Recollections of the Developments of Reflectance Spectroscopy for Solar System Exploration by one Planetary Scientist

by

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This is a short set of recollections of the development of a narrow field of science, and a short story about a man's career. Both are intertwined and embedded in broader activities, and both developed with a great deal of serendipity, as these things do. The field of science is reflectance spectroscopy as applied to the solid planetary surfaces of Solar System Objects, and I am the man. This presentation is limited to exploration of the Solar System and does not include the Earth itself, but that branch too was influenced by the history I am about to present.

If one were picky, one would say that reflectance spectroscopy is really a technique, and not a science. The science is what spectroscopy lets one do; namely, determine, map, and interpret the composition of materials illuminated by light, usually in the UV, visual, and infrared portions of the electromagnetic spectrum. In the case of Solar System Exploration, the illuminator is the Sun and the subjects are primarily the surfaces of solid objects: planets, satellites, asteroids, and comets. The science is in determining what the surface composition and chemistry processes reveals about the formation, evolution, and current state of these objects.

The man – I was born and raised in a small town (Elverson) of 450 people in southeastern Pennsylvania. Elverson was then on the edge of farm country, between Lancaster and Valley Forge, against the southeast side of the Appalachian Mountains and at the head of the Conestoga Valley. It was in Old Order Amish country, a beautiful, almost Old Europe-like country, idyllic for a boy growing up hunting and fishing, cutting corn and hanging tobacco using mules and horses. Now it is a suburb of Philadelphia, which is a chapter in the story of change during my lifetime.

Now there are many, many more people (and things) in the USA and World than there were in the 1940s and 1950s when I was growing up. The boy, now an old man, went from driving mules to plow fields, cutting tobacco and corn, and making hay for Amish farmers, and shooting squirrels and rabbits to augment the WWII - post depression family food supply, to helping fly spacecraft to distant planets working in major world cities. I point this out to illustrate the changes that have occurred in our world due to technology and to the huge increase in the living human population. There is a non-linear effect as a result of population growth and due to the increased wealth per capita, resulting in an increase in the number of things people have and the noise they make. This change is also evident in the science fields.

Consider this. I am 71 years old and was born in early 1939. If I reversed my life span, I would go back to 1868, just after the end of the Civil War. The mass movement of people to the West was just beginning. The first National Park (Yellowstone) was not yet designated. The first transcontinental railroad was under construction. The army was moving to quell the Indian uprisings and kill all of the bison; you get the picture. I would have seen the changes from the Civil War to WWII. Now, I am talking about changes from the Great Depression and WWII to today.

In 1868 there were around 38 million people living in the USA. The population grew to about 131 million in 1939, an increase of about 93 million or 244 %. Today, there are approximately 310 million people in the USA, an increase of about 180 million since 1939, twice the number for the previous period or another 137 %. In my lifetime, I have seen a factor of 4 to 6 increase in population and the subsequent effects of wealth. Add to this the technological developments like communications and transportation since 1939, and you have a dramatic change (a factor of 10?) in how people interact, and the amount of clutter. This change was likely as great between 1868 and 1939. Wealth and technology also increased dramatically during this period. 1939 was at the end of the Great Depression, and living standards were depressed then, giving my lifetime more apparent and dramatic affects from growth. My point being that people, institutions, and professions do not operate today as they did when I was a boy, and the quality of life is very different.

The small town and farm communities do not hold much attraction for an average, red-blooded American boy growing into a young man. While shoveling corn off a truck bed (no mechanical assistance in those days) in a cold rain a year and a half after graduating from high school, it suddenly occurred to me that there must be something better to do, but I did not know what it could be. What I did realize is that I needed new experiences and to change my scenery. I drove to the docks of Philadelphia and found a merchant marine employment office and applied to work onboard cargo ships. While waiting for that to process, I also checked out the military services. In those days, universal military service (the Merchant Marines counted) was mandatory for all males, so I had to do something about this obligation anyway. It was a natural, accepted, and effective way to go out and see the world. This unfortunately has changed and the motivation and mandatory “opportunity” does not exist, and in my view it is to the detriment of a significant proportion of our younger population. I joined the Air Force for a four-year tour of duty, hoping to fly. A month later I was called to work on a ship for the merchant marines, but I was already committed to the USAF and on my way to Lackland AFB near San Antonio, Texas, for basic training.

Six months after I joined the Air Force and after tech school in Biloxi, Mississippi, I was stationed in Salina, Kansas on a SAC (Strategic Air Command) base and had an epiphany: I wanted to be a physicist!

Why? As best I can figure, it was because of my father. My father had always dreamed of being a chemist working for Dupont, but had graduated from high school during the height of the Great Depression. His father had also died and he was alone with his mother. His (and everyone else's) Dupont scholarship was canceled and college was no longer in the cards. Still, my father continued to take me to events and places in Philadelphia (a major trip in those days with WWII still affecting the Country and its gas and tire supplies). Science was the central topic, and a favorite place was the Franklin Institute. I guess osmosis worked.

I then began taking courses at a local college near the Air Force base. This was Marymount College, Salina, Kansas, an all girls' catholic school. There were 498 girls and there was me. I was taken under the wing of a nun professor of chemistry and had a great start at college level instruction. After two years in Kansas, I volunteered for isolated duty in Greenland, that being the only apparent way to "see the world" (one of my major goals in the first place). I then spent a year there taking college courses with resident instructors from the University of Maryland and took advantage of a great program the military service ran in those days.

Near the end of my tour of duty in 1961, I applied to my home state university, Penn State, to enter the physics program and was accepted. In those days, universities accepted almost all former military service men, as they were viewed as hard working, dedicated, clean-cut men who were mostly successful academically. Then, a state resident could typically attend that state's university at very little cost. Fortunately for me Penn State had a great physics department and was very affordable.

Today, that is not true, as most state universities receive very little money from the state and must raise funding from tuition and research grant overhead. So, the concept of a State University is no longer a reality. I was a small-town boy on an historic Land Grant university main campus and feeling the motivation that comes with this experience. There is a new trend toward attending college on-line where there is no interaction with other students and faculty members. This is just taking courses, not really "going to college."

At Penn State, I was admitted with almost two years of course work completed. Most of that course work was in the humanities; so, I was left with primarily physics and math courses to take. This was a grunt. But, I was certainly motivated: I wanted to graduate and quickly. Being four years older and much more traveled than your class mates can lead to social isolation, as there is no common ground. Fortunately for me, there was a master's degree female student named Carol living next door to me, who later became my wife, but that is another story.

Completing the requirements for a BS in physics in a little over two years, including summers, quickly led to my search for a graduate school. Being normally insecure, I applied to 13 graduate schools in physics, and I was accepted by all of them. This was a marvel to me! I selected Caltech without any idea of what Caltech was, and with my faculty advisor suggesting that I would find it too much work. My reasoning was that it was farthest away from Pennsylvania and the in-laws I had just acquired. (Remember the women next door?) Also, I had never been west of Texas and Kansas and the West Coast seemed exotic. (Remember the see-the-world itch?)

Carol and I traveled to California early to attend a special summer workshop Caltech and UCLA had set up to introduce non-geologist physical scientists to planetary science. I had read a notice on the Penn State physic department bulletin board and it sounded interesting. I was ready to get away, so we drove across the Country to Westwood and UCLA for the summer in Carol's old Nash Rambler with all our belongings piled in the back. It must have appeared a little like a scene out of The Grapes of Wrath as we rolled into Beverly Hills while trying to find our Westwood apartment for the summer.

A small group of us, around 15 young students, spent six to eight weeks in discussions with three or so Nobel Prize winners and a number of other accomplished (and egotistical) scientists. This was organized and led by Gordon G. J. F. McDonald of UCLA and Bruce Murray of Caltech, and sponsored by NASA. The purpose was to entice us to study the Solar System. That is, join the then almost non-existent field of Planetary Science. I of course got much more out of this interaction than learning about the Solar System, but it worked and I was seduced into what was then a new push in science. I ended up studying both planetary science with Bruce Murray as my advisor, and in the astrophysics program with Jessie Greenstein as my advisor. Four years later I had a doctoral degree in planetary science and astronomy.

Recall, I went to Caltech in 1964 and science in the United States of America had received a gigantic boost from the WWII effort – the atomic bomb, radar, and much more. The war effort relied more than ever on technology, which requires a basis in science, especially the physical sciences. This produced a new generation of scientists. After The War these new scientists found their way to universities and government laboratories. Additionally, many who served in the military returned from the war and immediately completed their college training, some in science, under the GI Bill. This was a great thing for the USA and its citizens. This new generation of WWII scientists and returning soldiers trained students like me in new and growing departments in many universities, all within an expanding economy. This was a good time to be alive and studying science! Coupled with this new development of science education, the 1950s saw the establishment of Federal Government research funding through the Department of Defense (DARPA) and then through civilian agencies, especially NSF. This was to build on developments during WWII and was being driven by the Cold War, our standoff with the Soviet Union and the threat of nuclear war.

As part of this Cold War and just as I was finishing my year at the Greenland ice cap, about to return to The States and enter Penn State, in May of 1961 President Kennedy declared a new program to send a man to the Moon – The Apollo Program.

This Apollo program was conceived under the Eisenhower administration but became a reality under Kennedy out of fear that the Soviet Union would militarize space.

Remember that the Soviet Union put the first artificial satellite (Sputnik 1) into Earth's Orbit in October 1957. Our attempts failed many times. We and the Soviets were also building and testing large rockets carrying nuclear bombs – ICBMs. This was a scary time, the threat of nuclear annihilation was real and a lot of people believed it. In the Air Force, I was stationed in Greenland as part of the DEW Line effort – The Defense Early Warning system: an array of large over-the-horizon radar installations to detect incoming intercontinental ballistic missiles from the Soviet Union. Before I went to Greenland, I was stationed in Salina, Kansas at a SAC (Strategic Air Command) base, from which B-52 intercontinental bombers were flown every day sometimes to hover near the USSR with nuclear bombs on board and waiting for the command to charge forward to annihilate a portion of the enemy if they tried to attack us. I worked in close contact with two major strategic systems that were very active at that time, and the purpose of which I was deeply aware through my training and experience. About a year after I entered Penn State, and only a little more than a year after I was discharged from the Air Force, the Cuban missile Crisis occurred in October 1962. The Soviet Union was shipping missiles to station in Cuba and threaten the USA. I was put on 24-hour alert to appear at Dover Air Force Base in Delaware for active duty. Even after serving a tour of duty, one was required to serve an additional period during which one was subject to immediate recall. This period was war-like in nature and there was a serious threat of vast destruction. This threat motivated our technological and scientific efforts as well, and I was becoming a part of that.

At Caltech, I entered an environment filled with world-class scientists, but Caltech was a small school and there was a feeling of informality. I was able to interact with these scientists and experience how this elite group operated and how their research was accomplished, reported, and fought over. I continued to study physics (as well as geology and astronomy) at Caltech and I was impressed when I with only 10 to 15 other students took a series of solid state physics course from none other than Rudolf Mossbauer, the Nobel laureate from only a year or two before (1961) and the discoverer of the Mossbauer effect in 1957. This was definitely a social as well as technical education. It was also my first experience developing friendships lasting the rest of my life. (Alex Goetz is one of these people, as he became a graduate student there just before I did; we were essentially classmates.)

The Planetary Science program at Caltech, due to the vision of the then Division Chair, Bob Sharp, was just beginning and those of us entering it were to become leaders of the next generation. There were only a very few scientists studying the planets then and the new NASA Apollo Program was having trouble finding a sufficient number of planetary scientists to support the science side of the Moon Landing effort. Thus, NASA began a number of educational support programs, as well as basic research programs to develop the needed scientists and their knowledge basis. Caltech was one of the locations for these activities.

Another important Caltech association was the Jet Propulsion Laboratory, established in the 1930s and managed by Caltech around the rocket research of Theodore von Karman, then head of Caltech's Guggenheim Aeronautical laboratory. JPL developed the flight and ground systems to fly the first successful U.S. space mission, Explorer 1, in January 31, 1958 (a month before I entered the Air Force). When NASA was created near the end of 1958 as a new civilian space agency, JPL was transferred from army to NASA jurisdiction. Soon JPL became the lead center for developing, managing and operating deep space robotic spacecraft for exploration of the Solar System. This robotic deep space program was just beginning when I attended Caltech.

My first science activity was to study the orbits of "retrograde" satellites, those orbiting clockwise, as seen from the north pole, instead of the usual counterclockwise (prograde). I became interested in this during my summer session at UCLA and drafted my first paper there as my class report, submitting it for publication during my first Caltech year. It was published in 1966 and Caltech managed to get the NY Times to do an article on these unusual satellites. The media attraction was that these satellites tended to crash into the body they orbited, due to tidal forces, and thus there are few of them left. This interaction with the media was quite an education in itself. I learned the meaning of the term "journalistic license."

After another paper on this subject, I felt that I could not sit still long enough to do much theoretical work. Or maybe having to stay up all night submitting and resubmitting my computer program card deck to the campus computing center had something to do with it? Computers were just beginning to become a central tool in numerical calculations, like orbital dynamics, and they were slow, cumbersome, prone to breakdown, and difficult to access. Working through the wee hours of the night gave one better access and quicker turnaround.

I asked my advisor, Bruce Murray, for a hands on project. He assigned me the challenge of investigating whether the Moon glowed. That is, does the phenomenon of luminescence exist for the lunar surface material? Some astronomers had reported observations of glowing at times and at places on the Moon. We soon obtained some observation time on the 60-inch telescope on historic Mt. Wilson. I found myself on my first night at a telescope, sitting in the cassagrain observer's chair high off the floor of the telescope dome, staring through the eye piece of a spectrometer (the Bev Oke Scanner, it was called, after the Caltech Professor in astronomy that built it), trying to find my way around the Moon and learn which buttons did what things to move the telescope and operate the spectrometer. Bruce spent about an hour with me, and then left the mountain. He had done his job and now I was on my own. This was scary and exciting. After several sessions at the telescope I was unable to detect any luminescence and published a paper describing the null result. Not exactly Nobel Prize results, but it did teach me how to use a telescope and a spectrometer, and also I learned the Moon's geography. I had become an observationalist.

The next problem Bruce gave me was to demonstrate whether there were any differences in color from place to place on the Moon. Now, this question should give you some indication of how primitive was our understanding of Solar system objects. There had been some earlier work comparing the reflectance of sunlight at different parts of the visual spectrum with terrestrial materials, mostly by Russian scientists, and we studied their results first by having their articles translated into English. There was no Internet or Google in those days. Most of the early results suggested that the way the Moon reflected sunlight across the spectrum was consistent with basalt, the dark, heavy rock that comes out of mid ocean volcanoes, such as in Hawaii. Another controversy in those days suggested that we were just beginning the modern exploration of the Solar System. This question was: Were the craters on the Moon the result of volcanoes or from impact by meteoroids. The basalt interpretation suggested they were due to volcanoes, but most people studying the craters, like Gene Shoemaker then part-time at Caltech, felt certain they were due to impact.

After some preliminary observations, I felt that there were enough color differences to warrant serious observational study, but that we needed a new instrument more suited than the Oke scanner I had been using for this study.

In those days, and at least at Caltech, one was expected to become proficient at many different jobs—to be well rounded. Thus, we physicists and chemists continued to study physics, chemistry and astronomy, but had also to study geology and mineralogy, as well as take a field course in geology. We were housed in the Division of Geological Sciences after all.

This was not bad for me. The mineralogy was not far from my native solid state physics and the field geology was fun, hiking about and looking at colored rocks, trying to figure out how they got there. This was also fun for my wife, Carol. It turns out, I have what is called "poor color perception." I am partially colorblind. So, the colored rocks are not so colored to me, and this is a serious handicap when trying to map rocks. Bob Sharp, one of my Caltech mentors, demonstrated again Caltech's flexibility by allowing Carol to come with us on these mapping expeditions and be my color-eye. Her geology skills and knowledge were nil, close to mine, so she could not couch me into a better grade. Grades in this course were not so competitive anyway, and everyone seemed pleased to have a pretty girl along.

This requirement for well-roundedness extended to instrumentation, and I soon began designing and machining my own spectrophotometer in the Division basement machine shop, under the instruction of a master machinist, Sol Giles. Sol was also a character and perfectionist—new experience number umpety ump since leaving Penn State. I built and used several instruments, including one made from magnesium to reduce weight, a Sol Giles idea. Magnesium is also dangerous to machine in that it can catch fire, burning very hot, and it makes great fireworks. Imagine today a grad student machining magnesium in the basement of a building on campus. There must now be 10 government agencies formed to police this dangerous practice. Nonetheless, Caltech and I survived.

During this period, the exploration of the Solar System using robotic spacecraft was developing at JPL. The first effort was focused on the Moon during the 1960s, with the Ranger and Surveyor mission, to support the Apollo Program. The effort soon included deep space missions, with Mariner 4 launched toward Mars in 1964, the year I entered Caltech. Mariners 5 launched to Venus in 1967 and Mariner 6 and 7 launched to Mars in 1969. All these were fly-by missions.

My tie with JPL was growing, even though I was then using only telescope observations and not working on space missions. My advisor, Bruce Murray, had become very active in the Mariner series, which mostly carried cameras and space plasma instruments. He led, or was heavily involved, in the camera efforts. Secondly, I became acquainted with a JPL scientist, John Adams, who arrived the year before I did. He was working on the laboratory and petrology side of how the color of rocks was related to their composition and mineralogy. We soon became colleagues, he served on my Ph.D. dissertation committee, and we have worked together ever since, especially concerning Moon science. My association with JPL would continue and expand even to this present day.

At this point, we had just begun the modern era of reflectance spectroscopy for exploring the Solar System. I should briefly explain that chromatic decomposition of light had been known for a long time, if only through the rainbow. In the second half of the 17th century, Isaac Newton named "spectrum" the colored figure obtained by scattering sunlight through a prism. Beginning in 1666, Newton demonstrated the fixity of the colors thus formed, and synthesized white light by mixing these colors. In 1800 Herschel discovered that light extended into the infrared. In 1802, William Wollaston fitted the entrance of his spectroscope with a fine slit to improve resolution and discovered the presence of fixed black lines within the solar spectrum. In 1814, Joseph von Fraunhofer invented the diffraction grating (transmission). After fitting this onto a theodolite, he resumed Wollaston's work and marked the relative positions of several hundreds of black lines. He was, however, unable to provide a satisfactory explanation for their presence. Still, the field of spectroscopy for studying extra-terrestrial objects had begun. In 1859, Gustav Kirchhoff and Robert Bunsen demonstrated the reversibility of emission lines: "Within the spectrum, an element absorbs the light at the exact location of the lines which it can emit." They enunciated the basic law of elementary spectrometry, which states: "Each element has specific properties as regards the light it emits." They explained Fraunhofer's black lines as being caused by the absorption of solar light by metal vapors present in the colder layers surrounding the sun. They even identified the element responsible for some of these black lines. This work paved the way for atomic spectrochemistry and announced the advent of modern physics.

The last half of the 19th century and into the 20^{th} century saw a rapid development in the use of spectroscopy for studying stars and stellar systems. Scientists used increasingly large diffraction gratings and telescopes with photographic detectors to apply the technique. Sometimes, these studies were directed at Solar System objects, mostly large bright objects like Jupiter, but also to some solid objects. It is important to note that all of this early work was restricted to the visual wavelengths, where the photographic process and the human eye were sensitive. I recall reviewing the literature before starting my dissertation work concerning the Moon's color and being made aware of some Russian language literature comparing the reflectance of the Moon to basalt, speculating that the moon had experienced volcanic activity. Clearly, the concept that cold solids and rock-forming minerals (as well as gases and hot stars) had distinctive spectral features had emerged early. It turned out that the visual wavelength range was too restrictive.

The modern era of applying spectroscopic techniques to the study of solid solar system objects began with the development of solid state electronic detectors during WWII. These detectors were better than the photograph in several ways, but one is that they are sensitive to infrared radiation. They began to slither out of the classified world as I was entering graduate school in the early and mid 60s. One professor, with whom I worked, Gerry Neugebauer, was being given blackboxes containing such detectors to use at telescopes but was not allowed to open them. This effort expanded rapidly and soon we could buy the detectors and build them into our own spectrometers.

Spectroscopy for studying the composition of extra-terrestrial objects had been in development for more than a century before this “modern” era began and I came onto the scene. We were building on fundamental developments in the field of physics and then astronomy. We were even considered astronomers in those days, belonging to organizations such as the American Astronomical Society, although we were not truly noticed or respected by “real” astronomers. We were beginning to practice geochemistry and geology using remote sensing, as it is referred to today.

I finished my dissertation at the end of the year 1967 and stayed on at Caltech as a Postdoc until the end of the academic year in June 1968, as degrees were awarded only once per year. During that period, I was approached by Frank Press, a former Caltech seismologist who had left to head the recently renamed MIT Department of Earth and Planetary Sciences, about joining the new faculty he was building in planetary science. I had several other offers but the allure of MIT and Boston won. In June 1968, Carol and I ping-ponged back across the country and arrived to housesit for Frank and Billy Press while they were away and while we looked for long-term housing.

I could hardly wait to get started and tried to go into my new MIT office the next day, a mid-June Saturday. I found nobody around! This was very strange for me because at Caltech all of us worked every day, both day and night. It appeared MIT was a bit more relaxed. It was also much larger and set in an urban environment. And Cambridge, in the late 60s, was Berkley East -- a hot bed of a society revising itself. But again, that is another story. It was a very different and new environment for me.

The growth in Solar System exploration, and science in general continued, and I was strongly affected by this. Before I left Caltech for MIT, I was approached by several program managers, particular Bill Brunk and Bob Bryson, from the NASA Solar System Exploration Program about accepting some grant funds to start my research program at MIT. I quickly ended up with two research grants totally something like $180,000 in 1968. That is well over $1 million in today’s buying power!

This illustrates again how much planetary science and the world of science in general has changed. Then, NASA research program managers actually managed and led a program. They developed themes, sought scientists to fund, and followed the research closely. One felt part of a team, and it was very motivating. This is unlike today, when we spend a significant or major percentage of our time writing proposals for grants while most are turned down. This greatly reduces the amount of time available for true research. Research programs at NASA are run primarily by rules that NASA lawyers have written, supposedly to avoid legal suits by disgruntled and unsuccessful proposers who feel discriminated against. Proposal reviews by supposed peers result in number scores, with no rebuttal possible, and funding is handed out by running down the score list until the money runs out. There is an element of randomness in this process, with considerable bias in that the peer review panel members themselves have a basic conflict of interest and often, are young people who are anxious to secure their own funding.

I was off to a roaring start at MIT, buying equipment and hiring technical help to set up an instrument and data handling laboratory to build the next better spectrophotometer after my dissertation efforts, and scheduling time on many of the world's major telescopes. We soon had about 30 people in the MIT Planetary Astronomy Laboratory (MITPAL). There was also some additional funding for equipment from a large DARPA program Frank Press ran. Money was not a real concern. Time, energy and imagination were the real limits.

We soon were measuring the reflectance spectrum of any Solar System object that happened to be up when we were at a telescope and extending our spectral range out to 2.5 µm. We began publishing a whole string of papers, hundreds of them, describing and interpreting our measurements. MIT also decided they wanted an astronomical observatory and found a local donor to pay for it. I was put in charge with my instrument laboratory and we built the George R. Wallace Jr. Astronomical Observatory. To give it a research twist, we tried to make it automated, so that it could be operated from afar. It still is in use. NSF funded us to experiment with the novel concept and some of the lessons learned carried over to my work renovating the Mauna Kea Observatory 88" telescope when I later moved to Hawaii.

We had attracted a number of graduate students. These at MIT included Clark Chapman, Carl Pilcher, Carle Pieters, Roger Clark, Bob Singer, Bob Huguenin, Larry Lebofsky, Mike Gaffey, Lucy McFadden and our first postdoctoral fellow, Torrence Johnson. Later, at the University of Hawaii we added more people such as Bob Brown, Paul Lucey, Diana Blaney, Jim Bell, Karl Hibbitts, and several more postdocs. Some were European students or scientists, including Ralf Jaumann and Tilman Denk.

Some of these people are leading spectroscopy investigations themselves today, and some have produced quite a number of their own graduate students. We now have grand and even great grand graduate students. A family tree has been planted. This is what NASA hoped would happen in the old days when they were building a program. Also, it is clear to me that the development of people is even more satisfying over the long term than the science and technology contributions, which are more ephemeral.

At MIT, a major link in the reflectance spectroscopy chain of development was forged. Several other young scientists were hired by Frank Press in his attempt to rebuild the department. One was Roger Burns. Roger was developing an understanding of the relationships between the spectral features we were seeing in the laboratory samples, and the specific electronic energy transitions in the mineral elements, especially iron in silicates. This was based on crystal field theory and Roger wrote a classic book on this subject. Given my solid state physics background, I resonated (no pun intended) with his work and absorbed it readily. The triangle of efforts to develop reflectance spectroscopy in our own little world was closed: Roger Burns and the physics of electronic spectral feature formation in silicates with his geochemistry background, John Adams and laboratory studies of terrestrial and especially extra terrestrial samples and his geology and geochemistry background, and me with remote observations of extra terrestrial objects and a physics and chemistry background with some engineering and project management thrown in. Of course, we each had expertise and understanding in most areas, but there was a clear emphasis to each of our more specific efforts.

We began our research by applying reflectance spectroscopy of Solar System Objects using ground-base telescopes. This was the only observational tool available then. We built our own instruments, scheduled time on a number of observatories, shipped our equipment, mounted our instruments on the available bolt circle on the back of the telescope, planned our own observation scenario, recorded the data, and flew everything back to MIT to be serviced, and the data calibrated and analyzed. This was an end-to-end operation and we did it all. This is unlike today, when instruments are built mostly by observatories and professional shops. Observing is no longer done from the telescope dome floor but rather on line from afar. This is far easier on the astronomer, but lacks the romance of starring at the night sky out the dome slit and wondering about the mysteries of the universe, sometimes from a remote mountain peak in the Andes mountains or from Mauna Kea in the middle of the tropical Pacific Ocean.

The transition to spacecraft observing was, however, beginning to occur. We immediately became involved in the Apollo Moon Program, but using our telescope observing results to help select candidate landing sites and then, with John Adams, using the returned lunar samples as ground truth calibrators. We were also part of a color camera experiment carried by Apollo to obtain registered color images of lunar regions. The Principal Investigator was an up and coming young scientist named Alex Goetz. Also, I was appointed to the camera team on the Mariner 9 mission to Mars, launched in 1971. Still, these early deep space missions did not carry reflectance spectroscopy instrumentation, partly because we were just then proving that the technique was useful.

The transition from telescopes to spacecraft gathered momentum when, in the mid 1970s, NASA called for instrument proposals to fly on the planned Galileo mission to the Jupiter system. MIT had by then earned a good track record for developing and operating spacecraft carrying x-ray instrumentation. This was managed out of the Center for Space Research, a cross-organizational center at MIT, of which I was a part. It was developed around the x-ray research of Bruno Rossi, a famous Italian experimental physicist who came to MIT right after WWII (one of the earlier generation that I had mentioned). We also worked with several scientists (e.g., Bernie Burke, who discovered the radio emission from Jupiter) who had been active in the Radiation Laboratory (RadLab to us), the MIT laboratory where radar was developed during WWII.

I later learned that our MIT efforts to develop a proposal for an imaging spectrometer to the NASA Galileo mission caused some concern at JPL. A few weeks before the proposals were due at NASA and we at MIT were finishing our proposal, JPL apparently saw the risk we posed to their dominance in producing all the cameras for deep space flight. They realized that imaging reflectance spectrometers might become as common as cameras were then. Thus, JPL quickly developed a proposal and submitted it. In parallel, we suspect, JPL let NASA know that JPL would win this “competition,” thank you very much. This worked for them, and I was told by NASA to join the JPL team, who would build the instrument and provide the experiment Principal Investigator. The PI at JPL turned out to be Torrence Johnson, my first post doc at MIT a few years before and also a graduate of Caltech planetary science program a year after me. This was becoming incestuous, but one of my points is that our world was much smaller in those days. The first imaging reflectance spectrometer was selected to fly on the Galileo Mission to the Jupiter system in 1976. This was after the technique was first demonstrated for Solar System science on the Moon and asteroids. I was not the PI, but you take your victories where you can, and I don't recall being disappointed.

The transition would progress slowly and my research group and I would be very active with telescopes for several more decades. In fact, one of the main reasons I resigned my tenured professorship at MIT and joined the Institute for Astronomy at the University of Hawaii was to be closer to, and have better access to, one of the best telescope sites in the world. In that venue, we would grow our research group to around 60 people and $4 million/per year by the late 1980s. This is nearly $8 million dollars today. We built instruments, bolted them on the telescope, built data handling software and hardware, and published many, many papers. It was fun. I recall arriving back at Honolulu International Airport after a long trip and not being able to drive past the University and my laboratory on my way home without stopping for a few minutes and touching bases.

The slow transition to spacecraft was partly due to delays in the Galileo program, which was due to the Space Shuttle Challenger disaster. What became known as the Galileo Near Infrared Mapping Spectrometer (NIMS) was selected by NASA in 1976, and built by JPL in the early 1980s. However, it did not reach Jupiter until Dec 7 1995, a delay of 20 years. Another often-overlooked reality is that the technology that went into our NIMS efforts was frozen in 1976, or even a bit earlier. In 1995 we could have done much better, and we were informed of that many times, sometimes as if we were personally responsible for being so backward. That is the nature of long duration deep space missions in a period of technology renaissance.

NIMS was very productive in producing new compositional knowledge, especially about the Galilean Satellites. It was the first, but nearly every spacecraft mission to other Solar system objects after that carried, or is planning to carry, a visual-IR reflectance spectrometer. A number of these spacecraft spectrometer investigations are lead by alumni of MITPAL and our University of Hawaii group, called the Planetary Geology Division (of the Hawaii Institute of Geophysics), or PGD. The technique and the applications follow closely what we did with telescopes 40 years ago. But, the implementing technologies are vastly improved. Gratings, detectors, aspheric optics, cooling, electronics, etc., are very much better today, or did not exist when we started. One of the joys of living and working a long time in a field is experiencing this development and remembering when we "did it with sticks and stones."

Note also that our European colleagues are building and flying such experiments on most of their Solar system missions as well, and in close cooperation with US scientists. The technique is now applied in the European Solar system Exploration Program, and spectrometers are carried on most every one of their spacecraft missions to a solid body. The Italians have assumed the lead in Europe in building these moderate spectral resolution imaging spectrometers. I and other USA scientists are investigators on the Europe-led missions, and the European scientists participate in nearly all NASA missions. It is now truly an international science, with other, non-European countries beginning to participate.

As the measurement platforms were changing from telescope to spacecraft, the spectrometers themselves were evolving with technology developments. We started with individual absorption filters and then passband interference filters in front of photomultiplier tubes to obtain reflectance in 10 or so spectral bands (one by one) in the extended visual range for one pixel. Even this was an improvement on the early dispersion spectrometers with photographic detectors that were designed for stellar spectroscopy. The Solar System reflectance spectrometers evolved to now covering the entire spectral range where reflected sunlight is useful, using two-dimensional solid state electronic detectors and with enough spectral resolution to resolve most of the solid state electronic and molecular absorption bands for Solar System materials in this range. And, this is done simultaneously over the entire spectral range and also for a line of hundreds of pixels. Scanning this line of pixels can produce an image at close to 1000 spectral bands! Then, data rate and volume becomes the major technical problem.

Since soon after arriving in Hawaii, Carol and I started developing a small business of our own. It ended up emphasizing the support of the national defense-intelligence effort in providing software and instrumentation related to spectroscopy and remote sensing. This kept us busier than ever, perhaps more than we wanted at times. Still, this work was not as intellectually exciting as the initial Solar System Exploration effort and the development of this new technique. After 15 years, an opportunity came to sell the company with enough return to be satisfying, and we did. I moved back full-time to the University, after having taken a half-year leave of absence during a three-year period as I helped to build the company. This was in the mid 1990s. Carol was appointed as the director of the newly developing Maui High Performance Computing Center and worked on Maui during the week for several years. I returned full-time to science and technology development just as the Galileo mission to Jupiter and our NIMS began producing data. This was a good period. My then smaller group and I concentrated on analyzing and interpreting the Galilean Satellite data and it was more like the good old days.

I was, of course, growing older. The six or so flights to Europe each year from Hawaii were becoming less exciting, especially with the decline in the airline industry. Also, NASA’s funding was declining and the concept of Centers of Excellence within the NASA Solar System Exploration Program was abandoned. Our group and many other groups were forced to decentralize and become more of a random collection of under-funded and less well coordinated scientists. (My group for example lost half its funding base in this process.) The overall NASA Solar Exploration Program had become much less exciting. Adding stress to an already stressful situation was the general decline in state funding for state universities. Over about five years, this process resulted in approximately a 40% decrease in the budget of my home school (SOEST) within the University of Hawaii. Our experience was not unique or even unusual in the United States. This produced low morale and declining working standards.

I was now over 60 years old and had been in a university setting for over 35 years. It seemed like a change in my life was needed. Although it took a couple of years to prepare, at the end of 2001 I formally retired from my tenured professorship at the University of Hawaii and became Emeritus Professor. Carol had ended her work with the Maui HPCC and we were free!

We sold the house, got rid of most all our belongings, our beloved dog, Dandy, passed away, and we left Hawaii to live, at least temporarily, in our “second” home on the east slopes of the North Cascade Mountains near a small town in Washington State called Winthrop. Almost immediately we left to live for four months in Nantes France, where I was hosted at the University of Nantes by professor Christophe Sotin. We rented a small garret apartment in Nantes near a famous outdoor market and had a great time living more like graduate students. This was the first of four years of living in Europe for three to four months each spring, first France and then Italy, working more closely with some great European planetary science groups, including Sotin's in Nantes and Angioletta Coradini's in Rome, with considerable time also with Gerhard Neukum's laboratory in Berlin.

At the same time, it was clear that I did not want to retire from the Solar System science and the space mission work, so I set up a research center in an existing farm house on one of the properties we had purchased next to the property on which our second home was located. It was sort of, "If you build it, will they come?" The answer was, "Yes!" Within several years, we were funded to work with five different space missions, as well as reflectance spectroscopy laboratory investigations, and had several young scientists working with us. This was again an indication that the technique had matured. We are still in this mode as I write this. I am not sure how much longer this will be possible, as each mission has a smaller and smaller budget for participating scientists and basic research grants are more and more difficult to obtain, especially for an old guy. An optimistic point is that we are involved in at least three future mission planning efforts involving reflectance spectrometer investigations, and there is a good chance some of them will be selected. An even more satisfying experience is that former graduate students of mine lead most of these investigations, and they are still willing to work with me.

To bring us to the current time, I will mention one very recent and notable event: The India-led Chandrayaan-1 mission to orbit the Moon. We finally returned to the Moon in 2009 with a reflectance spectrometer to pick up on the earlier work that was accomplished by ground-based telescopes during the 1960s and 70s. Carle Pieters is the PI of the M^3 investigation (Moon Mineralogical Mapper), built by JPL, and she is scheduled to tell you of the M^3 Moon story here at this symposium. This mission is very successful in producing new knowledge, including the discovery of water on the lunar surface. Perhaps even more importantly, it is an example of a cycle in history.

Carle was one of the early graduate students during our beginning days of Moon work at MIT, and she never lost her fascination for the Moon. She has been trying to get a reflectance spectrometer to the Moon for over 30 years. We had to send spectrometers to the Jupiter and Saturn systems, Mars, Mercury, several asteroids and comets and have a spacecraft well on its way to Ceres and Vesta, before we could return to the Moon. Still, it is because of Carle's efforts and tenacity that we achieved the M^3 investigation. Her effort was based on our earlier work at MIT, learning how to apply reflectance spectroscopy to the study of the composition of extra terrestrial objects in our Solar System. As an aside, I must say that I deeply appreciate Carle including me in her team to accomplish M^3 and I look forward to her talk here.

I could continue with descriptions of other space missions and the accomplishments of other former students, but I think the story told so far gives enough of the flavor of the three themes I tried to carry here: 1. The development of reflectance spectroscopy for study of Solar System objects, 2. My life story as a planetary scientist, and 3. The dramatic changes in society during this period.

Finally, It would be quite normal for my recollections to be a bit different from those of some others who traveled a similar path, memory being what it is. I apologize for that. Let me know your vision and I will try to reconcile it with my remembrances. I also apologize to those who should be mentioned here but are missing due to space and time limitations. One obvious omission is mention of several other research groups that were active in spectroscopy or related planetary science fields, including the historic group at the University of Arizona. This article is about my personal experience and others could and should tell theirs, as they saw it. I also hope I have not offended or embarrassed anyone by mentioning names and places. These people are important to my life and to the profession, and their roles were important, in our little world. They should feel proud. And, last of all, this article is a first attempt at recoding my experiences. Perhaps I can do a better job next time, if another opportunity arises to do such an article. It is certainly an honor to be able to contribute whatever value this article might have to this special occasion to recognize some accomplishments by a friend and colleague of similar age and experience to mine. Thank you, Alex Goetz.