sort of the overall manager of money and so on in the Bureau, and interacted primarily with NASA. It was felt that they needed someone on staff to deal with the EROS Program Office and with NASA Headquarters.

So, I came back. At that time, this building that I am sitting in today [the USGS headquarters in Reston VA] didn't exist, and we were located downtown [DC] in the old GSA building. I was on the staff of one Dallas Peck who was just giving over the Geochemistry and Geophysics Program to Dick Fisk. So I worked mainly for Dick Fisk for two years--Gene Sheldon was the Chief Geologist at the time. And then, after two years of that, we did a Landsat follow-on project which NASA funded. One with Terry Donovan which wasn't terrible successful [Terry was located at the USGS Flagstaff Field Center at the time]. We were looking at one of the Permian oil and gas fields, and the resolution [of the Landsat imagery] was just too small--too limited for what we were trying to do. In the 80s, I worked primarily on the Shuttle Multi-spectral Infrared Radiometer with Alex Goetz as a CO-Investigator; on a TM [Thematic Mapper] experiment; and on a project in Spain. I still have contacts there [in Spain] and a very small existing project; mainly to do with mine waste, pollution, and that sort of thing using hyper-spectral data. I did a lot of that sort of thing.

I've done a lot of work with AVIRIS and these so-called hyper-spectral, high-resolution instruments. And then in 1989, I believe, we proposed an instrument to be on board EOS, and I believe it was [Astrogeology's] Hugh Kieffer Hugh who worked out the Acronym--and the acronym was TIGER (Thermal Infrared Ground Emission Radiometer). And NASA said, that's interesting, the Japanese have suggested building an instrument very much like this, so why don't you work with the Japanese and see if you can't help them come up with a very good instrument for environmental purposes and ecological modeling, etc. So for ten years, that's what we have been doing. But, it's been a reasonably good experience.

One thing just occurred to me that I should tell you. You recall that we [the science staff at the Branch of Astrogeology] were sort of always looking for people in those first few years at Flagstaff--people who might be interested in Astrogeology. At one point, we had a list that came around, and Jack McCauley showed me the list and said do you know any of these people? And I said, Terry Offield--that can't be. He said, what do you mean it can't be? I said, you know, I went to VPI (Virginia Polytechnic Institute), which at that time was all-military, except for Veterans--and there were a hand-full of those. And I said, in my sophomore year, there was a guy who lived across the hall from me with a roommate whose name was Terry Offield, a geology major--and I was then a pre-med major [Author's Note: Larry's dad was a pharmacist].

At any rate, he [Terry Offield] had a little bookcase with a lot of rock and mineral samples on it. That was in 1952, and the next time I ever heard his name was in 1965 I believe; he was coming back to the US, having been in Pakistan and Brazil. And I said [to Jack McCauley], well, I don't know anything about him but--I know that I used to know him--but not very much. I can't say that it was a renewed friendship because under those military conditions--second and third-year students barely spoke to one another--so I really didn't know him at all."

*Schaber: "So what were your general thoughts, in retrospect, about the years you spent with the Branch of Astrogeology in Flagstaff?"*

Rowan: "My general impression was that I really didn't know what I was doing most of the time. I didn't have the vaguest idea about lunar photometry. So it was just one of those situations where your just sort of thrown in, and you either sink or swim--and to have to learn in order to swim. So we all learned, but I found it very challenging--actually more challenging than graduate school because things had to be done more quickly. There were timelines."

*Schaber: "What we got done in such a short time between say 1964 and 1969 when Apollo went--it was unbelievable."*

Rowan: "Yes, And in retrospect, I think that the work went well, and I am pleased with that. On the other hand, I think I probably spent too many hours at work [and in travel] and not enough hours with my family. And, if I had it to do all over again, I would do it differently. But of course, that is also a change in attitude with time with the whole society--you know."

[Author's Note: This author will be ever so grateful to Larry Rowan, who I had known as a fellow graduate student at the University of Cincinnati (1960-1965). Larry was one year ahead of me in receiving his Doctorate in Geology. I found out many years later that it was Larry Rowan who

actually had mentioned my name to Jack McCauley in 1964 as a possible candidate to work at then fledgling Branch of Astrogeology. As fate would have it, I decided to go to the Geological Society of America Annual Meetings in Miami, Florida in the fall of 1964. One of my objectives was to look for possible job opening--- as I was then a junior at UC. Jack McCauley, having been alerted to my name by Rowan, put a note on the recruiting Bulletin Board there at the hotel in Miami where the GSA meetings were being held. Again, by fate, this author went by that bulletin board on a couple of occasions to see what job-interviewers might be there. I saw a small envelope with my name on it. I thought that very strange. Upon opening it, I found it was Jack McCauley; he wanted to meet with me regarding working for the Branch of Astrogeology. I could not imagine how Jack had gotten my name. It is indeed true that one's life path and career can be triggered by coincidental meetings, fate, or "favorable alignment of the stars" to some minds. No matter how or why it happened, I will be eternally grateful. Thanks Larry.]

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**John Running** (photographer; B.S., 1970, Northern Arizona University)

The following was taken from an interview with John Running by Gerald G. Schaber on 25 April 2001:

Running: "I grew up in Buffalo, New York and came to Flagstaff Arizona in 1965. I had just been released from the Marine Corp. After I got out of the service I wanted to come to the Southwest, and Flagstaff kind of beckoned. I went to Northern Arizona University (NAU) and started working on a degree; but I was married, and I had a daughter, so I was also looking for work. I applied to the USGS {Branch of Astrogeology} trying to get into their Film Documentation Unit; however, I was turned down by Hal Stephens. He didn't think I had enough background.

While I was in the Service, I worked in an intelligence unit--so I was doing a lot of air photo interpretation and stuff like that. I got a job in Flagstaff working for a soils-testing laboratory. Then, Don Elston at the Branch of Astrogeology suggested that I talk to him. Right after I talked to Don, I talked to Harrison Schmitt. That would be June 1965. Then Schmitt suggested that I talk to Larry Rowan. I did--and Larry hired me to work for him. So, probably in June 1965 I was hired to do work similar to what I did in the Marine Corps--photo interpretation, but basically on the lunar surface. I was hired as a Physical Science Tech (PST). After working for Larry Rowan and his group I started working for Ester Holm and Maurice Grolier. This was up on the fifth floor of the Arizona Bank Building. I remember that Astrogeology had this big computer [located at NAU]. It was not like one of our desktops [computers] today---it took up a whole room that had to be built for it (see 1965). They used it for terrain analysis.

I remember having to crunch out Chi Squares--doing statistical stuff--having to do them all by hand. Then, someone got a [desktop] computer [Singer computer with video screen] that would do square roots! That was revolutionary!

One thing that was really great about working at USGS is that there were good people. I just enjoyed it. It was challenging work. About the time of Lunar Orbiter I went back to Langley to work on that. Again, I was working with Larry Rowan, Ester Holm and Maurice Grolier.

On one occasion I was going back to Langley for Lunar Orbiter I, the first Lunar Orbiter. I was on a plane flying over Kansas when we had a bomb scare. So, the plane did this emergency landing. It just kind of did a dive. They evacuated the plane off the chutes--and I had maps with me. I was one of the first ones off because they said they needed someone strong to help; and so I volunteered myself, and was catching people at the bottom of the chute--but I kept my maps with me. I was going back for that map tube and people were saying what do you have in that map tube? Why are they so important? Then, we finally got to Langley. Oh, Duke Ellington was on that plane; so that was kind of neat getting to see him.”

[Author’s Note: At the time John came to Astrogeology and later worked on Lunar Orbiter, he really had no experience in photography, except in photo-interpretation of aerial photographs.]

Running: “I didn't start doing photography until my last three years at Astrogeology. Then, there was the Film Documentation Unit headed up by Walt Roeder. I talked a lot to Walt, and I really liked him. He took me under his wing. Walt went to Maury Brock [who was in charge of the Branch's support groups at that time] and he suggested that I didn't have enough experience, but if I could prove myself to him [my ability to make a movie], he would let Walt hire me. So I made an audition film. We showed it to Maury and he let Walt hire me.

Walt was a great boss. He was a German man who came over here just before WWII. He had been a photographer all of his life. He served an apprenticeship in Germany. He told me, I will teach you everything I know! So at that time, I took off school for a year straight. I was going to NAU at the time. Walt would give you all kinds of free reign--listen to your ideas--let you try them, and then he would gently correct you if he thought you were wrong. But he was very un-Germanic with his way of working with people. He was a wonderful boss.

So then I started doing film documentation with Don Hart. He was the Branch scriptwriter, soundman and narrator. So I finished out this great period of time with Astrogeology. Then, about 1969, the money started thinning out for the Apollo Documentation Unit. The Film Documentation Unit just didn't have any more work. So we started really more of a film library than making movies--and I didn't really care for that too much. I started doing some freelancing in photography about the time I was getting my degree in May 1970.

I got a chance to do a freelance photography job that was going to take about four months to complete. I requested a leave of absence from the USGS, but Irene Gedney [Astrogeology's Administrative Officer at the time] said she couldn't--so I quit. Everybody advised me not to do it. But I left USGS in June 1970. I have been a freelance photographer ever since.”

[Author’s Note: John Running still resides in Flagstaff where he has his own photography studio; John is now a world-class photographer and very well known and respected for his work.]

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**Ramon Sabala** (b. 1931) (draftsman/graphic illustrator), one of the Branch of Astrogeology's hardworking draftsman/illustrators arrived for work in Flagstaff on 27 July 1965.

The following was taken from an interview with Ray Sabala (Flagstaff, Arizona) by Gerald G. Schaber on April 23, 2001:

Sabala: "I was born August 28, 1931 in Scott's Bluff, Nebraska. I spent most of my school years there. I attended the Junior College on a football scholarship, and after that I went to Denver looking for a job and went to school at an engineering drafting school. Actually, I guess I went to work for an oil company at that time in 1952. After that I had gotten on with Melco Refinery. They decided to split up and move into other areas, so then I went to work for Superior Oil there in Denver, and that's where I met Jim VanDivier. The company that he had been working for folded, and he was working there for Superior. He was employed there with Superior Oil, and we worked together there for several years in Denver. They started to shut down their offices and Jim went to Farmington New Mexico and I went to Casper, Wyoming.

*Schaber: "About what year was this?"*

Sabala: "This was in 1963 I guess. And it was shortly after that Jim was laid off in New Mexico, and he came down to Flagstaff and was hired by the Branch of Astrogeology."

*Schaber: "Do you have any idea how Jim VanDivier made the connection to Astrogeology in Flagstaff?"*

Sabala: "It had to do with some friends of his that were working at the Survey there in Denver."

Jim just went ahead and applied; and then started working down in Flagstaff with Astrogeology. Superior Oil was continuing to close their offices around the country and Jim ask me if I would like to apply for Astrogeology in Flagstaff--and I did. Gene Shoemaker hired me at that particular time. So I moved to Flagstaff, and I was involved in mapping for the space program. At that particular time there was an opening at Astrogeology in Flagstaff for a Cartographic Technician. I started out drawing illustrations of astronauts using the lunar tools we were designing [at Flagstaff] -- that type of thing. That kind of opened the door. We worked with Jack Schmitt, and of course we worked closely with Putty Mills on the various simulated Lunar Roving Vehicles (LRVs) that were hand-constructed in Flagstaff."

*Schaber: "You were originally located at the ANNEX or Dance Hall down on old Santa Fe Avenue, right?"*

Sabala: "Yes, it was on Santa Fe next to that A&W root beer stand- [just east of Babbitt Ford]-you know where the electronic technicians had their office--and the surveying team was in there too."

*Schaber: "So you were there until the Fourth Street Building in East Flagstaff was completed in 1967, and then the drafting moved up there-right?"*

Sabala: "Yes, we moved over on Fourth Street where we had the facility there with John Nuttall and his group Electronics group [in addition to the Branch Surveyors and photogrammetry group]."

*Schaber: "So were you ever were in the Arizona Bank Building?"*

Sabala: "No."

*Schaber: "So what were you were working on during the Branch's Pre-Apollo field testing of Apollo lunar geologic methods was were going on in Hopi Buttes--say 1966 to 1968?"*

Sabala: "This is where we were laying out and designing different-scale maps for the Apollo landing on the Moon, and working with Thor Karlstrom on potential walking traverses and this type of thing. One day Thor Karlstrom came up to me and said you know Ray we've just killed the astronauts. I said what? He said our traverse is too long; they would have enough oxygen to get out there, but they don't have enough oxygen to get back to the LM."

*Schaber: "Were you out at any of the field tests out in Hopi Buttes or Meteor Crater? What was your group's role? You helped in some of the reports we did resulting from those tests as I recall."*

Sabala: "Yes, we did."

*Schaber: "You also supported Walt Roeder's Film documentation Unit with drawings, illustrations, and that type of thing---right?"*

Sabala: "Yes. I did work with Dave Roddy on his map of the impact crater feature at Flynn Creek, Tennessee."

*Schaber: "Were you out at JPL during any of the Pre-Apollo, unmanned, lunar missions like Surveyor?"*

Sabala: "Yes, Jim VanDivier and I both went out to JPL for all of the Surveyor missions. I guess our first visit out there; it was kind of interesting. We asked the JPL folks what we should bring with us as far as drafting instruments are concerned. They said you won't need to bring anything; we have everything here for you. So, when we arrived they didn't have anything for us. They didn't have drawing paper or anything. Finally, we went to a trailer that had caught on fire, and found some drawing paper that had gotten wet and was all wrinkled, but we used it anyway. We had to use a string and a pencil to draw craters (circles) with.

*Schaber: "To use as a compass-right?"*

Sabala: "Yes, as a compass!"

*Schaber: "This was at high tech JPL at that time—right?"*

Sabala: "Really! After that we took out our own instruments, and I used to call my kit--have T-square-will travel!"

Sabala: "During the Surveyor missions we laid the photos down, and catalogued them by number--each photo you know. That is where they would mosaic them in that concave aluminum hemisphere."

*Schaber: "Tell me about the experience of going out to JPL and being part of a mission. That must have been exciting!"*

Sabala: "It was very exciting because everything was so new and it seemed like you learn something every day. At that particular time it was very fascinating, and I enjoyed it very much."

When Jim and I arrived at JPL for one of the Surveyor missions, I didn't have a badge there at the security office for some reason. Jim was the only one who had one. So anytime we had to leave this particular area—and they had guard on the doors-- we'd kind of walk single file. I was right behind VanDivier step-by-step to get past the guard. The guard would look at us and think--I don't know if that one guy had a badge on or not. As soon as we got past him, Jim would give me the badge and I would put it on and I'd turn around and show him [the guard] the badge--and he would let us go. We did that for about two weeks."

*Schaber: "Jim VanDivier's cartography group at Flagstaff was incredibly busy. At the same time they were all working on the lunar geology maps that were on a very tight publication schedule; so did your being at JPL working with Batson's Surveyor group at the same time interfere with the publication of the lunar geology maps a bit?"*

Sabala: "Yes, we worked a lot of overtime. And one time we made up geologic maps--remember we used to do them by hand color using those oils--that was when we drew the people from the Government Printing Office out to Flagstaff wanting to know where our color printer was. We had to show them that we were doing them by hand."

*Schaber: "Now which maps were these?"*

Sabala: "These were of the Equatorial Belt [of the Moon] that Ester Holm [at Astrogeology 1964 to February 1966] was working on at that particular time."

*Schaber: "So tell me Ray, what you were doing during the actual Apollo 11 mission? Were you with our Lunar Geology Experiment Team down at Houston at any time during Apollo 11?"*

Sabala: "Well, I wasn't on the first mission, Apollo 11. Jim and I really didn't get involved until Apollo 12 because they [Shoemaker's Mission Apollo Lunar Geology Surface Experiment Team] took Astrogeology's photography crew down there rather than us for Apollo 11. For some reason or other they [the ALGSE Team] didn't think that they needed drafting personnel at that particular time; and it wasn't until Gene needed drawings--that's when Jim and I got involved in the Apollo 11 mission.

*Schaber: "During the last geology briefing before they went to the Moon for Apollo 12, Gordon [Swann] was told by Charley Duke and Al Bean from the Apollo 12 crew that they would like colored units on their geologic maps in their Data Package Maps. Gordon must have said*

*something like God--we would never be able to get this through the NASA/MSC approval mill in the short period of time now before the mission. Tell me about that."*

Sabala: "They decided to go with that system; so we were using "cut-a-color"; so we would cut these units out and place them on the map for the astronauts. We didn't finish them in time, so Gordon put us on the plane, and we flew to Orlando.

So Jim and I were cutting these things out using the airplane windows as light tables--so to speak. We got down there, and we got in with a drafting crew right there at the Cape; and golly, they just treated us like kings you know-- because it was all new to them too.

So there we were—the two of us-- finishing up these maps--and they had to run the [engineering] crew out of the spacecraft in order to paste these things on the wall in order to have them for the astronauts for their flight--Apollo 12."

*Schaber: "They had to run who out [of the spacecraft]?"*

Sabala: "All of the engineers and everything who were working on the spacecraft at that time--preparing it for the launch."

*Schaber: "Yes, they did this secretly [for the crew] because they didn't want anyone [like-those responsible at NASA for approving such things as the map on the spacecraft] to know what they were doing."*

*Now one thing is a conflict [compared] to the story I heard from Gordon. He said that there was a special kind of colored pencil that you guys had to use, that NASA said was flame proof and all of this that you guys were told to use. I didn't know that it was cut-a-color because I thought they gave you the name of a certain kind of pencil."*

Sabala: "They did give us a color pencils; I guess that it wasn't cut-a-color until later."

*Schaber: "So, Apollo 12 was the one [maps] that you colored by hand."*

Sabala: "Yes, with the colored pens"(see **Fig. 70** in text).

*Schaber: "I am told by Gordon that the guy at NASA/MSC that in charge of getting things approved to put on board the spacecraft was blindsided, and very upset, when we all heard from the Apollo 12 crew on the Moon when they said something like "gosh, these colored maps are just the ticket."*

*So you went down there to the Cape for this secret rush job; and you met the crew and everything--right?"*

Sabala: "Yes, which was a very exciting thing; it seems like they always had us tied to a drafting table somewhere."



*Schaber: "Well, they certainly didn't want you to tell anyone what you were doing."*

Sabala: "No, that's for sure!"

*Schaber: "How long were you down at the Cape doing that?"*

Sabala: "Well, we were there right up until launch time because. We were with Thor Karlstrom at that particular time--because I know that they said that we had to get out of that room because of the launch coming up--so they kicked us out of the motel."

*Schaber: "So you didn't get to see the launch then?"*

Sabala: "No, we had to come home!"

*Schaber: "So who accompanied you down to the Cape for this?"*

Sabala: "Well, it was Thor Karlstrom, Jim VanDivier, and me."

*Schaber: "I recall seeing you in Houston for nearly all of the Apollo missions; is that right?"*

Sabala: "Oh yes, I went down to Houston either during, or just following, all of the missions, clear up to Apollo 17 where I had an invite for the final Apollo mission from Jack Schmitt to observe the launch. That was the only one that I did get to see."

*Schaber: "We gave away all of our launch VIP tickets, and went out on the parkway [at the Cape] in a drive-your-own mobile home [RV]."*

Sabala: "When we went with George Ulrich."

*Schaber: "Yeah, right--so you were with us in that group."*

Sabala: "Yes."

*Schaber: "Remember we went to the KSC gift shop during the afternoon, and--there were all of these tourists there waiting for the launch that night. We just happened to see a family with several kids. We had about four VIP passes among the guys that were with us, and we just asked the father of this family if they were going to the launch--and they said yes. Then we said how would you like to sit in the VIP stands. They said Oh my God--so we gave them our launch VIP invitations."*

*Then George rented this drive-yourself RV and we all went out on the North or South Parkway; we didn't get back to our motel until 3:00 AM or 4:00 AM in the morning."*

Sabala: "That's right! But I recall that I got separated from you folks. I ended up driving out to the launch with Bill Muehlberger and his wife Sally—Jim Vandivier and I."

*Schaber: "Let's talk about the 45-day reports [for Apollo] that our Apollo Lunar Geology Experiment Team did down there in our little trailers at MSC, starting with Apollo 11. There were no computers, no fancy Photoshop-type software or scanners to do photo-scanning and this type of thing. Instead, we had this new state-of-the-art Thermofax thing; this little machine that we could (with considerable effort) make wet print copies from photographs—do you recall that?"*

Sabala: "Yes."

*Schaber: "You were kind of doing the photos and Tim Hait was helping put everything together for us, or coordinating. And we were printing our EVA reports over there [at MSC's printing shop]. Remember that guy Dave Straw, or whatever his name was who ran the printshop down at MSC?"*

Sabala: "Yes."

*Schaber: "We'd go over there in the middle of the night and print these EVA reports in between the EVA's, when everybody else was sleeping because the crew was sleeping. We [the ALGSE Team members] couldn't [sleep] because we had to put together [and print] these reports in order to put them on the desks of the mission control personnel before the next EVA. Remember that?"*

Sabala: "yes"

*Schaber: "Tell me what you remember about doing that."*

Sabala: "Well, in fact, that's when I developed the whole crew. That's when Muehlberger came down one night and helped me fold maps and get ready for presentation to the astronauts you know. We had the 1-day, the 2-day, and the 5-day reports that we had to submit for the NASA and media people down there during press briefings."

Sabala: "Well you know, we went down as a team and we performed as a team. It didn't make any difference what we had to do."

*Schaber: "We did whatever needed to be done."*

Sabala: "That's right!"

*Schaber: "We were so lucky to be part of history, and be at the right place and graduate at the right time--it's just amazing!"*

Sabala: "Well, when the Superior Oil Company I was working for shutting down--and that's when I applied for the Geological Survey--and I was just picked up right away. I just can't believe how fortunate I was."

*Schaber: "And it you hadn't known Jim VanDivier—worked with him earlier---you wouldn't have known about Astrogeology."*

Sabala: "Right!"

*Schaber: "You worked with Jim VanDivier's team getting lunar and planetary maps ready for publication, in addition to drafting and illustrations for various planetary missions that came along. By about 1980 or so, you guys over in the Cartography Group at Flagstaff had a different name--you broke into two sections as I recall--there was the photomechanical section run by Hugh Thomas and Thematic Mapping or something like that- right?"*

Sabala: "Yeah, it was shortly after that when I quit the group and went to work for myself [as Center draftsman] and worked for Gordon Swann."

*Schaber: "You went over to Building- Three at the Flagstaff Field Center I believe."*

Sabala: "Yes I did!"

*Schaber: So you left the group and went over to do drafting for anyone in the Flagstaff Field Center by then as I recall. "...?"*

Sabala: "That's correct."

*Schaber: "And you thought that that was just a better thing for you at the time?"*

Sabala: "Yes, but shortly after that is when [Hugh] Kieffer [then Chief, Branch of Astrogeology] told me that he didn't need me any longer--So that's when I accepted a transfer to Denver with the Survey's Weapons Program."

*Schaber: "So what year did you actually leave Flagstaff?"*

Sabala: "It was in 1990."

*Schaber: "So overall, looking at your years in Flagstaff, can you give me just a brief summary of what your feelings were, and how you enjoyed it?"*

Sabala: "Well, everything was so new at that particular time. I was very impressed that I was even asked to participate in that program--and I can hardly believe that some people didn't believe that we even went to the Moon!"

*Schaber: "Ray, remember the day that we were sitting at the bar in terminal two at Sky Harbor airport in Phoenix on our way to Houston for one of the Apollo missions? I forgot which one it was, but we were just sitting there drinking beer--a few beers actually-- and watched our plane take off. I recall we didn't even care at the time--and we didn't care at that time because we were going to Houston so darn often that there was always another plane."*

Sabala: "Yes I remember that event."

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**Gerald G “Jerry” Schaber** (b. 1938; geologist; Ph.D., 1965, University of Cincinnati)

[Author’s Note: see main text for additional commentary about my Apollo-Era memories]

I am a native of Covington, Kentucky, and went to Holmes High School there. I attended the University of Kentucky (Lexington, Kentucky), and receiving my B.S. in geology in 1960. I then attended the University of Cincinnati; receiving my M.S. in Geology in 1962, and my Ph.D. in Geology in 1965 (Dissertation: crystal chemistry, X-ray structure analysis and synthesis of high-temperature phases within the Bournonite Group of the lead, antimony, and arsenic sulfosalts).

I was interested in the classic array of Ordovician invertebrate fossils throughout the Greater Cincinnati area as a young boy, but was strongly influenced by my eighth-grade physical geography teacher, Louis von Walden, at Holmes High School in Covington, Kentucky. Von Walden had received a degree in geology and perhaps stressed geology more than other aspects of physical geography. In doing so, he strongly influenced my eventual college major and career.

During my youth, I explored the Big Bone Lick area (now a State Park) near Union, Kentucky (south of Florence, Kentucky) that is famous for the vast number and variety of Pleistocene-age vertebrate bones (bison, mammoth, mastodon, horse, sloth, and other species) found there. This sulphurous salt lick, first discovered by a French-Canadian explorer in 1739, became of very special interest to President Thomas Jefferson who, after sending George Rogers Clark to Big Bone Lick to investigate the site, had special rooms at Monticello and the White House for displaying and studying the bones from the site. Several bones from Big Bone were lying in the large rotunda of Monticello when I visited there many years ago.

While attending graduate school at the University of Cincinnati, I was asked to be the on-site-geologist during the first scientific dig of Big Bone Lick that was carried out from 1961 through 1963 under the direction of Bertrand Schultz (University of Nebraska, Lincoln Nebraska), in collaboration with Ellis Crawford (then Curator of the Science Museum, Devou Park in Covington, Kentucky). Frank C. Whitmore, Jr. a well-known USGS vertebrate paleontologist and Pleistocene geologist also participated in this dig.

Totally unknown to me at that time (1961-1963), Frank C. Whitmore, Jr., Chief of the Military Branch of the U.S. Geological Survey back in 1958-1959, had caught the "moon bug" from one of his own Branch members, Arnold C. Mason. Mason had plunged into a study of the Moon both on his own and in his official position as a geologist with the Military Geology Branch. Frank Whitmore brought Gerard Kuiper as consultant and obtained a commercial package of lunar photographs and maps costing a few dollars as raw material (Wilhelms, 1993, p. 38). It is indeed a small world.]

I was initially interviewed (as a final-year Ph.D. candidate at the University of Cincinnati) by John F. McCauley from the Branch of Astrogeology while both were attending the Annual Meetings of the Geological Society of America (GSA) in Miami in the fall of 1964. Lawrence "Larry" Rowan, who received his Ph.D. in geology in spring 1964 from the University of Cincinnati and arrived in Flagstaff with Astrogeology in August of that year, had given my name as a potential candidate for

the branch to Jack McCauley to look up in Miami. After that initial meeting with McCauley, I enthusiastically sent in a Federal Employment application to join Shoemaker's Astrogeology group in Flagstaff. Subsequently I had received job offers to teach at Queens College in New York and to interview with the Bell Telephone Labs. Bell was interested in me because of their interest in using sulfosalts and sulfides for early transistors. Thankfully, I was advised by my dissertation adviser at Cincinnati (Frank Koucky-now deceased) to wait until I heard from the USGS. That turned out to be good advice. Some months later (April, 1965), I was asked to meet briefly with Don Elston and Harrison "Jack" Schmitt at the Greater Cincinnati Airport for a final hiring interview.

I received my formal letter of acceptance from the USGS in June 1965. In this letter, like geologist John MGonigle about the same time, was instructed "to notify Harrison "Jack" Schmitt who will be your immediate supervisor as soon as you are able to determine the specific time you will report for duty." When my wife Sandy, our one-and-a-half-year old daughter Jennifer, and I arrived in Flagstaff by car on July 7, Jack Schmitt had only weeks earlier (in June) moved to Houston, Texas to begin training as a Scientist-Astronaut. As fate would have it, Jack Schmitt would in December 1972 (during the Apollo 17 mission) become the only geologist/astronaut--so far--to explore an area of the lunar surface.

I was originally assigned at the Branch of Astrogeology to work on the Apollo Applications Program and on the development of remote-petrographic (rock and soil) analysis and other analytical instrumentation techniques for use in post-Apollo lunar base missions. In addition to participating with geologists Bob Sutton, George Ulrich and others in the geologic mapping of the Hopi Butte test sites being developed by the Branch as upcoming field test sites of Apollo lunar geologic procedure and training, I also began worked closely with geologists Joe O'Connor and Dave Dahlem who, like me, had graduate university training in the application and development of state-of-the-art analytical instruments of the type that were used in the analysis of geologic materials. Analytical techniques that we researched included X-ray-diffractometry, X-ray spectrometry, and radio-isotope-and flame-spectroscopy, in addition to normal wet-chemical analysis, optical mineralogy, and both standard and automated petrographic analysis techniques. I, like all other geologists hired by Astrogeology in the early years at Astrogeology (both in Menlo Park, California or Flagstaff, Arizona), was also given a lunar quadrangle to map geologically, the Sinus Iridum Quadrangle, which was published by the USGS in 1969.

I participated in numerous Apollo Extension System (AES) and Apollo Applications Program (AAP) field exercises carried out by the Branch of Astrogeology and the Branch of Surface Planetary Exploration between 1965 and 1969 in Hopi Buttes and Meteor Crater, just 70-80 miles northeast and 35 miles east of Flagstaff, respectively. During these tests, I on occasion played the role of a non-suited astronaut in the field. More frequently, however, I was either located inside Astrogeology's mobile petrology laboratory van during such field exercises (performing actual analyses of rock and soil samples collected by the Branch's "astronaut" during the exercise), or was one of the geologists in the Branch's mobile Apollo Data Facility (ADF) van out in the field, or the ADF facility located back in Flagstaff. The ADF was Astrogeology's precursor to the actual Science Operation Room in Houston that became a reality during the actual Apollo Moon missions. My main interest, with regard to the analytical instrumentation being tested during those early field tests in Hopi Buttes and Meteor Crater, were the miniaturized X-ray diffractometer (lent

by JPL) originally developed by Phillips Electronics for the Surveyor Lander (but never flown) and the remote petrographic analysis of microscopic images of rock thin sections using a bore-mounted TV vidicon in place of the ocular on a standard Zeiss Petrographic microscope. I planned and directed several of these early exercises to test analytical instruments and remote petrographic analysis procedures to be used in then planned later Apollo missions (see 1965-1968 in main text for more details).

After Gene Shoemaker submitted his successful Proposal "*Geologic Field Experiment in Early Apollo Manned Lunar Landing Missions*" to NASA in November 1965, I began splitting my time the Advanced Apollo mission scenarios and the Early-Apollo planning under the general leadership of Gordon Swann. As a result, I was eventually included as a team member on what became Shoemaker's Apollo Lunar Geology Surface Experiment (ALGSE) for Apollo missions 11-13.

I participated in the science Operation Room (later called the Support Room, or SSR) in the Mission Control Building at MSC/ Houston during Apollo mission 11. Unfortunately, I was serious injured in an auto accident near my home in Flagstaff (19 September 1969, shortly before Apollo 12. I remained in the Flagstaff Hospital during that mission, and well beyond. Using crutches, I flew with the Flagstaff Mission Control Geology Team from Flagstaff to Houston for the "unlucky" Apollo 13 mission, but returned the next day to Flagstaff when the mission was in trouble and the crew could not complete their scheduled lunar landing.

When Gordon Swann submitted his proposal to NASA in October 1969 to be Principal Investigator on the Geology Experiment Team for the J-missions Apollo 14 and 15, I was formally invited by Gordon to be a Co-Investigator. Following the acceptance of Swann's proposal by NASA, I was tasked by Swann to lead the effort to plan for, designed and see to the production (by the cartographic group in Flagstaff) of the Apollo 14 Lunar Surface Map Package. I was also active in the planning of the Apollo 14 traverses, including the long traverse by Alan Shepard and Ed Mitchell (walking, pulling the Modular Equipment Transporter, MET) up to the rim of Cone Crater to photograph and sample the rim ejecta that was (correctly) thought to have exposed ancient Fra Mauro Formation materials ejected by the impact of the Imbrium Basin.

From 1970 through 1972, Gordon Swann asked me (and other branch geologists) to do the geologic mapping of various, and rapidly-expanding number of astronaut geologic training sites that were needed by the Branch of Surface Planetary Exploration. The objective was to map the site using only stereo-pair aerial photographs, so as to simulate the mapping of a lunar landing site using only Lunar Orbiter photographs or Apollo Metric Camera images. Whoever mapped the geology of the training site also planned the details of the EVA (Extra Vehicular Activity) traverses and station activities for that site. I participated in the mapping and traverse planning at sites in Northern Arizona, Nevada, Hawaii, and elsewhere, used by the Branch of Surface Planetary Exploration for the geologic training of the prime and backup crews selected for Apollo missions 14-17 (see Table I).

For Apollo 15, I was one of the four geologists (including Lee Silver-Caltech, Gordon Swann – USGS, and James Head-Bellcom, Inc., D.C.) who were informally assigned to train the Apollo 15 crew, and plan their traverses from their scheduled landing site adjacent to the Hadley Rille on the

East margin of Mare Imbrium (see 1970-1971 in main text for more details). As was the case for Apollo 14, I was tasked by Gordon Swann to lead the effort to plan, design, and production of the Apollo 15 Lunar Surface Map Package in Flagstaff. Working closely with the Apollo 15 crew (Commander David Scott and LM Pilot James Irwin), James Head, Gordon Swann, and I helped plan the Lunar Rover Vehicle (LRV) traverses (the first Apollo mission to use the LRV), in addition to walking contingency traverses in the case of a Lunar Rover Vehicle malfunction.

[Author's Note: I fondly remember that my father (Fred Schaber) and I, along with Lynda Sowers, Jody Loman, and Ray Batson of the Branch of Astrogeology, attended the Apollo 15 launch at the Cape together; watching from the VIP stands. I also recall that actor Robert Redford, singer John Denver, and the President of Mexico and his wife were also in attendance at the VIP stands for that launch. One of my most cherished possessions (and memories) is a photograph of my father and I in the VIP stands at the Cape to watch the launch of Apollo 15.]

I was not a Co-Investigator of the Apollo Geology Experiment Team for Apollo 16 (Principal Investigator- William Muehlberger (University of Texas, Austin, Texas and USGS, Branch of Astrogeology); however, I participated during that Apollo mission as part of the "Tiger team" at Mission Control in Houston. It was the tiger team's role to provide real-time analysis and make suggestions during and in between the lunar surface EVAs to the formal Geology Experiment Team in the nearby Science Support Room in Mission Control.

On Apollo 17 (November 1972), I played a dual role as Co-Investigator (with Flagstaff USGS colleague Dick Eggleton) on the Apollo 17 Lunar Radar Sounder Experiment Team and as a member of the geology Tiger Team for the Geology Field Experiment Team, lead once again by Principal Investigator William Muehlberger.

I contributed written scientific contributions to nearly all of the NASA-required post-mission science reports for Apollo missions 11, 12, and 14-17, and either senior-authored or co-authored numerous journal articles relative to Project Apollo data during and following the Apollo Era.

Unlike many of my colleagues, I remained with the Branch of Astrogeology after Apollo ended, and shifted my interests to planetary geology (and mapping) and terrestrial and planetary remote sensing, especially the potential use of synthetic Aperture radar in geologic analysis of terrestrial and planetary surfaces.

Starting even before my appointment as a Co-Investigator on the Apollo 17 Lunar Sounder Team, I had been (since about 1966) engaged in assessing the use of multi-wavelength airborne--and eventually spacecraft-borne--synthetic aperture radar (SAR) sensors for use in the remote analysis of various kinds of geology terranes. In 1970, early in the Apollo lunar mission period, I began (along with Walter E. Brown at JPL and Len Berlin at Northern Arizona University) what turned out to be a 15 year investigation into the relationship between SAR wavelength/frequency, incidence angle, and polarization (transmit and receive) in Death Valley, California.

In 1969, JPL engineer Walt Brown built a crude prototype L-band (25-cm wavelength) SAR sensor that JPL mounted on one of their large jet aircraft. One of the first targets over which they flew in 1970 was nearby Death Valley, California. The resultant L-band images were remarkably

detailed and geologically interesting. Having worked with me on the Apollo 17 Lunar Sounder Experiment, and JPL's Seasat radar-imaging satellite, Walt Brown showed the Death Valley images to me. I agreed to go to Death Valley and try to document some sort of correlation –if there was one—between the radar backscatter-brightness on the (then un-calibrated) SAR images and the micro-relief or roughness of the various salt pan and alluvial gravel fan surfaces there. I, along with geographer G. L. "Len" Berlin (Northern Arizona University in Flagstaff) spent a lot of time in Death Valley measuring terrain relief directly, and then and photogrammetrically from stereo photographs acquired at head level, and subsequently helicopter-derived stereo photographs. A paper was published on our early results as a Bulletin in the Geological Society of America (G.G. Schaber, G.L. Berlin and W.E. Brown Jr., 1976, Variations in surface roughness within Death Valley, California: Geologic analysis of 25-cm wavelength radar images, *Geological Society of America Bulletin*, v. 87, pp. 29-41).

Subsequent to the publication of that initial paper in 1976, these early, prototype, L-band overflights were supplemented in the late 1970 and early 1980s by overflights of the Valley using NASA's multi-frequency radar-scatterometer that acquired line radar spectra in three frequencies (C, L and P-bands) simultaneously; and later calibrated SAR images from three sophisticated, multi-frequency, SAR sensors mounted on aboard a new JPL jet aircraft (after the earlier aircraft burned upon takeoff).

A sophisticated Terrain Analysis computer program, developed in Flagstaff by Dick Pike and Wes Rozema (a consultant to the Branch of Astrogeology from Northern Arizona University for use in lunar terrain analysis at Flagstaff in the early 1960s, was applied by Walt Brown (JPL) and myself during the late 1970s through the mid-1980s to develop various terrain-scattering models that related radar frequency, radar incidence angle, and radar transmit-and-receive polarization to various terrain roughness parameters measured in Death Valley and elsewhere.

In 1972-1973 I had rectified oblique images made from photographs taken by the Apollo 17 metric camera from lunar orbit to identified and map three massive eruptive phases of lava flows in Mare Imbrium that I traced to the Euler Hills in southwestern mare Imbrium. Thus was the first ever recognition of major lava flows source vents on the Moon (Schaber, G.G., "Lava flows in Mare Imbrium: Geologic evaluation from Apollo 17 orbital photography; in Lunar Science Conference, 4<sup>th</sup>, Houston, March 5-8, 1973, Proceedings: *Geochimica et Cosmochimica Acta*, v. 1, Supp. 4, pp. 73-92.)

NASA's Mariner 10- Venus/Mercury probe was launched on 4 November 1973 and flew by Venus on 5 February 1974, where it took the first close-up high resolution images of the planets pervasive and complete cloud cover. Mariner 10 reached Mercury on 29 March 1974, passing over the planet at 705 kilometers (438 miles) above the surface. A second encounter with Mercury occurred on September 21, 1974, at an altitude of about 47,000 kilometers (29,200 miles). The sunlit side of the planet and the south-polar region were photographed. A third and last Mercury encounter, at an altitude of 327 kilometers (203 miles), occurred on March 16, 1975. About 300 additional photographs were obtained along with magnetic field measurements. Photographs of Mercury reveal an intensely cratered, Moon-like, surface and a faint atmosphere of mostly helium, resulting from solar wind bombardment. In addition to geologically mapping the Tolstoj quadrangle Basin quadrangle of Mercury (along with J. F. McCauley) (Schaber, G.G., and McCauley, J.F., 1980,



Geologic Map of the Tolstoj Quadrangle of Mercury: U.S. *Geological Survey Miscellaneous Geologic Investigations Map I-1199.*), I also researched the relationship between large, shallow structures and average crustal viscosity (Schaber, G.G., Boyce, J.M., and Trask, N.J., 1977, Moon-Mercury: Large impact structures, isostasy, and average crustal viscosity; *Physics of the Earth and Planetary Interiors*, v. 15, pp. 189-201). I also worked with colleagues McCauley, Guest, Trask and Greeley on unraveling the stratigraphy of the Caloris Basin on mercury (McCauley, J.R., Guest, J.E., Schaber, G.G., Trask, N.J., and Greeley, R., 1981, Stratigraphy of the Caloris Basin, Mercury, *Icarus*, v. 47, pp. 184-202.)

In the summer of 1976, I was invited by colleague Hal Masursky to be part of the Viking-Mars Landing Site Evaluation Team at JPL. Along with Michael Joseph Boyce and Art Dial from the USGS in Flagstaff, we performed numerous crater counts on various potential landing sites imaged acquired by the Viking Orbiters in real time after the preplanned Viking landing sites (resulting from photogeologic analysis of the lower resolution Mariner 9 images) were found to be too rough only after the Viking spacecrafts 1 and 2 were inserted into orbit that summer of 1976. At the same time, I was a member of the Viking Mars radar subgroup (led by G.L. Tyler and R.A. Simpson from Stanford University) within the Viking Site Certification Team. Our job on the radar team was to use Earthbased radar spectra (6-cm and 12-cm wavelength) to evaluate the micro-roughness (blockiness) of any newly proposed potential Viking landing sites.

Photogeology research using Viking images of Mars resulted in the following significant publications (Schaber, G.G., Horseman, K.C., and Dial Jr., 1978, Lava flow materials in the Tharsis region of Mars, in Lunar and Planetary Science Conf., 9<sup>th</sup>, Houston, March 13-17, 1978; Proceedings: *Geochimica et Cosmochimica Acta*, Supp. 9, pp. 3433-3458; Schaber, G.G., 1980, Radar, visual, and thermal characteristics of Mars: Rough planar surfaces: *Icarus*, v. 42, pp. 159-184.;

In 1978 I worked closely with colleague Hal Masursky at the USGS Branch of Astrogeology on NASA Pioneer-Venus mission (launched 20 May 1978; Venus orbital insertion 4 December 1978)), which would map the altimetry (accuracy of 150 m) and (albeit with poor resolution) the surface of the planet for the first time, using a 20 watt (peak power), 1.757 GHz radar-altimeter sensor. Masursky and others (including this author) would publish the first formal analysis of the radar reflectivity and geologic features on Venus acquired from an orbiting radar platform (not Earth based). (Masursky, H., Eliason, Eric, Ford, P.G., McGill, G.E., Pettengill, H.H., Schaber, G.G., and Schubert, Gerald, 1980, Pioneer-Venus radar results: Geology from images and altimetry; *J. of Geophysical Research*, v. 85, no. A13, pp. 8232-8260). This paper would win a major award.

About this time (mid-1970's) I was asked to be a member of the NASA Science Working Group for the Venus Orbiting Imaging Radar (VOIR) mission (later to be renamed Magellan) that would have as its major objective to map the entire surface of the cloud-covered Venus using long wavelength (12 to 25 cm range) synthetic aperture radar (SAR) signals. My active participation in the early VOIR Working Group and my previous field experience in Death Valley and elsewhere, with regard to researching the geologic potential of long wavelength radar, resulted in my later being selected as a Co-Investigator on the Magellan mission which resulted in the radar-mapping

at high resolution radar-mapping of 98% of the planet Venus between 1990 and 1994 (see below for more details).

My early work into documenting the value of SAR for interpreting geologic terranes and structures in Death Valley led me into a two-decade-long effort that would--starting in 1982--take me into Sahara Desert of Southern Egypt and Northwestern Sudan with USGS colleagues Jack McCauley, Carol Breed, and Maurice Grolier to document the unexpected penetration of up to a meter or so of loose modern sand sheet by the Shuttle Imaging Radar-A (SIR-A) flown aboard Space Shuttle Columbia in November 1982. It was during radar-photogeologic analysis of the SIR-A radar images that included parts of Egypt and Sudan that Carol Breed, Jack McCauley and I first discovered systems of west-trending, previously unknown-- and now totally aggraded and sand mantled--fluvial (riverine) channels of all sizes dating from Mid-Tertiary to Pleistocene in age. This discovery resulted in a widely-recognized and award-winning paper in *Science* (McCauley, J.F., Schaber, G.G., Breed, C.S., Haynes, C.V., Grolier, M.J., Issawi, Bahay, Elachi, Charles, and Blom, Ron, 1982, Subsurface Valleys and Geoarchaeology of Egypt and Sudan revealed by Shuttle Radar: *Science*, v. 216, no. 4576, pp. 1004-1020).

Other significant publications resulting from the SIR-A results in Egypt include the following:

Elachi, Charles, Roth, L.E. and Schaber, G.G., 1984, Spaceborne radar subsurface imaging in hyperarid regions: *IEEE Transactions on Geoscience and Remote Sensing*, GE-22, no. 4, pp. 383-388.

McCauley, J.F., Breed, C.S., and Schaber G.G., 1986, The megageomorphology of the radar rivers of the eastern Sahara: Second Annual Spaceborne Imaging Radar Symposium (April 28-30 1986), *JPL Publication 86-26*, Jet Propulsion Laboratory (Pasadena, California), pp. 25-35.;

McCauley, J.F., Breed, C.S., Schaber, G.G., McHugh, W.P., Haynes, C.V., Issawi, Bahay, Grolier, M.J., and El-Kilani, A., 1986, Paleodrainages of the Eastern Sahara: The radar rivers revisited; *IEEE Transactions on Geoscience and Remote Sensing*, GE-24,, no. 4, pp. 624-648.

Schaber, G.G., McCauley, J.F., Breed, C.S., and Olhoeft, G.R., 1986, Shuttle Imaging Radar: Physical controls on signal penetration and scattering in the Eastern Sahara: *IEEE Transactions of Geoscience and Remote Sensing*, GE-24, no. 4, pp. 603-623.

McHugh, W.P., McCauley, J.F., Breed, C.S., and Schaber, G.G., 1988, Paleorivers and geoarchaeology in the southern Egyptian Sahara: *Geoarchaeology*, v. 3, no. 1, pp. 1-40.

McHugh, W.P., Breed, C.S., Schaber, G.G., McCauley, J.F., and Szabo, B.J., 1989, Acheulian sites along the "radar rivers", southern Egyptian Sahara: *Journal of Field Archaeology*, v. 15, pp. 361-379.

McHugh, W.P., Schaber, G.G., Breed, C.S., and McCauley, J.F., 1989, Neolithic adaptation and the Holocene functioning of Tertiary paleodrainages in southern Egypt and northern Sudan; *Antiquity*, v. 63, pp. 320-336.

Three subsequent SIR missions (SIR-B in 1984 and 2 flights of SIR-C in 1994) provided multi-frequency (X-, C- and L-band) radar images of not only the Northern Sahara (for McCauley and Breed, and myself as Co-Investigators), but many additional sites around the world for other SIR Experimenters. Significant publications that included this author that resulting from these later SIR missions (most notably the two SIR-C flights in 1994) include the following:

- Davis, P.A., Breed, C.S., McCauley, J.F., and Schaber, G.G., 1993, Surficial geology of the Safsaf region, south-central Egypt, derived from remote-sensing and field data: *Remote Sensing of Environment*, v. 46, pp. 183-203.
- Schaber, G.G., McCauley, J.F., and Breed, C.S., 1997, The use of multi-frequency and polarimetric SIR-C/X-SAR data in geologic studies of Bir Safsaf, Egypt: *Remote Sensing of Environment*, v. 59, pp. 337-363.
- Schaber, G.G., 1999, SAR studies in the Yuma Desert, Arizona: Sand penetration, geology, and the detection of military ordnance debris: *Remote Sensing Environment*, v. 67, pp. 320-347.
- Schaber, G.G. and Breed, C.S., 1999, The importance of SAR wavelength in penetrating blow sand in Northern Arizona: *Remote Sensing Environment*, v. 69, pp. 87-104.
- Lancaster, Nicholas, Schaber, G.G., and Teller, J.T., 2000, Orbital radar studies of paleodrainages in the Central Namib Desert: *Remote Sensing Environment*, v. 71, pp. 216-225

My role as a Co-Investigator on the Magellan radar-mapping mission to Venus (1990-1994) resulted in some widely-heralded publications resulting from our study (with colleague Robert Strom, U. of Arizona, Tucson) of what exactly the rather limited, but spatially random—distribution (on a global scale) of impact craters on Venus are telling us:

- Schaber, G.G., Moore, H.J., Strom, R.G., Soderblom, L.A., Gaddis, L.R., Boyce, J.M., Kirk, R.L., Chadwick, D.J., and Russell, Joel, 1992, The geology and distribution of impact craters on Venus: What are they telling us?: *J. of Geophysical Research*, v. 97, No. E8, pp. 13,257-13,301.
- Chadwick, D.J., and Schaber, G.G., 1993, Impact crater outflow on Venus: Morphology and emplacement mechanisms: *J. of Geophysical Research*, v. 98, no. E11, pp. 20,891-20,902.
- Strom, R.G., Schaber, G.G. and Dawson, D.C., 1993, The global resurfacing of Venus: *J. Geophysical Research*, v. 99, No. E5, pp. 10,899-10,926.

B.S	University of Kentucky-(1960, Geology)
MS	University of Cincinnati (1962, Geology)
PhD	University of Cincinnati (1965, Geology)
Team Member:	Apollo Lunar Geology Experiment Team--Apollo 11-12
Co-Investigator:	Apollo Lunar Geology Experiment Team--Apollo 13-15
Tiger-Team Member:	Apollo Lunar Geology Experiment Team--Apollo 16-17
Co-Investigator:	Apollo Lunar Radar-Sounder Experiment-Apollo 17
GSA Commendation	for Astronaut Training, 5 February 1973
Team Member:	Viking-Mars-Landing Site Certification Team, 1976
Co-Investigator:	Space Shuttle Imaging Radar Experiment-A (SIR-A)
NASA Group Achievement Award	Shuttle Imaging Radar-A Development Team, 1982
Asst. Chief, USGS, Branch of Astrogeology	December 1978 to August 1983
Chief, USGS, Branch of Astrogeology:	July 1983 to December 1986
SAR Science Team Investigator:	Space Shuttle Imaging Radar Mission B (SIR-B)
Dept. of Inter. Meritorious Service Award	1988

Co-Investigator: Team	Magellan-Venus Radar-mapping Mission, Science
NASA Group Achievement Award (Magellan Radar-Science Group)	1992
SAR Science Team Investigator:	Space Shuttle Imaging Radar Mission-C, (SIR-C)

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**David L. Schleicher** (b. 1937; geologist; Ph.D., 1965 Penn State University) arrived in Flagstaff on 15 July 1965 as was assigned to the Apollo Application Program-Geologic Methods Group. Dave would become one of the Branch of Astrogeology's first "astronaut" test subjects to operate within the difficult spacesuit environment (on pure oxygen at first) during very hot days of the Branch's Early Apollo and Post-Apollo lunar geologic procedures development tests in Hopi Buttes starting in 1965. Dave was also active in the development of the Branch of Astrogeology's "science back room" communication and recording concept using USGS astronaut test subjects in the field. Dave Schleicher was a member of the Apollo Lunar Geology Experiment Team for Apollo missions 11-13. Dave left Flagstaff following Apollo 13 and relocated at the Geological Survey's Federal Center in Denver, Colorado.

The following was taken from an interview with David L. Schleicher (Golden, Colorado) by Gerald G. Schaber on 25 April-2001.

Schleicher: "I grew up in Pennsylvania, and eventually went to Penn State University. I started out majoring in physics but decided that I was really more interested in more tangible things. So late in my junior year I switched my major to Geology. That following Summer I went on to Caltech where I ended up with a M.S. degree three years later in 1962. Then I returned to Penn State. Professor Dick Jahns, whom I had met at Caltech, had gone back to Penn State as Dean of the College--as it was called at that time. So, I went back to Penn State and finished up my Ph.D. dissertation with Dick Jahns in 1965; and through Dick met [Harrison] Jack Schmitt who was interviewing people at Penn State for the Astrogeology program in Flagstaff. Jack interviewed both John MGonigle and me at that time—and I believe Tim Hait as well. The rest is history. I was offered the job; I accepted the job, and started with Astrogeology in Flagstaff on July 15, 1965."

*Schaber: "You didn't just switch from physics to geology your first time at Penn State because you just thought it was easy or something--you must have been interested in it-right?"*

Schleicher: "Oh yeah--very much so. Well, it was one these funny things. One of my favorite recurring stories is the fact that my entire life was influenced by a computer that [blindly] assigned my dorm room as a freshman at Penn State. A guy a couple of doors down the hall was dating Ella [later, David Schleicher's wife], and the two guys in the room next door were taking geology. I had just really never thought about geology before that time, but they talked about it with some enthusiasms and I guess I got caught up in the enthusiasm and too an introductory course in it. From that point on, I was hooked, so I changed majors."

*Schaber: "What about space interests?"*

Schleicher: "No, I didn't have any particular early interest in space; you know, other than the kid who is just generally interested in science and sees the aurora occasionally, and looks at the Moon, etc."

*Schaber: "What were your feelings when the Soviet Sputnik was put into orbit?"*

Schleicher: "Oh that was exciting! We walked out on the golf course across from the dorms in the wee hours of the morning to see the thing going overhead. Yeah, that was exciting, but I can't say that I was thinking of it in terms of a career.

I went through a period back in high school of reading all kinds of books on rocketry, escape velocity, and the equations for two and three-stage rockets, and that sort of thing. But, that was sort of a passing thing. It was actually the result of one of the student teachers we had in a biology class."

*Schaber: "Okay, so you get to Flagstaff and what did you start out doing? You had a lunar geologic quad assigned as I recall."*

Schleicher: "Oh yes; John MGonigle and I worked on the Plato Quad [located adjacent to this author's Sinus Iridum quad]. Like John, I was on the Apollo [manned lunar] side of the house. I think, from the start, we were gearing up for those Apollo landings; [developing] and working with the Lunar tools with John and Ivo Lucchitta. We were heavily involved in sorting out just how things ought to be described [orally from the lunar surface]. Yeah, these guys were going to be scurrying around on the surface talking about what they saw."

*Schaber: "So, you were mostly interested in the Lunar Tools and how an astronaut could actually do geology on the lunar surface."*

Schleicher: "The descriptions [how one actually describes what one sees--in an efficient and clear manner] are the thing that I remember most, because it is the thing that has had the most profound effect on what I've done with the rest of my life."

*Schaber: "You were involved in most of the early tests out at Hopi Buttes, and you were one of the few Branch geologists who actually donned the early Gemini and prototype Apollo spacesuits during our early field tests, under extremely hot condition sometimes, out in Hopi Buttes. John MGonigle told me some horror stories about that."*

Schleicher: "The one specific bit that I remember about the suits is that I used to get Gus Grissom's suit, because he was the only guy who was short enough that I could fit into it.

[Author's Note: -Gus Grissom was one of the three astronauts killed during the Apollo 1 fire at the Cape in 1967].

*Schaber: But you routinely reached dangerously high body temperatures and water loss through perspiration in the suits during our summer exercises out in Hopi Buttes, as I recall. Gordon Swann told me that his temperature in a suit out there reached 107 or so at one time."*

Schleicher: "Oh, my pulse rate on one test was up to 180 or something astronomical; and the suits were confining, stiff, and heavy. Good God, you're working hard lugging around an extra 150 pounds or so."

*Schaber: "And the Mercury and Gemini space suits weren't made for walking--they were made for sitting."*

Schleicher: "Yeah, and certainly not walking in one-G [gravity on Earth]."

*Schaber: "What if any astronaut training trips did you participate in early on?"*

Schleicher: I went on two astronaut trips with the astronauts in the Grand Canyon, and I went on an astronaut trip to Iceland. The first Grand Canyon trip included a mob of astronauts, and the second trip was Bruce Mc Candles and Ed Gibbons. Ella [Dave's wife] and I took them down the Canyon. For some reason they couldn't make the main trip earlier [with the larger group of astronauts].

*Schaber: "Dave, tell me your overall feelings about the Apollo Era experience working at the Branch of Astrogeology in Flagstaff."*

Schleicher: "Well, you know one of the strangely surrealistic aspects of the whole experience is that it did feel sort of like a movie."

*Schaber: "It did. It was very surreal!"*

Schleicher: "There was a sense of excitement certainly, but it was very subdued relative to what was happening at the time."

*Schaber: "We were just so busy; we couldn't think about it. And if we did, we would have kept daily journals like Red Bailey did."*

Schleicher: "Yes, that's true."

*Schaber: "So, it was Apollo 11 time; please summarize for me what you were doing for the Geology Team down at Mission Control in Houston for that historic Apollo 11 landing."*

Schleicher: "I was in one of those little side rooms. I was actually in on one of the simulations [called SIMS] just before Apollo 11. I sat in a little glass booth just off the main control room. Myself and someone else--I have forgotten who--were the astronauts. We had to come up with descriptions of what we were doing and what we were seeing while the people in the Control Room listened and took notes, etc."

*Schaber: "So you were on the crater field at the Cape?"*

Schleicher: "No, we were actually sitting in the other room [at the MSC Control Center]."

*Schaber: 'Oh, just making up things?'"*

Schleicher: "Yeah, sitting in the other room. We probably had some sort of scenario sketched out--but I don't remember at the moment."

*Schaber: " We had remotes from Houston down to the CDRA/ADF on the fifth floor of the Arizona Bank Building before we built Fourth Street (ADF) building. Then we had remotes from Cape Kennedy and MSC Houston to our Fourth Street facility, as I recall. All I know is that Johnny Nuttall [head of the Branch Electronics group] and his associates were amazing at getting all of these remotes set up with AT&T, etc.*

*So let's talk about Apollo. I think I participated in the whole Apollo 11 Mission in a daze.*

Schleicher: "Well, probably all of us did; we were like drunk--without sleep."

Well, I certainly think that there was some sense of history. I think the thing that I marvel at most was sort of the social effect--for a few hours the whole world had just stopped and watched what was going on. And then the thing that was amazing was the anticlimax that almost instantly the effect was gone. It had been such a spectacular effect--and then it was so short lived."

*Schaber: "Well, I don't know. I suppose that I felt that there was more to come, and we were so busy training the next two or three crews!"*

Schleicher: "Yes, and I stayed on through Apollo 12 and then 13--which of course was a bust--and then after that I was gone."

*Schaber: "What did you actually do with us during the missions?"*

Schleicher: "I sat in one of those back rooms, and you know thought hard, and tried to formulate questions; most of which never got sent up to the crew on the Moon."

I think, in fact, we had very little input into the [early] missions—actually during the missions. After the missions of course, dissecting everything that they had said [of geologic relevance] was a chore. I think on Apollo 11, almost right away, I started working from the transcripts to create a coherent transcript of everything that was said-. And then, we created a paraphrased transcript which was--again--something that probably led me to where I am now [working for Quest Communications in Denver], working with words a lot--thinking about what they meant to say rather than what they had literally said.

*Schaber: "Yeah, getting rid of the noise in the transcripts in other words!"*

Schleicher: "Right--Getting rid of the noise, exactly!"

*Schaber: "Our edited transcripts of the Apollo missions were very nice documents because they were basically summarized all of the science."*

*I think you might agree. With our training out in Hopi Butte, we were so far ahead of the game--and the geology guys in Houston. It wasn't until after Apollo 15 that the Houston geology group finally gave up trying to beat us at our own game; they said you win, we couldn't do it any better than you have done. On Apollo 15 you would have been very proud Dave, we printed EVA reports ports after each 7-hour EVA-- and printed them (sometimes in the wee morning hours) using MSC's own print shop. We would even have hardcopy pictures from the EVA video in these interim reports. We would put our green USGS covers on them, over in the print shop, and lay them on the mission control people's desks--on all seventeen rows, or whatever, in between the EVAs so the mission control people (including Gene Kranz) what they accomplished during the previous EVA. It was quite a coup.*

*In fact, Gene Kranz came up to Gordon Swann after the first EVA on Apollo 15 (after finding our report on his desk) and said hey Swann, this is really great. You guys must have three 8-hour shifts like we do huh; do you have guys who work in between the EVA's? Gordon said no, it's all the same group of people. Gene Kranz said that's impossible! Gordon said no! Kranz said when do you sleep? Gordon said hell, we don't!"*

Schleicher; "Yes, we had been thinking about how to do the missions for years [since 1964] in a disciplined way."

*Schaber: "So you left Astrogeology after the failure of Apollo 13 and went to Denver. Was there a job offer?"*

Schleicher; "It was an opportunity. I think that John MGonigle and I had many of the same reactions. I don't remember any particular run-in's with Shoemaker. I just had this idea that I had to be a field geologist --you know--that I had to do the thing that I passionately wanted to do when I went into geology. There was an opportunity to do that sort of thing on a project that was actually an outgrowth of the lunar project. It was a project to reinvent terrestrial geology, applying every technique that we had in the books--many of them of course that came directly out of the Apollo program. So, we spend a few years trying to reinvent geology. We weren't very successful."

*Schaber: "Reinvent in what way?"*

Schleicher: "Reinvent the techniques of doing field geology; just looking at new ways of doing basically the same tasks."

*Schaber: "Who in the Survey was smart enough to admit even being interested in trying this idea to reinvent the way we do geology?" In those days such an idea would have been considered crazy in the Survey right?"*



Schleicher: "It was a clever idea that I believe came out of MGonigle collaboration with Harry Snead, because Brokaw had had the imagination to send off some of us field geologists want-to-be's to work with real field geologists during one of those summers. MGonigle worked with Snead, Ivo worked with Al Proasca and I worked with Jerry Richmond."

*Schaber: "In general though, you learned a great deal about how to do the things you are doing now from your work in Astro-right?"*

Schleicher: "Oh I think so. I think it had a profound effect! As I say, those paraphrased transcripts [from the Apollo missions]; thinking about how things need to be described. It had to do with classifying information and presenting it so other people can use it. The other aspect of the job [at Astrogeology] that really spoiled me for all later work was--getting me to be a real junky for doing things that had never been done before."

*Schaber: "Yeah, all of us in early Astro had to do that."*

Schleicher: "Yeah, but it's followed me throughout my career and is the source of my frustration right now that I'm being forced to do things precisely the way they have always been done--which is not very good. They were done not very well.

Then, not too long after that, along came environmental impact statements. I was thrust into them kicking and screaming. However, they were fascinating Jerry because again--here was a new game. Nobody knew what the rules were because the rules hadn't been invented yet--they hadn't been made up, and it was really fun just sorting out the rules and deciding what kinds of information that people need, and how do we assemble it. It was geology in its classic sense of coming up with information needed to make critical decisions when you don't have enough information, and you have to squeeze every scrap of useable content out of the information you do have. It was fun, and it continued through jobs like that throughout my whole Survey career. I think those were the best things I've done n the Survey."

*Schaber: "I'll bet. It seems to me that in writing an environmental impact statement, it would be real hard not to be biased if you're pro-environment, etc."*

Schleicher: "You very carefully learn to take a careful middle ground, but to lay out both sides so that somebody can make a rationale decision. In fact, I think that most people I worked with were strongly pro environment and yet managed, for the most part, to put that bias aside."

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**Harrison Hagan” Jack” Schmitt** (b. 1935; geologist/astronaut; Ph.D., 1964, Harvard University), who would, as LM Pilot, explore the Taurus-Littrow Valley on the Moon during Apollo 17 with Commander Eugene Cernan in December 1972, started working in Flagstaff with the Branch of Astrogeology in mid-July 1964. Schmitt's assignment was to work with Gordon Swann on the development and testing of Early-Apollo lunar geologic exploration procedures. In June 1965, Schmitt would be selected as NASA's only geologist/Astronaut; he left Flagstaff June 1965 to begin NASA flight training.

The following biographical information on Jack Schmitt is from [astronautix.com](http://www.astronautix.com). (<http://www.astronautix.com/astros/schmitt.htm>):

“Jack Schmitt was born 3 July 1935 in Santa Rita, New Mexico. He received his Bachelor of Science degree in science from the California Institute of Technology in 1957 and Doctorate of Philosophy in Geology from Harvard University in 1964. Jack worked for the Norwegian Geological Survey on the West Coast of Norway and for the U.S. Geological Survey in New Mexico and Montana. He also worked two summers as a geologist in Alaska. Jack was involved in photo and telescopic mapping of the Moon with the U.S. Geological Survey's Branch of Astrogeology [in Flagstaff, Arizona] when NASA selected him in June 1965 in its first group of scientist-astronauts [astronaut group #4].

Because he was not a pilot, Schmitt attended a 53-week course in flight training at Williams Air Force base, Arizona. Graduating first in his class, he received his Air Force jet pilot wings and later his Navy helicopter wings. While training for a Moon mission, Jack provided Apollo flight crews with detailed instruction in lunar navigation, geology, and feature recognition. He also assisted in the integration of scientific activities into Apollo missions and helped analyze lunar soil samples returned by the astronauts.

Jack Schmitt was originally assigned to the Apollo 18 crew, but when that flight was cancelled, he was moved up to Apollo 17 under pressure from the scientific community. He became the only geologist to set foot on the Moon.

Apollo 17 was launched on 17 December 1972, and three days later Schmitt (LM Pilot) and Commander Gene Cernan landed their Lunar Module in Taurus-Littrow, while Ron Evans orbited overhead in the Command Module. Jack Schmitt and Gene Cernan made three Lunar Rover traverses over as many days. On 19 December, the last two men on the Moon during Project Apollo came home, splashing down in a safe landing in the Pacific Ocean.

Schmitt resigned from NASA in 1975 to run for the U.S. Senate in his home state of New Mexico. He was elected 2 November 1976, and served one six-year term. In his last two years, he was chairman of the Subcommittee of Science Technology and Space. He then became an independent consultant in science, technology, and public policy in Albuquerque, New Mexico [See main text for more details on Jack Schmitt's role during his brief stay at the Branch of Astrogeology (1964-1965).]

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**Laurence A. Soderblom** (b. 1944; Geophysicist; Ph.D., 1970, Caltech), an exceptionally gifted and broadly-talented scientist entered on duty in August 1970 with the Branch of Astrogeology at Pasadena, California in WAE status while a post-doc at Caltech after receiving his Ph.D. in geophysics from Caltech (Pasadena, California) in June 1970. As a post-doc at Caltech between June and October of 1970, Larry was working closely with Professors Bob Sharpe and Bruce Murray on analysis of the Mariner 6 and Mariner 7-Mars flyby data. Thus, he had already been engaged in Mars (planetary) data even before coming to Flagstaff in October 1970.

The following was taken from an interview with Larry Soderblom by Gerald G. Schaber on 13 February 2002:

Soderblom: “I was born in Denver, Colorado on 17 July 1944. My father worked at Lowry AFB as one of the first computer specialists in B-29 gunnery computers. This was significant because it gives a little on my background. We moved to Las Vegas, New Mexico a year later (1945) and I grew up there and went to Robertson High School in Las Vegas, New Mexico. That high school was unique in that Las Vegas was a small college town, and most of the spouses of the college professors taught at Robertson High School. So I had four years of Russian; I had a course in topology and set theory, and several courses in Philosophy--Aristotle, Plato, etc. Anyway, it was kind of a unique education. From there I went to New Mexico Institute of Mining Technology--now known as New Mexico Tech. I started out in geology, and halfway through switched to physics; so I decided to get two degrees. I ended up with a double (BS) degree--one in physics, the other in geology. That was from 1962 to 1966.

I graduated in 1966 from New Mexico Tech. and went to Caltech (Pasadena, California) from 1966 through 1970 in the Division of Geological and Planetary Sciences. I started out with Bruce Murray and Gene Shoemaker as advisors (see above for description of Ph.D. research).

Then, as a post doc at Caltech, I ran into Hal Masursky and Jack McCauley. They suggested that I ought to come to work at Flagstaff for Astrogeology. As far as any Apollo-related work at that time, the kind of thing I worked on mostly was analysis of lunar imaging data with the Branch of Astrogeology. I was working with Jack McCauley doing things like looking at the crater distribution in the near-terminator regions at 0.5-degree grazing angles.

I did publish a couple of papers on my lunar crater work in one or more of the Apollo Science Reports. Then, I got involved in using the kind of things that I did while a post-doc at Caltech [i.e., age dating of the maria using his crater erosion model, and determining the compositional variations using reflectance spectroscopy, etc.] to classify the characteristics of the Apollo landing sites. Sorting out ages and chemistry, I concluded that the ages of the landing sites ought to decrease from east to west and Apollo 11 ought to be higher titanium; while Apollo 15 was in part lower, etc. So, all of that is a matter of record [Larry was correct of course]. But the primary thing, as far as contributing to Apollo was this use of the crater erosion model in order to get ages of very small areas of the lunar maria.”

When asked by Gerald Schaber during their interview to briefly describe his crater erosion model, Larry responded as follows:

Soderblom: “Basically what you do is start out with a very fresh impact crater with a characteristic internal slope of the order of the angle of repose--like 30 and a few degrees. Then you model its evolution through time through the rain of small impacts gardening. It changes its shape in a predictable way. I wrote down the equations. Basically, they looked like a heat-flow equation. I solved it; the craters take on the form of a zero order Bessel Function and basically just decays in an exponential way. Then, by looking at individual craters; by looking at the largest crater that was just shadowed in a region of the maria, you could say that a crater at that diameter would be worn to that depth--which was set by the Sun angle on that image. Then, that could be related to a

net-erosion for that surface. Then, of course, the largest craters would be steeper with more shadow because they wouldn't have been around long enough to have been eroded to that level. And then, of the craters that are smaller than that size, some would be shadowed--because they are fresh--and some would be unshadowed because they have been highly degraded. Therefore, it was vital to determine that critical diameter where some of its members of that particular crater size had gotten eroded down to that slope. That is basically the way the model works.

The Mariner 9 project was over in 1971-1972. The Voyager project started in 1972, and we launched the first probe in 1977. Then Viking Mars (lander and orbiter) started taking data in the summer of 1976. Parallel to Voyager, I got involved in Magellan [Venus Radar Mapper, formally called Venus Orbiting Imaging Radar, or VOIR], in Galileo and Galileo NIMS, and then Deep Space One. I also worked on Mars Pathfinder and Mars Global Surveyor. Fortunately, I wasn't really involved in either one of the failed mars probes.”

[Author's Note: To properly do justice to the remarkable contributions and accomplishments of Larry Soderblom to date in his career at the Branch of Astrogeology with regard to both lunar and planetary science is beyond the scope of this work that is focused on the role of the USGS in the Apollo Program. However, the unique talents and varied contributions by Larry Soderblom to the U.S. Geological Survey, NASA, and the overall Space Science Community to date, can perhaps be summarized here by listing a sampling of Larry's impressive career achievements to date, as abstracted from his Professional/Technical Record at the U.S. Geological Survey:

#### A. Voyager Imaging Science Experiment

In 1971, Larry was selected as one of the few original team members for the Voyager Mission Imaging Science Experiment. He became Deputy Team Leader of the experiment with responsibility for conducting that part of the scientific experiment focused on the satellites of Jupiter, Saturn, Uranus, and Neptune, and with the responsibility of technical leadership of the Teams' digital image processing and data processing activities. Soderblom often took on the full responsibility of team leadership and represented the Team in major Project decisions, owing to long-term commitments abroad of the Team Leader, Brad Smith. Larry often accepted full responsibility for coordinating the Team's science analysis and preparation of scientific reports. Although Larry twice received NASA's medal for Exceptional Scientific Achievement for his contributions to Voyager, he always believed that the real reward was his exposure to the trials of scientific team leadership.

#### B. Development of Imaging Spectrometers for Multiple Planetary Missions

Imaging spectrometers are devices that require digital images in hundreds of wavelengths; we often refer to the data that such devices generate as "image cubes." The scientific potential for such devices in revolutionizing remote sensing is enormous. They combine the compositional analytical capabilities of laboratory spectroscopy with high quality synoptic digital imaging. By the late 1970's these instruments became technologically feasible. A small group of planetary scientists (of which Soderblom was one) proposed a simple imaging spectrometer (NIMS, Near Infrared Mapping Spectrometer) for flight on the Galileo mission to Jupiter. (Galileo was launched in October of 1989). Similar instruments were tested for Earth observation (for example,

AIS, the Airborne Imaging Spectrometer). In 1983, an engineering group at Caltech's Jet Propulsion Laboratory and Larry proposed to develop a generic suite of advanced imaging spectrometers for flight on multiple NASA planetary exploration missions. The activity was funded and started. Their scientific group was expanded to include two other smaller teams who had proposed similar developments. Soderblom was asked to lead this activity. In addition to Galileo's NIMS at Jupiter, Cassini, carrying VIMS, was launched for Saturn.

### C. The Planetary Image Data Problem

The archive of digital images collected by (mostly NASA) space probes of planetary surfaces, atmospheres, rings, and satellites are now quite large. The images in their original digital formats constitute the primary scientific data resource because the digital dynamic range far exceeds that of hardcopy; quantitative analyses mandate the data in numerical form, and in order to capitalize on technological breakthroughs in analytical techniques the original data is essential. Larry Soderblom was one of the pioneers in pushing to preserve these invaluable planetary data resources in their original digital format.

By the early 1980's the situation was as follows--according to Larry: most research investigators had easy access to hardcopy images only; a small number of institutions had the ability to access, process, and utilize the digital data. The data themselves were being lost through deterioration of storage media. Technical reference material, calibration data, and spacecraft ephemerides needed to process the data were disorganized, dispersed, and usually difficult-to-impossible to relocate. Even if an investigator was successful in accessing the digital data and necessary supporting material, software to process the complex, raw spacecraft image data was difficult to come by---and for most, impossible to recreate. The planetary community stood at the brink of losing a national scientific resource.

The Branch of Astrogeology has developed and maintained a large digital image processing capability to support its commitment to the NASA Planetary Program in cartography, photogrammetry, and scientific research. In 1984, as part of a larger NASA Planetary Data System effort, the USGS group, under the leadership of Larry Soderblom, undertook a major effort to help solve the planetary image data system crisis. Their activities focused on four primary components. First, they became involved in efforts to protect the basic image data archive. By catalyzing efforts of the PDS and Voyager Projects, the Astrogeology group in Flagstaff played a leading role in initiating archives of images on CD-ROM's for wide-scale distribution. The Voyager and Viking, and Magellan (radar) images have now been preserved in this manner.

Second, Astrogeology's digital data preservation group, under Soderblom's leadership, undertook a major effort to restore radiometric calibration data (from in-flight data if necessary) for Mariners, Viking, and Voyager to convert them to a form tractable for distribution. Those files are now widely used throughout the community.

Third, the Astrogeology group played a leading role (along with JPL navigation) in an effort to standardize the essential ancillary geometry data (spacecraft, target-body, and solar relative position and pointing files). Astrogeology's own Cartography program adopted and utilized this standard system, so now investigators worldwide can gain easy access to the highest quality

geometric data. Fourth, the Astro group under Soderblom's direction organized, documented and released an image processing software system (called PICS, Planetary Image Cartography) derived from Astrogeology's own capability. This system, now also used widely by the community, allows the investigator to start from raw spacecraft images, perform a range of radiometric and geometric corrections, control and map images to a variety of formats, and perform a wide variety of scientific analyses.

Scientific Leadership--Larry Soderblom considers that perhaps his most significant contributions in leadership of the NASA Planetary Exploration and USGS Astrogeology Programs have been his heavy involvement in five episodes of long-range planning of the NASA planetary exploration program. Unlike most committee work (which Larry says is usually a total waste of everybody's time), he is--and rightfully so--extremely proud of contributions that he has made in these studies. This included the original SSEC (Solar System Exploration Committee) of the early 1980-'s (which Larry co-sponsored as chairman of a NASA Advisory Council Subcommittee). The SSEC study defined a Mars climatological orbiter (which became Mars Observer), a Saturn orbiter and Titan probe (which became Cassini-Huygens) and a Venus radar mapper (which became Magellan). During these periods Soderblom served as member and/or chairperson of: the NASA Advisory Council, the NASA Space Science and Applications Committee, the Solar System Exploratron Subcommittee, the NASA Committee of Planetary Exploration, in some cases on more than one occasion. After the failure of Mars Observer, Larry served on the Mars 2000 Committee that defined the 10-yr. long Mars Surveyor Program that we are engaged in today.

During the 1995-1997 timeframe, Larry co-chaired the Solar System Roadmap activity along with Charles Elachi of JPL. Through this activity, they defined and promoted elements of the NASA Origins Programs that eventually became included as congressional budgetary line items for Outer Planet Exploration (which is generating the upcoming Europa and Pluto missions) as well as a major augmentation to the Mars exploration program to secure funding for a returned sample in 2005. Larry has also been participating in the Mars Architecture Committee, which is dealing with technological priorities, and political and societal ramifications of both returned Mars samples and the definition of a combined robotic-manned Mars exploration program.

During the last few years, Larry Soderblom feels that his most important contribution has been in promotion and introduction of advanced technologies into the NASA planetary exploration Spacecraft program. This has come in two areas--in solar electric propulsion and in the most technologically advanced instrumentation package ever flown. Soderblom has been heavily involved in the definition and development of the Deep Space 1 mission now in flight. As NASA and JPL management will testify, as a member of the team that originally defined the first few New Millennium missions, Larry hounded the system to get solar electric propulsion chosen as the principal new technology to be demonstrated and flight validated by DSI. Larry strongly believes that electric propulsion will open the gateway to the next major epochs of space exploration (and ultimately space travel); DSI has now completed its first 1000-hour test of the xenon ion engine.

The second area of contribution, with regard to his promotion and introduction of advanced technologies in the NASA system stems from a project started about 1987. Recognizing that spacecraft must become lighter (thus cheaper to launch) and less expensive to build and fly, Larry became concerned that the ambitions and capabilities of these missions might well fade into

mediocrity. He proposed and led a project to define an instrument package for a flight to Pluto [the only planet--if it is one--which NASA has not visited up close)]. [The Pluto Mission remains a personal goal of Soderblom]. The design goals of PICS (Pluto Integrated Camera-Spectrometer) was to weigh in at 10 kg, operate on 10w, and cost <\$10M a copy. This instrument was to retain the potency of the Voyager, Cassini, and Galileo remote sensing payloads that all weigh, cost, and use power at tens times these rates. The resultant PICS instrument was built out of silicon carbide to make it ultra-light but retaining high optical performance, used integrated optics, electronics, structures, thermal control, shared focal planes to create an instrument that combines the full-blown capabilities of (a) near-IR imaging visible imaging spectrometer, b) a near-UV imaging spectrometer, and (c) two high-quality long-focal-length visible imaging channels. PICS met the challenges of cost, mass, and power and was chosen (under the name MICAS) as the primary remote sensing package to be flown on DSI. It is undergoing test flight at the present time. Soderblom believes with this introduction of electric propulsion and advanced instrumentation; NASA's faster-better-cheaper program can now actually yield better science. Larry admits that his earlier fears of a mediocre future for NASA and planetary science are over.

[Author's Note: Larry Soderblom is an amazing person with a brilliant mind to match. His decision to join the Branch of Astrogeology in August 1970 was without doubt one of the most important things that ever happened to the Branch and the U.S. Geological Survey. Setting his love of, Flagstaff, nature and outdoor activities ahead of accepting the more lucrative offers he has passed up for over 35 years from Universities, NASA Centers, etc., points to Larry the person, not Larry the exceptionally gifted scientist. Larry always has his priorities straight. The post-Apollo record of the Branch of Astrogeology would certainly have looked different without Larry's forward-looking leadership and talent for making things happen. The comparison of Larry and Gene Shoemaker, his mentor at Caltech, is obvious. Larry still works for the Branch of Astrogeology and he and his wife Barbara still reside in Flagstaff.]

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**Hal G. Stephens** (b. 1917; geologist/photographer) received his AB and then MS degree in geology from the University of Missouri. In 1941 he went to Houston Texas and worked for the Mott-Smith Corporation doing gravity surveys in the Southeast U.S.-for six months. He went back to Columbia Missouri and worked for the Continental Oil Company for about nine months; then joined the U.S. Geological Survey, where Hal started out working on the Survey's bauxite project.

He was drafted in 1945 and went into the Army Air Corps where he worked as a photogrammetrist in various military bases in the U.S. until the war ended. Hal rejoined the U.S. Geological Survey in 1947 and went to the Black Hills of South Dakota on the Survey's pegmatite project. Lincoln R. Page [one of Gene Shoemaker's "Angels"] was the project Chief. At the end of 1948 or early 1949 Hal joined the Survey's uranium project on the Colorado Plateau. It was there that he first met that brash young, 19-year old geologist, who had just received his Masters Degree from Caltech, Gene Shoemaker. Hal was on the Colorado uranium project until 1953 when he went out to the Survey's San Francisco offices, then in the old U.S. Mint Building (before the Survey's center at Menlo Park, California was completed). In December 1953, when Building One at Menlo Park was completed, Hal moved to Menlo Park and was headquartered there until he moved to Flagstaff in June 1965 to work for the Branch of Astrogeology.

The following was taken from an interview with Hal Stephens by Gerald Schaber on 4 April 2002:

Stephens: "I was born 8 June 1917 in a little seacoast village, a few hundred miles south of Shanghai, China. My father, while in China, was with a shipping agency. He would meet the incoming ships, go out on a launch and meet them, check their cargo and things like that. He was born in Quanyen, China. My grandparents were both missionaries there.

We moved to Columbia, Missouri, but there was a little town about ten miles out of town called Stephens Store--then it became simply Stephens. This town was named after my Great Grandfather, and this was the reason for their [his parents] settling there. I went to elementary schools in Columbia. We didn't have Kindergarten then. I started in at U.S. Grant Elementary School up through the sixth grade, and then went to Jefferson Jr. High School for seventh and eighth grades. I then went to Hickman High School for ninth through twelve grades. I graduated from that high school in 1935.

The summer of my graduation I enrolled in the University of Missouri at Columbia, and my major was geology. My Masters was also in geology. I leaned more toward mineralogy. I didn't know a thing about geology until I was a freshman in college. I was enthralled when I was introduced to it by my first geology professor there, Dr. Walter D. Keller, who lived until he was 101 years old. He was doing research up until a week before his death.

After graduating, I went down to Houston, Texas for job hunting. I was then 1A (draft status) because it was 1941. It was tough finding a job. I got a job with the Mott-Smith Corporation. They were doing gravity surveys in the Southeast U.S. That was 1941. I worked for them for a few months. The day I resigned was a Friday. That following Sunday was Pearl Harbor day. So, we moved back to Columbia, Missouri and I had a letter waiting for me offering me employment at Continental Oil Company, which I accepted shortly afterwards. I worked for Continental Oil Company about nine months, and in 1942 I joined the U.S. Geological Survey.

When I joined the Survey we were working on a bauxite project, and we were getting [draft] deferments six months at a time. We got several deferments, and finally they said no more deferments. So, about six of us went to Memphis, Tennessee and applied for Navy commissions. All of us were accepted except one--in 1942. But before I was offered the commission, the word came through that we were getting another deferment, and if we declined the deferment we would lose our jobs with the Survey. So, I had to turn down the commission--and that was that.

Then, I was drafted in 1945 and went into the Army Air Corps. I went to Jefferson Barracks [near St. Louis, Missouri], and I took my basic training at Shepard Field, Texas. Then I was assigned back to Jefferson Barracks in a photogrammetry Unit. I was qualified either for photogrammetry or topographic surveying. I was actually given a choice--and I chose photogrammetry because I was more interested in that subject. I learned later that those who had selected topographic surveying were shipped out to Africa during the North Africa campaign. So there was a little bit of fortunate choice.



Then, from Jefferson Barracks I went to Buckley Field out of Denver, Colorado. They closed that field and I went to McDill Field in Florida where we were working on aerial photographs that were taken in all parts of Asia.

After the war I applied for a position at the University of Missouri, and was an instructor in geology for a year. That was prior to taking my Civil Service Exam--and teaching helped me prepare for it.

I rejoined the Survey in 1947 and went to the Black Hills of South Dakota to work on the pegmatite project. Lincoln R. Page Author's Note- Later to be one of Gene Shoemaker's "angels"] was the project Chief. I worked on the pegmatite project during 1947 and part of 1948, and then went to the Survey's Colorado Plateau uranium project. This is where I first met Gene Shoemaker. He was just fresh out of Caltech with his MS degree--he was just nineteen years old. Gene was working under Fred Cater in geologic mapping. Gene was a very energetic fellow.

I was on the Colorado Plateau Project until 1953 when I went out to the Survey's offices in San Francisco, California--then in the old U.S. Mint Building. This was before the Survey's offices in Menlo Park were completed. I was in San Francisco from August until December of 1953 when Building One at Menlo Park was nearly completed. I was headquartered at Menlo Park then from December 1953 until I moved to Flagstaff in June 1964.

While still in Menlo Park, I had a variety of assignments. The first job I had was radioactive reconnaissance of California, which I took over from George Walker. That involved examining mines and prospects the whole length and breadth of the state. That went from 1953 to 1955. Then I worked with Lorin Clark on the metamorphic belts along the west slope of the Sierras, and then from there I worked with Wayne Hall on the lead-silver-zinc district in the Darwin area. Anaconda Mining Company had quite a few lead mines, with silver and zinc around there. We started out mostly on mine mapping, and when Wayne got the Panamint Butte Quadrangle, we spent two or three years on that--up into 1960."

The following quotations were taken from a filmed interview with Hal Stephens by Harry Ryan [Flagstaff, Arizona] on 29 June 1996:

"Since I had known Gene since 1948 or 1949, I liked to keep up with what he was doing after Gene arrived at Menlo Park in 1958 after the close of the Survey's Colorado Plateau uranium project.

When he [Gene] got interested in impact-type studies, he was doing some laboratory-type tests at Ames Laboratory (Ames Research Center) down at Mountain View, California. They were using a hydrogen gun--which is a means of propelling a projectile at extremely high velocities. They'd use a normal type of explosive charge that would compress nitrogen with a piston until it ruptured a membrane. Then, that rush of hydrogen gas would propel the projectile at speeds up to 20,000 feet per second. It would impact a block of sandstone and form a crater. The crater was usually 6 to 8 inches in diameter and 2 to 3 inches deep. The projectile would completely disintegrate, but the cratering effects were left.

I saw the work he was doing--and was fascinated by it. They would examine the craters in detail, and I said why don't you map it?" Gene said, how do you mean? I said, well, take a couple of photogrammetric cameras and set them up 3 or 4 inches above the cratered specimen, and simply take a stereo pair; and use that to draw topographic contours of the crater configurations. Gene thought it might work. He tried that, and he did this for a number of years."

I knew a machinist in Monterey, California; we shared a common interest in shooting. He was using impacts of rifle bullets on steel plates in order to get an idea about velocity. If it penetrated 1/8 of an inch--that told him something. If it penetrated 1/4 of an inch plate of steel--that told him something. If it just simply cratered a thicker plate, that gave him some crude idea--a crude measurement of velocity. So, cratering or penetration is a factor of velocity more than--say--bullet substance.

Well, I had one gun that fired a bullet at 3,600 feet per second. And on a 1/4 inch plate of steel, it made quite a crater, maybe 1/2 inches deep; and it "peeled" the edges around--just like a normal impact crater would form, except that it held together--being steel--rather than shattering and throwing pieces in all directions [like crater ejecta]. I was intrigued with that. So, I got the machinist to saw it [the crater] in half--right through the center of the crater. That gave you a beautiful cross-section of it. I showed it to Gene. He was interested in the mechanics of it. It was a crude way to measure velocity, but the cratering aspects were quite successful. The bullet residues remained as a little lens-shaped piece of lead in the bottom of the crater. I indicated my interest in what he [Shoemaker] was doing, and this led to other activities that were a prelude to the lunar mapping.

Gene wanted to start a lunar mapping program about this time--about 1962-1963. So, he arranged for Elliot Morris and me to go down to Mt. Wilson, California, to copy their best lunar photographs. The best [telescopic] photographs ever taken of the Moon were taken around 1928, because after that the sky was being degraded by light pollution, in addition to other forms of pollution.

I did spend some time at the Lick Observatory for Hal Masursky while I was at Menlo Park. Hal was doing some observing there.

Elliot Morris was one of the first [of us] to come to Flagstaff. He came there about 1963 with Jack McCauley and Russ Wahmann. They established the first office there.

In those days Gene had very little time and very little money to do a lot of work. I recall one time early in the game when Gene had already committed about \$300,000 to the program--and he said, now, I have to go to Washington and visit NASA and find that money! And he did! Otherwise, we would have collapsed. But Gene worked on these principles. He was extremely confident in his ability to sell the program--and he did a whopping good job of it!

The first time I came to Flagstaff was on 15-20 June 1964 to participate in Astrogeology's first suited pre-Apollo astronaut field exercise at Sunset Crater and the Bonita Lava Flow northeast of town. We had acquired a Gemini spacesuit from NASA and Jerry Harbour and Gene Phillippi

were the only Astro personnel that would fit into it. They both wore the suit at different times during the conduct of the exercise [See 15-20 June 1964].

Well, the Gemini suit was built for a person [astronaut] in a seated position in a small Gemini capsule. Well, they tried to stand and walk in that suit and it put them in a rather bent-over position. Nevertheless, the exercise went very well, and we got a feel for what the astronaut would be doing as a geologic observer. We were not trying to make geologists out of them--just to observe geologic features, and to be able to relay in recognizable terms what they saw and what they were doing--say in collection of a sample.

So that was my first assignment from Gene, to cover the activities of that first suited exercise in Flagstaff. And during the remainder of that year [1964], I was in touch with Gene about the progress of the program. He then asked me if I would like to join the Branch--or what would become the Branch of Astrogeology.

The following was taken from an E-mail sent to the author by Hal Stephens on 23 October, 2002 following his review of an early version of this manuscript:

Stephens: "I came to the Branch of Astrogeology primarily to set up the Film Documentation Unit. I hired Tom Lee, Jim McCord and Walt Roeder. Tom Lee and Walter Roeder later transferred to other parts of the Branch, but Walt Roeder remained and supervised the making of films documenting the astronaut field exercises. I also hired Don Hart [in October 1965] whom was expert in mixing sound, narrating, and editing the films. When Jack McCauley was Branch Chief [of Astrogeology] in 1976, he commissioned me to set up the Data Facility. At that time, large numbers of images from the Viking mission (to Mars) were coming in. The Data Facility at that time was done in Building-One (the original Branch of Astrogeology Headquarters building on Switzer/McMillan Mesa), with limited space."

*Schaber: "Hal, I ask everyone I interviewed do briefly describe their thoughts on working with the Branch of Astrogeology during the pre-Apollo and Apollo days."*

Everybody at Flagstaff was excited and highly motivated. Gene brought some people in from Caltech where he had been teaching. They were highly skilled in their fields--and some are still there [in Flagstaff]. The morale was extremely high. We knew--each one of us knew-- we were living in a very exciting time--that we were going to see an astronaut reach the Moon--and of course- we did! My feeling was that all the people involved in Astro were very dedicated people. They never watched the clock they'd take an assignment and do it--and whatever it took, they would put in the hours required. They never thought of overtime, and most of their work was done, you might say, in overtime periods. Looking back, I didn't think so at the time. But it's been fascinating--we were living in an extraordinary period of time."

[Author's Note: One of Hal Stephens' non-Astrogeology-related achievements [early in his USGS career], that he did not mention during our interview, was his world class body building awards in the late 1940's. Hal's picture appeared on the cover of *Iron Man Magazine* in 1948. Hal Stephens is considered one of the top body builders of the "Golden Era of body building"--and without any steroids! Hal Stephens retired from the USGS in August 1977. Hal and Dorothy Stephens, two f

the most wonderful and kind people that my family and I have ever had the pleasure to know, still reside in Flagstaff.]

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**Desiree E. Stewart-Alexander** (geologist; Ph.D., 1967, Stanford University) arrived in October 1966 to work part time as a Physical Science Tech (PST) for the Branch of Astrogeology in Menlo Park. She became full time with the Branch in 1967 following the completion of her Ph.D. dissertation. Des worked on various lunar geologic maps, including the map of the Apollo 17 landing site (Taurus-Littrow). Des was a member of the Lunar Sample Preliminary Examination Team at the Lunar Receiving Laboratory (LRL) at MSC, Houston, Texas for Apollo missions 16 and 17. She was also a member of the Apollo Lunar Geology "Tiger Team" at Houston during Apollo 16 and 17.

The following was taken from an interview with Desiree Stewart-Alexander (Aptos, California) by Gerald G. Schaber on 4 September 2001

Alexander: "I was born in England, and we moved to the West Indies when I was four, and back to England--then we went to the Mediterranean and then to the West Indies--and then up to Canada in the early 1940s--then to the States. So I had my beginning schooling in a mixture of Canada, the Southern States, and New York State--and up and down.

Then I went to undergraduate school at the University of Richmond in Virginia, 1948-1952 with a major in history. Then, I went to work in New York City; I walked into the wrong employment agency, and ended up with American Metal Climax--and started being involved in their various mining operations and things like that."

*Schaber: "So how did you go from history to working in geology?"*

Alexander: "Ah well, that's because, apparently, my student visa had run out and I didn't realize it--so the IRS came knocking on my door. So this American Metal Climax had a small office in Toronto, and they sent me up there when I got it all squared away. I couldn't do anything up there because it was a geology exploration office--but they had me filing maps and all of those good things. So, when I got through with that--it was about four weeks--I came back down and worked for them for another year--then I went down to Dallas. Then, after six months of as a claims examiner in an insurance Company, I thought back to school Des; you've got to get training in something you wanted.

Just from my experience from the company up in Canada, I was interested in geology, so I went to SMU [Southern Methodist University] and took all the basic course in geology--I hadn't had any--so I took them all. I was in Texas in 1955-1956, working for six months for Texas Wmployeers, Inc., and then I attended SMU while working for DeGolyer and MacNaughton, an oil company.

Then I applied to graduate school and was accepted at Johns Hopkins and Stanford. I ended up in Stanford from 1956-1958 and got my degree from there while working at the usual assortment of odd jobs for the School of Earth Sciences during that time. It took me a while because I still had to

make up some math courses, and biology and all of that stuff. Henry Moore was there at Stanford when I was there.

Between getting my masters and Ph.D., I taught geology for a couple of years (1963-1965) at Halle Sellassie1 University in Ethiopia, and then came back and finished up the Ph.D. My thesis was on the metamorphic rocks in the Sierra Nevada foothills. While still finishing up my dissertation, I went to work in 1966-67 as a PST at the USGS [not Astrogeology] in Menlo Park, California. I worked for Utah Construction and Mining in Menlo Park for six months or so--I was in Wyoming for them-1958-1960. From 1965-1966 we also worked part time for Dale Jackson at the Survey conducting studies of ultramafic rocks and eclogites.”

*Schaber: "How did you hear about Astrogeology?"*

Alexander: "I forget who interviewed me and asked me to come there. I certainly worked for Don Elston--but I just flat don't remember. I started working for Don Wilhelms doing some of the Moon maps. I authored the published Geologic map of the Rheita quad of the moon (Map I-694) (included the Nectaris Basin) and the Geologic Map of the Fracastorius Quad of the Moon (authorship is Stewart Alexander and Tabor). I also wrote a paper with Don Wilhelms entitled, "The Nectarian System, a new lunar time-stratigraphic unit." I also did a Preliminary geologic map of site V-8 Apollo site (area photographed by Lunar Orbiter V) and a map of selected geologic features in part of the Apollo landing site I (1969). I then started doing some Mars maps before I transferred to Western Environmental Geology (see below).”

I hadn't intended to stay with Astrogeology for very long. I stayed there longer than I thought I would. Then when Apollo started, I had done some of the small-scale maps, and then I did a map of Mare Nectaris--a large-scale map. Then Howard Wilshire got involved in the Moon rocks--and went down there--to Houston.”

*Schaber: "Didn't you get involved in mapping the Lunar Quad that one of the Apollo spacecraft landing on?"*

Alexander: "I did do one of the landing maps. I remember meeting with Jack Schmitt, so it must have been Apollo 17 [the Taurus Littrow site]. I was down at MSC in Houston helping identify the rocks when they first came in.

The big map I did was of the Nectaris Basin; and I also did a map of the farside of the Moon--he first one that was done.”

*Schaber: " How did you get tied in with Howard Wilshire?"*

Alexander: “Howard Wilshire actually got me into the Lunar Receiving Lab looking at the Moon rocks. I was the first woman to be down at the LRL actually looking at the Moon rocks. We were identifying the rocks as they came in--and it was interesting.

I had been down there helping with the Apollo Group [Field Geology Team] helping with the geologic maps—along with helping Howard Wilshire with the lunar rocks.

It was very interesting--but frustrating in a way--because the rocks were unopened initially by MSC contract people); then we examined them after they weighed them in the morning. They started exactly at 8:00 AM and closed up the LRL exactly at 4:30 PM, or whatever it was. It was frustrating because you couldn't stay and work longer hours. But anyway, it was fun. A lot of the time we worked in pairs--just because it as easier."

*Schaber: "So you took a lot of petrology when you went to school?"*

Alexander: "Oh yes, Igneous/metamorphic petrology was my field, so we had to work with the microscopes etc."

*Schaber; "What was you general impression of the Moon rocks--besides the fact that there wasn't any rust in them?"*

Alexander: "Mostly that they were pretty dull compared to terrestrial rocks, because again, there was no water. The only strongly-colored mineral was the epidote. All the rest of them were dull-looking. But the whole process [of working with the Moon rocks] was interesting. You'd put on plastic gloves, and then you stuck your arms through those [vacuum box] windows. We all had to wear the same gloves; and here was this big guy; he was about 6'4" or 6'5" with big hands trying to stuff them into these two big gloves. And there was I, quite a bit smaller, so they had to bring me a box to stand on."

*Schaber: "So, you were down there for which missions?"*

Alexander: "I think both Apollo 16 and 17--as I recall. I know I was down there for two of them."

*Schaber: "And after the Apollo missions, did you contribute to some of the data reduction and mission reports?"*

Alexander: "We were in the group that sat in the back room while they were doing the lunar walks. I know on Apollo 17 we got in and stayed up late that night so that we would be ready to sleep during the daytime the next day--and then they changed the blooming mission [planned traverses] on us. Anyway, we made a geologic map for them based on what they were saying and doing--and we handed it to them when they landed [back on earth]."

*Schaber: "Yeah, they were pretty amazed. We had it down to a fine art by then!"*

Alexander: "Yeah that's right. So I did that, and then we would come back down to MSC again when the rocks came back.

I guess then after Apollo I was detailed in 1975 for a year or two to NASA Headquarters as program scientist in the Lunar Program Office. Roberson was my boss. I had about one day a week out at the Survey [downtown D.C. or in Reston, Virginia]. The agreement I had with Mike Carr [who was Branch Chief of Astrogeology then] was that after I did that, I could then transfer out of Astrogeology."

Schaber: "What were you doing at NASA Headquarters?"

Alexander: "Helping to advise them as to what they should and shouldn't be doing with regard to the lunar stuff. It was just a general adviser. Steve Dwornik was there.

I was in NASA Headquarters with the managerial Group--Space Sciences. We were mostly involved to see who got what money--the science proposals and grants.

I was there only for one year. I left Astrogeology about the mid-1970s. Later, between 1976 and 1986, I was Chief of the Branch of Western Regional in Menlo Park."

Schaber: "What did you do after you were Branch Chief for Central Regional Geology?"

Alexander: "I spent one more year working the Survey and then took early retirement on 30 July 1986."

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**Robert Leeds Sutton** (1929-1982; geologist, Haverford College and the University of Colorado, Boulder) entered on duty with the Branch of Astrogeology at Flagstaff on 1 March 1965. Bob attended the West Town School [a Quaker school] in Westtown, Pennsylvania (class of 1947), and Haverford College in Pennsylvania (class of 1951 with honors). Bob's father taught physics at Haverford College at the time Bob was a student there. Bob then attended the University of Colorado at Boulder, Colorado between 1951 and 1962. Bob was an outstanding field geologist, petrologist, and human being. He spearheaded the geologic mapping of Astrogeology's pre-Apollo test sites in Hopi Buttes. He also played important roles in the geologic training of the astronauts and in Apollo mission operations at Houston--especially in the descriptions, classifications, and photo-documentation of the returned Apollo lunar samples in the Lunar Receiving Laboratory at MSC.

Unfortunately, Bob Sutton succumbed to cancer in Flagstaff, Arizona on 28 December 1982, cutting short a productive career of over 20 years with the U.S. Geological Survey. On 15 February 1983, a bronze plaque was dedicated to Bob Sutton in the "Apollo Room" at the U.S. Geological Survey's Planetary Data Library in Building Four on the USGS campus on McMillan/Switzer Mesa in Flagstaff, Arizona. The citation on the plaque reads as follows:

*"On this day, the 25<sup>th</sup> of February, in the year 1983, this USGS Library Facility housing data obtained of the Earth's Moon by NASA, is hereby dedicated to the memory of Robert L. Sutton, member of the Apollo Lunar Geology Experiment Team. For his exceptional dedication to the training, traverse planning, sample analysis and geologic investigations, the lunar science community will be eternally grateful. By his outstanding character, friendship, and positive influence on our lives, we who were fortunate to know him have been enriched."*

USGS, Branch of Astrogeology colleague George Ulrich remembered Bob in the Geological Society of America Memorial Series following Sutton's death in 1982 (Ulrich, 1982, Geological Society of America Memorial Series:

*“Sutton's education included a B.A. with honors from Haverford College (Pennsylvania) in 1951 and graduate work at the University of Colorado during the years 1951-1962. The latter period included intervals of temporary employment with the U.S. Geological Survey and three years with Seaboard Oil Company. During this period, he acquired broad field experience in geologic mapping, working with J.H. Hartshorn on the Malaspina Glacier in Alaska [in 1953], with Hal Masursky on a study of uranium-bearing coals in Wyoming, and with George Bachman in New Mexico. Additionally, he did surface and subsurface mapping for petroleum exploration in Colorado, Wyoming, and Nebraska. He developed skills in mineralogy and map compilation while working with Howard Powers, Gershon Robinson, Ray Wilcox and Steve Oriel.*

*Sutton became a part-time employee with the Survey shortly after a 1964 project mapping coal deposits in southern Utah with Harry Waldrop and Fred Peterson. He was headquartered in Flagstaff for the next twelve years, which were among the most stimulating and interesting in his career. Pre-Apollo planning for scientific missions to the Moon, geologic training of astronauts, active participation in the Science Support Room at Mission Control in the Manned Spacecraft Center (now the Johnson Space Center) at Clearlake, Texas, and the interpretation of geologic results following the six lunar landings all demanded and received, his creative dedication to understanding the geology of the Moon. His knowledge of lunar rock samples--their identifications, locations, field orientations, and geologic environments--was encyclopedic. His assistance was often sought by scientists studying lunar samples, firmly establishing his reputation in the NASA community.*

*As a team worker, Bob was superior, and teamwork was indispensable in the Apollo missions. His positive and friendly manner and his sense of humor often smoothed ruffled feathers and calmed competing egos among the scientists and engineers engaged in the missions. He was a steady and supportive influence on the Field Geology Team, serving on all missions as lunar sample expert during excursions by the astronaut crews on the Moon's surface.*

*Bob wrote or collaborated with others on more than 60 publications, including 27 on the geology and rocks of the Moon. Among his staunchest admirers, Gene and Carolyn Shoemaker recognized his contributions to the geology of the Moon by naming a recently discovered asteroid (no. 2532) after him. Bob was a Fellow of the Geological Society of America and a member of the American Association of Petroleum Geologists and the Montana Geological Society.*

*Bob Sutton was one of the most astute and meticulous observers of geologic field relations this writer has known. He had a superior talent for communicating his observations to associates in the field and for gently pointing out details that colleagues had overlooked. His description of the cut-and-try method of locating geologic features on topographic maps has had wide application in the canyon and mesa country of the western United States. The quality of his fieldwork is not adequately represented by the four published geologic maps that bear his name. His extensive field studies in the Hopi Buttes region of northeastern Arizona in the late 1960s are summarized in two publications (1974) which unfortunately, are not widely available.*

*From 1976 to 1979 Sutton served as executive officer for the USGS Interagency Task Force in Billings, Montana, which conducted an environmental impact study of the northern Powder River Coal Basin. For his performance in this leadership role, he received a Special*



*Achievement Award. During the years 1980-1982, which closed out his career, he focused on the surficial deposits of the San Juan Basin and Four Corners area as part of the Energy Lands Program. During his reconnaissance in the La Plata Mountains, he began to recognize previously unmapped glacial deposits, important to the understanding of the Pleistocene history of the area. As the cancer took its toll, he was forced to abandon this promising new work.*

*Bob's contributions went beyond his professional work. He was an accomplished musician who played and composed for the piano. He also played the clarinet, guitar, lute, and recorder; he sang tenor in choruses with the greater Denver Opera Company, the Boulder Civic Opera Workshop, and the Northern Arizona University Opera Workshop. He also performed memorably in several USGS Pick and Hammer shows. The outdoors was one of his great loves; he was a member of the Rocky Mountain Rescue Group in Boulder and the National Ski Patrol in Flagstaff, and he was an avid mountaineer, backpacker, and river runner. His social conscience found expression in the sixties in the freedom march on Washington.*

*Raised in the Quaker faith, he was a true friend to all and a teacher to many. He had a profound and positive influence on those privileged to work with him, younger field assistants and older colleagues alike. He is survived by his wife Nona [deceased (2001) since Ulrich wrote this memorial], a brother, David, three daughters, three stepchildren, and eight grandchildren. He is greatly missed."*

In a letter written the year before his death, Bob Sutton wrote:

I missed my [college] graduation [in 1953] because I had already headed to Alaska for summer work as a geological field assistant on the Malaspina Glacier. That fall I started graduate work at the University of Colorado, in geology. In 1953, I joined the U.S. Geological Survey in Denver and attained the rating of geologist. That year I married Virginia Veach, of Shaker Heights, Ohio. The next year I joined Seaboard Oil Company for four years. I almost left geology as a career as a result, but decided to go back to graduate school while I was at loose ends. I had two wonderful girls by 1959 when I returned to school, so I worked part-time for the USGS in Denver. We had a third girl in 1961. In 1963 I tried my hand at teaching in a small private school, but one year of that was enough. The summer of 1964 was spent with the USGS, mapping geology near Lake Powell, in Utah and Arizona.

Still wondering about "a permanent job", I took a trip to Scripps Institute to check out the possibilities in Marine geology. On my return trip to Page, Arizona, I met a former U. of Colorado grad student and Jack Schmitt (who later became astronaut Schmitt on Apollo 17), both with the USGS. They were working on the upcoming lunar program with the idea of obtaining the maximum geologic return. It sounded like exciting work, so I applied, was accepted, and the rest is history. I began work in March 1965, and have been there ever since. I feel fortunate to have had a part in all of the Apollo missions, helping to train the astronauts in geology and to be at Mission Control [in Houston during the missions]."

Gordon Swann remembers: "I knew Bob Sutton at school [University of Colorado in Boulder] pretty well. I was coming back from Houston or some place and spent the night in Phoenix [in the winter of 1964]. I got weathered in and I called and found that Dick Godson was there in Phoenix also spending the night--and that he had a government car. I called Dick and made arrangements

to ride up to Flagstaff with him the next morning. I got up early and went to the restaurant to have some breakfast--and there was Bob Sutton! He was working with Pete Peterson up around Lake Powell, and he was to meet Pete in Flagstaff--but he didn't know how he was going to get there. So I said I can give you a ride to Flagstaff. On the way up to Flagstaff I talked him into joining us. I told him we were really looking for people [to join the Branch of Astrogeology]. So, I committed to hiring him right there [on the road to Flagstaff]" (from an interview with Gordon Swann by Gerald G. Schaber on 2 January 2001).

Bob Sutton met his second wife Gale New while both were working at the Branch of Astrogeology in Flagstaff shortly after he arrived in 1964. Unfortunately, Gale, who was a diabetic, got stuck in her car up on the remote Schultz pass Road north of Flagstaff without her insulin, and passed away before she could be located.

[Author's Note: I recall being with Bob Sutton in the swimming pool at the Nassau Bay Hotel at Clear Lake City (across from the Manned Spacecraft Center in Houston) around the time of Apollo 11, when Bob and I met Nona and her daughter, Barbara. Nona and Bob would subsequently marry and reside in Flagstaff. Sadly, Nona Sutton passed away in 2001.]

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**Jody Swann** (secretary; B.S., Eastern New Mexico University)

[The following information was abstracted from an interview with Jody Swann by Gerald G. Schaber on 16 February 2001.]

Jody (Loman) Swann, was born in Texas, but was reared primarily in northeastern New Mexico where her mother was high school teacher and her father owned his own water-well drilling company. She received her Bachelor's Degree at Eastern New Mexico University with a double major in secondary education and secretarial science. When she graduated she got a job as a secretary, and in a few months got a job working for the Civil Service. Her first husband [Sam Loman] had been transferred to Flagstaff, Arizona, and Jody was anxious to keep her Civil Service continuity, and so ended up getting a job out at the Navajo Army Depot (NAD) [at Belmont, just west of Flagstaff] as a secretary in the personnel office.

While Jody was working at NAD in 1963, Don Elston and Wanda Wilkinson (Administrative Officer for the Branch of Astrogeology at that time) came out to NAD to look at the register of competent people to be considered for recruitment as secretary to Gene Shoemaker. Elston asked Jody if she would like to come into town and talk to them at Astrogeology [at Astrogeology's wing at the Museum of Northern Arizona.]

Jody Swann: "So I did. I went in, talked to Don and looked the place over a bit--and I called him [Don] back the next day and said no, I didn't think I could do that. Then, Don called me one Sunday and asked if I would reconsider coming to work for Astrogeology. And I said no again. Then, he called me at my office at NAD and said I know you don't want the job--you said that--but I really want you to just come and meet Gene Shoemaker. I [finally] said okay, I'd come out. This was November 1963.

I arrived at MNA and it was a hubbub; everybody was still working; the lights were still on and everybody was doing their job--and I was amazed! I had come from the Department of Defense. I had worked for the Department of Agriculture in New Mexico, for the Air Force; and I was currently working for the Army. I was used to people working until 4:30 PM. You leave work at 4:30 PM; that is what you did--and you don't stop or you're going to get run over [by the people scrambling to leave the building].

I remember thinking that we should get on with this [Don was not there at MNA to meet her when she arrived] because there was a young man there in ragged Levi's and a flannel shirt that I feared wanted to clean up the place. I was soon informed that he was Gene Shoemaker! I sat down and talked to Gene--and I was hooked! I quit my job [at NAD] the next day and went to work for Astrogeology. Gene was the most charismatic personality-plus person I have ever known. He could sell ice to Eskimos! So that was when I started to work for Astrogeology. That was December 2, 1963 out at the Museum of northern Arizona.

Gene was different from anybody. I came from a very small town and a relatively small university, and I was incredibly naive and unworldly. I had never flown on a commercial aircraft in my life, and Gene just assumed that I could make travel reservations, get tickets and hotel reservations, and take dictation. I could take dictation because that is something I learned to do in college.

Gene wanted me to listen in on every phone conversation he had--from beginning to end. And we had telephone receivers that had a little button on them that was sort of a press to talk button. Otherwise, I could pick up the receiver and keep it tucked between my ear and my neck----and my neck is still a piece of trash! I would listen to every word he said on the phone. And for good reason for Gene--that was to keep track of any commitments that he may have made whether on the calendar, a trip, something he said that he would send to someone--which I would just take care of. He wouldn't have to write it down and remember it--think about it. It would just get done! And that makes a lot of sense.

I couldn't possibly absorb all of that [Gene's phone conversations]. I didn't have the background to absorb it--but even if I did, you would still have to make a selection of what's important to your mind and what you're going to write down--what you're going to do--what you're going to remember to do. Anyway, if they only had headsets then!

I remember when Jack Schmitt first came to Astrogeology in Flagstaff. Like a lot of brilliant and energetic people, Jack had written something for Gene--and he wanted it typed. I was the only secretary then--and when Gene was in town, Gene was my priority. Gene was gone so much, and always had a pile of stuff on his desk--he had to get things done. I had to do it then. I recall onetime when Jack Schmitt brought me something to type; and I guess I was just naive enough to be kind of rude. No, that's not true--but anyway I was frantic trying to get things done for Gene--and transcribing. Jack later came up and said, have you got that typed yet, and I said if you don't get your ugly face out of here, you're never going to get it typed! Now go away and I'll let you know when it's done. Jack said, yes ma'am! After that, we got along just fine. But sometimes under the pressure of trying to do too many things at once--you're not always at your best."

The first time I actually flew in a commercial airliner was to go out to Pasadena [CA] for Surveyor 1 to type reports and take dictation, among other things.”

*Schaber: "People tell me that first Surveyor landing made things quite hectic at JPL, as they never expected it to land—much less take over 11,000 pictures."*

Jody Swann: "Yes, and we were there. I was out there over one weekend for which Gene [Shoemaker] was going to be out of town. My sister lived in the San Bernardino area, so she came up to Pasadena to pick me up. She sat in the parking lot and waited to me for six hours because Gene was dictating, and he said it'd just be a little bit longer--just a little more. I ended up taking a complete steno pad--front and back--in one session for a Surveyor-1 report. But NASA was big on these reports--interim reports; but that was good because it got things out to the public.

But from then on--from Surveyor and on through Apollo--there were so many real-time things we had to do. Everything got done [because it had to]. A lot of things that got done--and got done well--were done on the fly. It was sort of what-if-let's try! There was a lot of imagination and a lot of youth.”

*Schaber: "And really old technology compared to today's standards-right?"*

Jody Swann: "Well, that's true! Oh, my goodness--if I'd had a personal computer in those days--gosh!"

*Schaber; "Or even a word processor!"*

Jody Swann: "Yes! We just typed, and re-typed [with a typewriter]. You couldn't make typographical errors--it just took way too much time.”

*Schaber: "Actually, we scientists were doing the science—and thoroughly enjoying it, although it too was hectic. But you know, many of us never thought about the pressure that was put on the secretaries at that time--it was amazing. You all absorbed some of the excitement of course, but then again, you were doing the hard work like the drudgery of typing and taking dictation [and putting up with the scientists demands 12 to 16 hours a day]."*

Jody Swann: "One day [in 1964] I was working over at Gene's house. It was a cold and snowy day and Gene had been sick. People were coming over to their house to keep appointments with Gene, or to see Gene, and I was being the secretary over there. Gene had an appointment that morning with Jack Schmitt. Gene wanted Jack to apply to be an astronaut. And Jack said, Gene, I don't think I can make it because several years ago I had to have major surgery and they took out several [extra] feet of my colon or large intestine, and I don't think I could pass the physical. Gene said, I don't see why that would be a problem. So, that morning, Gene and Jack dictated his application for me to type up for him to apply to be a scientist astronaut--and the rest is history.”

*Schaber: "Gene already knew he had physical problems by that time [I am here referring to the fact that Gene had earlier been diagnosed as having Addison's disease; see main text, spring of 1963]"*

Jody Swann: "Gene couldn't even get a pilot's license. He had to quit flying on a private airplane by that time."

Also, in those early days [of the Branch] we had only the old telephone switchboard that was in the Forest Service Building [now the Federal Building on San Francisco Street in downtown Flagstaff]--and everything had to go to the switchboard. There were only so many trunk lines. Dialing numbers to get out- meant getting many re-orders. There were no touch-tone phones then; you dialed every number all the way through. It could take a while. You'd dial this whole long number, and you'd get a re-order and you would start all over. It could be a challenge."

[Author's Note: The name Bob Wildey [astrophysicist] came up during our conversation, and Gordon Swann, who was sitting in on my interview with Jody, made the following comment]:  
Gordon: "Ken Watson hired him. Bob Wildey is the guy that Hal James, the Chief Geologist at the time, commented on during a visit to Flagstaff. He was looking over the Branch roster and said, what the hell is the Geological Division of the Geological Survey doing with an Astrophysicist on its staff?"]

Jody Swann: "Bob Wildey really irritated me one time. Gene got a lot of correspondence from people interested in what we were doing. Well, one of the letters was in German and I didn't have any idea what it said, so I asked Bob Wildey to translate it for me. And he did--he translated it into Spanish. [Gordon Swann's comment: "But that was Bob Wildey's sense of humor!"]"

Well, I didn't see the humor in it."

*Schaber: What were you doing for our Apollo Lunar Surface Geology Team in Houston during Apollo 11?"*

Jody Swann: "I did stuff for Gene; and for the Geology team when we were actually in the Science Support room [at Houston's Mission Control]. I was either typing up notes during the EVA's or typing up sets of questions and getting them circulated through the higher-ups so they could be transmitted up to the surface [of the Moon]. That is the kind of thing I was doing. Then in late 1970, early 1971, Sam and I moved to Flagstaff; and Sam and I were divorced soon thereafter. Sam went to work for the State and eventually moved out of Flagstaff--and I stayed working for the Branch of Astrogeology."

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**Gordon Alfred Swann** (b. 1931; geologist; Ph.D., 1962, University of Colorado):

Gordon, who grew up on the west slope near Grand Junction, graduated from high school in 1949 and went to the University of Colorado in Boulder (the first time) from 1949 through 1952 where he majored in Chemistry. He took geology rather by accident --as an elective--in his third year as an undergraduate.

He then joined the Navy (1952-1956) and studied electronics, but decided during his last year (fourth) there to go back to school and become a geologist. He returned to the University of

Colorado after the Navy and majored in geology. Gordon worked WAE for the USGS from 1960 through part of 1962 working with samples out of the twenty-six-mile long Harold D. Roberts tunnel in Colorado. He earned his BS in Geology in 1958 and Ph.D. in Geology from the University of Colorado in 1962.

The following was taken from a filmed interview in Flagstaff with Gordon Swann by Harry Ryan in 1966; and from an interview of Gordon Swann (in Flagstaff) by Gerald G. Schaber on 2 January 2001).

Gordon Swann: “Upon graduating in 1962, I got a job at a consulting company in Boulder, and worked there for 2 to 3 months until that company went under. I looked for another job, and through a CU friend Joe O'Connor, found temporary work with Carl Roach at the USGS in Denver. That's when I started hearing lots of Shoemaker stories. The work –funded by ARPA, involved the study of high intensity shock on rocks. The cold war and arms control with the Soviets had created the urgent need to determine how one would go about detecting underground nuclear explosions. Arms control with the Soviets was going on at the time. The reason the military was interested was in those days they were talking about a test ban and inspections; and Shoemaker thought we could develop ways to prove that an underground nuclear test had been fired.

And that's when I first started hearing about Gene; of course, he was the Branch Chief --although he was in Menlo Park (CA) at that time, and just in the process of moving to Flagstaff. He came through Denver probably in the later part of 1963 (I think) and gave us all a little bit of a pitch about how we all ought to move to Flagstaff--and nobody wanted to. Then ARPA pulled the rug when the test ban treaty kind of went under, and we found out on Tuesday that there was no more money past Friday.

Once I was in Flagstaff, I liked it very much. I had only been to Flagstaff once in the middle of the night when I got discharged from the Navy--so I never really knew much about Flagstaff. Carl Roach (describing Flagstaff) had been to Flagstaff once or twice. He said it's just like going to a movie set to go to Flagstaff-Gene's operation. You walk down the street and there's here's the laundry, and here's the Chinaman's restaurant, and here's the saloon, and then another saloon--and then you open the door and all you see on the other side is sagebrush.”

*Schaber: “What was your first job assignment when arriving in Flagstaff?”*

Gordon Swann: “Working with Jack Schmitt on what are the kinds of things that an astronaut can or should do on the Moon. And then, my very first job that was astrogeological in nature; I got assigned temporarily to Houston on the astronaut-training thing. That would have been about February or March 1964. The reason that happened was that Gene had put down a number of man-years that would be devoted to it. And Jack Eggleston in Houston started counting people and multiplying--and there weren't as many man-years coming out of this team as had been budgeted for. So Danny Milton and I got send. We weren't really needed because the Survey guys already down there were doing a good job. They didn't need any extra help.”

*Schaber: "And the MSC geologists were kind of alienated against us anyway-right?"*

Gordon Swann: "Oh Boy! And that was mainly on account of Eggleston. NASA kind of went back on their word on telling Gene that they wouldn't compete with us--and they turned around and started hiring geologists--and it was clear, that their marching orders were to take that job away from us. They were kind of young, inexperienced guys, and Dale Jackson was kind an old experienced hand; they didn't get too far."

*Schaber: "So how did Dale Jackson get involved?"*

Gordon Swann: "Gene knew him and asked if he would take it over for the first year. And Dale agreed. He was working for the Survey in Menlo. Dale was a very well known black-rock geologist [mafic rocks]. Dale then led the Houston astronaut training group from the USGS that at that time consisted of Al Chidester -who was from Denver--Don Wilhelms from Menlo, and Marty Kane (a geophysicist) -- and a secretary. And that wasn't as many man-years to fill the square. And so, they plugged us in [Swann, and Danny Milton]--and actually, it was a little bit over-manned because there were a bunch of Houston geologists working on the project too. And it turned out, most of those MSC geologists were pretty good guys; they were just doing what their boss told them."

*Schaber: "What Houston geologists were involved in the astronaut training program at that time at MSC?"*

Gordon Swann: "Ted Foss was the boss--and Elbert King. Ule Clanton was the second guy there, and then John Dietrick. The first guy there was actually Curtis Mason. Curtis was a pretty good guy--he had a lot of limitations, but he recognized them. He was their lone geologist until they hired Ted Foss who was a good geologist. Then they hired Dave Strangway."

*Schaber: "How many Astrogeology science personnel were at Flagstaff when you arrived?"*

Gordon Swann: "Not too many. It was kind of in a state of flux of people moving, and just getting moved. But Jack McCauley was here, as well as Elliott Morris and Hank Holt. Joe O' Conner and I came about the same time- in 1964. We hired Bob Sutton. I knew Bob at school pretty well. I was coming back from Houston or some place and spent the night in Phoenix. And -that's right-- Dick Godson was here [in Flagstaff at that time]-- and I got weathered in and I called and found out that Dick Godson was there too spending the night [in Phoenix] and had a government car-- and I called him and made arrangements to ride up with him real early in the morning. I got up early and went into the restaurant to have some breakfast--and there was Bob Sutton. He was working with Pete Peterson up around Lake Powell--and he was to meet Pete in Flagstaff--but he didn't know how he was going to get there. So I said I can get you a ride to Flagstaff. On the way up I talked him into joining the Branch. We were really looking for people, so we got him on."

*Schaber: "So anyone in the Branch could just hire someone then without formal authorization?"*

Gordon Swann: "Almost- And so that's how Bob got on. Oh, I forgot, Dick Godson, Ken Watson, and Marty Kane were also here [in Flagstaff]."

*Schaber: "Larry Rowan should have been here about that time."*

Gordon Swann: "He came about that time. Then Jack [Schmitt] came on about that time."

*Schaber: "How did we get hold of Schmitt?"*

Gordon Swann: "That was primarily Gene Shoemaker. Gene, I think he knew Jack a little bit from Caltech, and I think he knew Jack's father [an economic geologist of some note]. [Jack went to Cal Tech as an undergrad and then went to Harvard for graduate school]. Gene's the guy who talked Jack into applying to be an astronaut. If it hadn't been for Gene, Jack wouldn't have even applied."

*Schaber: "How and when did the idea of developing and testing the pre-Apollo lunar geologic field studies at Hopi Buttes and Meteor Crater come about? This was a brilliant idea because it really influenced NASA and got us the Apollo Lunar geology experiment."*

Gordon Swann: "It kind of developed from the work that Jack Schmitt and I were doing [together]. It kind of grew like Topsy. When I first came here, Don Elston agreed to do a bunch of time-and-motion studies. Frankly, I didn't think that the Survey should be involved with much of that. Jack agreed. We kind of got that thing turned around to where we were doing more field testing [developing lunar geologic procedures]--and doing just some time and motion along with it. What they were doing was-- things like the following: they'd get fifteen [geology] students from NAU and sent them out into the field and have each one of them go out and break a rock, photograph it, and then describe it."

*Schaber: "So this was part of Jack and your Astronaut on Foot study?"*

Gordon Swann: "Yes. One of the things we did learn from those early tests. The first thing one of those NAU students would do in the field was to break a rock and "blow" the dust off of it. That wasn't going to work very well with an astronaut in a space helmet on the lunar surface!"

*Schaber: "So you sold NASA on funding the Manned Lunar Group of our Branch to carry out these pre-Apollo field tests of astronaut geologic procedures around Flagstaff. What part did NASA HQ and MSC play? The MSC geologists monitored our test once and a while as I recall."*

Gordon Swann: "Oh, they participated a little. But when we used an actual space suit [out in Hopi Buttes, for example], this didn't have anything to do with S&AD [Science and Applications Division]--and those [MSC] geologists. We arranged that whole thing with the Engineering department at MSC. They were very cooperative. The final agreement we came to was that if they would furnish suits and the proper technicians and supporting equipment to use suits in the field, then we would furnish test subjects to do metabolic stuff down there."

*Schaber: "So, NASA decided or we decided that our field tests should be film documented?"*

Gordon Swann: They wanted it. I think that Shoemaker came up with the notion in the spring of 1964--before Jack [Schmitt] arrived. Shoemaker said I want to run a mission out at the Bonita



Flow, I want to run it in [space] suites, and I want it documented; we'll use this to sell the program [to NASA] [see 1964 in main text for description of Astrogeology's very first suited test 15-20 June 1964 at the Bonita lava flow adjacent to Sunset Crater just northeast of Flagstaff] (see Fig. 16).”

[Author's Note: As mentioned in the main text (see June 1964 above), the Manned Lunar Studies Group at Flagstaff put together a report on this test that included a filmed documentary. We showed the report and film to NASA and got \$600,000 for the next year. That is what really got us started [in our NASA-funded program of developing and testing procedures around Flagstaff for the Apollo astronauts to use on the lunar surface.]

*Schaber: "How and when did the Astrogeology's Electronics Support Group get started at Flagstaff?"*

Gordon Swann: “Well, there were two engineers here when I came--Bill Mason [arrived in Flagstaff 5 August 1964] and Ray Barnett. Barnett (electronics engineer) came from Denver. Mason was on the same project I was on in Denver [with Carl Roach]. Shoemaker talked Barnett into coming to Flagstaff and setting up an electronics group--and he hired Mason. And then they hired Eric Bramsoe [arrived in Flagstaff in February 1964] and Oliver Grieve [arrived in Flagstaff 20 September 1964] who were electronics technicians.”

*Schaber: "Then Johnny Nuttall somehow arrived at Flagstaff to work for the Manned Studies Group of the Branch."*

Gordon Swann: “Well, Ray Barnett and those other guys [mentioned above] weren't getting anything done. Eric Bramsoe was a pretty good hand with the radios, but when we started getting into the suit business, we recruited and brought on an electronic technician at NASA named Johnny Nuttall. Johnny would instrument us, and he would work the radios. He'd get me up at 4 in the morning and shave my chest and stick all of these probes on me. We also needed somebody of his caliber who also knew quite a bit about telemetry, given that we were at that time getting into the telemetry business. And so I talked Johnny into coming...to moving up here [Johnny Nuttall came to Flagstaff from Houston in February 1967]; and I think he did a pretty darn good job for us.

He worked that whole system up--all the politics and everything-- in getting the field radios down to the control Center in Houston. He set that whole system up through the Telephone Company--the relays and all that.”

*Schaber: "Tell me how our field simulations from Hopi Buttes, Meteor Crater or our tests run out of our Apollo Data Facility in Flagstaff came to be joined electronically [audio and video] to MSC Mission Control Room, and to the so-called "rock pile" training site at KSC? And that brings me to the whole CDRA [Command Data Reception and Analysis] or ADF [Apollo Data Facility] concept we developed."*

Gordon Swann: That was pretty much our idea, and we sold it to NASA. They liked the idea and the mission control people were very cooperative with us. Jack Schmitt liked to say, "Yeh, they liked you guys, they found someone they could drink with!"

But you know Jack has told me that Jack was quite highly thought of in mission control [see the book *Failure is Not an Option* by Gene Kranz (see Kranz, 2000, 415 pages)]. Jack was thought of more as a flight controller than he was as a member of the astronaut team. He spent a lot of time over there."

*Schaber: "How important was it to have Jack Schmitt act as a liaison between Astro and the astronaut with regard to geology training?"*

Gordon Swann: "Oh, very important. He did a very effective job of selling management on it."

*Schaber: "This was despite the fact that a lot of the astronauts didn't care a lot for him according to Gene Cernan's book "The Last Man on the Moon" [Cernan and Davis, 1999, 356 pages]."*

Gordon Swann: "I think Gene was fair enough to Jack [in his book]. The problem they had with Jack was that these guys were all test pilots. And to them, it was a test pilot program. They were testing hardware, and then test flying it to the Moon. They didn't need a bunch of dang scientists hanging around--guys just starting to learn to fly. But, it turned out that Cernan said [in his book] that there wasn't anyone he had rather gone with than Jack--once he got to know him. You know, Jack finished second in his class at Williams [jet-training school at Luke AFB in Arizona]. He is the only scientist-astronaut that they allowed to finish helicopter training."

*Schaber: "When did the idea to build the Rovers come about, and how did Puddy Mills and those guys come into the picture? That was a major positive activity for Astro, and the astronauts really loved the rovers and the crater field."*

Gordon Swann: "Our lunar rover program [at Flagstaff] all began with the old MGL, the Mobile Geologic Laboratory [that NASA/Huntsville had General Motors design and built for us [see 1965 in main text]. I got here just a little before it arrived, but it was arranged by Shoemaker, and I think Don Beatty at NASA HQ. The Marshall Space Flight Center in Huntsville, Alabama, funded it and supported it. They contracted General Motors out in Santa Barbara built this big horrendous machine. Puddy Mills, it turns out, lived in Santa Barbara, and he was very interested in this machine. He'd gone over and watched its progress. He ended up getting a job with us in Flagstaff to take care of this albatross when we got it. We had it about a week when Congress cancelled the project. And that was another \$600,000 investment [that is what it cost to build]. Putty was so interested in that thing--he wanted to take over the thing [its maintenance and refitting for use in our field tests]."

*Schaber: "How did the MSC geologists participate in our pre-Apollo field tests, and who were our major contacts both at NASA HQ and at MSC during that period of pre-Apollo testing?"*

Gordon Swann: "Well, our major contact at NASA HQ was always Don Beatty and Jerry Goldberg--Jerry worked for Don, and to a lesser extent Bob Bryson and Jack Handley. Our major

contacts at MSC--once S&AD was started --started out being Bill Hess, and became Tony Callio. We had to an awful lot of end running around Callio to get anything done. Our major contacts with the mission control center were Jim Saultz, John Cooper, and later on we had quite a bit of contact with Gerry Griffin and Gene Kranz. Gerry Griffin went on a couple of the geology field trips with us.”

[Author’s Note: Gordy later mentioned to me that Gerry Griffin went with the Apollo 16 crew (Charles Duke and John Young) to Schooner crater on the Nevada Test Site [27 October 1971]. Rocko Petrone [NASA Director at that time] also went on that one. I mention to Gordon about my trip with Rocko on the Apollo 17 trip to Hawaii 22-29 June 1972; see 1972 in main text].

*Schaber: “So you were saying that the Branch’s pre-Apollo field testing in Northern Arizona really sort of cemented the deal for us to end up doing what we did in Mission Control in Houston--the Apollo Lunar Geology Surface Experiment Team--right?”*

Gordon Swann: ”I think it helped a lot.”

*Schaber: “Tell me about the Apollo Data Facility on Fourth Street. How that got started?”*

Gordon Swann: “Our first--what we called CDRA [Command Data Reception and Analysis] facility was on the fifth floor of the Arizona Bank Building in Flagstaff, right behind Al Chidester's office. As we grew, we needed more space, so we set up the one down on Fourth Street in East Flagstaff. The building was built for us- and the adjacent building for photogrammetry. They were privately built for the Branch by local builders, [Buttrum and Jamison]. We just rented them through GSA [see January 1967 in man text].”

*Schaber: That brings me to the senior, academic geology advisors that monitored our Apollo procedural tests and the astronaut training exercises. How did that come about?”*

Gordon Swann: “Well, Shoemaker asked Hoover Mackin [U. of Texas], Aaron Waters [U. of California, Santa Barbara], and Edwin Goddard [U. of Michigan] to be CO-I’s on his proposal for the Apollo Lunar Geology Surface Experiment. Part of it was that he wanted to get some gray beards, well-known and respected geologists from outside of the USGS on his team. So that is what got them involved. Unfortunately, Hoover died shortly before Apollo 11.”

*Schaber: “I recall that they contributed a lot of good ideas and critique of our work?”*

Gordon Swann: “I think quite a bit, especially Hoover and Aaron. I guess Dick Jahns got involved through the training trips down in the Pinacates Volcanic Field in Mexico. He was kind of the area expert. And Jack Schmitt knew him at Caltech.”

*Schaber: “The Cinder Lake crater fields in Flagstaff and the Verde Valley; -that was another good idea.”*

Gordon Swann: “The Apollo 11 crew was scheduled to train on our [recently completed] Cinder Lake crater field [just east-northeast of Flagstaff] [in June 1969] about one month before the left for the Moon. Walter Conkrite [CBS News anchorman] came to Flagstaff to be part of that event.

We had him Conkrite set up at the Branch Headquarters on the Mesa about 7:00 AM; then Conkrite, his CBS cameraman, and I drove out to the crater field where Walter interviewed me as part of the preliminary planning for the arrival of the Apollo 11 crew. At that time, we were still planning on them coming. Then, because of some NASA scheduling excuse, the Apollo 11 crew’s trip to Flagstaff was cancelled at the very last minute.”

[Author’s Note: Although there probably was some legitimate conflict with the crew’s intense training and meeting schedule; this author also believes that the fact that the NASA engineer-types and administrative types (with few exceptions) considered lunar geology--or science on the Moon in general-- to be the very lowest priority at that time—and perhaps rightly so considering Apollo 11 was to be Man’s first lunar landing and safety was the primary concern.]

*Schaber: "How and when did Jack Sevier become such an important friend to Astro, and what role did he play-I know he was one of your best friends?"*

Gordon Swann: “I knew who he was from [Ray] Zedeker's LSOP [Lunar Surface Operation Planning committee] meetings; so I knew who he was. After Apollo 10, they had a working group assembled to work on the Apollo 10 photographs of the lunar surface. And they divided us into two groups. One was the science group that was supposed to interpret the photographs for science; and the other -- the engineering group—was to interpret them for trafficability and land-ability. James “Jim” Sasser headed up the science working group, and Jack Sevier headed up the engineering working group. I was on the engineering working group—land-ability and that stuff--and that's how I first started getting acquainted with Jack [Sevier]. He got more and more into it, until he finally was more or less formally assigned to integrate the science and the engineering. Jack did a very good job.

Jack Sevier mostly helped to integrate our science objectives with the spacecraft--you know. At the same time he did a very good thing showing us why it wasn't exactly the same as flying a Cessna over to Sedona [Arizona] or something, and then getting out and taking [rock and soil] samples. There were real concerns. He was a lot of help in the lunar surface traverse planning--on keeping us on the straight and narrow as to what could and couldn't be done. Traverse distances versus backpack consumables, and that kind of stuff. So, he was very good.”

*Schaber: “We had many problems with Tony Callio [Administrator for Science and Applications at NASA/MSC] in Houston. What was the problem main between him and the USGS?”*

Gordon Swann: “He didn't like the USGS, he didn't like geologists, and he was a real empire builder. He was a physics-engineer. I first met him when he was NASA headquarters- and he and several NASA HQ guys came out and we did some kind of little demonstration at the old CDRA [in the Arizona Bank Building in downtown Flagstaff], and then we took them out to Meteor Crater and Hopi Buttes for a 2 or 3 day exercise. Callio was in my car on the field trip, and we got along just fine and all. Then, when he came to MSC as Wilmot “Bill” Hess's deputy, I'd

mentioned something to Jerry Goldberg [another USGS lende to NASA] about Callio seemed like a nice guy. And he [Goldberg] said yes, but there is a side of Tony that I hope you don't see. Well, as it turned out, that side of him was all that we ever did see. He just was very ambitious and he wanted all the [Apollo] geology [activities] to be done by his gang [at MSC]. He didn't like us--and that was about it. And then Hess resigned and he went to ENCAR in Boulder; then Callio stepped up and took over his job."

[Author's Note- Bill Hess eventually resigned his position [September 1969] as director of science and applications at MSC--"he finally got tired of bucking the entrenched anti-science interests there" (Beatty, 2001, p. 208; Wilhelms, 1993, pp. 237-238).]

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**Hugh F. Thomas** (b. 1943; photo-mechanical technician/cartographer)

The following was taken from an interview with Hugh Thomas by Gerald G. Schaber on 1 April 2001:

Thomas: "I was born in Flagstaff, Arizona and raised there. I went to high school and college, and then joined the Navy, serving some time in Vietnam. I was discharged from the Navy in 1965. I wasn't actively looking for a job, but just kicking back and enjoying being home when a friend of mine I had gone to school with, Joe Dushek [hired by the Branch of Astrogeology in July 1964], called me and asked if I was looking for part time work? I said yes, I'd only been out of the Navy for about three weeks. I said what is it? Joe said well, we have some part time work as a field assistant, and we're doing a lot of time-and-motion studies [pre-Apollo field studies]. I had no clue what he was even talking about.

So I went in and interviewed with Don Elston who was Acting Branch Chief at the time, because Gene [Shoemaker] was out of town. Everything was fine--they had this work--and he said great--come in Monday morning. So, I said fine.

I went home--and this was on a Friday, and Saturday morning about 8 o'clock, the phone rang--and it was Don. He was talking about all this work that they needed done on a semi-annual report, and he wanted to know if I could come into work early. I said fine. So, I interviewed on Friday and went to work Saturday. We worked through on Sunday. [Author's Note: Hugh Thomas was officially hired on by the Branch of Astrogeology on 14 February 1965.]

They were printing the annual report down at the old ANNEX building downtown where the old root beer stand used to be. It was on Santa Fe where Baskin-Robbins was for a long time. The building also used to be called the old city garage years ago.

They had the press set up there, in addition to the photolab--as they called it. We worked all weekend on this annual report. That's when I first met Don Elston. Karl Zeller was running the old press that we printed it on. It was an old Multilith I believe.

That's also where I first met Jim VanDivier. He was working on collating the report along with the rest of us. There wasn't a drafting section then. Gosh, we worked--I don't know--at least ten to

twelve hours a day for at least four or five days there. I figured that that was a pretty good introduction for my first part time job with the Survey. This part time job seemed to just keep going on and on; and I finally got a Career-Conditional appointment that turned into full time (around 1967-68).

At that time Astrogeology had the entire fifth floor, all of the fourth floor, and part of the third floor of the Arizona Bank Building. About the only thing left at the Museum of Northern Arizona (MNA) was the machine shop.

My first supervisor I worked for was [Harrison] Jack Schmitt [later, the Apollo 17 LM Pilot]. After he left for Houston to become a scientist/astronaut, I did several months in the field with Bob Regan [a geophysicist with Astrogeology]. We did a lot of work out at Meteor Crater. We ran a portable seismic rig down at the bottom of the crater. We'd hike down every day, and set up the rig, and we'd run several tests. I wasn't doing any photography at all at the time.

Bob Regan was quite a character. Everyday, we'd drive out there [from Flagstaff] and have to hike down into the crater with all of our supplies--and part of it was carrying dynamite down for the seismic rig. These were 5 pound-sticks of dynamite--a good chunk! We'd get out there and I'd get all set up, and I'd usually have five or six of these sticks of dynamite and a box of caps. Bob would say okay, you can take off now and when you get about halfway down, I'll start down! He wouldn't even walk with me. He wasn't about to walk close to me!

I had worked for the county highway department for about a year and a half; and I ended up doing almost all of their blasting--before I was in the Navy--right out of high school is when I did that.

After that I worked with Yukio Yamamoto and Jim Crossen [surveyors at Astrogeology] doing field surveying. I worked for them for about a year and a half. We did work at Meteor Crater, at Zuni Salt Lake (New Mexico). We also went over to Mono craters in California--all doing base maps for potential astronaut training sites. We spent months and months out at Hopi Buttes. I worked for them for quite a while, and then I decided that I needed a little more education. Then, I did the unthinkable, I'd had my full-time appointment, and I requested to be put back on WAE status, and I went back to NAU for about two years. I worked part time whenever they had a job that I could do.

I took mostly math and drafting classes. I took nearly all of the drafting classes they had to offer because that's the direction that I thought I wanted to go.

I came back to Astrogeology one time to borrow a Leroy drafting set from Jim Vandivier, the drafting supervisor at Astrogeology, and told him what I was doing. I had a drawing that I wanted to spiff up a little bit and do some inking on. Everything was done in pencil; so, I inked the drawing, and it went over fine. I got a pretty good grade on it. Jim was kind of impressed with my concern over doing a good job and he asked me if I was interested in working for him. I said gee, yeah, I still have a WAE appointment.

Jim's shop was located down on Fourth Street at that time. They had just gotten into the new building on Fourth Street. When I told him that I already had a WAE appointment, he almost

broke his neck getting up to Building-One on the Mesa to check the records because they had a big hiring freeze on at the time and couldn't hire anybody new. He came back down and called me about a half-hour later and said do you want to come to work Monday morning? I finished up my classes and I've been here full time ever since. It was extremely lucky that I had requested to be changed to WAE instead of resign, because I would have never got back on [with the Survey].

I started out doing engineering drawings--some of the prototype Apollo Hand Tool instruments that they were designing for the astronauts to possibly use on the Moon. We did four or five of them that our [Astrogeology] machine shop actually built. I switched from engineering drawing to illustrations. When they got Building-Two done up on the Mesa, we came up there from Fourth Street--probably in 1968-or so--because I wasn't down there on Fourth Street more than about a year [Author's Note: Building-Two on McMillan Mesa was indeed completed in 1968].

We had several different names [over the years]. There were two groups together. The group that Roger Carroll went to head up after Russ Wahmann left did the actual cartographic mapping--all of the landing site maps for the missions [Apollo and others]. Then, the other group--I forgot what they called us. We did all of the illustrations and technical drawings.

That's when they moved the photolab out of Building-One [on the Mesa] into Building-Two. I had always been interested in photography, so I kept bothering everybody back there, helping out when I could, and sort of just learned on the spot. So, after about two years I started spending more time back in the photolab helping out than I was drafting; so Jim asked me if I wanted to work back there--and I said sure.

Russ Wahmann was the head of it [the photolab] but his office was always at the Bank Building. He never even had an office close to us. He was a "removed supervisor." Russ was an old Topo Division man who was at Rolla Missouri [when we hired him]. That's where Cal Hazelwood came from also [Rolla]. He was the photographer at the time when we had the ANNEX building [also called the Dance Hall] down on Santa Fe [downtown Flagstaff], and he moved up to Building-One.

Jim VanDivier was a draftsman for Superior Oil before he came to Flagstaff. It might have been in Wyoming. After he got more or less established here, and they started expanding with more work, that's where he got both Ray Sabala and Roger Carroll. They were both working for the Superior Oil Company [but in a different location than Vandivier]. Ray came down first, and then about a year or so after that, Roger came down."

*Schaber: "Did you ever get to go to MSC in Houston for any of the Apollo missions?"*

Thomas: "I only got to go one time to Houston. That was for Apollo 11."

*Schaber: What did you do there?"*

Thomas: "Just some of the post-mission reports-drafting. I was there when they let the astronauts [Neil Armstrong, Buzz Aldrin, and Mike Collins] out of quarantine [at the Lunar Receiving Laboratory at MSC in Houston]."

I think we were actually the busiest [over the years] during the Apollo Program, because we had to do the Apollo landing site maps at 1:5,000,00-scale, which was a formal [USGS] publication. Gosh, those maps would take 6 to 8 months if you just put them on the Survey's usual publication procedure, but we had to do them in 90 days!

Roger Carroll's cartography group worked on the formal publication at Astrogeology, and we did all of the other larger scale maps. There were three or four different scales of maps. It was a big hassle. Normally, we worked from 9:00 AM to 5:00 PM, and then we'd come back in at 6:00 PM and work until 10:00 PM every day."

*Schaber: "And nobody ever complained!"*

Thomas: "We had 12-hour days for I don't know how many years [Author's Note: For about a decade], actually. In those days it was just--get it done. I know that there were a lot of us who put in a lot more time than we ever wrote down."

*Schaber: "Tell me how the photolab came about [and how it was divided]."*

Thomas: "Well, when Building Two was set up [1968], we had two photolabs. The [regular] photolab was run by Karl Zeller. He did all the color work and slides; and at the time, we did a lot of aerial photography--500-foot rolls of film and paper.

The other lab that supported all of the cartography and everything [for Batson's group] was called the photomechanical lab. So there were basically two photolabs with very different functions."

When we moved into Building-Two on the Mesa, it [the photomechanical lab] was put under Van Diver's group since it dealt primarily with illustrations and text, etc. That's where the two labs spit so to speak. We were still within Astro when we moved into Building-Two.

We all stayed within Astrogeology until--I can't remember when it was."

*Schaber: "In the 1980s?"*

Thomas: "That sounds about right. We went into what was called the Publications Division [of the USGS]. We worked under them for a couple of years, and then we went to national Mapping Division [of the USGS]."

*Schaber: "Was that because the NASA funding slowed down pretty much after Apollo to support all of you photolab and cartography people?"*

Thomas: "Yes, basically. The Branch of Astrogeology said that they couldn't afford us, but they still wanted all of their work done by us. As an example, after the Magellan spacecraft sent back all of those radar images of the surface of Venus [1990-1994] I personally printed 15,400 feet of 42-inch-wide glossy paper of those images. That was a big rush job [as usual].



*Schaber: "Tell me about when the computers came along, and we switched from scribing, drafting by hand, etc. to computers. I keep wondering if that would have been a boon during the Apollo days, or not. It's hard to say, but we did so much with the technology we had. I can't imagine us being more efficient than we were, but I suppose we could have been if we had had all of this fancy equipment that we have today."*

Thomas: "I'm definitely one of the old stick-in-the-mud holdouts, but I can see it now. It is so much labor saving. One of those maps, with the hand scribing, the peel coats, and the photomechanical work--your looking at 1,500 to 2,000 hours of labor. Materials, I'm not even going to guess at. The same job using the graphics computers available today takes between 110 to 150 hours at the most--and every bit of the quality is there."

*Schaber: "You can also make changes quickly."*

Thomas: "Oh yes. If you were to change a color on a map using the old systems [we used during the Apollo days], where you had to go back and change peel coats, you're talking about \$300 to \$400 and three-or four-days labor."

*Schaber: "That makes it even more amazing what we did with what [technologies] we had during the Apollo Era. If you want to go into graphics or mechanical drawing in college today, you probably never even see a drawing board."*

Thomas: "They don't even know what a pen looks like."

*Schaber: "Tell me about your impressions of the exciting Apollo Days."*

Thomas: "It was far more than exciting!"

[Author's Note: Hugh Thomas, who provided truly outstanding photomechanical work for the USGS, Flagstaff Field Center, for almost four decades, retired in 2002 and still resides in Flagstaff.]

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**William G. "Bill" Tinnin** (vehicle mechanic):

Bill Tinnin started work in June 1965 with the Branch of Astrogeology in Flagstaff, Arizona. He resigned from the USGS in December 1965 but was rehired in July 1966 to work with Putty Mills on his Lunar Vehicle Systems, Field Operations, and General Support Project. Bill would immediately be assigned to maintaining the MOLAB and Trespasser vehicles. Bill Tinnin, Dick Wiser (starting in spring 1966) and Putty Mills would together play a major role in the design and constructing of the Branch's two hand-made Lunar Rover Vehicle (LRV) simulators (Explorer and Grover-see main text and below) used extensively in geologic field training of the crews of Apollo missions 15-17. Bill Tinnin remained with the USGS' Flagstaff Field Center for many years as head of the Center warehouse activities before retiring from the Survey. Today, Bill is active as a maintenance manager of a number of realty properties around Flagstaff. Bill and his wife still reside in Flagstaff.

The following was taken from an interview with Bill Tinnin by Gerald G. Schaber on 12 Jan 2001:

Tinnin: "I had been running a garage in Flagstaff--as a mechanic--in 1963 and I wanted to get something that was permanent and with benefits. So I applied with the USGS. That's when Don Elston was in the Arizona Bank Building downtown. So he hired me.

I started off with Bob Regan [a geophysicist with the Branch of Astrogeology] doing gravity surveys--Meteor Crater, Hopi Buttes, and other areas. We were doing surveys out there at Bonita Lava Flow (adjacent to Sunset Crater just northeast of town) when they were doing the time-and-motion studies [in preparation for astronaut training.] We set up the surveys with Jim Crossen [a surveyor for the Branch of Astrogeology].

I think I started out for about \$1.40 per hour. I had been trying to get a raise and couldn't, so I went ahead and quit. About nine months later, there was an opening with Putty Mills down at Astrogeology's vehicle shop; so I went to work with Putty in 1965. The only vehicle we had at that time was the "Trespasser" which was a [commercial] ten-wheeled vehicle. Then, the MGL (Mobile Geologic Laboratory built by GM) was delivered [also called MOLAB for Mobile Laboratory]. We did quite a bit of work with the MGL, taking it to different locations and seeing what it could do, and having the geologists do little surveys or traverses out of the MGL. We took it to the Hopi Buttes area. We also took it out at the Cinder Lakes (just Northeast of Flagstaff) even before Astrogeology's crater field was constructed out there. We also took the MGL out to Belmont (near the ice cave/lava tubes just across the road from the Navajo Army Depot) just west of Flagstaff.

When I got to Putty's shop I started keeping [maintaining] the Trespasser field test vehicle down at the old Byrd Building at 1990 Huntington Drive [just east of Enterprise across the tracks between East and West Flagstaff] (see Fig. 45a,b in text). The "Explorer" [Astrogeology's first hand made lunar rover simulator] was also later built down there (see Fig.45h, 47a in text.) The "Grover", our final lunar rover [built to be a terrestrial version of the actual Lunar Roving Vehicle, or LRV) was constructed at a facility at 1720 East Street in East Flagstaff (see Figs. 75). That's also where our canvas-covered LM simulators were later constructed."

*Schaber: "Tell me first about the Explorer vehicle; how did that come about?"*

Tinnin: "Well, it was [fashioned from] a jeep. They brought a Jeep frame in with the motor and transmission and wheels. No body on it at all. They-took the motor off the transmission and transfer case; used the axles and built suspension that would fit the frame body that we built. We built the body out of inch and 3/8 chrome-moly tubing. And then we put suspension on all four wheels. We took the wheels that came with the Jeep and put on Cub Tractor tires on it. It had a single joystick control on it, which was electric-motor driven to drive the wheels."

*Schaber: "Was that an airplane-type stick?"*

Tinnin: "Yeah, it was kind of like an airplane stick"

*Schaber: "Is that where you got it; or did you build it from scratch?"*

Tinnin: "Well, we built this one. It had servos in it with little switches--when you turned the handle to the right it would engage the switch which would drive the motor."

*Schaber: "Was the design of Explorer influenced by our Branch science staff?"*

Tinnin: "I'm not really sure where the design came from. I know that the design changed a little bit after we started building it the vehicle."

Tinnin: "We had a box covering the top part of the motor [in the middle of the vehicle]. We had one seat on the front to start with--with a joy stick control. We had one seat on the back for a passenger. We also put on 360-degree cameras; we put magnetic devices on long poles and forward and straight up. We did have a lot of the navigation gear on it in the form of a navigation plotting board."

*Schaber: "Do you recall what kind of navigation system it was--or where you got it?"*

Tinnin: "It was excess property off some aircraft--it was Air Force stuff."

*Schaber: "It was supposed to plot its own position as it went?"*

Tinnin: "Right, Explorer had an XYZ plotter on it--and it had a gyro on it (see Fig. 55 in text). Then, we also fixed it so it would run remotely, so we could stand off with a model airplane remote device and control the vehicle. We could run it where we wanted to run it. We were testing the navigation system out on the [Navajo] Reservation one time, just running this vehicle [the Explorer] around. There wasn't a soul around but us when we started. The next time we took it out running it around out there, there were probably ten or twenty Native Americans out there [watching]--and the next time there were probably fifty."

*Schaber: "And there was nobody driving it!"*

Tinnin: "No, the TV just showed the terrain ahead. There weren't really any gauges on it. But we were looking at the terrain ahead. We built in a 3-second delay so if you were coming up on an obstacle like a rock or something like that--you had to judge where you were going to be in three seconds--whether you were going to be over it or behind it, or wherever. So, it was pretty tricky to run. We had a little incident out there. We were out on a field trip and--pause----anyway, this vehicle was run right into the bluff. There was a rock wall there. And it was run right into the bluff. We had a person sitting on it [most likely Putty Mills was driving]."

*Schaber: "On the back I hope!"*

Tinnin: "No, he was on the front. When they run it into the bluff, well it folded the footrest up against him. It didn't hurt him but it did do some damage to the vehicle."

Then, another time, they had it climb the back of one the USGS carryalls. It comes around the corner there--with the delay built in--and it runs right up on the back of the carry all. This time it was in remote mode."

*Schaber: "So, locally, we had it out at Hopi Buttes--and where else?"*

Tinnin: "Merriam Crater [just NE of Flagstaff in the east side of the San Francisco Volcanic Field, Meteor Crater, and elsewhere."

[Author's Note: The Explorer vehicle was used in a test of geophysical instruments in Amboy Crater, California (17 February 1966 (see Fig. 23); to the National Boy Scout Jamboree near Farragut, Idaho (1-9 August 1967) (along with MOLAB) (see Fig. 52), the Arizona State Fair in Phoenix (4-14 November 1967 (see Fig. 54).]

*Schaber: "Then, in April 1970, Putty Mills, Don Wiser and you started construction of Grover, the Branch's Earth-gravity version (1-G) of NASA eventual Lunar Rover Vehicle. Putty Mills told me the story of how Don Beatty [NASA Headquarters] called and said NASA needed a 1G version of the Lunar Rover built because the Rover simulator that Boeing built for them to use in astronaut training kept breaking weld seams. Is that the way you remember it?"*

Tinnin: "Yes, that's the way I remember it. It just wouldn't hold up on their crater field [at Cape Canaveral]. So, they wanted something that was stronger--that still looked the same, and acted the same, and controlled the same. So we built the thing out of chrome-moly alloy tubing."

*Schaber: "And of course Don Beattie told your shop that NASA needed the vehicle hand-built in 90 days for an important test—and Putty Mills actually agreed to do it. Unbelievable!"*

Tinnin: "There again, you were messing with chrome-moly, so you used a heliarc welder on it. Everything had to be ground to fit--the electric drive motors in it, for instance, the boxes had to be built to house the motor, as well as all the suspension. So everything had to be hand-fitted. There again, it was single-stick control. It was built as close to the specs on the actual [Boeing] Lunar Rover as we could get."

*Schaber: "So you, Putty and Dick Wiser built the Grover. When did Dick arrive?"*

Tinnin: "He come to work after I did--about a year after I did [in November 1966]. We were still in the Bryrd Building down there on Huntington Drive [in Flagstaff] when Dick arrived."

*Schaber: "Was Dick local?"*

Tinnin: "Yeah, he had been working for Kaibab Lumber in their yard there as a general handy man. Dick has passed away."

*Schaber: "So, you get the Grover completed in the promised 90 days. What happened when the first astronauts came for a demonstration and test ride on the new astronaut training vehicle? I*

*recall that the Apollo 16 crew, in addition to other astronauts and NASA types, were there at your shop on East Street [1720 East Street] in East Flagstaff where Grover was constructed."*

Tinnin: "I think that's probably right. I've got some pictures of the astronauts who did come to check it out. It wasn't even quite finished at that time (see Fig. 78 in main text)."

*Schaber: "Tell me about some of the other accessories you put on Grover."*

Tinnin: "Well, we built simulated backpacks like the actual backpacks that would be used on the Moon; using the same specs. Then we built the seats on the Grover that would accommodate the backpacks. It was single stick control. We had the [S-band and high gain] antennas that we put on there. The vehicle was all battery operated; it had four, six volt, batteries in it that run the steering and the control system. It had a tool carrier on the back to carry the [lunar] tools. Then, after that we used a lot of instruments on it. You know, again, the cameras, etc."

*Schaber: "I know that the astronauts really liked it."*

Tinnin: "Oh yeah. We even got a set of actual flight fenders and put on it. We were over in Nevada one time--a lot of sagebrush over there, etc. We tried to talk them out of putting those flight fenders on because they were just thin fiberglass. But, no, NASA had to have them on. It turns out that we ran out of battery powers about two-thirds the way into the test;, so Dick Wisner and I hooked a rope onto it and we were towing it to finish off the field exercise that they had going there. And of course we run it over sagebrush that ripped the fenders right off."

*Schaber: "Well, you know, on Apollo 17 they lost a fender too; they used Chronopaque maps from our Lunar Surface Map Package that we made for them, along with duct tape to make a new fender [See Apollo 17, December 1972 in main text}. So, our geology maps were used twice for non-geology things. One was on Apollo 13 to save their lives to make those little adapters to save their lives on the way back from the Moon on Apollo 13, and then on Apollo 17 they used them with duct tape to replace a broken fender and keep the dust from flying around."*

*Do you remember recall how many places we used the Grover in astronaut exercises starting with the Apollo 15 crew?"*

Tinnin: "Well, we had Grover so many places. We were at the Nevada Test site for a while. We ran the vehicles up there on the north end of the test site. They would fly the astronauts in by helicopter for the test, and then they would fly them into Las Vegas where they were staying. In the meantime, we [the Branch's Vehicle Field Vehicle and logistical Support group] would truck them, as well as all the gear, to the next location and then we'd start all over again. Usually, that's the way it went. It went for two or three days."

[Author's Note: The Grover was actually used in geologic training exercises for Apollo astronauts in Hopi Buttes (north of Winslow, Arizona), the Nevada Test Site, the Cinder Lake Crater Field and Sunset Crater National Monument (northeast of Flagstaff), Merriam Crater northeast of Flagstaff, Coconino Point north of Flagstaff, near Boulder City, Nevada and the Rio Grande Gorge near Taos New Mexico (see Table I; see 1970-1972 in main text for more details and dates

for these exercises using Grover). On 25-26 Jan 1971, Grover was taken down to the Branch's Black Canyon Crater Field in Verde Valley, Arizona (south of Flagstaff) for a non-astronaut test of its remote TV system].

*Schaber: "Yes, we were training two [Apollo] crews at once there for a few years."*

Tinnin: "Yes, the astronauts were pretty straightforward. When they came in, they had a job to do; they got her done and they were gone."

*Schaber: "So, what was your shop basically involved in after Grover was built?"*

Tinnin: "Well we had to maintain it. And we also transported it to the different astronaut training sites--wherever they decided they were going to go. Then they put us to building these LM units that had the window design the same as the actual lunar lander (LM). They were built on 3 inch chrome-moly pipe with tractor disk feet as footpads. And if I remember right, they were somewhere like 12-feet in the air at the base."

*Schaber: "The canvas ones you're talking about. Before that you had the plywood LM mockup that NASA had built-right"*

Tinnin: "Yeah, NASA had somebody [Huntsville] build that old full-sized plywood one [simulating the actual LM ascent stage; [it was very heavy]. They contracted it and shipped it to us early on. But we used to haul it out to Hopi Buttes, the Cinder Lake Crater Field, and other areas. Two men could pick up the canvas units easily. We got it out there in the field; we just spread the legs out, and set it right up. We had four of those canvas LMs at one time."

*Schaber: "So then, after the period of Grover maintenance and trucking that thing around to our many astronaut field exercises, you eventually took the Grover up to the Survey's National Headquarters in Reston, Virginia and put it on display-right?"*

Tinnin: "We first took the MGL to the Marshall Spaceflight Center in Huntsville, Alabama. Dick Wisner and I drove it back there on a lowboy trailer. And later on, we took the Grover back to Reston, Virginia. It was kind of funny. Everybody was standing around there figuring out how they were going to get this vehicle into the building. They were going to take out this window--knock this wall out, and they were going to do this--they were going to do that. I went back there alone, so I got a couple of guys with me, and we kind of rocked it up on two wheels and slid it in sideways."

[Author's Note: Yes, and eventually the Grover got moved down into a dark hallway [in Reston.] Then, when I was Chief of the Branch Chief of Astrogeology (1983-1986) I got a call from the people at the Space Museum at Alamogordo, New Mexico. They wanted to borrow it [the Grover] for two years to put it in their space Museum there—just on a two-year loan (we thought). I thought at that time that it was a good idea, given that we actually didn't have a good place to display it at the time and it would not too far from Flagstaff when we wanted to retrieve I in two years.]

However, that turned out to be a bad idea because when the two years were up they didn't want to part with it. They said that our Lunar Rover Grover was an important part of their most popular exhibit. Of course, I showed some resistance to their keeping it any longer. As a result, the museum people there decided to leave Astrogeology out of the loop; they instead they had their rather influential U.S. Senator at the time call Dallas Peck, then Director of the USGS. Dallas, for some reason told them that they could keep it for ten more years—without even letting us know in Flagstaff.

After that ten years went by we discovered that the museum had, again without our knowledge, secretly petitioned the management at Reston and successfully had the loan contract for our Lunar Rover extended another ten years. I did have the foresight, however, when I wrote the original contract with the Museum [back in 1983] that we could get the vehicle back at any time we wanted to—without notice, when we had a place to display it properly.

So, when the plans for the new Shoemaker Building ( the new home of the Astrogeology Team) at the Flagstaff Field Center were completed, and the building was being constructed, Wes Ward, then Chief of the Astrogeology Team, instructed the staff at the Space Museum at Alamogordo that we would be wanting the Grover back—and soon. The Grover was finally trucked back shortly before the completion and dedication of the Shoemaker Building in September 2002. It now permanently resides in a special alcove built just for it in the foyer of the new Shoemaker Building at the Flagstaff Field Center in Flagstaff (see Fig. 77d in main text.)

*Schaber: "So what did Putty Mills and Dick Wiser and you do when Apollo was over?"*

Tinnin: "After Apollo ended, we started cleaning out the shop and transferring equipment we had there. Of course there wasn't any need for the vehicles or anything, so, they were going to lay us off or find a hole elsewhere for both Dick Wiser and I. So, Dick and I went to Denver and we looked around up there at the Federal Center there. Both of us decided that we didn't want to live in Denver. So, we came back to Flagstaff. Then the Flagstaff Field Center offered me the maintenance position at the warehouse [Building-Five at the Flagstaff Field Center]. In the meantime, you know, I had been involved in moving everything and everybody up at the Center. As soon as they finished the warehouse--building five [1976]-we started moving everything out of the Fourth Street building, the machine shop etc."

*Schaber: "Where you out at the Cinder Lake Crater Field when Walter Conkrite came out right before the Apollo 11 crew were supposed to come out there in 1969?"*

Tinnin: "Yes, he was the most kicked back guy I ever saw in my life. He sat out there in the seat of his car and he wrote out what he wanted to say--and he came out and he said it--and he was gone. Jules Bergman (ABC science anchor) was just the opposite. I mean, he'd get out there and he'd clear his throat."

*Schaber: "So Bill, tell me what you thought about your time with Astrogeology, especially during the Apollo Era."*

Tinnin: "It was the greatest job I ever had! Like you say--everything was new. Everything was different and exciting.

*Schaber: "Everybody worked hard."*

Tinnin: "That's right. We worked any hours. Whatever they wanted we did; nobody complained. We'd go out on a geologic exercise or astronaut training exercise, get where we were going say maybe 4:30 to 5:00 in the morning, then, we'd pull out sometime 7:00 or 8:00 in the afternoon. Then, we would have to drive back maybe 30 miles or so back to the motel or whatever."

*Schaber: "You know Bill I've been thinking about this a lot since I've been interviewing people for this Branch history document. It seems to me that the difference between then and now is like day and night; what you must have is a national program--a real exciting national program that's of interest to nearly everyone's exploratory nature—one of Man's natural instincts I believe. Yes, the Soviet race to the Moon spurred the Apollo Era on. But, you know--that is what this country needs now. We need a countrywide, dedicated task to get people excited--and the money would be forthcoming. We had enough money in the Apollo days to really get the job done. People say, well, those days are over. Well, not if you have the national interest behind it--and the congressional people behind it--like if there is a Mars mission someday. The world will be as excited then as we were about going to the Moon--I hope. But it won't be for a while because people's priorities now are other things. People say, why spend that money on space--we should spend it here on Earth. Where do they think that money is spent? It's not spent on space--it's spent here. That is what we need to tell the people—then truth for a change."*

Tinnin: "Yes, you have a pride in something like that."

*Schaber: So, I also heard that you and your group helped make a Tang commercial?*

Tinnin: "Yeah, we worked all one night for Tang. That is when they were putting Tang in the suits for the astronauts to drink on the moon. And best I can remember, one night--I can't remember what crew it was--but they were up there and the inner bottle broke and he had Tang in his suit. So, we just drove the Grover for them, and then they put it on TV as a commercial. I guess it was on for six to eight months."

*Schaber: "So, I'm surprised. Well, it's a Government thing I guess there no reason not to."*

Tinnin: "Yes, they had to give us one dollar. And they were supposed to send us a case of Tang--but we never received it."

*Schaber: "That's just as well!"*

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**George E. Ulrich** (b. 1934; geologist; Ph.D., 1963 University of Colorado, Boulder) arrived in Flagstaff to work with the Branch of Astrogeology in September 1965. George, who was born in Cortland, New York, grew up in rural North Carolina in the tobacco-growing country. He then moved to Providence, Rhode Island. His father was a Universalism Minister. George went to Jr.



High school in Providence, and then went to high school in Leominster, Massachusetts and Concord, New Hampshire. He then went to Brown University on the Navy Scholarship Program and graduated in 1955. George was in the Navy from 1955 to 1958 on a Destroyer Escort. After the Navy, George went to the University of Colorado at Boulder (from 1958 through 1963), where he received his Ph.D.

The following was taken from an interview with George Ulrich by Gerald G. Schaber on 15 February 2001:

Ulrich: "I got interested in geology in high school because a good friend of the family liked to go for walks, and he would take me along sometimes through the woods and show me rocks--just talk about rocks--and I got kind of interested. Then, when I got to college I intended to major in chemistry. However, after the first year of organic labs all afternoon--and Calculus, which I didn't feel very comfortable with--I switched to geology and thoroughly enjoyed the rest of my undergraduate years.

After completing the Ph.D. at Boulder, my next job was with the USGS in Hopkinsville, Kentucky and I worked for two years there on the state geology-mapping project. Towards the end of the second year, Joe O'Connor called from Flagstaff, and I think Gordon Swann called later on. They knew that I had done my dissertation on Pre-Cambrian structure (at the University of Colorado). The stereo plots [used in structural geology work] and all that kind of stuff they thought might have application in lunar geology.

When Joe O'Connor and Gordon Swann called from Flagstaff and asked, would you like to work out here [at Flagstaff], I said you bet ya! When I was in graduate school, I had gone on a field trip down through Arizona and New Mexico--and we ended up in Flagstaff. I fell in love with the town the first time I ever saw it. Matter of fact, we slept on Bill Breed's (staff geologist at the Museum of Northern Arizona) living room floor,-another guy and I. Bill took us over the next morning and showed us the spectacular view of the San Francisco Peaks from the Museum. We had some good Mexican food down there on Route 66, and I just knew that that's where I would really enjoy living if I ever got a chance. So, when the guys called about the lunar program and all that--I jumped at it. I had watched the Ranger [lunar impact] mission on a television. So, I was beginning to get interested in what was happening out in Flagstaff. All I knew about Gene Shoemaker was that he was one of the prime people on the Ranger Mission and that he was in this program called Astrogeology--or lunar geology at that time. I was just watching it from the outside. Then, I got a chance to go to Flag. We drove across the country to Flagstaff with an eight-year-old son, an eight-week-old daughter, and a dog. We arrived in September 1965 to the magnificent view of a Labor Day snowfall on the Peaks.

I was assigned space in the Arizona Bank Building. You and I were in the same office, and I think Dave Schleicher was in there with us. At least you and I started out together. We were on the third floor of the Bank building. The first operation that I remember was--I think two days after we got there--we were on our way out to Hopi Buttes [Trading-Post diatreme] for a field test. We were working up on top of the mesas there doing visual descriptions and recording them back in the LM [the full-scale plywood lunar Module, or LM, which NASA had built for Astrogeology]. It was near Castle Butte. There was another butte over there across the valley from the Trading-Post

diatreme where we had another test later; we called it Apollo Butte. I remember the film documentation people [from Astrogeology] were doing the movies. We were all living in motels in Winslow, Arizona. That was a great time."

The following statements were added by George Ulrich on 24 July 2002 during his review of an early draft manuscript of this work:

Ulrich: "The nine years following that initial field test produced many fond memories for me of the great adventure of Apollo. The experience of mapping a quadrangle on the Moon, beginning with cold nights at the telescope on Anderson Mesa, and finishing with Lunar Orbiter photographs under the guidance of Don Wilhelms was a unique opportunity for a field geologist. Another interesting exercise was helping to create the replica of the early Apollo landing site in the Cinder Lake area near Flagstaff's sanitary landfill with colleagues Red Bailey and Hans Ackerman. This cratered terrain in the dark cinder deposits of Sunset Crater was heavily used both for landing location exercises from the windows of the mockup LM, and later for lunar roving vehicle tests and astronaut training. I enjoyed the freedom to develop field tools like the sun compass, both staff-mounted and vehicle-mounted, with enthusiastic help from Howie Pohn and Walt Fahey's machine shop, to demonstrate surveying techniques in a nonmagnetic environment.

The work I am proudest of was participation with the engineering community in the design and development of the lunar roving vehicle (LRV) for the final three Apollo missions. Henry Moore, Richard Pike, and I prepared a terrain analysis and traverse document that was a primary basis for Bendix and Grumman's design studies. This work also led to the building of the Survey's own working model of the Grover (Geologic Rover), used extensively in astronaut training, for which Putty Mills and his team earned the highest award given by the astronaut crews to any Survey employee—the Snoopy Award.

Beyond these adventures, I remember clearly the mission simulations and the excitement of the Science Support Room [or Science Operations Room] in Mission Control during Apollo missions 15, 16, and 17 with the Swann and Muehlberger field teams. The collegial relation with these teams was a model of cooperative effort, not to be found in many other careers within the earth sciences."

[Author's Note: George Ulrich and his wife Sally presently reside in Sarasota, Florida.]

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**Joel S. Watkins, Jr.** (b. 1931; geophysicist) arrived in Flagstaff to work for the Branch of Astrogeology on 7 April 1964. Watkins was trained formally as a geophysicist specializing in magnetics and gravity techniques.

Joel had originally joined the U.S. Geological Survey, Branch of Geophysics in Denver in the summer of 1961. Joel met Marty Kane after arriving at the Survey, and they became close friends. After a year or so, Joel discovered that the magnetics program in the Survey was sort of dying out because of lack of funding. He had become involved in ground water seismic refraction work between 1962 and 1963, but then learned from Marty Kane that the Branch of Astrogeology group in Houston (MSC) was looking for geophysicists to help train the astronauts for lunar exploration.

Kane asked Joel if he was interested--and he was. Astrogeology's astronaut training group at MSC in Houston had disbanded before Joel could take an active part; so he moved directly to Flagstaff (April 1964) (from an interview with Joel Watkins by Gerald G Schaber on February 6, 2001).

The following was taken from an interview of Joel Watkins (Hearne, Texas) by Gerald G. Schaber on 6 February-2001

Watkins: "I was born in Poteau, Oklahoma. I grew up in Warren, Arkansas during the depression for the most part; and then WW II came along. I graduated from high school there in Warren in 1949. Then, I took a ROTC scholarship to the University of North Carolina, where I graduated in 1953. I guess my principal claim to fame there was being Phi-Beta Kappa in 1953.

I went into the Marine Corps for three years and got out in the summer of 1956. I spent two years at the University of Wisconsin, in Madison, working in gravity and magnetics; and then transferred to the University of Texas in Austin in 1959. One of the classic geologists--big name in the Survey--came to the University of Texas my final year there working on my Ph.D. and taught a three year course that I took. I apparently impressed him and he encouraged me to apply for a job with the USGS--which led to my joining USGS in the summer 1961. I had finished my Ph.D. there earlier that year (1961) in gravity and magnetics in the Washatau fold belt all through Texas."

*Schaber: "How did it happen--who did you contact first at the Survey]?"*

Watkins: "Well, I just applied. I knew Isadore "Izzy" Zeits and Roland Henderson were quite well known in the magnetics business by virtue of all the magnetics work that had been done by the Survey in the late 1940s and 1950s."

*Schaber: "Did you know Marty Kane?"*

Watkins: "I didn't know Marty Kane until I arrived at the USGS. I met him--and Marty and I became close friends. After a year or so there, I discovered after I got to USGS that the magnetic program was sort of dying off because of the lack of funds. Also, I got a little disillusioned with gravity and magnetics because of the fundamental ambiguities involved. I got involved in 1962, I guess, in my first ground water refraction project. I got really heavily involved in that in 1962 and 1963.

I guess about 1963, Marty went with the group in Houston that was training astronauts, and he encouraged me to join the Branch of Astrogeology. Marty Kane and I jointly put together a proposal [while he was still at MSC in Houston with the Survey's astronaut training group], and submitted it to NASA. It was funded. The proposal was for the investigation of In situ physical properties, which was basically a refraction methods investigation [that was carried out for the Branch of Astrogeology in Flagstaff]."

*Schaber: "On volcanic rocks-right?"*

Watkins: "Not necessarily volcanics, but potential lunar-analog rocks of all types. This turned out, of course, to be mainly volcanics. We went to Sunset Crater, SP Crater and other places around Northern Arizona—in and around Flagstaff for the most part. We also worked on some sands and some limestone to determine the physical properties and to try to anticipate what the physical properties of the near surface rocks would be on the Moon."

*Schaber: "When did you actually come to Flagstaff?"*

Watkins: "I came there in April of 1964. At that time, the Branch was still out at the Museum of Northern Arizona. Gosh, there weren't too many people there at that time, but I guess a year or so later, we moved into Building-One [Astrogeology's new headquarters building] up on the mesa. I was only there a little over two years, but we did a lot of things. I hired Dick Godson. I was in charge of the project. Marty didn't come to Flagstaff. Marty went to graduate school at that time to work on his Ph.D. I was pretty much in charge of the project out there at that time. I guess I was called a Section Chief--or something like that."

*Schaber: "Who all did you have working with you at that time?"*

Watkins: "Well, at one time, I think I had thirty-one people working in the Section. Dick Godson was in charge of the field operations. We had a couple geologists working with us out of Menlo Park. We had a fairly good-sized technical staff. Oh yes, Norman Bailey was also there with us for a while. Emeline M. Hull [hired by the Branch of Astrogeology in July 1964] was my secretary."

*Schaber: "Down in the Bank Building--or up at the Mesa."*

Watkins: "She started at the Museum. That's right; we were in the Bank Building for a while too.

Well, we moved in over there [on Jamison Street in east Flagstaff]. [Author's Note: We used to be neighbors and good friends with Joel Watkins and his family back in 1965-66.]

Watkins: "Remember, we were over at a party at Russ Wahmann's house one night--and an entire wall fell down. Those houses that we were in part of Upper Greenlaw Estates were pre-fabricated and, somebody apparently didn't get the wall nailed up very well.

Anyway, Dick Godson and I went around all over the country. Godson did most of the fieldwork. I was also involved in astronaut training. Let's see--I was over at Philmont Ranch in New Mexico [3-6 June 1964]. I was on the pre-training trip to Hawaii, but I didn't actually get to go there. There wasn't much we could do with explosion work. I was also at the Nevada Test Site.

The other thing you might be interested. There were four of us who were seriously interested in applying for astronaut training. I was one of the four--and of course [Jack] Schmitt got it. But Gordon Swann and several others from the Branch applied. I started out with some of the preliminary tests--but I have a blood condition. So, before submitting the final application to

NASA, I went up to Salt Lake for a day and went through a series of blood tests up there to get some expert's opinion on whether I might pass. They told me no; so I backed off. But Gordon and Schmitt went on through to a more advanced stage—and Jack Schmitt was the one selected. Yes, it was a good time.

Then, I guess, the summer of 1966, I was feeling some stress. I had taught myself seismology and felt the need for more formal training in seismology and information processing; so I prevailed upon Frank Press (perhaps with a boast from Gene Shoemaker) to take me on as a post-Doc at MIT. So, I went there, and the following spring my wife Carolyn had some medical problem, so I bailed out and went to North Carolina in 1967 and took a professorship there. I stayed there for five years, and then went to the Maurice Ewing-lab in Galveston, Texas--as part of the University of Texas system. I went there in January 1973. I stayed there for five years, and then joined Gulf Oil for eight years. Then in 1985 when Chevron bought Gulf, I took early retirement and came to Texas A&M University.

I've been teaching seismic interpretations. I've gotten into the oil--into seismic reflection. When I went to Galveston, we had an opportunity down there to put a ship to sea and get into some deep-water seismic reconnaissance surveys. So, I got involved in that and been there ever since."

*Schaber: "You know, I can't think of you without thinking about that time we went up to take those ski lessons together at the Arizona Snow Bowl [a ski area at 9,800-11,800 feet elevation atop the San Francisco Peaks north of Flagstaff]--remember that. One night, it was 45 below zero in Flagstaff and we were up there at perhaps 50 below. As a matter of fact, I found out that the night we were there the thermometer that they had up the Snow Bowl--a very fancy one--broke [at -52 degrees F]. So, that was amazing.*

*That's when I gave up skiing.*

Watkins: "I remember trying to learn how to ski in those old boots. It's a wonder I didn't break my leg. I still ski a little bit--but not very much. I have a bad knee and I'm getting rather old for that kind of thing."

[Author's Note: Joel Watkins currently resides in Hearne, Texas where he is still Emeritus with Texas A&M University.]

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**Don E. Wilhelms** (b. 1930; geologist/lunar expert; Ph.D. 1963, University of California, Los Angeles)

The following has been taken in part, or paraphrased from, a biography of Don E. Wilhelms by Branch of Astrogeology geologist Mary G. Chapman, (<http://astrogeology.usgs.gov/About/People/MaryChapman/>) written 15 December 2002 for the Branch of Astrogeology website (<http://astrogeology.usgs.gov/About/People/DonWilhelms/>).

Chapman: "With his geologic mapping of the Moon, Don Wilhelms set the methods and standards for planetary mapping. He is author of *Geologic History of the Moon* (1987, USGS

Professional Paper 1348) and *To a Rocky Moon, A Geologist's History of Lunar Exploration* (1993, University of Arizona Press), as well as many other science publications and planetary geologic maps. Don helped train Apollo astronauts and served as a professional expert on news broadcasts in Germany during the Apollo 11 Moon landing. He is an elected fellow of American Association for the Advancement of Science, American Geophysical Union, and Geological Society of America, and was awarded the prestigious Geological Society of America G.K. Gilbert Award in 1988.

The only child of William and Allene Wilhelms, Don was born in Hollywood, California, on July 5, 1930. William was an engineer who kept the electronic equipment at NBC working; Allene was a housewife. When he was about 12 or 13, Don became interested in astronomy, making many visits to Griffith Observatory in Los Angeles. His introduction to geology began with the observatory's interesting displays that included narrated histories of Earth. In Fairfax High School in Los Angeles (1945-1948), he showed an aptitude for science and had his first geology class in Pomona College as an undergraduate student. Pomona had a humanities and liberal arts requirement, and to help fulfill it, he enrolled in a Greek drama class. However, lucky for Astrogeology, he found the drama class so boring that he switched to beginning geology instead. He graduated from Pomona in 1952 with a Bachelor of Arts degree in geology and attended the University of California, Berkeley for one year. Don disliked the professors at Berkeley and gladly accepted being drafted into the Army in 1953. Except for basic training, the Army was a positive experience because he was stationed in Schwetzingen, Germany, near beautiful Heidelberg.

After his 2-year stint in the Army, Wilhelms returned to California in 1955. That winter, not sure what to do, Don took a trip in the West, as far as Zuni, New Mexico, for the Shalako ceremony, then returned home via Route 66. Admiring the colorful, rocky landscape around the continental divide, Don decided to return to academia and pursue an advanced degree in geology. While supporting himself partly by pumping gas for the movie stars at a station in Hollywood, Don attended the University of California, Los Angeles (UCLA) from 1956 to 1960. He took 2 summer jobs during graduate school: one with the Vermont Geological Survey in 1958 mapping high-grade metamorphic rocks in the Green Mountains, and another in 1959 with Richfield Oil Co. describing sedimentary rocks in northern Alaska. He earned Master of Arts degree in geology in 1958 from UCLA, with a thesis topic based on mapping in Plumas County, Sierra Nevada, California. Undecided whether to go on for a Ph.D. degree, he took the plunge because at the time the requirement for two languages was easy for him—he knew German, and scientific French was no problem. In 1960, his knowledge of German also led to the award of a Fulbright scholarship, and Don spent a year at the University of Munich, Germany. He returned to UCLA to finish work on his doctorate degree from 1961 to 1962. His advisor was John Crowell, and the dissertation topic was the structure and stratigraphy of Paleozoic sedimentary rocks and Tertiary volcanic rocks in the Nopah and Resting Springs Ranges, Basin and Range province, California and Nevada. Although he is not proud of his two thesis mapping projects, and he found that he disliked doing field geology, the work made him pay attention to field methods and principles. This concentration led him to successfully apply traditional field-mapping principles to mapping the Moon.

In 1962, the space program was really getting started. A friend told him that someone—Gene Shoemaker—was over at Caltech interviewing people for jobs in lunar geology with the USGS. Don got an interview with Gene. During the interview, Gene and Don discovered mutual

interests in astronomy, Quaternary geology, geologic processes, and non-uniformitarianism, but Don did not receive a follow-up letter. This job, combining his old love of astronomy with his new immersion in geology was the only thing in life that Don really knew for certain that he wanted. He wrote Gene, restating his interest. Gene started the paperwork to hire Don in 1962. When Don was hired by the USGS, he was still writing his dissertation. Friday, November 30, he turned in his dissertation, and drove to Menlo Park to begin work on Monday, December 3rd, 1962. He was awarded his Ph.D. degree from UCLA in 1963.

After 6 months mapping the Moon using the telescope at Lick Observatory, Shoemaker sent Don to Paris to study polarization as a technique for interpreting lunar geology with Audouin Dollfus. Polarization was supposed to show the structure of the lunar surface soil, but proved little except that it correlates with surface albedo (relative brightness). Science aside, the two months in Europe were among the most pleasant in Don's life. The optimism of the Kennedy era had reached France, which was prosperous enough to sustain the good things in life, but was not yet excessively modern. He is still a Francophile.

Don went on to live in Houston for a year to participate in astronaut instruction. He returned to Menlo Park and became deeply involved in the Lunar Orbiter and Apollo Programs, notably landing-site selection. His career evolved to include studies of Mars, Mercury, and Ganymede. He retired from the USGS in 1986. In retirement, he has been occupied with completing maps of Ganymede, studies of the Martian highlands, and writing the book *To a Rocky Moon*. Of his many scientific accomplishments, Don feels the high points of his career were providing the overviews of the history and findings of lunar exploration in *To a Rocky Moon* and *The Geologic History of the Moon*.

Don Wilhelms is a focused, honest man, and he likes both details and the big picture. A confessed "pack rat", he keeps complete files. He is also afflicted with Asperger's Syndrome, a mild form of high-achieving autism. The disorder makes it difficult for him to recognize people or to be socially proficient, which others interpret as having a gruff personality. He has had to memorize and practice social skills, like learning a language. He believes planetary geology is a science that suits introverts and his personality. However, anyone who knows Don realizes that for all his gruff talk, he is really a very sweet, loyal guy who enjoys pleasurable experiences, such as good food, wine, women, and discussions. Don loves the Moon, San Francisco, his rustic Russian River second home, historical books, and things well done.

A really great time is to go to San Francisco and spend an afternoon with Don Wilhelms, talking, eating, drinking, sight-seeing, bar-hopping, and meeting people. Don is a smart, well-informed man, always fun, knows all the best places, knows all the good stories, and can introduce you to the most interesting people.”

Career Highlights for Don Wilhelms include the following:

- Instructed astronauts in lunar and terrestrial geology, 1963-1965, 1968-1972
- Lunar Orbiter target selection, mission operations, screening, 1966-1967
- Member, Site Selection Subcommittee of the Group for Lunar Exploration (Apollo site selection), 1968-1970
- Apollo orbital photographic target selection and screening 1968-1973

- Co-investigator, Mariner 9 television experiment, geology team, 1968-1973
- Elaborated and codified the lunar stratigraphic scheme and methods of stratigraphy-based lunar and planetary geologic mapping first conceived by Eugene Shoemaker; 1970, 1984, 1987, 1990
- Coordinated the USGS program of 1:1M-scale geologic quadrangle mapping, 1965-1972; and authored or co-authored 5 of the 44 published maps
- Coordinated the 1:5M-scale synoptic geologic mapping that covers the whole Moon and was lead author of 3 of the 6 published maps (Wilhelms and McCauley, 1971; Wilhelms and El-Baz, 1977; Wilhelms et al., 1979)
- Devised popular reconstructions of the former appearance of the Moon (Wilhelms and Davis, 1971)
- Geologic mapping and topical studies of Mars, 1972-1989 (Wilhelms and Squyres, 1984)
- Geologic studies of Mercury, 1975-1976
- Geologic mapping of Ganymede, 1981-1993
- Synthesized and summarized current knowledge of lunar geology (Wilhelms, 1984, 1987)
- Wrote book-length history of lunar exploration from a geologist's viewpoint (Wilhelms, 1993).

Don Wilhelms briefly described his early interest in the Moon, and how he came to join the Branch of Astrogeology, in his book "*To a Rocky Moon*" (Wilhelms, 1993, p. 60). [Author's Note: some of the text below was also used where relevant in the main text of this work:]

Wilhelms: "On numerous weekends during my childhood and youth, while my peers were at the beach or wherever, I had haunted the Griffith Observatory above Los Angeles. I discovered both stars and rocks in this beguiling place, but was more inclined to astronomy. Among the many things I remember learning there was that what you see on the Moon depends on how high the Sun is in the sky--the lower the better, down to a point. I even made a (non-geologic) map of the Moon when I was 15 or 16.

Because my first love was astronomy I chose Pomona College in southern California for my undergraduate studies (on the basis of personal advice from astronomer Seth Nicholson, who is often credited as a co-determiner of the Moon's surface temperature, and who added that I would become a social misfit if I went to Caltech as I had been considering). But real-world astronomy was not for me, and I majored in geology at Pomona and the University of California at Berkeley and Los Angeles. Between 1957 and 1962 (with a year's interruption for a rewarding though not very geologic Fulbright Scholarship at the University of Munich), I was at UCLA preparing to deal as geologists usually do with messy oil fields or mines or heat, cold, rattlesnakes, cow pies, and poison oak. During the UCLA grind I visited JPL (the Jet Propulsion Laboratory in Pasadena, CA) and saw Ranger spacecraft being built. Knowing of my interest in such matters and my lack of interest in the oil companies that hired most geologists, another student told me about some guy who was at Caltech interviewing people who might want to work on the Moon. Shoemaker presented an unsurpassable opportunity to combine my childhood interest in astronomy with my adult profession of geology. After later reminding him who I was by means of a letter that included words to the effect "Obviously I'm



your man," I arrived at Menlo Park on 3 December 1962, a month and a half after the missile crisis [in Cuba] and three days after finishing my Ph.D. dissertation.

Fantasy became reality within a week as I took my first turn observing visually with the magnificent 36-inch refracting telescope of Lick Observatory. All astrogeologists were assigned a LAC quadrangle to map geologically, as well as to one or more other projects that more or less matched their interests or talents. Dick Eggleton became our teacher in the methods and facts of lunar geology once he returned to work after a serious automobile accident that had occurred on the weekend I was driving north to Menlo Park. Until the first spaceflights provided better data, all of us were required to observe the Moon on good nights whenever the terminator (boundary between illuminated and dark zones) was in or near our assigned quadrangle. We hoped to capture moments of superior seeing and favorable shadows that would reveal some critical detail for learning a feature's origin or relative age. I relished this telescopic observing more than anything else I did during my career" (Wilhelms, 1993, p. 60-61).

[Author's Note: Don Wilhelms still resides in his beloved San Francisco by the Bay. Because of his pack-rat compulsions to save old records, letters, etc., he [provided this author with many documents which filled in many holes in this history of the early years of the Branch of Astrogeology. For that Don, and your impressive work in directing us all through lunar geologic mapping, the Branch of Astrogeology and future historians of the Apollo Era will be ever grateful.]

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**Howard G. "Howie" Wilshire** (geologist; b. 1926; Ph.D., 1952, University of California, Berkeley);

The following was taken from an interview with Howard Wilshire (Sebastopol, California) by Gerald G. Schaber on 4 February 2002:

Wilshire: "I was born in August 1926 in Shawnee, Oklahoma. I grew up in Tulsa, Oklahoma, and went through high school there (finished in 1944); and then I was drafted into the Army. When I came back from the Army I went to undergraduate school at both Kansas and Oklahoma Universities on the GI Bill. I graduated from the University of Oklahoma in 1952. I then went to graduate school at the University of California a Berkeley, and graduated with a Ph.D. in 1956.

*Schaber "Tell me about your major when you went to school. Was it petrology?"*

Wilshire: "Yes, it was primarily petrology that I focused on, along with structure and geomorphology--and all sorts of things that were of interest to me. The first job I had was lecturer at Sydney University in Australia, and I stayed with that until 1960. Then, I had a research fellowship with the Australian National University from 1960 through 1961. I joined the USGS in 1961.

I came back to the US [from Australia] in 1961, and I had my application in to the USGS--and they were hiring at the time. I didn't have a particular job in mind--but they [the USGS] did--they

had a choice of two. One was at last two years of library research on geochemical sensors in Washington D.C.; and the other was mapping in Kentucky. I chose mapping in Kentucky. I sure didn't want to spend my life in the library. So I was in the Kentucky project until 1963. And then for personal reasons I insisted that they get me out of Kentucky. And at that point Carl Roach had need of people in his solid state investigations in Denver. So that is how I got into Astrogeology.”

*Schaber: "So were you were there with Carl Roach, Gordon Swann-and Joe O'Connor?"*

Wilshire: "O'Connor and Gordon Swann were there before I arrived. This project was designed to develop solid state methods of detecting clandestine nuclear tests. My personal opinion is that it was all a crock! But there were a lot of interesting things to do. And mapping in the Rockies in the Twin Lakes area was a lot of fun, and very informative--and led to publications on petrology that had nothing to do with the project.

I also got down to Mississippi and logged cores in the salt domes that were scheduled for nuclear tests and things like that. So it was geologically rewarding.”

[Author's Note: Howard Wilshire tells a “Shoemaker” story below]:

Wilshire: “An interesting event took place in Denver after I got there [probably 1961-1962]. Carl called us all together one day and said that Gene Shoemaker was coming up for a visit, and he is going to try to talk us into transferring to Flagstaff because he needs critical mass [there]. Carl said he wants everyone's opinion. Well it was unanimously decided that we did not want to move to Flagstaff. So Carl said I just wanted to make sure so I can tell Gene when the subject comes up.

Well, Gene arrived and he gathered us together in a little auditorium and gave an absolutely brilliant review of what was going on in Astrogeology Flagstaff. This was my first meeting with Gene. This was 1965 -maybe earlier. Anyway, at the end of Gene's talk, Carl jumped up and asks when do you want us to move to Flagstaff? Well, that's what happened, and we were all shocked!

I was there sitting in Denver doing this stuff when Gene wanted to move me out to Menlo Park because there was a lot of Annual Report writing and stuff like that. So, I moved out to Menlo in 1967 and I was there in Astrogeology through 1976. So what I was doing there was helping to write these [Astrogeology] Annual Reports--which was a ghastly job--with help from Danny Milton of course.

So, in addition to annual report writing, I did some lunar quads, and I developed what we called the Lunar Analog Studies program. I first developed a project under it, and then Masursky put me in charge of it. I guess there were ten or twelve projects going on that had something remotely to do with the Moon. My little special project was to continue something I had started way back before I joined the Survey--studying upper Mantle samples. This was primarily zenoliths in basalts. I said well, if we have the opportunities to get deep samples on the Moon, then we should know how to go about sampling them. So, that was my little project--a real stretch! I spent a lot of time doing fieldwork.”

*Schaber: "At Sierra Madeira (West Texas) and other places, right?"*

Wilshire: "Yes, that came in that same period of time. What happened with Sierra Madeira was that Gene, along with Carl [Roach], had mapped a pie-shaped wedge along the center of the structure. Gene thought that he had it all figured out, and that it represented what you would find if you exposed the underpinnings of Meteor Crater. But he wanted that finished. So I was assigned the job of finishing it. It was also a very rewarding job. I didn't agree with Gene at all in the long run, and invited him multiple times to come out into the field to look at it. But nevertheless, it was a rewarding study.

That of course got me in touch with Phil Keyes work--he had mapped the area--and he, in a very prescient moment, compared Sierra Maderia with the Vrefefort structure. That's what led Bob Dietz to come out to Sierra Madeira with Shoemaker to look for shatter cones there--and they found them. And later on--out of curiosity, we mapped them and showed that when you restore the beds they [the shatter cones] point in and up in the classic Vredefort style. And it also allowed me to justify a trip to Vredefort in 1972 I believe it. There was an international conference on the Upper Mantle in South Africa; and I attached a trip to Vredefort. I got to collect the pseudo-tachylites and things like that associated with those structures. So it was all in tying-things-together kinds of work.

Let me tell you why I didn't agree with Gene [on the interpretation of his geologic mapping at Sierra Maderia. The only thing that Gene ever published [on the structure] was the *GSA Bulletin* article. The map he had was just one column wide--so it was really a strange map. Anyway, he drew this circle in the structure and said that it was inside the mega-breccia and core, and said that it was equivalent to what was under Meteor Crater. There was one point along that line that I could identify in the field, and that was an oil well site--an old wooden rig that was still up. So I knew exactly where I was at that one point. Then I talked with Carl Roach and Dick Eggleton who had walked around this mega-breccia core--Gene going one way and these other guys going the other way. I talked to them and said, well what did you see there that actually made you draw the line. Well, Carl was very vague about this, and he said well you were in this bedded stuff, and suddenly you were in this stuff where you couldn't find bedding-- and when you did find it, it was going every which way. So that's all I had to go on.

So when I really got into my mapping I discovered that this transition that Carl was talking about was actually a transition in formations; it went from a well-bedded dolomite-sandstone, intermittent, unit to a reef-forming deposit--so you didn't find bedding in it. And that was the boundary, and inside of that boundary there were lenses and so on of highly shocked breccias--but the whole thing was not a mega-breccia core boundary--but a formational boundary. That is what I tried to tell Gene, and he simply wouldn't accept that. You know, once you made up your mind it is hard to change."

*Schaber: "Let's try to put all of these activities in some kind of chronological order if you can."*

Howard: "Okay, I told you that Kentucky was 1961-1963, and then 1963-1966 was the solid state investigations for Carl Roach. The Sierra Madeira work was started in 1970 and went through 1972."

*Schaber: "Okay, somehow you got involved with working with the Apollo samples."*

Wilshire: "Well that happened primarily because of Dale Jackson. Dale and I had been working on zenoliths and mantle plumes together--so he kind of dragged me into it. I did astronaut training work--actually set up a test training areas for Sierra Madeira, Black Hawk Landslide and Lunar Crater, Nevada. That was very interesting. This was for Apollo missions 14 through 17. I had a lot of contact with Jack Schmitt in that process, and we banged heads quite a bit.

At the same time, I joined with Dale on the Apollo Field Geology Team. I wasn't officially on the Geology Team, but I did a heck of a lot of work. I really didn't start until Apollo 14."

*Schaber: "Oh, so we didn't really have anyone except Bob Sutton from the Lunar Geology Team working down there at MSC in Houston actually working with the lunar rocks that early then?"*

Wilshire: "Yeah, that's right. I really didn't work directly with Bob Sutton on lunar rocks. I came into it fairly late because Dale [Jackson] needed some help.

I was a member of the Lunar Sample Preliminary Examination Team (LSPET) and I provided hundreds of the first systematic descriptions of samples brought back. I was on the line describing them as soon as they were dusted off. So I did a lot of hand sample petrologic descriptions of rocks, and that went to the team of people who distributed samples to the sample PIs--so we provided them with knowledge of what the rocks were so they could distribute them appropriately."

*Schaber: "That must have been pretty exciting looking at those [lunar] rocks."*

Wilshire: "It was, and the Apollo 14 stuff was real "garbage rock". I'll never forget the old guy on the sample distribution team from the Smithsonian--he remarked that any PI receiving an Apollo 14 breccia sample ought to be allowed not say anything about it!

Those samples were very intriguing from many aspects, but it was the first time that I was able to focus on the little zap pits-little dust impacts. That turned out to be of greater interest to me later in my career. So I did this countless number of samples; I spent many harrowed hours on the line there. I got to know the technicians very well. We had a very good rapport.

I recall another thing that happened when I was a member of the Science advisory Team-the Geology Tiger Team. This was for Apollo 15 through 17. Our job was to have a more relaxed view of the science accomplishments--and the need after an EVA; and we were responsible for writing a report on the accomplishments on the EVA and what we thought were the science needs immediately after the EVA. That was a lot of fun--but very taxing too. I never believed that I could operate under those circumstances. We had three TVs going on simultaneously give us information from different sources"

*Schaber: "Did you participate on any of the astronaut geologic training trips?"*

Wilshire: "Those trips were all educational. I remember going to the Blackhawk landslide. We went there because there was an interpreted landslide at the Apollo 17 site. When we got there, on the morning when the crew started their EVA's, Gene Cernan started asking questions. Someone said what are the rules about asking questions and getting answers? Gene Cernan said the rules are that we come out smarter than we came in!

I consider the Blackhawk one of the wonders of the geologic world. It's a marvelous place. There is solid rock that looks like it had become liquid.

At Lunar Crater--that was another one (see Fig. 96e in main text). It was a fairly simple training exercise. Our rules to the people running it was that we didn't interfere in any way with where they went or what they did--they were supposed to follow their cuff checklist. If they got off the track, they got off the track.

Anyway, one of the little exercises was to determine the stratigraphic relations of Lunar Crater itself, which is a maar crater that threw out big blocks of stuff and the nearby cinder cone. And you could see where the cinder cone stuff overlapped the stuff that was thrown out of the crater.

So Jack's [Schmitt] comments when he got there was well, the cinders are lying around these older-looking tuffs, but I can't tell whether they are bedrock or blocks thrown out of that crater. So I just noted that. Then when we came back on the debrief, I said well Jack, here we are 100 feet above any potential bedrock and there is that crater over there that threw out big blocks of this stuff; so what do you think? He said, well I can't tell.

I said damn it Jack, we are sending a geologist up there to give us a geological opinion and you're telling us a 90-10 chance is a 50-50 chance. So that made him a little angry. Well we had a couple of interchanges, and I said well Jack, extrapolation is the nature of our art! Then every time afterwards that I saw that crew, Gene Cernan would strike a theatrical pose and say extrapolation is the nature of our art! He even said it when they were in orbit around the Moon."

On this Lunar Crater trip--on a lunch break--Gene Cernan stretched his arms out and said, oh what it is to be one of your country's heroes! I thought ah hah, this describes this guy.

But after the Apollo program, and Jack ran for the U.S. Senate, I was in New Mexico after he was elected--and the billboards were still up. They had a mug shot of Jack on them, and they said try honesty for a change with Schmitt. But the wind had peeled little strips of this off [for a change with Sch...itt], so I took a little shot of it and stuck it in an envelope, and sent it to Jack with a little note saying dear Jack--just to help keep things in perspective. I can see the tight little smile he had after seeing that.

That more or less terminated my career with Astrogeology. I left in 1976, but I actually went on and did some more Astro work cleaning it up."

*Schaber: "Before we get to that, what did you do between the end of Apollo, 1973 and 1976 when you left Astro?"*

Wilshire; "I worked primarily on finishing up zenolith reports; and of course I was donned quite a bit with people writing mission reports. For example, I don't know how many hours I spent on the phone with Ed Wolfe."

*Schaber; "So you weren't doing anything outside of Astro work during that period?"*

Wilshire: "I spent a lot of the analog study work down in the desert; so I couldn't help but observe the effect of human activities in the desert. That's when I got interested in that. And just at that time--1976--Jack what's his name who ran the Environmental Studies-- he asked people to suggest projects that would make use of the approaches developed for lunar studies. During the Apollo 17 mission I was very impressed with the fact that we could sample different strata on a side of Littrow Valley by going up to boulders that had fallen down the hillside. You could tell which layers they came from because they left scars in the soil as they rolled down. And those scars to my eye looked as if they were made yesterday. But they did exposure ages in soil shaded by the boulders and exposed to solar radiation, and those things had been there for something like 35 to 50 million years.

So my suggestion was based on that observation. I said I would like to do some studies of tracks, events that left imprints on the least active surfaces on the earth with the point of trying to understand how long they were going to be there. I had in mind looking at the old wagon trails, etc.

I wanted of course, things that were made at a known time. So that was my project suggestion, and that was approved. I was going to look at the Pony Express trails. They were used for a limited period of time, and then abandoned--but you could still see them."

*Schaber: "Now was this after you left Astrogeology?"*

Wilshire: "Yes, this was actually just at the point that I left Astrogeology. I went into Regional Environmental Geology. Then they changed the name for political reasons. But just at the time that I was gearing up to do the study, the Barstow to Vegas motorcycle race had become a major conflict between major environmental groups and the Bureau of Land Management. So I decided that I would study that because I sure as hell knew it happened, and I could get a baseline on the impact it had. So that's when I got into that stuff.

But I was interested still in those Apollo 17 observations. I wondered exactly how long Man's imprints were going to exist on the lunar surface. I went to Henry Moore of course, and ask him how long one can reasonably expect Neil Armstrong's famous footprints to be visible and recognizable as a footprint. Well Henry did his calculations all based on the flux of dust and impacts and things like that--and he said millions of years. And I have used that extensively.

So that was the Apollo experience which was instrumental in what I did later on human impacts in the deserts.

So I developed a project called Surface Processes Project. It was an umbrella for doing all sorts of environmentally related stuff. I worked on just about everything--mining, grazing, pipelines--you name it and I did it. And that took up the years leading to my retirement. I continued the upper mantle studies, and in 1978 I studied the San Joaquin Valley windstorms in 1977. That was a unique event. I tried to talk to Jack McCauley and Carol Breed into coming out and looking at it but they were too busy to come out with me at that time.

Anyway, I busied myself with that for at least a year, and then I had various mapping projects. Then 1993 to 1994 I led an interdisciplinary team, which included Keith Howard and Dave Miller of the Survey, looking at the critical effect of a proposed low-level radioactive waste dump in Ward Valley (west of Needles about 20 miles). That was very political and the Survey didn't want me doing that at all. It was highly controversial and we just sent a memo to Bruce Babbitt (then the Secretary of the Interior) saying that we have some concerns, and we are your experts in that area--if you want to talk about it--we are available.

About three months later they said tell us your concerns. This was in 1993. So we sent them a two and a half page memo outlining our concerns, and never heard from them again. So I went off that summer of 1993 to Europe for a vacation. I came back and I had a telephone call from a guy I never heard of in an organization I never heard of--an opponent of Ward Valley. He said are you aware that your two and a half page memo resulted in 30-page rebuttal by the company that wants to run the dump. I said its news to me. And he couldn't believe that we were the Survey's experts in that area. They got a response to our concerns and they never got back to us. He said anyway, you're going to hear from Barbara Boxer. So that led to lots of poisonous things including a National Academy NRC review of our concerns. I guess that was the last real thing I did in the Survey; I was involved in that through 1995."

*Schaber: "And resulted in possibly your getting riffed in 1996 along with Keith Howard."*

Wilshire: "Yes, then I got riffed by the Survey. Then in 1996, about 15 months later, I was put back in--without explanation. I spent until the end of 1997 doing various things including wiring reports on waste dumps and stuff like that--and a USGS Bulletin on surface processes of the Earth. That was my swan song. I retired at the end of 1997. I am now writing a book on all of those things I studied."

[Author's Note: Howard Wilshire currently resides in Sebastopol, California.]

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**Edward W. Wolfe** (b. 1936; geologist; Ph.D., a961, Ohio State University)

he following was taken from an interview with Edward W. Wolfe (Prescott, AZ) on 9 April 2001 by Gerald G. Schaber:

Wolfe: "I was born on January 21, 1936 in Brooklyn New York. I grew up on Long Island and went to Wooster College in Ohio where I got a Bachelor of Arts Degree with a major in geology. I graduated from college in 1957 and went to graduate school at Ohio State, where I received a

Ph.D. in geology in 1961. During the last two years of graduate school--while I was working on my dissertation--I lived in Wooster, Ohio, and taught full time after college at Wooster. I joined the USGS in June 1961--got my Doctorate on a Friday and went to work with the USGS on Monday in Paducah, Kentucky. I spend two years there working on what was then--the famed--Kentucky geologic mapping project.

Following that, I was transferred to the Branch of Pacific Regional Geology in Menlo Park, California. I spent five years (1963-1968) there, during which I spent four summers working on geology in far southwestern Washington. I mapped the geology of a 15-minute quadrangle and studied the rocks. I spent one summer mapping in the Klamath Mountains of Northern California with Porter Irwin. I then got involved in detailed mapping of part of the San Andreas Fault [in Northern California], mapping all the details and segments along the fault. I also got involved some with remote sensing--which was a brand new idea back then. I got involved with remote sensing--as a geologist--trying to evaluate some of the remote sensing techniques that were being tried back in those days.

*Schaber: "What remote sensing frequencies or wavelengths?"*

Wolfe: "Visible, radar, thermal infrared, primarily. I guess some near-infrared too. I got quite wrapped up with thermal IR for a time. I spent nights sleeping in the desert and measuring the temperatures of rocks with a radiometer. People used to kid me that I made my living taking temperatures of rocks. I guess I was a rock doctor.

Anyway, sometime along about early 1968 (I would guess) Arnold Brokaw appeared in my office at Menlo Park and invited me to go to Flagstaff. I went to Flagstaff for a visit, and the visit included seeing the San Francisco Volcanic Field.

I had been working in rainforest country in folded, faulted Tertiary marine rocks with intercalated volcanic rocks in a region where it rains over 200 inches a year in western Washington. It was really frustrating; just because the exposures were terrible and the terrain was so overgrown that it was difficult to walk anywhere.

Here was the San Francisco Volcanic Field in Northern Arizona with all these cinder cones and lava flows and things--everything all laid out almost pristine. I thought, man, I'd like to do this! I think that very likely; it was really the San Francisco Volcanic Field and the opportunity to learn something there that really drew me to Flagstaff. I got to Flagstaff in the fall, about September of 1968.

*Schaber; "So, you didn't meet Gene Shoemaker along the way?"*

Wolfe: "No, I didn't meet Gene until after I had been in Flagstaff. I really didn't get well acquainted with Gene until after the Apollo program."



*Schaber: "He wasn't here much!"*

Wolfe: "He was at Caltech much of the time. While, I'm sure I met him early on, I really got well acquainted with him after the Apollo Program was over and I was working in the San Francisco Volcanic Field."

*Schaber; "So what did you originally start doing when you first arrived in Flagstaff."*

Wolfe: "I got there in September 1968 and joined SPE (Branch of Surface Planetary Exploration). There were field exercises going on with the ADF [Apollo Data Facility], so I got involved with some of that. However, I also had the latitude to start mapping in the San Francisco Volcanic Field. So I guess I really took as much time as I could, and went out to do that. I started working out in the vicinity of Merriam Crater (NE of Flagstaff)."

*Schaber: "Well, that makes sense because we needed new astronaut training sites."*

Wolfe: "Right, sure. It was justified in Apollo program terms. I kind of came with an agreement that yes I could work in the San Francisco Field. I probably wouldn't have come to Flagstaff without that agreement. I wasn't particularly drawn to the space program--I wanted to be a real earth geologist. Somewhere in the early years-1968, 1969--Dick Moore showed up as a young student between his Masters and Ph.D. degrees; and he wanted to study volcanic rocks. So I took him on as an assistant, and he started working in the San Francisco Volcanic Field too. He actually turned his work in the eastern part of the Field into his doctoral dissertation.

Well, I got drawn into the Apollo Program. I don't think I had much of anything... I participated with some of the SPE things--which really weren't Apollo-related so much."

*Schaber: "You mean the field tests? They were pre-Apollo."*

Wolfe: "Maybe so--yeah, I guess they were pre-Apollo."

*Schaber: "The ones out in Hopi Buttes, etc."*

Wolfe: "Well, I really didn't go to Hopi Buttes, but there were lots of exercises around the Crater Field and the volcanic features around Flagstaff like around Merriam Crater."

I got involved in astronaut training in Apollo 12. I went on several training trips with the Apollo 12 crew, along with a couple of guys from NASA (MSC/JSC)--Eul Clanton and Bob somebody. I can remember going to Hawaii with them [9-11 August 1969] to plan the Apollo 12 training exercise. I didn't have a very good understanding of why I was there, and what was expected of me. I certainly did the best I could, but I didn't have much of an agenda that came from anybody at the USGS.

Well, I didn't have a great deal of involvement [immediately] after training the AP 12 crew. I must have stayed at work on the San Francisco Field. In fact, I'm sure that happened because during the AP 11 landing, I can remember watching it in my living room with Doug Stoesser [Douglas B.

Stoeser who joined the Branch of Astrogeology in June 1969], who was in Flagstaff working on xenoliths in the San Francisco Field. I must have been actively involved in the SF Field. I really didn't get involved in Apollo until immediately after the AP 14 mission when I got asked to go down to Houston and help do the post-mission analysis and reporting. From about that time on, I was involved full time with Apollo. I didn't do anything else. I was involved--not with AP 14 training--but with 15, 16 and 17 training and mission analysis."

*Schaber: "So, let's see if we can follow that through from where we are now. So you were mapping out in the field during AP 11. After AP 11, you started helping to train the AP 12 crew. Then comes along AP 14; were you involved in that mission?"*

Wolfe: "I wasn't involved with the mission itself for AP 14. I came down to Houston shortly after the mission was over to help with the big job of trying to collate the data and make something out of the science.

Wolfe: "I don't think that I went to Hawaii with the AP 16 crew."

*Schaber: "I'm surprised that you didn't because that was one of the two Apollo missions (AP 16 and 17) that you worked on the most as I recall."*

Wolfe: "Yes, but as AP 16 was coming up, I was already deeply involved in [planning] AP 17. Val Freeman and I prepared the detailed site map, etc. I did a lot for AP 17. I think I was already involved in that during the AP 16 mission."

*Schaber: "We were doing so darn many things at once--we were training two crews at the same time. It was really difficult."*

*Let's take a little aside here. Let me ask you about all the guys that [Arnold] Brokaw brought onboard [Astrogeology]. We had some good ones like Bill Muehlberger--who he put on as the PI of the Apollo Lunar Geology Experiment for AP missions 16 and 17--which I consider perhaps his best decision. Then, we had Gene Boudette, Val Freeman, Phil Schafer, and Joel Pomerene given to the Surface Planetary Geology Branch out of the blue."*

Wolfe: "Yeah and I never really understood just why they showed up."

*Schaber; "Well, I understand it was because USGS management needed to find them other jobs in the USGS Survey because things had folded up, or whatever where they were working at the time."*

Wolfe: "They were at loose ends, yeah. Well, the politics of it I really never understood. In fact I really never understood Al Chidester's agenda either. I have never talked to anybody outside of Flagstaff as to why Arnold Brokaw came to Flagstaff, or what that was really all about.

Well, I came in as an outsider with no idea of what was going on. I guess I should have known that funny things were going on because when I came to visit Flagstaff [before coming to Astro] I met George [Ulrich] and got the idea that I'd probably be working with George--and George told me not to come!" [Author's Note—Now that is interesting!]

He said, you know, if I were you, I wouldn't come here. Well, I wanted to work in the San Francisco Field, and it seemed like an opportunity to do that.

*Schaber: "Why did George say that? What did he say what was his reason?"*

Wolfe: "You know, he never really said."

*Schaber: "Was Brokaw here then?"*

Wolfe: "Oh sure, Brokaw was here because Brokaw was the one who came to Menlo Park and asked me to come to Flagstaff."

*[Schaber here discusses the unfortunate mis-interpretation of a breccia deposit--a smooth facies of the Cayley Formation [ejecta from the Imbrium basin] at the Apollo 16 site]*

*Elston and Boudette, I guess, did the detailed mapping of that site [Apollo 16 site]. Many of us thought that that stuff was not volcanic, including me, and other guys in the Branch of Astrogeologic Studies who had looked at it.*

*Gordon and several others said that--as soon as the crew mentioned the word breccia on the surface-- and said that it wasn't volcanic, and a few other comments, that Elston and Boudette left the Mission Control room—and Houston--just left the mission.*

Wolfe: "I've heard that too. I don't think that they were involved at all after that. You know, they should have realized that they had just built an incredible house of cards.

[Author's Commentary-We must now reflect as to perhaps why the geologic mapping for Apollo 16 left something to be desired, given the fact that some of the "newcomers" from elsewhere in the USGS thrust onto the Branch by Arnold Brokaw--just in time for Apollo 16 [with the notable exception of Apollo 16 Geology Experiment PI, William Muehlberger from the USGS and the University of Texas, Austin) [a great selection by Brokaw], did not actually "want" to come to Astrogeology (or Flagstaff), but were at loose ends or out of a project to fund them at the time.]

*Schaber: "They were mapping noise in the data."*

Wolfe: "And how anyone with any experience in geology could do such a thing--it just defies imagination. They just got off on this incredible fantasy. I guess they must have convinced themselves it was real and that they were going to... (undecipherable)."

*Schaber; "Well, Danny Milton was helping them with the interpretation of that site too [Danny had mapped the larger scale map]. He tells me that we just didn't believe that a facies of the Cayley Formation [Imbrium basin ejecta] could be that fluidized and flat-lying. Well when we saw Venus up close much later, we saw all of these fluidized craters--the melt coming out of the impact craters on Venus--you realize that even on the Moon--a cold body--that there was a lot more fluidized outflow of ejecta from those things than one would suspect. So, Danny kind of defends*

*that and said, well I actually thought that it was [volcanic] too [Danny said that he even mapped it that way on his regional map.]”*

Wolfe: "Well, I guess there were others who were able to step in after the mission."

*Schaber: "Yeah, George and you stayed there and worked."*

Wolfe: "Yeah, I remember being very much involved after the Apollo 16 mission, during the data analysis. There was one of those missions where I was on the backup team--the team when the crew was sleeping. I can't remember which mission. I remember that Dallas Peck was on the Tiger Team--whichever one [mission] that was. I was involved in all or most of the training on Apollo 15. I was on the Tiger Team for the actual mission."

*Schaber: "And on Apollo 17 you were right there in the science operation room at MSC--right?"*

Wolfe: "Yes, I was there during Apollo 17."

*Schaber: "Bill Muehlberger was a good selection for PI of the Geology Experiment Team for Apollo missions 16 and 17 I think."*

Wolfe: "Bill was an excellent selection."

Well this is one of my favorite Muehlberger stories. He and I were flying somewhere in preparation for Apollo 17 and we were in the no-smoking section of the plane. I guess we were in the second to last row of the no-smoking section. Well, you know how big Bill is, and he hated cigarettes. Well, the guy in the seat immediately behind ours lit a cigarette. And Bill stood up--unfolded from the seat-- and powered over to that guy and he said are you going to put out that thing or am I going to put it out for you?"

*Schaber: "I would have put it out immediately!"*

Wolf: "I think the guy did!"

I worked very closely with Bill. We were really good friends. I think by the time that Apollo 17 came along I don't think Bill had as much time as he had had earlier.

I did a lot of busy work for him--went to meetings in Houston and down to the Cape to work with the [Apollo 17] crew when they were down there getting ready for the mission. I was commuting to the Cape once a week for a period of time--to work with Schmitt and Cernan there at the Cape. I would leave Flagstaff about 5:00 AM, fly all day and get there just in time to have dinner with the crew."

Schaber: "Yeah, I did that on Apollo 15."

Wolfe: "We'd have dinner, work for several hours, and I'd go crash in a motel and about 5:00 AM the next morning I head back to Orlando to the airport to return to Flagstaff to get ready for the next session."

Val Freeman and I did the detailed mission map for Apollo 17, and I was really involved in designing the traverses that were done. Then, as I said, I was in the science backroom on Apollo 17; and I wrote the USGS Professional Paper on Apollo 17."

Schaber: "Tell me what you thought of some of the Apollo 15, 16 and 17 crews."

Wolfe: "They were excellent. My feeling is that Dave Scott [Commander of Apollo 15] set the tone for the entire last three missions. I think he could have made it or broken it--and he made it by his enthusiasm for really doing a good job on the science. So, Dave was critical, and I think the other guys followed suite fairly well. I thought Charley Duke and John Young were really nice guys, and nice to work with. I found Gene Cernan to be kind of an egotist."

Schaber: "Did you read his book?"

Wolfe: "No I haven't."

Schaber: "You should read it--The Last Man on the Moon." It's pretty good, but he says some pretty derogatory things about Schmitt at first--until the very end when he says well, I guess there is no one else I had rather gone with. But you are correct, Gene is basically an egotist."

Wolfe: "I thought so. He wasn't really a delightful guy to be around. However, I thought he came around and worked pretty well with Jack Schmitt."

Schaber: "He tried real hard--he said in the book--to just put all of these things aside and.... They originally thought that the scientists were going to kill them up there--you know. But he did say some nice things at the end. He said he [Schmitt] did do better in the jet training than most of them--and to him [Cernan]--that was important!" But it is a very good book."

Wolfe: "Well, I think one of the things that was nice about having Jack Schmitt on Apollo 17, was that when we got down to the real details in those last weeks at the Cape--Jack really made it possible for us to have a lot of in-depth discussions."

Schaber: "Oh, Jack was a great coordinator and go-between for the geoscience communications between the astronauts and the geologists in Flagstaff. There is no doubt about that. In fact, if he hadn't gone to the Moon--but still did that job-- he would have served a tremendous service to NASA. As it is, he did an excellent overall job—as did Gene Cernan."

Wolfe: "One of the things I enjoyed. Remember the big final dress rehearsal we had for the Apollo 17 prime and backup crews out there at Apollo 17 using our Grover LRV simulator out

there at Sunset Crater and the Cinder Lake Crater Field?" [Author's Note: This test was carried out 2-3 November 1972].

Anyway, I designed a traverse in the S.F. Field, and there was supposed to be a problem in it. And I was down in the science backroom in Houston, and they had the whole mission control thing up for this big [field test] in Flagstaff--just as if it was the Moon.

I knew that I was supposed to build a problem into the traverse--so I designed the traverse so that they would end up coming right up against the Bonita Flow--which there was no way they could cross. That was kind of a kick for me."

*Schaber: "So, what did they say?"*

Wolfe: "Well, I don't remember but it was a pretty elaborate traverse you know--with stops and things that they did."

*Schaber: "Yes, I remember; I was out there in the field with the crew during that final exercise for them at Sunset Crater."*

Wolfe: "It was fun for me because I knew it was coming [during the traverse I planned], and I hadn't told anybody. Of course, it wasn't so much a test for the crew as it was for the science backroom team in Houston. The backroom had to come up with an alternative, and come up with it fast. Whatever they did--they worked it out okay, but that was kind of a fun thing for me. I took over the job that Tim Hait had been doing in the backroom--so I knew this was coming all the time."

I think some people we haven't talked about at all in this conversation who really deserve a lot of credit for the success, are the science-astronauts who were involved with the crew in training. They were the mission-scientists--Joe Allen for AP 15, Anthony "Tony" England for AP 16 and Bob Parker on Apollo 17. You know, those guys really did a magnificent, sort of selfless job, because they weren't going to the Moon or anywhere else, and they just threw themselves into their job. I think that they just played a tremendous role in being a go-between between the geologists and the astronaut crew--and they did that throughout the training period--not just during the mission."

*Schaber: "That is a really good thing to bring up; and I am so glad that Joe Allen finally got to go up on a Shuttle flight and retrieve a satellite later on."*

*It was really remarkable the difference between the astronauts. Like Al Shepard didn't really give a damn about geology--and said so on occasion. Gordon said that Al came up to him right before his last Apollo 14 geology briefing at the cape and said you know Gordy by now that geology is not my thing don't you? Gordon said, yes Al, I've got that general impression. But then Al told Gordon, I'll do the best I can for you--but geology is just not my thing. Gordon put his hand on Al's shoulder and said, hey, I am a lousy Aeronautical engineering. Al followed up with--that's good, we know where each other are coming from." Gordon said you bet."*

Wolfe: "Well, that's good --but Shepard just struck me as incredibly arrogant."

*Schaber: "Yeah, he was. But if you weren't a jet jockey, then you weren't anybody--and scientists were just like-- in the way at that point."*

Wolfe: "Well, I think that Dave Scott [Apollo 15 Commander] fixed that to a great degree."

*Schaber: "He really liked geology--he got into it! Now I must say that I give a lot of the credit to Lee Silver who was a major force on AP 15 with regard to astronaut geologic training for us. Dave Scott and Jim Irwin liked Lee a lot as a person and as a geology tutor. So I give him credit--of course along with Gordon Swann, Jim Head and yours truly-- all four of us actively participated in the geologic training and traverse planning for the Apollo 15 mission."*

*So after AP 17, what did you do?"*

Wolfe: "I really went back to work in a big way on the San Francisco Field. The Geothermal Research Program funded the work on the S.F. Field. So George Ulrich, Dick Moore, and I really worked full time on that for several years. I did put together a Professional paper on AP 17 during that period as well. I guess those were the main things that I did."

I did some Wilderness Area mapping down on the south side of the Verde Valley. George had an area nearby on the Flagstaff side of the Verde Valley, and we shared a trailer down there for two or three months. You know-- all the areas that were to be set-aside as Wilderness Areas as so-called Rare II areas--had to be examined. I had an opportunity to really look at the geology pretty carefully."

*Schaber: "Yes, Gordon Weir worked on that, didn't he?"*

Wolfe: "Yeah, he did."

*Schaber: "Was your work on the San Francisco Field originally scheduled to be a published map? George is still mad that it never got published in color--the final map."*

Wolfe: "Well, yes, at some point. I think when I originally started mapping back in 1968-69 there really was a specific plan--just working on the geology. But about 1974, the Geothermal Program took it over. There was a meeting of the Cordilleran Section of the GSA in Flagstaff. We had a field trip in the S.F. Field. At the end of the field trip, Patrick Muffler, who was the leader of the Geothermal Program decided that he would like us to really go to work with the geology on that map."

*Schaber: "Well, I want to tell you--and I told George--that you did a phenomenal job on that map. I especially loved the idea that all of the dates on all of the [many] flows are given on the map. It is a very complicated volcanic field, but when you have the dates on all of the flows, it makes a heck of a lot more sense."*

Wolfe: "Yeah, well sure, that helped make sense, and then we had magnetic polarity on almost all of the flows.

Once we added the magnetic polarizations we were going to publish those maps as colored maps. By that time, we were in Central Regional Geology and Dave Schleicher [who used to work for Astrogeology in Flagstaff] was the leader of the TRU in Denver; then, somewhere along the way, Dave decided that it was just too expensive. I got a letter from him [Schleicher] saying that we would do the taxpayers a great favor if we would just publish them in black and white.

Well, I was in Hawaii by then, and I couldn't mess around with it much longer, and I thought well, lets just get the darn thing done."

*Schaber: "These things always linger on and on. Give me some of the dates when you moved out of Astrogeology into Central Regional-- and then when you went to Hawaii."*

Wolfe: "Well, it was sometime in the mid-to-late 70's when we moved from Astro. I think it may have been whenever the Branch of Surface Planetary Exploration dissolved and was re-integrated back into the Branch of Astrogeology?" [Author's Note: This occurred on 13 October 1974].

So that's when George, Dick Moore, Ivo Lucchitta and I went over to Central Regional Geology I believe."

*Schaber: "However, you all stayed at the Flagstaff Field Center and moved into Building-Three as I recall."*

Wolfe: "Yes; and as long as I was still in Flagstaff, we were in Central Regional Geology. I left Flagstaff in March 1982, and I was still in Central Regional at that time."

*Schaber: "So who invited you to go to work at the Volcano Observatory in Hawaii?"*

Wolfe: "Well, Dick Moore had already gone to Hawaii. Dick had gotten to be friends with Dale Jackson someday; and I think Dale was influential in getting Dick to Hawaii."

*Schaber; "He [Dale] died soon after that."*

Wolfe: "Well, it was quite a while--well, I can't remember. Anyway, I suspect that Dick probably suggested me to the Survey people at HVO (the Hawaiian Volcanic Observatory); so I got a call from the Scientist-in-Charge."

*Schaber: "So your work here in the San Francisco Volcanic Field really helped you lot in getting that job perhaps?"*

Wolfe: "Oh yeah. It was really the work in the S.F. Field that made me of interest to HVO; and I was interested in them. I had earlier been working in these awful field conditions in western Washington."



*Schaber: "Oh--you wanted to see the rocks and not the vegetation, is that it?"*

Wolfe: "Yeah, My first real exposure to them was working in the rain forest in western Washington, and you know--I though they were interesting. But then I came out to Flagstaff and I saw cinder cones and lava flows that you could follow right up to the cone. It was being able to see these things in their pristine state where you could really see how they worked; and that is what really drew me to Flagstaff. But anyway, as a result of my work in the S.F. Volcanic Field, I got invited to go to HVO."

*Schaber: "What was your position there?"*

Wolfe: "Well I went to HVO to become the so-called Staff Geologist. It's a rotating job and people normally go for three years. There's been a whole bunch of people who have been there. Wendell Duffield was there on the staff, actually long before I was. He was gone before I got there.

For the first couple of years I was involved with activities at the observatory itself, and primarily with the current eruption which began in January 1983. I got there in March 1982. We had small eruptions in the summit caldera in 1982, which sort of gave me a little experience--a chance to wet my feet. These were events that lasted less than a day. Then, in January 1983, the current eruption began, and it became my full time occupation for a couple of years. I studied it in tremendous detail--and I did write a Professional paper on my part of that eruption. One of my really close associates in that was Tina Neil. She had had some small involvement with Astro I think. She came looking for a job at a time when I was getting ready to work on the Hawaii Geologic Map--and I had the money and authority to hire somebody. I hired Tina; she actually came on to work on the Hawaii Geologic Map, but we were up t our necks in the eruption, so for her first year or so there we just worked on the eruptions. Then, instead of returning after the eruption, I was asked to lead the group putting together the Geologic Map of the Island of Hawaii. So I worked on that for several years with a group of about 15 or 16 other geologists.

George Ulrich [also from Astrogeology] came to relieve me as the Staff Geologist, probably earlier than would have happened if it hadn't been for the Hawaii map. Prior to that, while I was working on the eruption, I wanted to go back to the Mainland to AGU and visit family, and that sort of thing; and I had asked George to fill in for me--which he did--while the eruption was going on. I think he really enjoyed it and I think the people at the Observatory really liked him.

Anyway, when I needed to get started on the Hawaiian map, we had a meeting at HVO about who we should get to come--and George was a shoe in. We talked about other people, but they knew George, and they already knew that they liked him, so George was the obvious choice. So when George came, which was about mid-1984, I stopped working on the eruption--although I spent about another year working on the data, and writing the Professional Paper.

Then I went to work on the Hawaii Map, and I worked on that the remaining time that I was at HVO. I think it is published as map I-2425."

*Schaber: "So after you left Hawaii, you came back to the Mainland, and then and then what?"*

Wolf: "I went to the Cascades Volcanic Observatory (CVO). I went there in January 1989 specifically to be the Scientists-in-Charge. They guy who had been the Scientists-in-Charge was Norm McCloud, who decided to take an early out--to everybody's dismay. They got Don Swanson to fill in, and Patrick Muffler, who was my Branch Chief by that time, came to HVO. I had really fallen in love with HVO work and I mentioned to Patrick Muffler if I had been ready to leave Hawaii I would have been happy to be considered for that Scientist-in-Charge job [at CVO]. I was completely busy with the Hawaii map. There was no way I could leave Hawaii at that time. But a few weeks later I got a call from Patrick and he said well plan on moving to CVO when you're finished with the Hawaiian map--and that was two or three years down the road. So I knew I was going to CVO."

*Schaber: "So what did you do there?"*

Wolfe: "Well, I spent five years being the leader of the Geologic Division group at the Observatory--a lot of administrative work. I did get to some eruptions, most notably, the Monte Pinatubo. I did get some things done there. I had to get the Hawaii Map finished and published--which I did while I was there; and I completed a Professional Paper on Mauna Kea while I was there.

I was Scientist-in-Charge there [at CVO] for five years, and I started a project on the geology of Mt. St Helens, which I am actually still working on. I spend my summers in the field. So I'm still working on that. I spend about 8 weeks in the summer up there."

*Schaber: "So when did you actually retire?"*

Wolfe: "I retired in December 1998 and moved immediately to Prescott, Arizona.

*Schaber: "Did you always want to move there?"*

Wolfe: "Yeah, we bought a lot here about ten years ago, so yeah, our intention was to move here. I still have three kids in Flagstaff and two down in the Valley."

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**S. "Sherman" Shou Chou Wu** (b. 1927; photogrammetrist; Ph.D., 1976, University of Arizona) came to work for the Branch of Astrogeology on 10 August 1965. He was recruited while attending Syracuse University by Astrogeology's Jim Alderman to help in mapping the Moon, with the promise that the government would send him back to school to finish his Ph.D. if he would join Shoemaker's group of lunar scientists at Flagstaff. In 1968 the U.S. government did send Wu to Ohio State University to continue his education, but after only one year Alderman called him back to Flagstaff to work on their new APC Plotter--right before Apollo 11 in the summer of 1969. He was never able to get back to Ohio State. However, in 1970 Sherman enrolled at the University of Arizona at Tucson, Arizona; where he both went to school and attended to Astrogeology work once a month or so. He completed his Ph.D. at the U. of Arizona in 1976 with a dissertation title "Topographic Mapping of Mars", which was part of his work at

Astrogeology. Sherman's most significant contributions to Astrogeology while he was in Flagstaff centered around his pioneering accomplishments in the topographic mapping of the Moon, Mars, and other planetary bodies based on conventional, as well as creative photogrammetric techniques.

The following is taken from an interview with Sherman S.C. Wu by Gerald G. Schaber on 9 July 2002:

Wu: "I was born in 1927 in a very remote, mountainous area. People there were very poor. There are "three" statements that describe my province: there was only three days a week of sunshine—it was raining all the time; there are no three continuous flat miles—everywhere there are mountains; and nobody had three ounces of silver—very poor. I got out of the mountainous area and went to the Capital of China, NanKing, in 1947. I got into the military college, so I didn't have to pay anything. The government provided everything, including some money for other things also.

But because the Communists took over China, we moved from China to Taiwan in 1949. If I hadn't moved, perhaps I would no longer be here today. So In Taiwan, I graduated in 1951 and even went to the Chinese Air Force Academy to learn how to fly because, since my major was photogrammetry, I thought it would be nice if I could fly for myself so I could go out and take aerial photographs.

After that I taught at a technical high school, and also taught geodesy at the university. During the three years teaching I gave my books away, so most of the technical high school used that book as a technical book.

Then, I had a chance to work with the U.S. Army Corps of Engineers in Taiwan to build a big airport and two missile bases. Then I worked the (unclear) Reservoir, called the Sherman Dam, with about 120 people in my group. After completion of that project, the Chinese government sent me to the United States for one year for investigation (unclear) and construction. I came to the USA in 1963 and just went right away to Syracuse University to get my degree.

Then in 1964 Jim Alderman [from the Astrogeology Branch in Flagstaff, Arizona], came to Syracuse to try to find someone to help map the Moon. Finally, he found me. Actually, at that time I would have had a job with (unclear) in Washington D.C., but I wanted to work for my PhD. That's when we [Jim Alderman and I] made a deal. He let me quit school and he promised to send me back to school for my PhD with the US government paying all expenses. Not only did Jim Alderman sponsor my permanent residence—he sponsored my wife's permanent residence, and even paid for her airline tickets from Taiwan to Flagstaff. So on 10 August 1965 I joined the USGS, Branch of Astrogeology, in Flagstaff, Arizona.

Then in 1968, the US government sent me to Ohio State University to work on my PhD. After only one year Jim Alderman called me back to Flagstaff in 1969 to work on their new APC plotter—right before Apollo 11 landed in July 1969. I was never able to go back to Ohio State. In 1970 I ended up transferring everything from Ohio State to the University of Arizona in Tucson. In the meantime, while I was in school there, I could come to Flagstaff once a month or so, to take care of Astrogeology business. So, I ended up finishing my PhD at the University of Arizona in

1976 with the dissertation title “Topographic Mapping of Mars”—which was part of my Astrogeology work.

So when I returned to Flagstaff, I continued with the planetary mapping starting with Lunar Orbiter, Apollo, and Mars Mariner 9. For Apollo, we prepared topographic maps of all of the landing sites. We made the maps before landing, and following the landings, of course, we received more data to improve these maps.

Hal Masursky was very anxious to make a topographic map of the Moon. Every time Hal went to NASA to talk about the lunar mapping program, he told me there was a group there from the US Army Map Service telling the group what they could do—or were planning on doing. That is when Hal showed up with these maps that our group in Flagstaff had already completed.

Perhaps most significant with regard to our topographic mapping during the Apollo years was the fact that we used virtually all of the coverage—all of the Metric camera coverage from Apollo missions 15 through 17. We compiled a contour map that covered about 30 percent of the Moon [the Apollo orbital belt].”

*Schaber: “Sherman, who else was working with you when you were running the Branch’s photogrammetry Unit?”*

Wu: “In those days the people who worked with me included Frank Shafer—a very good worker, and Ray Jordon. Ray was originally working with the U.S. Defense Mapping Agency in St. Louis, Missouri. He was interested in mapping the Moon, so he transferred from the Army Map Service to join us. So Frank Shafer and Ray Jordan were two very good people we had back then. We also had Gary Nakata and Jim Derrick. In the later period I had a number of people join us like Annie Howington, Patricia “Patty” Garcia. We worked on the Apollo Metric camera’s 30 percent coverage of the Moon. Of course, as you remember, everybody said in those days—those are Sherman’s “angels.” The reason for “Sherman’s angels” was because I had a lot of female college students that worked for me then. The reason was because the USGS had a special program called MPES (Minority Participation in the Earth Sciences)—and females were considered minorities then—even though females eventually made up a majority of my photogrammetry group.

As you know, I only picked up straight A students for that program. Patty Garcia, for example, was number one in her graduation class. Annie Howington was also number one in her graduation class. Her sister Lisa Howington was also number one in her class. There was also Julie Simpson, who is also very smart and worked for me back then [including Leslie Tamparri who now works at JPL]. [Author’s Note: Julie Simpson eventually married Jeff Plescia, a later geologist with the Branch of Astrogeology].”

[Author’s Note: Sherman Wu worked in Flagstaff until 30 September 1994 when he retired from the USGS. He re-located to Las Vegas, Nevada where he currently works for the U.S. Department of Energy, and Lockheed-Martin and the Bechtell Nevada Corporation doing highly classified research in state-of-the-art, digital, photogrammetry and remote sensing.]

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**Karl A. Zeller** (scientific photographer):

The following was taken from an interview with Karl Zeller by Gerald G. Schaber on 17 October 2001:

Zeller: "I started with the Branch of Astrogeology 28 October 1964. I was assigned to work down at the old county building [the Dance Hall or county Annex at old Santa Fe Avenue; now Route 66] where Baskins-Robbins East was located for years.

I originally came to Flagstaff from Phoenix in 1956, and worked for the old Jean and Trox photo store in downtown Flagstaff for five years. Then I went to work for Dye-Oxygen delivering welding and oxygen supplies and that kind of things. Joe Dushek [with the Branch of Astrogeology from July 1964 to January 1967] called me and said, hey, you're not doing photo work any more?" I said no; I am driving a truck out here. Well we need a photographer up here, you'd better come put in an application."

*Schaber: "So Joe Dushek was here at Astrogeology then?"*

Zeller: "Yes, he was here before I was; I think he worked in Administration or something.

Last I heard Joe [Dushek] was in Seattle, Washington with the FBI. I haven't seen or heard from him since. Anyway, I went ahead and put in an application [to the USGS] and Don Elston called me in reference to my application, and hired me on.

I was at Astro [about six months before Ramona Boudreau got there. After Building One was completed on McMillan Mesa in town [late 1964-early 1965] we moved everything out of the county building [on Santa Fe] and into Building-One."

*Schaber: "So who else was there at the photolab when you got there--Jim McCord and who else?"*

Zeller: "Jim McCord came later when they built the Documentation Unit. When I came there was photographer Ed Gand (a Chinese fellow) and Bob Mathis, both who had been who had been photographers with Astrogeology back when they were located at the Astrogeology wing constructed on the Museum of Northern Arizona."

I had the big copy camera for the map stuff--the big work. It was more the photomechanical stuff rather than the standard photolab stuff."

*Schaber: "Hugh Thomas was another early employee with the Branch."*

Zeller: "Yes, he was first working for Don Elston. I think he worked in the field with [surveyor] Jim Crossan as a rodman."

*Schaber: "Who was your immediate supervisor when you first arrived--Don Elston?"*

Zeller: "Well, Elston was kind of second in command in those days [Acting Branch Chief]. Gene Shoemaker was gone at the time on temporary assignment at NASA Headquarters in D.C."

*Schaber: "About that time the Branch was working on getting ready for Ranger and Surveyor--right?"*

Zeller: "Ranger was the first; and then we had all of the Astrogeology Annual Reports. I also ran the offset presses."

*Schaber: "The offset printing press was also down at that county building [the Dance Hall or ANNEX] on old Santa Fe Avenue--right?"*

Zeller: "Yes the printing was done at that building too; we didn't move until we went to Building One. I was also located [at various times] in our building on Fourth Street in East Flagstaff. When I started the color lab, they moved me with my equipment from Building-One to the Fourth Street Building. They had the computers there; the electronics lab, the Surveyors, and Sherman Wu's photogrammetry group in there."

Actually, between Building One and Fourth Street, I went up to the Bank Building and worked at the Documentation Unit as a still photographer. So, from Building-One [on McMillan/Switzer Mesa] I went to the Documentation Unit on the fifth floor of the Arizona Bank Building downtown. From there I went back to Building One--and then to Fourth Street."

*Schaber: "So you ended up back in building-One a second time. What was that all about?"*

Zeller: "They took all of the still photography out of the Bank Building and transferred Jim McCord into Building One. So he called me back and brought my lab back up too. He started off in the Bank Building in the Documentation Unit. Then Gene decided to consolidate the still photography together in Building One and he had McCord take over that section."

*Schaber: "Now Calvin (Cal) Hazelwood was doing some kind of photo work. What was he doing?"*

[Author's Note: Cal Hazelwood was transferred to the Branch of Astrogeology from the Topographic Division at Menlo Park, California and arrived in Flagstaff in August 1965].

Zeller: "He was doing the graphic arts stuff too--the big vacuum frame and the copy camera. He came after I did. Russ Wahmann was the one who brought him over to Flagstaff."

*Schaber: "When did Walt Roeder show up down there?"*

Zeller: "Walt showed up when the original Documentation Unit was closed; they just dropped that whole section."

*Schaber: "Well, the Branch formed the Film Documentation Unit then—right? That is what Walt Roeder was to head up."*

Zeller: "Yes movie film. So then Walt Roeder came to work at the still photography lab up at Building-Two [built on Switzer Mesa in 1968], and Tom Lee [hired by the Branch on 14 August 1964, and later moved to the Survey's EROS Data Center at Sioux Falls, South Dakota] came down with him. He came from the USGS in Menlo Park, California, to Astro."

*Schaber: "Give me your general thoughts and impressions of those early days at the Branch of Astrogeology in Flagstaff."*

Zeller: "Well, it was always my feeling that it was pretty exciting work. We were doing something that nobody else was--you know."

*Schaber: "You got to see the space pictures a lot of times before anyone else."*

Zeller: "Yes, and we did a lot of things in photography that nobody had tried before. When I started at Astrogeology, we had no automatic photographic equipment whatever; everything had to be done manually."

[Author's Note: Karl Zeller, one of the Branch of Astrogeology's most good-natured, hard working, and talented support personnel, stayed on with the Branch of Astrogeology's widely heralded photolab for many years following Project Apollo. Karl saw his Astrogeology photo lab's prestige increase even further in the lunar and planetary science community as a result of the high quality and innovative photo work carried out by he and his associates [Ramona Boudreau, Hugh Thomas, and others] following NASA's numerous unmanned lunar and planetary missions that showed the world its first ever close-up images from every planet and major satellite in the Solar System (with the exception of Pluto and its moon, Charon). Karl Zeller retired in April 1994 after thirty years with the USGS, Branch of Astrogeology. Karl Zeller and his family still reside in Flagstaff.]