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# The Memoirs of a Retooled Geoscientist or How I Came to Know and Love Remote Sensing

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or

## How I came to Know and Love Remote Sensing

By

Dr. Nicholas M. Short

# THE MEMOIRS OF A RETOOLED GEOSCIENTIST

by

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Most older professionals now practicing or utilizing the technology that is now generally called “remote sensing” did not start their careers trained in this field. They came in by the “backdoor” or perhaps the “sidedoor”. This contrasts with younger generations of scientists and applications-oriented specialists who were exposed to remote sensing during their collegiate years or soon after they started their careers. Those individuals probably were exposed to one or several courses dealing with remote sensing and GIS - a few may even majored in this now fast-growing line of work. In any event these more recent entries into the professional ranks either moved directly into jobs based on or requiring skills in remote sensing or assimilated the necessary knowledge to apply remote sensing because their educational background gave them a strong foundation.

This self-revealing Memoir is directed to some extent to these youthful practitioners, mainly to give them a sense of the history of the “early” days when their elders helped to lay the ground work in the rapidly developed realm of Remote Sensing in its modern version. I am one of these elders but not by choice - instead, as will be explained later, I literally came into remote sensing as an abrupt change in career quite by accident. My story, which will be unfolded in this online article, is certainly unique ---- it may in fact be typical. Many of my colleagues and peers during my first years have similar tales as to how they entered this field despite having been trained in other fields. They converted to remote sensing as an occupation either by seizing the opportunity or in response to circumstances.

Because my experiences after my conversion tend to parallel what has gone on since remote sensing became “fashionable”, my goal and approach in this article is to recount how this happened and to show that my activities mirror much of what is an integral part of the history of this field as experienced by my peers and colleagues who pioneered this new field.

Just so you, the reader, and I are on the same waveband, I will begin by presenting my own definition (a bit longwinded perhaps) of what remote sensing means to me. Thus, the term remote sensing [*] as I see it is….

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[*] **The term "remote sensing" is itself a relatively new addition to the technical lexicon. It was coined by Ms Evelyn Pruitt in the mid-1950's when she, a geographer/oceanographer, was with the U.S. Office of Naval Research (ONR) outside Washington, D.C.. No specific publication or professional meeting is cited in**

*The measuring of force fields, electromagnetic radiation, or acoustic energy employing cameras, the acquisition and measurement of data/information on some property(ies) of a phenomenon, object, or material by a recording device not in physical, intimate contact with the feature(s) under surveillance; techniques involve amassing knowledge pertinent to environments by radiometers and scanners, lasers, radio frequency receivers, radar systems, sonar, thermal devices, seismographs, magnetometers, gravimeters, scintillometers, and other instruments.*

Boiling this down to a simple, practical working definition, to most remote sensing involves using instruments flown on aircraft or on spacecraft to sense radiation coming from the Earth's surface (land and water) and/or its atmosphere in a systematic way, i.e., one that relates the radiation pattern to a geographic framework, yielding pictures, images, and numerical data sets. But, this leaves out two other domains: remote sensing is the main method, so far, of learning about the planets and the vastness of the Cosmos (the Hubble Space Telescope is, in my opinion, the greatest remote sensing device yet built by mankind).

I think the argument came be made, with conviction, that of the money spent by NASA and other international space programs and now industry, more has been directed towards projects and missions depend on remote sensors to acquire the requisite data than on any other category of expenditure. Thus, geophysical measurements of force fields, meteorology, oceanography, land observations, environmental assessments, management of resources, together with planetary exploration and astronomical observations of stars, galaxies, and the deep regions of cosmic space, make up a partial listing of remote sensing-dependent endeavors that over the years have involved the investment of many tens of billions of $dollars.

With this preamble behind us, now let me turn personal as I recount my 33 years of intimate connection with remote sensing.

A quick biopic: I am a native of St. Louis, Mo. My decision to become a scientist - specifically, a geologist - traces to the age of 12 when I bought a mineral set, and became hooked on rocks ever since. I gained a B.S. from St. Louis University, an M.A. from Washington University, spent a year at Penn State, and received my Ph.D. in 1958 from MIT. All degrees were in Geology, with my specialties being sedimentology and geochemistry. After spending two years in research at Gulf Research and Development near Pittsburgh, I joined the Lawrence Livermore Laboratory in California in 1959, being the first geologist hired there to support the underground nuclear testing program. Soon, I switched at LLL to the Plowshare program for 'peaceful' uses of underground explosions. My main contribution was to study the effects of huge shock pressures from these explosions on the rocks surrounding them. I discovered several intriguing phenomena in these rocks which had never been reported before.

Most of my studies used samples from major nuclear explosions. The most important of these was the Sedan Event -

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**literature consulted by the writer (NMS) in which the words "remote sensing" were stated. Those "in the know" claim that it was used openly by the time of several ONR-sponsored symposia in the late '50s at the University of Michigan.**

100 kilotons - which produced this 1100-ft diameter crater. A wide variety of shocked rocks could be collected as ejecta.

**The Sedan Crater at the Nevada Test Site**

In my last year at the Radiation Lab in Livermore, I conducted some rather "way out" experiments to shock rocks. One involved the firing of a flat-nosed shell from a 16-inch cannon removed from an old battleship:

**The cannon I used to shock a variety of rock samples**

I also "invented" a new method to shock rocks to very high pressures. To accomplish this I placed a sealed metal tool containing rock samples inside a cylinder filled with liquid explosives. When detonated, shock waves moved both outward and inward - the latter causing an implosion that squeezed the samples to pressures exceeding a half megabar.

**The implosion tube method of shocking rocks**

These shock features proved to be the "missing link" between these controlled events and the peculiarities found in rocks associated with known or presumed meteorite (asteroidal) impact craters; at that time about 75 such structures had been located. But, there was a high degree of skepticism among the scientific community in this idea of impact. Most, at first, claimed the craters or their scars (astroblemes) to have a volcanic origin. However, no measurements of high pressures during volcanic eruptions had ever demonstrated significant overpressures, and theory suggested top pressures of only a few 10s of kilobars at most. The impact advocates argued that the pressures generated as the crater was made could be in the 100s of kilobars. In my studies of underground explosions, I could show conclusively that instrumental measurements yielded transient shock wave pressures well in excess of 500 kilobars. The rocks I recovered from nuclear explosions in granite, tuff, basalt, salt, and alluvium that were determined to have experienced these high pressures showed very distinctive shock features. I also developed a method to shock rocks experimentally to similar pressures.

The importance of these observations was realized when I gave a paper at the New York Academy of Science in 1963. Several impact devotees who were in attendance showed slides that containing identical microscopic features to those I had discovered in the nuclear explosion rocks. This provided the first and decisive evidence that the impact features were subjected to very high pressures, unattainable by near surface terrestrial processes such as volcanism but the expected range of values that theoretical models posed for impact. Those of us who now recognized the implications of this dualism named the process *shock metamorphism.* This involvement with the 'impacters' changed my career emphasis drastically.

Another noteworthy achievement was to have had technicians build the first TV cameras designed to go into boreholes; one was 2.6" wide. I can argue with some conviction that this was my first "remote sensing experience.

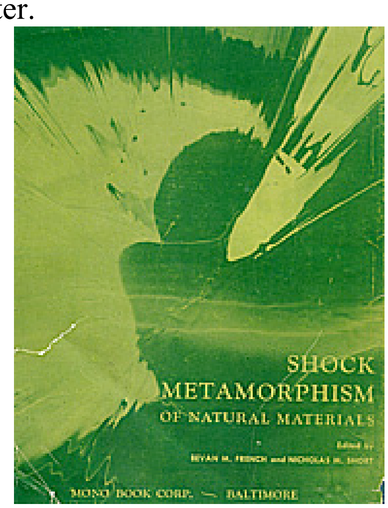
**The Nx core TV camera (in my hands)**

In 1964 I left Livermore for a teaching position at the University of Houston. I was hired as the department's geochemist. But funding for that was hard to come by. So, I applied to NASA for a research grant to study one of the small impact craters, the West Hawk Lake structure in western Ontario, Canada. As a follow-up, I began to concentrate on studying the petrography (appearance of the rock in thin section under the microscope) of rocks from a large number of reputed impact structures. This led to a series of published papers that helped me to join the ranks of those who were swiftly reaching the conclusion that impact cratering is one of the most fundamental of processes affecting the Earth's and other planetary surfaces.

The next shift in career came about in 1966 when a NASA scientist, Dr. Bevan French, and I dreamed up the idea of the "First Conference on Shock Metamorphism of Natural Materials", held at the Goddard Space Flight Center (GSFC). It was hugely successful, and its Proceedings (1968) is still the basic reference for this new field. While at Goddard, I was introduced to the facilities of the Planetary Branch and, awed by the plethora of equipment not available to me at Houston, I readily accepted the invitation to apply for a National Academy of Sciences Research Associateship, which was quickly awarded. So, in 1967 I took a leave of absence and with family journeyed to Greenbelt, MD for an exciting and productive two years.

**Attendees at the "famous 1966 Shock Metamorphism Conference**

While there, I continued my work on shocked rocks using electron microscopy, electron microprobe and other instruments never before available to me. With Dr. French, we spent much spare time in honchoing the Shock Metamorphism Proceedings, my first book. I was fortunate to be chosen as one of the 142 investigator teams to work on the first lunar rock samples returned from Apollo 11 and 12. More later.




**The 1968 "classic": Shock Metamorphism of Natural Materials**

So, where does remote sensing come in. Not quite yet. Of course, I had used remote sensing products in the form of aerial photos for years as I did field work in geology. But, I encountered the notion of "remote sensing", with its implications of more sophisticated instruments than an aerial camera, at a Short Course on Remote Sensing given by Dr. Robert Reeves (then at Colorado School of Mines) at the 1968 annual meeting of the Geological Society of America. I learned a fair amount but never suspected it would soon become my "bread and butter" activity.

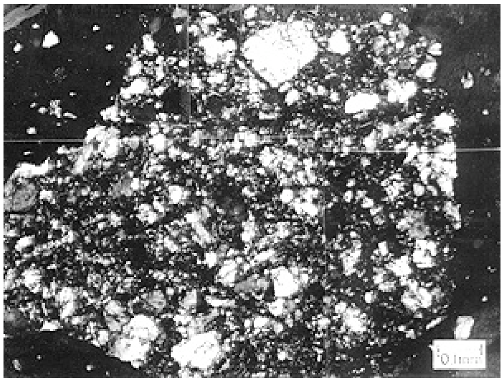
In 1969 I decided I would, for health reasons, not return to the University of Houston. Staying at Goddard did not seem an option, since the Planetology Branch was facing a scaleback in personnel (and did disband three years later). I applied for other teaching positions and received an offer of a Full Professorship at Kent State University, with the commission to upgrade their research programs in the geosciences. But, after accepting, we ran into a severe housing problem (related to the fact that local ordinances prohibited dogs in apartments). My wife made two driving trips to seek a solution. One, though inadequate, was found. Then, two days after her return, as she was dressing for us to celebrate our wedding anniversary, she suddenly screamed and collapsed. At that precise moment, I had no clue that this was to be a seminal moment in my professional life. She had somehow ruptured two spinal disks and was partially paralyzed. She was in traction for a month and the doctors advised that she not be involved in any major move for the year, especially since her condition was limit her care for our 5 year old rambunctious son. I asked for an extension before coming to Kent, but since Ohio public education required the position to be readvertised if not filled on schedule, this excellent opportunity was closed.

So, I faced a super-dilemma. A return to Houston was out, as I had resigned in the Spring. The Planetology Branch had filled my slot. Where was I to work?

Sometime prior to the end of my tenure with the Branch, I had met Dr. William Nordberg, a meteorologist at Goddard, who had been given the lead role as Project Scientist for the forthcoming launch and operation of the first Earth Resources Technology Satellite (ERTS-1). For him, this was also somewhat of a career redirection but his exceptional versatility in land sciences and his leadership made him a superb choice for this position. He had decided that spring that they needed at least one geologist on the Goddard earth sciences team, he knew I was searching for new vistas, so he made me an offer to join their program. But, to me at the time, this seemed to be a radical shift in career goals. Frankly, I didn't think inwardly that I would be much interested in looking a large area "aerial photos", so I rebuffed his offer. And, my knowledge of remote sensing was pretty much limited then to its spelling.

But, with the wife's broken back - my choices were few and "any port in the storm" became my guideline. I accepted, and just stayed at Goddard and in our rented home. I was able to work out a very favorable deal with Bill Nordberg. Since I was a lunar samples investigator, and Apollo 11 had landed and returned successfully, with samples guaranteed, I was urged to finish my studies through Apollo 12, maintaining one office with the Planetology Branch and another in Nordberg's Division. The arrangement demanded only that I attend certain meetings and that I started to learn the rudiments of remote sensing in spare moments.

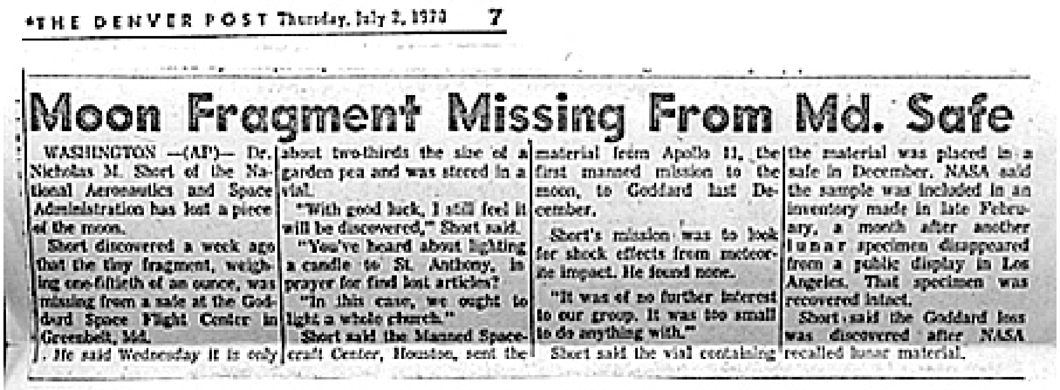
After receiving my Apollo 11 samples, as a one-man team I had to work 10-12 hour days, and Saturdays, to do my prescribed research on pinpointing shock features in the lunar rocks. One night I prepared a mount for an x-ray asterism study of a single crystal of a lunar basalt. When I brought it to the technician's lab for the next day analysis, I laid the vial containing the sample (about the size of an eraser tip on a pencil) on his table, then simply forgot about it. That analysis was a small part of my investigation and I got enough done to prepare a paper for the first Lunar Science Conference in Houston, in January 1970. I had found definitive signs of shock effects mainly in the lunar regolith (sometimes called "lunar soil"). My main contribution: I co-discovered independently what John Wood, an MIT classmate now at the Smithsonian Astrophysical Observatory, had also found: the earlier proposal by A. Turkevich that the lunar highlands seemed to composed of Anorthosite (a feldspar-rich igneous rock) was indeed valid. I stayed in this mode until I finished my studies on the Apollo 12 samples.



The single tiny "instant rock" clast that to me the Highlands was composed primarily of the rock type called Anorthosite

In Spring of 1970, I moved to Nordberg's building and proceeded to immerse myself into remote sensing and ERTS in particular. But, a traumatic debacle intervened.

This came from inventorying my lunar specimens prior to shipping them back to the Lunar Receiving Laboratory at the Manned Spacecraft Center. One small vial was missing - the one mentioned above. I scoured the Planetology Building with no luck. I reported this to the Branch Chief, Dr. Louis Walter. He intensified the search but also drew a blank. We then reported this to Houston, and shortly people from NASA Headquarters visited us, accompanied by two agents from the FBI. This was to be the first sample reported lost. I was grilled but had no clue (I didn't remember that late night). Another thorough check of the facilities failed once more. Two days later, a press conference was held at Headquarters to announce the loss. I was not permitted to attend my own demise. Since Moon Rocks were still then a popular subject, this missing rock made headlines around the world - front page in the New York Times and the Washington Post.



THE DENVER POST Thursday, July 2, 1970 7

Moon Fragment Missing From Md. Safe

WASHINGTON —(AP)— Dr. Nicholas M. Short of the National Aeronautics and Space Administration has lost a piece of the moon.

Short discovered a week ago that the tiny fragment, weighing one-fiftieth of an ounce, was missing from a safe at the Goddard Space Flight Center in Greenbelt, Md.

He said Wednesday it is only about two-thirds the size of a garden pea and was stored in a vial.

"With good luck, I still feel it will be discovered," Short said.

"You've heard about lighting a candle to St. Anthony, in prayer for find lost articles?

"In this case, we ought to light a whole church."

Short said the Manned Spacecraft Center, Houston, sent the material from Apollo 11, the first manned mission to the moon, to Goddard last December.

Short's mission was to look for shock effects from meteorite impact. He found none.

"It was of no further interest to our group. It was too small to do anything with."

Short said the vial containing the material was placed in a safe in December. NASA said the sample was included in an inventory made in late February, a month after another lunar specimen disappeared from a public display in Los Angeles. That specimen was recovered intact.

Short said the Goddard loss was discovered after NASA recalled lunar material.

Mentioned on Network News by Walter Cronkite and Huntley-Brinkley. I then compounded the problem by agreeing to a telephone interview by the Baltimore Sun. As I responded, being rather disgusted and shell-shocked at the moment, I reverted to my old habit of cracking puns. As I drove home that evening, I tuned into WTOP radio to listen to Doug Llewellyn's 5 minute human interest program, only to hear him describe this NASA scientist who lost the Moon - each comment punctuated by one of my puns. This tid-bit was so amusing that it was broadcast nationally.

The sample was found 2 years later when the x-ray technician cleaned out his room. By then no fuss about the prodigal sample's return. Two good things came from all this: 1) several old girlfriends wrote me, having seen the news release, and 2) because we had to delay our vacation trip to Arizona, I was on hand when a fierce storm blew over the willow tree in our backyard, which with a neighbor's help we righted and replanted.

Thus, by spring of 1970 my decks had been cleared and I was now ready to be metamorphosed (without the shock) into a fulltime remote senser. By this time, the advent of ERTS had begun to dominate my activities and that of many colleagues. But, first I needed to master the fundamentals of remote sensing. Fortunately, there were several in our Division quite willing to tutor me.

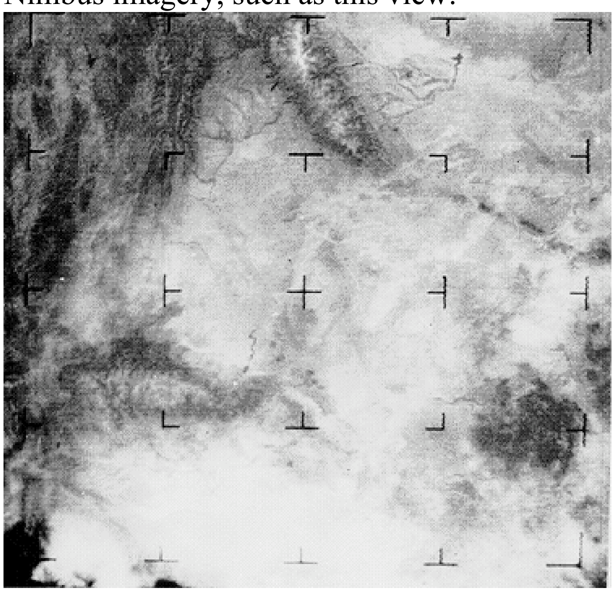
After returning from the western trip, I now was committed to full time in Remote Sensing. I was fortunate in the two people who were my immediate supervisors. My Branch Chief was Dr. Warren Hovis, a spectroscopist, specializing in the Infrared, who had learned his "trade" in the U.S. Army. Warren was a tall and large man who always treated me with respect, being aware that I was coming in "cold" to this new field. He patiently and gradually taught me the "ropes" in aircraft remote sensing and was the one who steered me into Nimbus image analysis. He encouraged me to learn about the reflection spectroscopy of rocks and minerals, using his instrument called the Hovis Sphere (his reputation was built as a sensor designer while in the Air Force earlier) for laboratory spectral analysis and then his field spectrometer (which I would operate out of doors under natural conditions.) The Assistant Division Chief was Mr. Bill Bandeen, also a friendly boss, a meteorologist who added to my knowledge based on Nimbus and TIROS image analysis.

But, the most influential of all, even though I dealt with him much less often, was Dr. William Nordberg. He was an Austrian (who fought in the Second World War on the German side) who came to this country in the late '40s, along with his lovely wife, Trixie. Both by then spoke excellent English with only hints of an accent. Bill joined NASA in the early '60s and rose rapidly because of his expertise in meteorological satellites. He was a dynamic (and somewhat impatient) leader who had the gift of inspiring those under him, or associates, to become fully committed to their particular involvement in space programs. I was often asked to sit in on his weekly staff meetings and witness how he operated. His mind was going full tilt and his demands for results were persistent. Most in the meetings stayed rather docile, with participants being a bit intimidated by him. But for some reason, whenever something came up about which I knew or wanted to do, I became argumentative with him, if I thought my position was firm, rather than just acquiescent to his viewpoint. I think Bill enjoyed the spats we would sometimes have, because he was naturally combative and hoped to have people disagree with him. Several of our sparrings were rather brisk but he never downgraded me if I was firm in my thinking. Despite these clashes, my respect and admiration for Bill Nordberg remained high throughout our times of interaction.

Most of my time at Goddard in the Fall '70 to Spring '71 was committed to learning more about the theory and applications of remote sensing. I worked with Dr. Norman MacLeod (for 3 years, my room mate) on analysis of multispectral imagery. We recognized a need for an optical instrument that would allow individual spectral band images to be superimposed to make a color composite projected on a screen. I designed a basic concept and then got NASA to fund a company, $I^2S$, to build a prototype for our use (they then put the same instrument on the general market).

Norm and I published this simulation study as a NASA paper. Meanwhile, I had to prepare a proposal for being selected as a Principal Investigator when ERTS-1 went up in the Summer of 1972. I decided to make Wyoming geology - of which I knew much from fieldwork there during my Gulf Oil days - the test site area for study. I contacted the Chairman of the Geology Dept, Dr. Robert S. Houston, to ask that he and other staff join me as Co-Investigators. They eagerly became part of my effort.

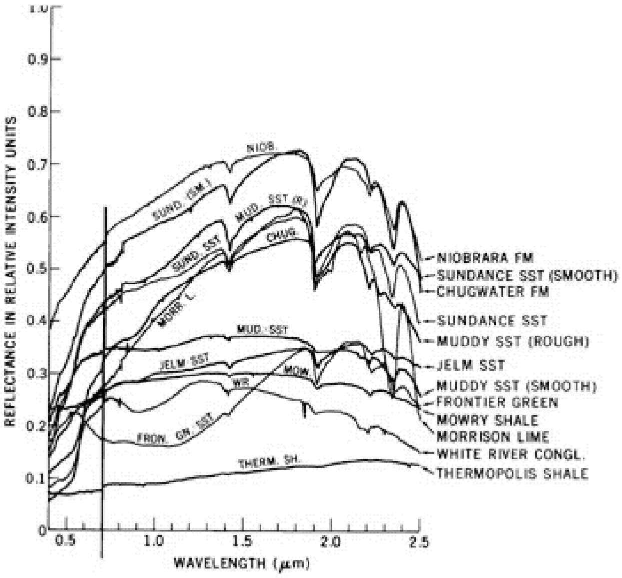
At that time, all I had to work with in preparing the Wyoming project was Nimbus imagery, such as this view:



**Nimbus vidicon image of parts of Wyoming and Utah**

Honestly, while it was neat to see much of Wyoming and surrounding states, I was not impressed or excited by the prospects of doing geology from space because of the poor resolution. I learned almost nothing new and began to wonder what ERTS could provide that would be instructive and novel. In other words, "What had I gotten myself into?" Maybe staying at Goddard wasn't such a good idea; maybe I should look into academe again and get back to my areas of training and interest.

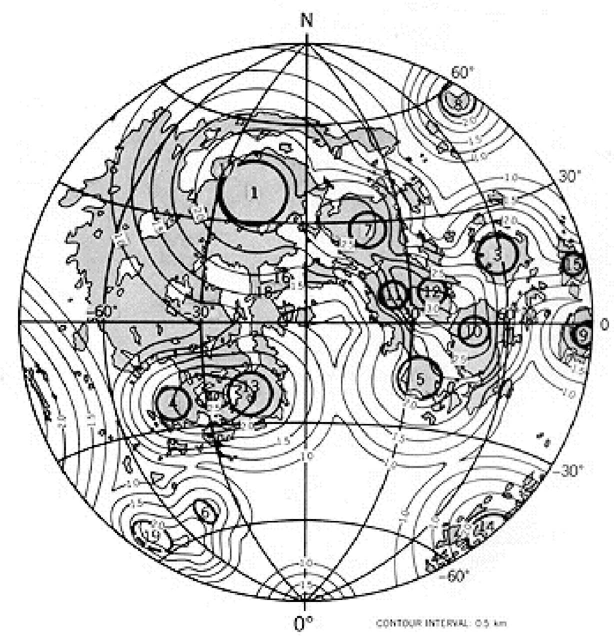
But that would be feasible only in the longer run. For now, I had better prove to my management that I could metamorphose into a bonafide remote sensing specialist. I did have one advantage to build on: my Ph.D. thesis on trace elements in soils involved using an optical emission spectrograph, so I had some background in spectroscopy. I decided to learn more about the kinds of spectro-instruments available to me in my Division. One was the Hovis Sphere, a black-lined sphere combined with a scanning spectrometer, which produced definitive spectral plots of target objects within it. I had a collection of samples from Wyoming obtained during my Gulf Oil days. Spectral curves (shown at the top of the next page) for these different stratigraphic units were thus run on Hovis' instrument. And the results made some sense to me.




**Spectral Curves for some typical Wyoming stratigraphic units.**

Warren Hovis' group had also built a portable field spectrometer which he urged me to learn to use. It was rather big and cumbersome. One day, with the aid of a technician in the Branch, I brought the unit outside of Building 6 at Goddard. But at first I was unsure as to what targets I should examine. Grass (by the building), concrete (walkways), and asphalt (parking lot) didn't seem too appropriate, although I did obtain their spectra. Then, I hit upon a diverse target category - the many colored vehicles in the parking lot. How well can the spectrometer differentiate these colors? We set up a the first car - blue as I recall, and got its spectral curve. We must have appeared as a weird pair to those going to the building's cafeteria. Did about 15 more, and proved to myself that the vehicles could be distinguished this way, while realizing that color will perhaps be a (or the) prime parameter in interpreting imagery from ERTS and similar systems.

But, my mind still dwelled on cratering and lunar geology. One day in February of 1971 I called Mike Dence in Ottawa to ask his opinion about the effects of crater wall slumping on crater diameter and volume of fill. After hanging up the phone, I sat back and stared at a map of the Moon's frontside, focusing on its craters. In an instant, I had a profound inspiration - that "electric light bulb" moment that scientists seek in hopes of a quantum leap insight. If all those basins and craters, from a 1000 kilometers down to a few meters, were to toss out a certain fraction of the target rock beyond the immediate confines of the excavated basin/crater, and if that ejecta didn't leave the Moon (as was most probable), then it had to come to rest elsewhere on the lunar surface. Thus, if I just calculated that fraction for each larger crater or basin, using the dimension information we already had from terrestrial observations, then I could compute the total volume of all such ejecta spread around the Moon. Using a broken slide rule (the sliding plexiglass with its vertical hairline used to mark a new spot was missing), within an hour I had arrived at a number which ranged between 1.4 and 2.4 kilometers as the average (assuming a uniformly spread) thickness of the so-called impact ejecta blanket that had been presupposed to exist on the Moon (exposed in the Highlands but buried [largely removed from sight] in the Basins, such as Mare Imbrium). I realized that this thickness would vary depending on the distribution of the larger craters and basins. So in the next few days I refined my model to yield an isopach (thickness variability) map adjusted for the actual Moon with its different-sized Basins.




**Isopach map showing generalized distribution of lunar ejecta blankets**

I was very excited. This seemed a major discovery of great import in future lunar missions. But, I felt that reporting my calculations as made by slide rule wasn't too elegant or convincing. So, I decided to redo the whole process using a computer. I needed a programmer to help produce the algorithms, etc. required to run the model. I enlisted the aid of Mr. Michael Forman. We obtained a set of computer tapes from the Lunar and Planetary Lab at the University of Arizona, on which were all the diameters of crater/basins from 3.5 to 500 km. I refined the model and tested it. When satisfied, we ran the program. The results came back almost the same as those with the slide rule. I decided to publish this as a paper. Because I was on the Editorial Board of Modern Geology (from 1970 through 1985, when that periodical went defunct), I could arrange for a rapid review. It was quickly accepted. As I was reading the galley proofs, a press conference released the results of Gary Latham's seismometer readings at Apollos 11, 12, and 14 sites (thus providing 3 points on the lunar surface). He noted that there was strong evidence of a 2 km-thick low velocity zone in the Highlands. This confirmed my conclusions, and I tacked on a Note added in Proof relating this information. As soon as I got reprints, I mailed them to many of the astrogeologists who would be interested. (Let me say here that there was enough doubt within the lunar community about my conclusions that, over the years, at least 6 more papers were published to report other approaches to the same topic; in all cases, they affirmed my general results.) In one sense, this was to be the most important paper of my career since it could influence the choice of Apollo landing sites for the next missions.

I had obtained my insights during the time that the Apollo Site Selection committee was choosing a site for Apollo 16. The USGS team supporting that process had recommended the Cayley Plains in the Highlands because, they guaranteed, the principal rock materials there would be volcanic deposits (both ejecta and ash). My model said that the dominant rock would be impact ejecta. I then phoned Bill Muehlberger (U. Texas) to tell him that I thought the site would not show any evidence of major volcanic deposition. He passed this prediction to the committee (of which he was a member) but my model didn't sway them in their decision (who am I to challenge and contradict the USGS!). Off went Apollo 16 - it found only impact ejecta.

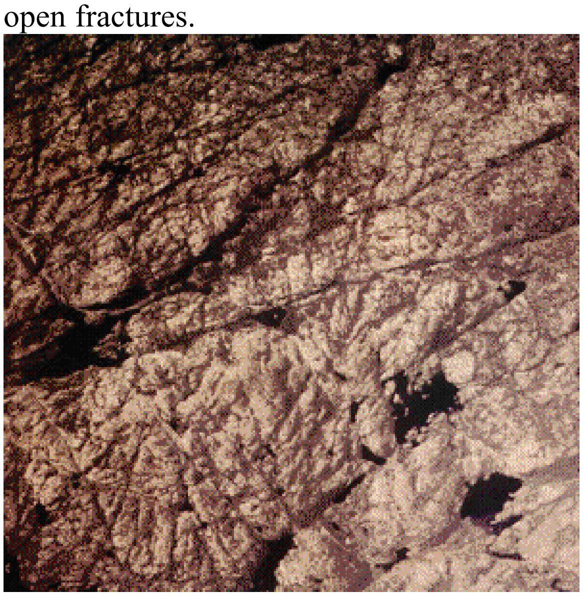
One might think the committee would have learned from this. The Apollo 17 committee (chaired by my friend, Dr. Farouk El Baz) met to consider the recommendation from the USGS. This time the boys from Flagstaff were blunt in their certainty that their first choice of sites, Taurus-Littrow, would be replete with volcanics. My isopach map showed that there would be 3 kilometers of impact ejecta there. Once more, Muehlberger was my advocate; again, I was overruled.

So, off went Jack Schmitt (a geologist) and his buddies to the 17 site. On the first day's excursion, Jack looked up at the 1.5 km high North Massif, and the rocks within it, and said from the Moon that Nick Short was right - its all impact ejecta. The next day, they operated a small seismograph-thumper to determine what was below the landing site. The result: 1.5 km of low velocity rock. Adding the two 1.5's gave just 3.0 km - precisely my prediction (I think I was lucky). So, about $5 billion dollars had been spent going to USGS sites - interesting in their own right - but not finding what they were after. My view of the lunar highlands was confirmed. No one from the Branch of Astrogeology, the site selection committee, or anyone in NASA ever had the simple guts to acknowledge my correct prediction (El Baz did phone me to say that I was on to something). Later, when I talked with Jack Schmitt in Florida (1974), he did personally praise my contribution. But, in the literature, it is hard to find any reference to this, my most important contribution to Science. Who says the system is "honest" and "fair".

By 1971, a humongous task awaited me: the evaluation of more than 500 proposals for ERTS-1 investigations. This was to be the largest such effort of its kind in the history of NASA. It was scheduled for two full weeks (including Saturdays) in late June at a Ramada Inn located just of Hwy 450 and the Washington Beltway. Hours were from 8:30 until well into the evening each day. The whole top floor of the hotel was rented by NASA. More than 80 evaluators were involved.

Although I was by then the Geology Discipline leader for the ERTS program, I was not able to participate in the geology proposal evaluations because I had my own proposal in that group. So, I was switched instead to the Geography Group, serving as co-chair; most evaluators were from Universities or Industry. Our group had about 70 proposals, some of which I had started to read even before going to St. Louis. I read them all, and appraised each carefully. This was a new experience for me. It proved very hard for me to decide which are the better (best) ones, partly because I was somewhat out of my field, but mainly because most seemed good, and I had qualms about quashing enough of them to pare the collection down to a workable few. We all spent hours reading, then met twice each day to summarize. The last day found us making the final reduction and organizing our recommendations. These were given in a plenary session to the ERTS Review Board; we had to defend our choices against some strong questioning. After that, the NASA personnel needed another two weeks to put all the deliberations and results into a document form sent to Headquarters. It was at this Ramada exercise that I first met Mr. Robert Stewart from Johnson Space Center. He was to become my assistant in the Geology Discipline Program for the next few years; I must have made about 6 trips to Houston during those 4 years.

I learned (sub rosa, from Paul Lowman) that my Wyoming proposal had been accepted (was well thought of) but with a major revision. The reviewers decided that I would not be given enough field time (since they knew how tight the travel budget was) to be the leader in carrying out the investigation. So, they on their own selected Bob Houston as the P.I., making me a Co-I. Although I was totally p.o'ed, I was forced to accept this compromise. But later I learned that this was an illegal action - no P.I. can be reassigned without his permission. I was never consulted. I considered making a formal complaint but decided that I might "win the battle but lose the war." Anyway, getting the grant allowed the U. of Wyoming to hire a full time remote senser for their staff, Dr. Ron Marrs, who had just completed his Ph.D. under Bob Reeves when the latter was at Colorado School of Mines. I worked with Ron on Wyoming projects until 1978. In October of 1971, I had NASA fly a U-2 aircraft mission over areas of Wyoming that would become our prime targets for ERTS study. The entire Wind River Basin was covered, including also a swath across the Wind River Mountains, which was split by huge, open fractures.



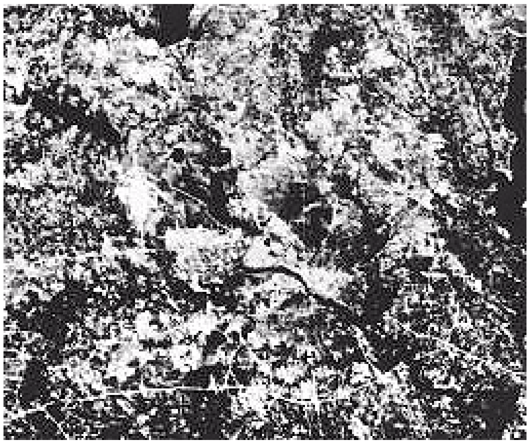
**Joints and fractures in granitic rocks of the Wind River Range**

I experimented with mapping using the resulting aerial photos (taken from an altitude of 70000 ft). The results were given as a poster paper at the 1972 GSA Annual Meeting in Minneapolis (Herb Blodgett as co-author). I then contracted with the AeroService Corp. in Philadelphia to have them make a controlled mosaic (removing the vignetting at the joins) which was almost as good as the one they had made of Los Angeles. I also completed my spectrographic signature development for Wyoming rocks but never did succeed in applying that information to the aerial photo analysis. I was still, up to then, an amateur in remote sensing.

Anticipation reached a high pitch as the launch date for ERTS-1 approached in mid-'72. I myself had remained unconvinced that it would become a powerful new space tool, since my experiences with aerial multispectral images hadn't yielded much that was new. I was openly and outspokenly skeptical.

Launch was on July 23, 1972 in the early afternoon. It was a cliffhanger: as the countdown went down to about 20 seconds, a voice at Mission Control shouted "Hold!". There was a 30 minute delay to fix a problem. Then, the count resumed and everything was picture perfect. We watched over closed-circuit TV. Orbit was achieved and the sensors (MSS and RBV) activated flawlessly over the next several days.

Three days later, more than a hundred scientists and technicians gathered in the Mission Control area awaiting the first returned images. What was brought to us was a polaroid picture of some area on an orbit line that went from Minnesota towards Texas. There were very few gray levels in this image. Nobody had a clue as to where they were looking.



**The first ERTS scene, after reprocessing: Dallas-Fort Worth, Texas**

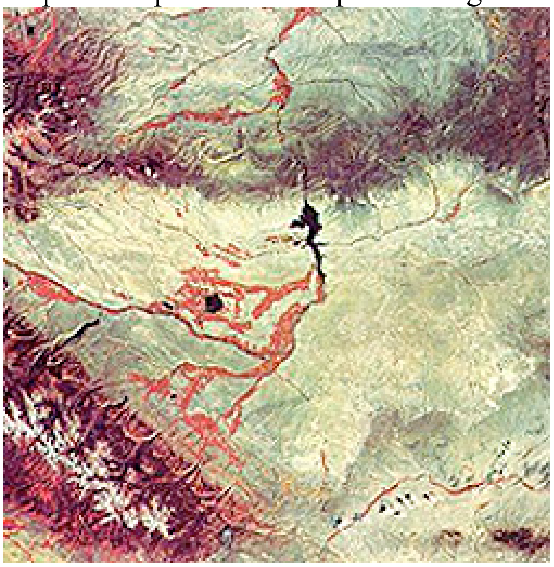
We got out an Atlas but that didn't help at first. Someone thought it might be Dallas-Fort Worth but the black patterns explained as water didn't fit the assemblage of lakes in the area. Only when someone noted that the Atlas was 10 years old did anyone postulate that these lakes were part of a reservoir system in which at least two new bodies of water had developed after the publication date. It was Dallas but no one was inspired by that particular polaroid image, which had just two levels of gray.

Several hours went by and then a technician entered with a small can containing a role of 70 mm film from the very first orbit. No one knew - except me - how to mount the film in a viewer in the room. As I reeled through the images on this viewer, big chiefs crowded behind me. For the first 10 frames, nothing but clouds. Then the cloud bank abruptly ended and a full cloudfree scene occupied the screen. The area was of the closed folds of the Ouachita Mountains (part of the Appalachians) in western Arkansas-eastern Oklahoma.

**The first identified ERTS scene: Ouachita Mtns. of Eastern Oklahoma**

The scene was <u>stunning</u> - I gasped in surprise. The details were amazing. It was then that I made two cogent statements: First, I turned to Len Jaffe, the Associate Administrator for the Earth Sciences, and uttered these immortal words "I am so wrong in my prediction, that it will not be enough just to eat crow - I MUST EAT RAVEN !". He used that line in talks he gave to Congress and across the country. Then I prophesized that "If the rest of the ERTS pictures are like these, then someone has to put them in a Picture Book for the world to see." Little did I think then that I would be the creator of such a book. I was even more a convert to ERTS when the first color image came out the following day, showing the central California coastal area around Monterrey. From then on, I visited the image production facility almost daily.

Soon thereafter I began agitating for an opportunity to go into the field with ERTS imagery to check it out. This time Nordberg gave it his full blessing and cleared the way. A cloud free pass over Wyoming occurred in August. It was not until late the day before I was scheduled to depart for Wyoming that four key black and white image sets were processed and converted into transparencies and prints. One image, of the Wind River Basin, was a color composite. I picked them up at midnight.



**False color composite of the Wind River Mtns and Basin**

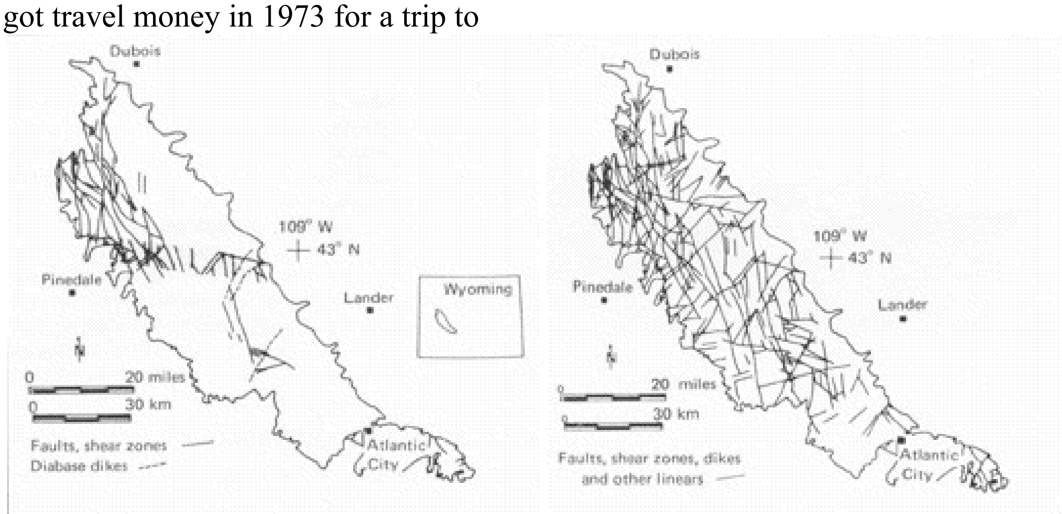
The next morning I was on a plane to Wyoming via Denver (it was on the Denver-Casper leg on a Frontier DC-3 that I dozed off at night, awoke to what I thought was tumbleweeds rushing past the wings, and shouted that we were crashing - minor panic among the passengers; it turned out that the pilot was flying into a freak snowstorm and I was seeing flakes).

I was joined there by Bob Houston and Ron Marrs. We all went into the field for two days, learning how to locate ourselves and to read the imagery. A novel experience. (Our trip to the Medicine Bow Mountains included a stop for lunch at a rustic restaurant where I had a stew concocted around a mix of deer-elk-antelope meat.) They then went back to Laramie and I spent three days more touring other parts of the state. When I returned to Laramie, Bob asked me to accompany him and Ron to Cheyenne to make a presentation to officials of the Natural Resources Department of the State of Wyoming. This we "winged" with the imagery on hand. The officials seemed nonplused and perhaps unimpressed. I learned a few weeks later that they had actually been fascinated. They were involved in a three year, multimillion dollar inventory of the entire Powder River Basin. They contracted (for $50000) with the Geology Dept. to see what ERTS could do to help. The Dept. finished its study in December of 1972, turned over 5 hand colored copies to the State, who then decided that it was sufficiently complete such that the rest of the project could be halted. When I got my copy (a large folio, blue-bound), I took it to Headquarters, and Len Jaffe and John Denoyer in particular, quickly recognized its propaganda value and took it with them to Congress during hearings as proof of the cost savings ERTS could bring to various applications.

I arrived back at Goddard just in time to be a star attraction - as the first NASA scientist to go into the field with ERTS - in a hastily called Conference of Investigators designed to exchange experiences in the use of ERTS. I did an impromptu review of the Wyoming trek, supported by quickly developed 35 mm slides showing ground features to compare with their representation in the ERTS image. I made a point that intrigued the attendees: I could see in the images dirt roads that were less than 20 m wide, detectable below the 79 m resolution quoted for the MSS because the contrast of the light dirt against the sage background swamped a pixel with high reflectance, thus making that pixel appear much brighter than off road ones.

After the conference there was a frenzy of activity as new, often spectacular images were produced daily. I came to haunt the Image Processing facilty in Bldg 22, near Mission Control. Throwaway pictures could be retrieved, so I started building a collection. My thoughts of an Atlas were crystallizing but I wanted to have a year to acquire a diverse set and to decide what the best format would be. I also started working on the Wyoming imagery, using the color additive viewer and then, for the first time in my life, the capabilities of computer processing. At that time we were relying on JPL's processing program -VICAR- developed for analysis of planetary probe data. We soon added IDIMs as an alternative system.

As I learned more about the Wyoming scenes, I felt the time had come to make an extended field trip there. Nordberg vetoed my travel request. I hit the ceiling - saying that it was his idea for me to honcho a major study like that - but he cited tight travel money (tight because he was traveling almost weekly, along with the senior staff, leaving us lackeys to just look at pretty pictures). I had a real donneybrook argument with him in his office - did he get mad! I finally got travel money in 1973 for a trip to Wyoming, during which I field checked some of the classifications (this time using Penn State's ORSER program). The one of the Willow Lake area on the southwest flank of the Wind River Mountains was the most convincing. On the day I was to visit that site, it rained incessantly. So, I bought a set of colored pencils and in the motel I enhanced the map by assigning different colors to the different feature categories for ease of reading. Meanwhile, Ron Marrs was finding out a lot about applicability. His colleague, Prof. Ed Decker had used ERTS to map fractures in the Wind River Mtns, producing a case study that was so powerful, it became the standard for that application. The next graphic underscores that point. Prior to ERTS there was no map of Wind River lineaments, as this mountain block was too rugged for extended field work. The U-2 flight line gave Dr. Decker a control base for his ERTS interpretation. He told me later that the whole interpretation session took only three hours to make the map below. To test veracity he did spend some field time later, finding evidence for these linear features wherever he checked.




**Lineaments maps of the Wind River Range (Left: pre-ERTS; Decker's early work at top left, with U-2 Flight strip; Right: using ERTS)**

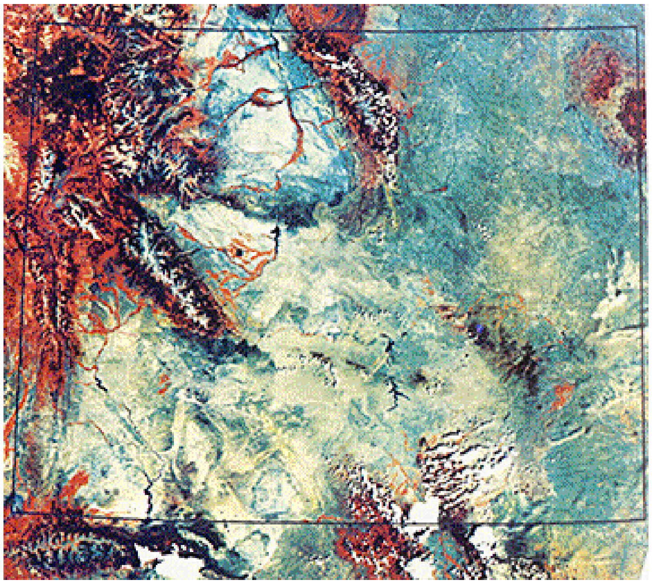
Most of my time was tied up in continual monitoring of the Geology Principal Investigators reports. I was "cursed" (?) with the largest number of grant contracts (about 60) to be monitored of anyone at Goddard. This required constant phone calls and other interactions. Some investigators were doing a great job, providing much insight; others were flops. The most common use of the ERTS data was in mapping fractures (lineaments; the term "linears" crept in but was eventually discarded as slang). Yngvar Isachsen of the N.Y. Geol. Survey made a superlative study of fractures in the Adirondacks. The other common use was in recognizing alteration around ore deposits; the work by Larry Rowan and by Alex Goetz were exemplary, and several foreign ones were notable.

As 1972 closed, Norm MacLeod, Vince Salomonson and I prepared an important review and position paper on using ERTS-1 as a global mapping tool. I also submitted a paper describing how ERTS could be used in geological studies, which was the Cover Story article in a 1973 issue of GeoTimes. In 1973 the Second ERTS Symposium was held at Goddard; the Third Symposium in 1974. I had the task of summarizing the significant results in Geology for the Publications that evolved from these meetings. In 1973, I teamed with Paul Lowman to write another position paper on the Outlook for using Space Imagery in the Geological Sciences; published as a NASAwide document.

In 1973 I came up with another idea: preparing a mosaic (using black and white ERTS images) of the conterminous 48 United States. This stemmed from a recommendation in the global mapping paper mentioned in the previous paragraph. It was approved and funded at a cost of nearly $500,000. The Soil Conservation Service aerial photo lab was contracted to do this job. My colleague, Art Anderson, was placed in charge of the routine interaction with the SCS. Chuck Bohn, a sort of "jack of all trades" at Goddard joined in the effort (he would work with me on occasions over the next 10 years until he left GSFC). The mosaic was completed (along with Alaska and Hawaii) in 1974 and made quite a sensation (one version occupied more that 12 feet of a wall when mounted). So much interest arose that the National Geographic Society commissioned a color mosaic (both red false color and green natural color) produced by the General Electric Space Sciences image lab in Beltsville. Here is part of the original SCS map, showing most of the desert southwest:

Part of the first b & w ERTS mosaic of the coterminous United States

During the same time, I contracted with GE to produce a color mosaic of all of Wyoming, shown below. This may have been the first such mosaic that covered an entire state.


**First ERTS color mosaic, showing all of Wyoming**

In 1973, the Planetology Branch was disbanded. Most of its personnel stayed at Goddard: to MIT). The Branch Chief, Lou Walter, was grabbed by Bill Nordberg to be Head of the newly created Earth Sciences Division. Along with Paul, the two Herbs - Tiedemann and Blodget, and Herm Thomas, Charles Schnetzler, and Phil Cressy, I was reassigned to Lou's Division, although for a while I stayed in Hovis' Branch.

I remained quartered with Norm MacLeod at this time. He had teamed with Jane Schubert on various applications of space imagery. But, he, being an idealist and humanitarian, found the travel limitations at Goddard too stifling for him to contribute. So later, he talked his way (literally) into an appointment with the American University, where he was on staff for many years. Jane eventually migrated to Ottawa to work for the Canadian Center for Remote Sensing; she later married its Director.

In 1973, I was asked to be the mentor for a visiting NAS Associate, Dr. William Finch, a geographer at San Diego State University. We worked very closely; he decided I was a go-getter and latched on to me, so that we were jointly active in several projects (my wife, Ellie and I became good friends with him and his wife Vivian). He was a real sleuth at finding outstanding Landsat images; his discoveries reactivated my original vision the day the first images came back of a picture book featuring images that would give a world view. I talked both Paul Lowman and Vince Salomonson to join us as co-authors. But, soon Lou Walter got wind of our idea and told us bluntly that this was not a proper project for scientists; if we try to carry out our plan, we would be disciplined (fired?). Vince backed out; the others agreed to my approach of putting together a strawman version on my own time, which I would then plop on Nordberg's desk, going over Lou's authority, and getting Bill to sign off and provide funding. So, for much of '73, I assembled three loose leaf binders full of images. This I did at home (even as I struggled to complete the Planetary Geology book), often by getting cardboard boxes containing 500 or more images from the Image Processing Lab, placing them next to my chair in the TV family room, and going through them one by one (viewing the TV 20 seconds out of each minute - enough to get the gist of the program), extracting candidates and winners.

By early '74 I had a superlative collection of more than 200 outstanding ERTS images, some with captions. This I delivered to Nordberg. Within a week he called me in, bursting with enthusiasm. I told him about the Lou Walter veto and threat; he hit the ceiling, chewed Lou out, and gave me carte blanche to work (parttime) on the book (whose title Mission to Earth: Landsat Views the World was suggested to me by Bevan French; ERTS by 1975 had been renamed Landsat). Lou never recognized the book's existence, after it came out, and never praised it despite its huge success; in fact, he never really forgave me for going over his head, and got even (explained later). I will discuss the highlights of putting this book together (we replaced Vince, who had bowed out, with Stan Freden who had become a Branch Chief in another Division) in several paragraphs later.

But the highlights of this year both occurred in May. In early May Walter Cronkite, the famed news broadcaster, came to Goddard to gather material for a Special CBS was doing on the applications of Space to society. I was chosen to be the technical person he interviewed on camera. When I met with him, he tried to put me at ease while I tried to hardsell him on ERTS. We discovered that both are native Missourians, so that smoothed the conversation. My bit was to explain how black and white images can be converted into color and, especially, what the false color red version means. This ended in about a one minute segment in the special, which was aired that Fall.

**Walter Cronkite with Bill Nordberg and yours truly before my interview that was later shown on CBS**

This reminds me: Just a few months earlier, I was the technical adviser for a 30 minute NASA film that summarized ERTS and remote sensing. I helped to define the contents and prepare the script. I was filmed using the $I^2S$ Additive Viewer to produce a false color composite image. When the film was finished and released, I found to my horror that some imbecile had edited out the key moment when the third image is added to give the red color. Thus, this process made no sense to most viewers of the film. I raised the roof with the NASA Headquarters people doing this project, but the film had been finalized, so the error would have to be tolerated. This, in my opinion, is an indictment of the great failing of NASA and Government Science - accuracy (truth) takes second to costs.

The biggest trip of my life began in late May of 1974. I was selected to be part of a two-man mission, sponsored and paid for by AID (Agency for International Development) of the State Department. Five countries in SouthEast Asia had planned to hold separate symposia on the use of ERTS for applications in their respective nations. My traveling partner, and Senior in the Mission, was Mr. John Boechel, Chief of the Engineering Directorate (Code 700) at Goddard. My role was as the technical expert. We would meet in Bangkok for the first program. Each chose a separate itinerary. Mine took me first into the Southern Hemisphere.

I had previously received invitations to give talks in both New Zealand and Australia. I requested travel support to honor these requests and to my surprise I got the funds from NASA Headquarters. What I thought of as a small detour was actually a 7000 mile side trip. I elected the option of returning via Europe, so I could claim that I had actually gone around the World. After a 17 hour flight, I arrived in

Auckland, to be met by two officials and to interview of their national TV.

That day we drove as far as Lake Taupo, and visited the Wairakei geothermal (geyser) field. That night I took a dip in the motel's heated (by the thermal waters from Wairakei) swimming pool - and received a major shock. The full Moon, as I floated on my back, was ***Upside Down*** relative to the orientation in the northern hemisphere. I'd written a book - *Planetary Geology* - largely about the Moon but had never heard of this flip-flop.

The next day we drove to Wellington, and specifically to the suburb of Lower Hutt, where the Department of Natural Resourcess is headquartered. My host there was Peter Ellis, a young Englishman who had emigrated to N.Z. to set up that country's remote sensing program; he had stopped enroute six months earlier at Goddard where I was his principal contact. I stayed in their home for two nights. On the first day, I gave my first lecture of the trip to about 60 personnel. I remained in contact with Peter for the next 12 years.

Then on to Melbourne in Australia where I lectured at their University. Next, to Canberra where I gave a talk not about remote sensing but about my lunar studies. On to Sydney, another talk, and finally completing my 6000 mile detour via Manila, Hong Kong, and then to Bangkok, site of the first of four ERTS symposia.

At Bangkok, we went to the Thai Institute of Technology for the two day program. In the audience was a student who, today, is the proprietor of a Thai Restaurant in Shawnee-on-the-Delaware in eastern Pennsylvania - he remembers me from that occasion. We had Thai fast food for lunch. The talks by John and me went well; locals gave most of theirs in English. One couldn't help but feel important in such an environment. I developed a strong liking for the Thai people. The real highlight was a dinner hosted by the Symposium that included 8 (visually stunning) Thai dancing girls. I was coaxed, red-faced, on to the floor to dance with one.

I ended up giving the keynote address for the Symposium.

**Nick Short at the Bangkok Symposium**

While at the meetings, one of the Thai scientists came to me with an offer: When I returned to pass through Bangkok enroute home, why not stay and accompany him to his field project: measuring elephant track distribution - their footprints filled with water and became puddles for malaria-carrying mosquitoes. Wanted to but my schedule had been firmly locked in. Next stop was in Kuala Lumpur, Malaysia.

From there, we went on to Jakarta, Indonesia . After being met by a State Dept. representative, we were driven to the finest luxury quarters in the country - the Hotel Indonesia. That night John and I had what, as I look back on life, was the most extraordinary and sumptious (banquet) feast of my life. Every Sunday evening the Hotel put on a huge Java "Smorgasbord" for both tourists and leading citizens. There were two long (20 ft) tables each loaded with native foods and delicacies. In between a Javanese sat cross-legged while cooking meat wrapped in a peanut butter paste. At the far end was a dessert table more delectable than any I've ever gazed at. The meal started with alcoholic drinks. I truly "pigged" out, trying as many different dishes as I could manage. While we ate, a 20 piece Gamelin band (mostly brass and cymbals) played for our entertainment, and later 12 dancing girls from Java performed. I went to bed that night having apparently survived the orgy of my life.

The next day was the start of the 2-day Symposium. It was attended by several hundred scientists and technologists, and by a half dozen or so government ministers (Heads of Agencies) and by a few Army Generals. We were placed center stage. Both of us gave our now routine talks (with slides) and then the rest of the morning (until 1 PM) was given to talks, unfortunately all in native language(s).

Afterwards, a General had us as guests at the city's most exclusive Chinese Dum Sung restaurants. Chinese girls came by every few minutes with carts laden with "goodies", which the General kept having put on our plates. This was almost the equal of the previous night.

That night I was violently sick (from the gastric cuisine overload). Threw up for hours. But, by morning I had purged my system and had a hardy breakfast. John had experienced no problems. Off we went to the second day of meetings. About an hour into the proceedings, I looked at John and his face had turned the most amazing shade of medium gray-green I could image a human to assume. He was clearly quite ill. We rushed him off stage; the General called for a military vehicle to take him to the American Embassy medical facilities, where he was treated by a Doctor who was a native of Bowie, MD (my home at this time). John was returned in the evening, more or less recovered. Just too much rich food!

But, by default, I was now the acting head of the U.S. mission. We had a lunch for all participants. I was the guest of honor, seated at a round table that included the U.S. and Canadian Ambassadors, 3 Ministers (including Energy, and Commerce), and 2 generals. Every one focused on me, with questions. This was "top dog" stuff, and I was a bit queasy about my role. So, I decided to take the offensive. I turned to the Canadian Ambassador and asked him about his country's abundance of impact craters - about which he professed no knowledge. Then I glanced at one of the Ministers, and saw a look of envy creep across his face. I sensed that I had probably just committed a diplomatic "faux pas". Sure enough, he asked if Indonesia had any such wonderful things. A moment's panic, then I answered "Well, no, Sir, the geology is not suited to their preservation, but, of course, your country has the World's greatest collection of Volcanoes" I was greeted with a big smile - World War III averted.

Singapore was our last formal stop. I returned home by way of New Delhi, Karachi, Beirut, and overnights in Istanbul and Athens.

Bill Finch returned for a second summer (1974) as an Associate at Goddard. Paul and I had by then finished most of the captions we would write for Mission to Earth. But Bill had finished only a few of the 40 images assigned to him. Thus, he had become the rate-controlling factor in our plan to have the Book finished in time for its appearance during the Bicentennial celebrations in 1976. He suffered from a mental block, or a lack of confidence, in doing this task which should have been simple for a geographer. I came up with a expedient scheme to get him to do the job. In July the two families rented a cottage at Avon, in the North Carolina Outer Banks (from there the Finch's were to go to their childhood home in Wilmington, NC). While my son and I played each day (I was done with my quota), often swimming in the ocean, I had Bill working most of each day on his writing. My inducement ("carrot on a stick") was to control the evening cocktail hour - nothing for him unless and until he had finished 8 for the day. It worked - like a charm.

During that summer, an amusing incident occurred. I had been studying a computer-enhanced image of the Washington Landsat MSS scene, which I had printed as an enlargement on high quality photo paper. There were several areas northwest of DC that had some anomalies I couldn't explain. Time to check them in the field. One was in the Catoctin Mountains northwest of Frederick, MD. There, the Presidential retreat at Camp David is located. Bill Finch and another summer intern went with me by government (NASA) car. We got to within a few miles of Camp David, stopped, and got out to look around. I had the ERTS picture on the back of the vehicle when a Maryland State Trooper happened by. He got out, questioned us as to why we were there. He spotted the picture, which was a false color composite in which vegetation is shown in red .

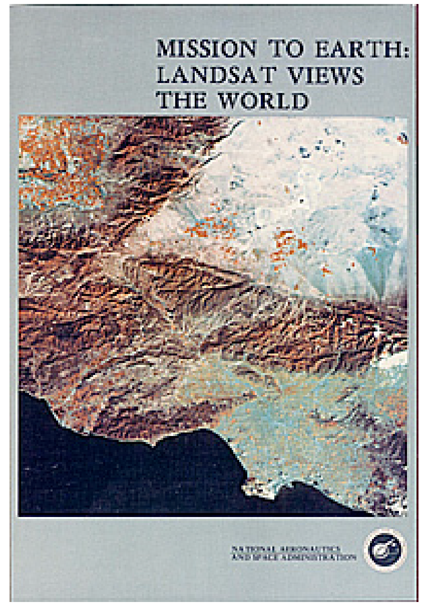
**Maryland State Trooper asking about the Landsat image of the area**

He asked just what this photo was. I tried to explain. He then asked why it was red. I (foolishly) answered (in jest): "Because it is the Communist version". That did it. He detained us while he radioed first the police, then the FBI, and finally at my behest, NASA Goddard, where our identity was verified. I tried to explain my "humor" but it was a half hour before he let us go, with instructions not to proceed toward the Camp David compound. In retrospect, I still think it was funny.

In December 1974, I summarized what was being learned about the use of ERTS to search for new oil fields and mineral deposits at a Symposium in Miami, FL on Remote Sensing for Energy-related Problems. Apollo 17 Astronaut Jack Schmitt was the keynote speaker, and Nick, met him, impressing Jack with his knowledge as a 9 year old.

In this period into the middle of 1975, I finished the major components of Mission to Earth. The editing then took over a year. The person assigned by the NASA Headquarters Technical Publications Office (by then under the direction of Kay Vogelwede, with whom I had further dealings for the next ten years) was a wonderful lady, Anne Schmidt, who worked with me on this and two other (later) NASA books. Many problems ensued but all were resolved and a final version was assembled in late 1976. The printing was done in Indianapolis during January 1977. Both Paul Lowman (for part of the time) and I (full time) had to be present at the Printers for that month to supervise the production and especially to give advice on getting the colors "just right" (the chief printer was very fussy, trying too hard for perfect duplication, not realizing that the versions we were using weren't necessarily optimum, being arbitrary). We had to be present or available from 8 in the morning until the end of the second shift at Midnight. Life in that town was tough, this being the coldest January (one night down to a minus 26° F) ever on record there.

The book began distribution in February of 1977. Because we had hoped to have the book's publication coincide with the Bicentennial, we backdated its publication to 1976. People in the NASA family received copies right away, from Administrator on down. Praise for the end result was very high, indeed. Over the next two years, it won almost uniformly strong approbation from reviewers across the Globe. The first printing of 25000 copies (the entire cost of the book to that time was more than $350,000) sold (at the bargain, subsidized price of $14) out in months and a second printing of 25000 copies followed.




**Mission to Earth: NASA SP-360**

Perhaps the best proof that such a book was a valuable way to publicize NASA and the Earth Resources program was that in the next five years at least four other similar books using mainly Landsat were published. The book received a Group Achievement Award from NASA and the Autometrics Award from the American Society of Photogrammetry, both in 1977. The book was complemented by another: ERTS - A New Window on our Planet, consisting of articles by various staff of the U.S. Geological Survey who had conducted studies of geology and various other applications; its editors, Dr. Richard Williams, Jr. and William Douglas Carter were both receptive to sharing information between the two books (theirs came out in 1976, beating Mission to Earth by almost 9 months); that book complemented Mission by concentrating on in-depth case studies.

But, for me, the highest honors were these. I sent copies to Richard Nixon, who answered graciously, and to Hubert Humphrey, then in the hospital dying of cancer, who sent me a wonderful personal note indicating that he had read through it carefully as a diversion during his time of great suffering. I sent two copies to President Jimmy Carter (one specifically earmarked for Amy). Several weeks later I received a call from Dr. Frank Press (with whom I had contact during the Lawrence Livermore cratering days), then the Scientific Advisor to the President, requesting that 50 more copies be directed to the White House, where Pres. Carter would give a copy to any visiting Head of State if that person's country were pictured in the book. In sum, Mission to Earth proved to be the second most popular book ever sponsored by NASA. I certainly proved Lou Walter to be wrong.

Returning to 1975, one change in my job status was important: at Lou Walter's bidding, a small Geology Group was established, with Paul Lowman, Herb Blodget, Herb Tiedemann, and Mel Podwysocki as personnel, under my direction. Paul left the group not long after, since his interests in space imagery were supplanted at the time by studies of tectonics. But the others worked with me on projects for the next two years. The high point would come in 1976, as described later.

Research-wise, I spent much of my time still coordinating Landsat investigations and, personally, extending my studies of space imagery for lineaments mapping. I did have to interrupt this work for a month to prepare a report for the Earth Resources Program Office at Headquarters on the Practical Uses of Landsat for Mineral and Energy Resources that was then submitted as is to the U.S. Congress' Committee responsible for NASA Appropriations.

But in the Spring of '76 I received an open and disturbing warning from Lou Walter: he had evaluated what our Geology group was doing and had decided it wasn't going anywhere or competing against the much larger JPL group (who had unlimited travel and much more funding). In the Spring of 1976, he told me in no uncertain terms that I had better come up with something dramatic and scientifically sound and useful OR he would disband us. This had two effects: First, Herb Tiedemann and Mel Podwysocki decided to look for jobs elsewhere. (Herb went to Phillips Petroleum in Ponca City, OK and Mel quickly found a better position with the USGS in Reston, VA). Second, I came up with a very solid field experiment: the Utah project - which actually happened.

I arrived at this idea as follows: What was not being properly addressed in current Landsat investigations? The answer: The ability to recognize different sedimentary rock types and units and thus to do at least reconnaissance level mapping from the imagery alone. At the outset, I considered selecting Wyoming as the study region, where I knew the geology well, but dropped that because 1) the Wyoming project had already demonstrated that under the right circumstances rock units could be differentiated, and 2) I wanted to go to another state whose geology I didn't know well. Based on my travels, I easily narrowed possibilities down to just Utah. I got a hold of the Utah geological map and found about 10 promising site targets, based on the already mapped stratigraphy. I decided to add several mineral deposit localities, following a suggestion from Mel Podwysocki. I examined several Landsat images including the one covering our chosen prime area (below; look for black line) and found some of my sites to be in mountain ranges that had heavy forest cover. Too much vegetation would kill any attempt to map exposed units. A preliminary field survey was needed (this is always true when a study like this is proposed) to determine whether rock exposure was sufficient. Then, suitable sites could be specified as flight lines for an overflight by a NASA aircraft with a multispectral scanner.

**Landsat image of part of south-central Utah**

So, I submitted a travel request to check out the potential sites in Utah before the NASA flight (which cost $50000 to do, with another $50000 to process the scanner data). I could do this scouting trip for less than $1000. Lou Walter turned it down flat. He said it wasn't justified and besides I could rely on the Landsat imagery to find rock exposures with little cover (just not usually true). The real reason was that they were overdrawn on travel money for that quarter because the big chiefs had consumed it all. I hit the roof and confronted Lou with all guns blazing. My main thesis: how can he expect our group to succeed in the JPL manner if we couldn't even preview our field experiment sites. His answer: that's my problem - solve it.

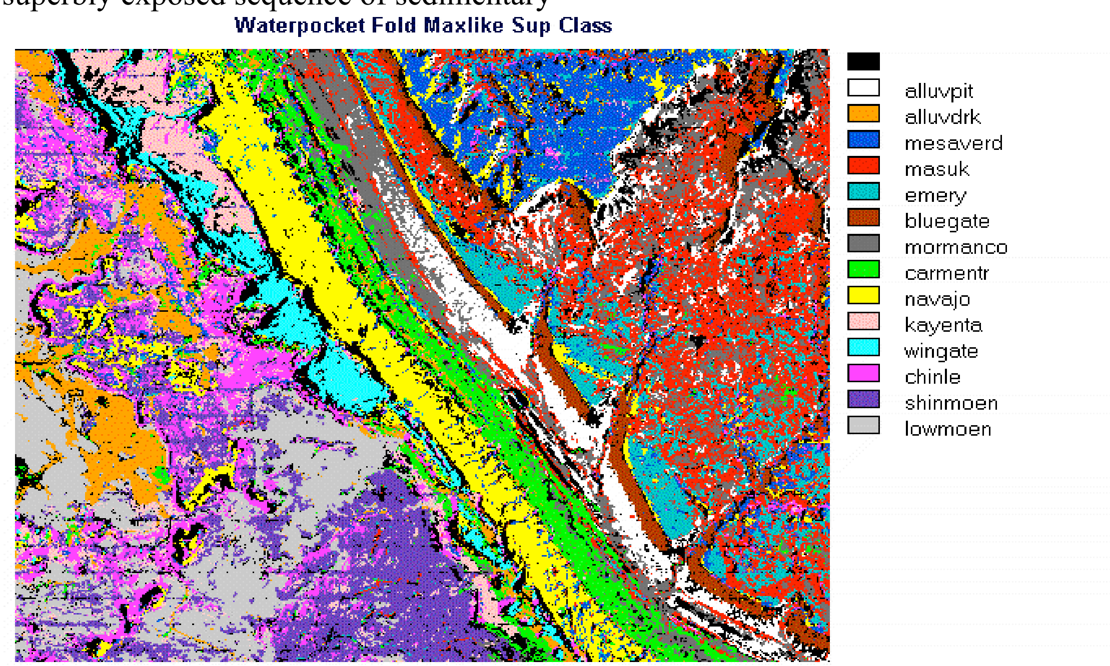
And I did! I had previously received a standing invitation to speak at the University of Utah if I were ever in Salt Lake City. This came from Dr. Larry Lattman, by then the new Dean of the Earth Sciences College at U. Utah, and before that one of my Investigator responsibilities when he was at Penn State. I called Larry, explained my problem, asked for help, and received an invitation to come to Utah at the University's expense, both to give that talk and to be assisted by them in the field checking. I took a week's vacation in May '76 (Lou wouldn't even let me go as duty time, because he felt that would be a conflict of interest), and flew off to SLC.

What a superb response awaited me. First, Larry had arranged for one of his graduate students, who was a pilot in the Utah Air Rescue Service, to fly a single engine plane over all my test sites. This we did in a two day period. I photographed every site. We would fly until refueling became necessary, then he would set the plane down in a desert runway for gasoline. This in itself was sufficient for me to pin down the right sites. (Tragically, this pilot, whose name I forget, was killed just a few years later during a rescue mission). Then, I was given a field vehicle and an assistant, allowing me to check more closely certain sites on the ground. Finally, I met with about 10 faculty to report my observations and to have them evaluate the worthiness of the sites. They helped me to decide to reject 3 sites and add 3 others that they recommended (one being a geothermal field near Roosevelt, UT; another the Big Rock Candy copper deposit). This whole episode was the very model of how to do this kind of experiment properly.

With specific flight lines now specified, the NASA aircraft (NC-130B) with its 24 channel Bendix scanner covered 11 chosen sites on a cloud free day in June in Utah. Everything went perfectly, except that afterwards it was discovered that several of the thermal channels had malfunctioned. But, we ended with a remarkable data set that compared well with any of the JPL missions. I was to spend years on the analysis of these sites, leading to several important papers. The results were so good that the GEOSAT Committee (an amalgamation of mining and petroleum companies that was set up to determine the value of remote sensing in exploration and to advise NASA on future satellites and missions; its chief executive then was Dr. Fred Henderson; I sat in on their meetings when these were in Washington) designated four locations (Waterpocket Fold, White Mountain, Lisbon Valley, and Big Rock Candy Mountain) as National Test Sites for Geology. Of these, the Waterpocket Fold, a superbly exposed sequence of sedimentary rock units inclined in a monocline, proved to be the one research area where I was able to produce significant results from my studies in the 1980s (more on this later).

**The Waterpocket Fold, taken during my aerial reconnaissance**

Using the maximum likelihood classifier, I produced geologic units maps that achieved greater than 80% accuracy, such as this one




**Supervised classification of the stratigraphic units exposed on and around the Waterpocket Fold**

Needless to say, while Lou Walter praised my initiative, he never congratulated me or the group for coming up with an experiment that met his standards for competing with JPL.

A tragic, sorrowful event happened in 1976. Bill Nordberg was diagnosed with a rapidly spreading, untreatable form of cancer and on October 6 he died. His friends came to the funeral parlor in droves, being especially kind to his widow, Trixie, whom I had gotten to feel at home with. There was a later Memorial Service, attended by hundreds. The eulogies could not convey the sense of loss that all of us (and myself deeply) felt as the dynamic leader of both major meteorological satellite programs and ERTS/Landsat would no longer be there for us as the Earth Resources program, begun by Bill Fisher of the USGS and culminating in Nordberg's vision, was now on firm ground, fully accepted, and pointed towards a great future. I vowed at the Service to honor Bill by dedicating Mission to Earth to him. Losing Bill really hurt; I admired him above all others in the ERTS program.

I had now been at Goddard for ten years. It was becoming obvious that geology was increasingly out of place there - the emphasis in remote sensing was shifting to vegetation studies. We continued to look bad relative to JPL in our limited, patently underfunded geological efforts. Returning to teaching was an option and I had sent out feelers to see if any school wanted to start a program in geological remote sensing. This proved premature - it began to infiltrate programs in the 1980s as people such as I published papers proving its value. If I were to entertain a job change, it could be in the raw materials industry, which was now using remote sensing almost routinely, but I placed the added requirement that it should be in the western U.S.

Almost by accident an opportunity was presented to me. In March of 1977 I received a phone call from a Dr. Peter Alexander, who was at that moment visiting the Dept. of Energy in Washington concerning the NURE (National Uranium Resources Estimation) program. Officials there were suggesting to him, as head of the DOE's Field Program based in Grand Junction, CO, that remote sensing should be integrated in the exploration methods being improved as new means of finding additional deposits of uranium. My name was given to him by the Bendix Company in Michigan, who knew about my Utah work.

Peter called me at Goddard, explained the nature of the job, and asked if I might be interested. Ellie (reluctantly) told me to go ahead and check it out. This led to a trip in April by her and me to visit the DOE operations there. It turned out that I would be hired by their prime contractor, Bendix Field Engineering, and my duties would be to coordinate all ongoing and future studies in which remote sensing could become an integral, perhaps key, component. The salary offered was more than I made with the government. I really took a fancy to Grand Junction (its setting among red and yellow sedimentary rocks at the edge of the Colorado Plateau was beautiful to me. We were wined and dined (including a trip to the pine-forested Black Mesa). The job was offered on the spot, with a reporting date of July.

Independent of this, Stan Freden, one of the co-authors of Mission to Earth (who by now had become a friend) had approached me to see if I were interested in joining the ERRSAC program which he headed, with Phil Cressy as the Group Leader. I politely declined but this offer was to become critical shortly as my "escape valve".

Then, two bombshells exploded: First, I learned in April that Peter Alexander had quit the Grand Junction program and had accepted a position in the Washington area, starting June 1. This should have been a warning sign that something was amiss with the NURE program. Then, late in the month I got a frantic call from G.J. saying that NURE was closing out two contracts and that I would be absolutely needed there at once to check out that they had met the terms of the contracts. That was another indication of trouble, but I never connected this to any danger since contracts were often not renewed. But I would have to report at once to evaluate the contracts. I would then be allowed to return to Bowie (remaining on salary), of course after I had quit NASA, to close on the house and move the family west.

I had little choice but to barrel-ass out to G.J. I submitted my resignation (through Stan Freden who was now in my job loop) and my colleagues threw a sudden going away luncheon (among the gifts was a small elephant statue made of sea shells). Because I needed a car (and we had two), I decided to drive the blue Ford to G.J. and then leave it there. My hurried departure was on a Friday morning - I allowed four days to reach Colorado. The trip out was strenuous and I slept rather poorly but I reached G.J. on the Memorial Day Monday.

There, my problems really started, got worse, and finally destroyed this whole venture. I was checked in to the Holiday Inn. As I tried to fall asleep, exhausted after driving, some wild noises swept through the Inn, keeping me awake for several hours. There had been a regional playoff of American Legion baseball teams in G.J., the champion had been crowned, and everyone celebrated with vigor.

I reported on Tues. AM to the Bendix facility. Bleary-eyed, I filled out the employment forms, met my immediate new boss, Dave Emilia, and by afternoon was on a plane to Casper, WY via Denver. There, a motel had been reserved. After dinner, I retired early but to my horror heard train engine noises just outside - the motel was next to the railroad and a switcher was constantly moving cars around. Terrible night, with sleep deprivation besetting my misery.

The next two weeks turned out to be a living hell. But too involved for me to treat the details in this tome. I'll just summarize in this brief synopsis. On the day after my arrival I was sent to Casper, WY to meet with Ron Marrs of the University of Wyoming. Got up very early and spent 15 hours in the field the first day, then 10 the next. Then a horrible weekend sleepwise visiting my brother, Fr. Anthony Short, pastor of an Indian Mission. Payday for the Indians led to drunken binges. Upon return to Grand Junction, immediately after checking into the office, I was told to drive my car west to meet the JPL crew, headed by Alex Goetz (not there at this time) and Mike Abrams and Jim Conel. They were taking field spectra with their new portable spectrometer; it had been out of commission but was now repaired, so I must hurry to see it in action. Once in the field, it malfunctioned from the start. Trip aborted. Then back to G.J. that night where I was told we were leaving at 6 AM, again for Casper, and an emergency meeting with the third contractor.

The meeting was attended by several Bendix people, whom I accompanied as an observer, and the mining company personnel. The following day was spent entirely on site in rough country near Lost Cabin (a nearly deserted mining town) looking at the relevant geology. That evening I was now near collapse from so little sleep but did not tell anyone from Bendix. I started a serious introspection about whether this job change was a positive step. In fact, the next morning I phoned Stan Freden, told him of my plight, and learned that he had not gotten around to submitting my resignation for processing. Thus, the ERRSAC job was still open.

I was on the fence until the Denver airport. One of the Bendix staff was a portly youngish man who was a chemist. Our plane to G.J. was off schedule, so we had drinks in the lounge. He quickly became inebriated (drunk, folks) and opened up on what was going on at the Bendix facility. His startling revelation: management had decided to back off from adding remote sensing to NURE - my inspections of contract work were terminal. A general scale back in the program was why Peter Alexander jumped ship (without being honest up front with me). So what do they have in mind for me: Well, I would be assigned to a field geophysical team and would be responsible for electric log interpretation. This was so far afield from what my experience and interests were that by the time I got to my motel in G.J., I called Stan Freden at his home, and said I would return immediately - they would count the two weeks I'd been gone as vacation.

The most ironic outcome of the Grand Junction diversion: We had sold our home so when I returned we had to find a new place. We moved to splendid home in Ellicott City, MD. I learned a few months later that the President of Bendix Field Engineering - my erstwhile employer for those difficult two weeks - lived just around the corner on the next street.

Now, on to ERRSAC, which is the acronym for the Eastern Regional Remote Sensing Applications Center. This was one of four centers established by the NASA Headquarters Technology Transfer program, others being in the South, the Midwest, and the West. The idea, strongly supported in the U.S. Congress, was to train and assist State and Local Government Agencies throughout the country in using remote sensing technology, based largely on Landsat data, to address environmental, land use, and agricultural concerns amenable to the new information sources stemming from air and space image coverage. Issues was a key byword that was used by the ERRSAC staff in their dealings with these agencies. For me, joining such an operation was a drastic change in my interests and work activities. Frankly, I wasn't at heart enthusiastic about this new career shift, since it was only peripherally involved in science - to which I had a life long commitment. But, I had no other options. To contribute to ERRSAC I really had to learn aspects of remote sensing other than the geological applications I had pursued since joining NASA. Above all, I had to master computer image processing, especially the IDIMS and ORSER systems that ERRSAC had adopted from outside sources. I had to learn to talk with people who were politically minded or who were technicians in various fields.

ERRSAC was headed by Dr. Phil Cressy, a geochemist who had been in the Planetology Branch when I was an NAS fellow there. For him, this work was a complete departure from his career goals, but he made the transition very effectively, and discovered he had gifts as a leader, a planner, and a skilled politician. Others in ERRSAC during my tenure included Jimmy Weber (the master politician), Arlene Kerber (with whom he has lived, without marriage, for decades now), Bill Alford, Bill Campbell, S. Ramapriyan, Betsy Middleton, Marc Imhoff, Scott Cox, Mike Goldberg, Janet Gervin, Herb Blodgett (another geology retread), Diane Kugleman (Cressy's Administrative Assistant), and Pam Bolling, the Center secretary.

At the outset, it was not clear to Phil just what I would do for ERRSAC. I wasn't mentally tuned to the dealings with the Agency people. However, after a few months of "getting my feet wet" as one of the technical people (applying any Geology was not useful, since very few of the 15 states we were responsible for were in terrain where rocks could even be seen, because of the vegetation-covered East), Phil decided that my educational interests would make me the right person to establish the formal Education program that became an integral part of ERRSAC's mission. Thus, from 1978 through most of 1979, my responsibility was to run one or two week training programs for groups from one to several states. This meant that I needed to be especially familiar with computer applications. I mastered this by doing both geological and non-geological studies. Fred Gunther was an ex-geologist who had converted to a programmer working for our contractor. He was my interface with computer studies. But, gradually I started to master the actual manipulation of IDIMS. I did this by asking him or others to show me how to do the processing through the keyboard routines. In other words, I became something of a programmer myself, relying on my own inputting rather than on technical support.

I began to coordinate the training sessions in 1978. I was rather formal in the way I did things: lining up ERRSAC staff as presenters, developing exercises for computer processing, working with individuals from the states on their particular problems. But, I was probably too pedantic and stiff doing these tasks. In late '78, Cressy hired Bill Campbell, who had just received his master's from the Rose-Hulman Institute of Technology in Terra Haute, IN with specialization in remote sensing. Bill had become interested in this subject when he was a senior at Southern Illinois University and had been inspired by reading Mission to Earth to get into the Landsat business. Cressy decided to have Bill work with me. His presentations were much more folksy and casual and he seemed to get through to the state attendees better than I. So, in 1979 Bill took over the Training duties and I was renamed Program Scientist for ERRSAC.

It was during this transition that Bill succeeded in getting Jack Dangermond, one of the founders of the new field called Geographic Information Systems (GIS) and the President of ESRI (Environmental Systems Research Institute) in California, to speak to us at ERRSAC in early 1980. Dangermond had developed his concepts through his association with Ian McHarg, the writer of Design with Nature, a classic that laid the groundwork for the GIS approach. I was much inspired by Dangermond's dynamic overview and have been a "fan" of GIS ever since, despite having never actually mastered the technique or used it practically.

I made several good friends among one of our two-months summer training programs that we held exclusively for college professors. Hugh Bloemer, a geographer from Ohio University, has remained in contact with me up to the present. Al Thompson, of the Geology Dept. at the University of Delaware, was another. Jim Campbell of the University of Virginia developed into a specialist in image processing (he wrote a textbook on the subject) because of his stay with ERRSAC. My leadership in these programs proved invaluable to them, and I learned also, by teaching.

**One of ERRSAC's summer institutes for teachers**

In 1979 one of my duties was to organize, as Chairman, the first ERRSAC Conference, to be attended by more than 150 people from various walks of government. I chose the Tidewater Inn in Easton, MD on the Eastern Shore, a hotel whose history and comfort was ideal for such a Conference (3-days). About 60 papers were in the program (I edited the Proceedings that followed). We ended this October meeting with a boat cruise and catered dinner around that part of Chesapeake Bay, stopping at St. Michels. In sum, the affair was a huge success and really caught NASA Headquarters attention. Here I am as Master of Ceremonies:

**Introducing the program at the First ERRSAC Conference**

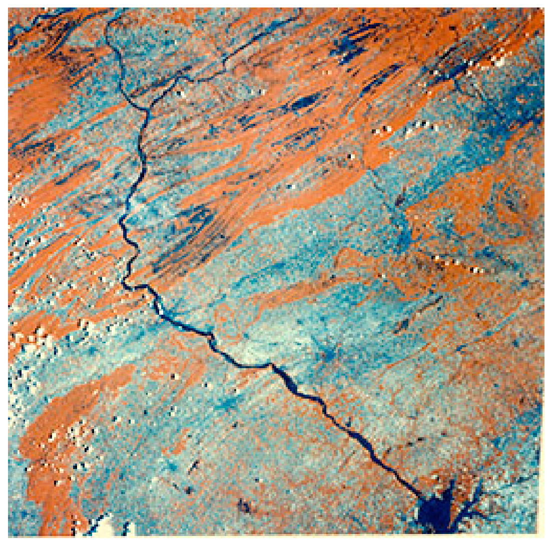
Then, in 1980 I was given funding by Headquarters to put together the first national CORSE (Conference on Remote Sensing Education). This would bring together more than 200 specialists and educators from across the country. I decided to have CORSE at Purdue University, with David Landgrebe and Shirley Davis, of LARS (Laboratory for Agricultural Remote Sensing) as co-organizers. Again, this went off well and I served as editor for the Proceedings. A second CORSE, for which I had no responsibility, was held in 1985.

In 1981, Cressy felt the time had come to have a second ERRSAC Conference. Again, I was Program Chairman. But this time Cressy decided to have a contractor handle the arrangements, site selection, etc. However, a conflict of interest was discover, so, finding a site was "dumped" back onto me. I located an excellent one in the Radisson Hotel in Danvers, Massachusetts. I set up the program and scheduled it for June. Cressy pulled some strings and got us use of the 16 passenger NASA aircraft normally available only to bigwigs (I flew on it after the Second ERTS Conference in Houston). Our departure was to be from BWI at 2 PM on Sunday. But at the last moment, a bigwig from Headquarters bumped us off the plane. We had to drive up in NASA station wagons instead. I managed the meeting well - the Hotel had superb facilities - and chalked up another success.

In 1980 I set out on one of the most ambitious and challenging writing tasks I was ever to take - one which in fact did more for establishing my reputation than perhaps any other endeavor. My idea came from realizing that the agency trainees would get much more out of their ERRSAC experience if they had been exposed to many of the basics of remote sensing before they came to GSFC. This could be accomplished by developing a training manual that had examples of the types of data they would encounter plus some pedagogical aids such as questions and exercises. With such knowledge already mastered they could start right in with the principal reason they'd been assigned or had elected to spend time at ERRSAC - the computer-based image manipulation and ways to interpret their results.

So, I presented this concept to Phil Cressy who talked it over with Jimmy Weber and Bill Alford, and then with the Headquarters people running the Regional Applications Program. Almost at once, they all showed strong enthusiasm for my project, and funding was found to implement it. My efforts would spread out for nearly 18 months, during which I ended up really coming to understand much more of this technology since I had to be its master if I wanted to construct a comprehensive and technically sound training vehicle. In other words, I had to teach myself first in order to organize the essentials of the relevant aspects of remote sensing into a surrogate for the instructions that we would otherwise give on site at Goddard.

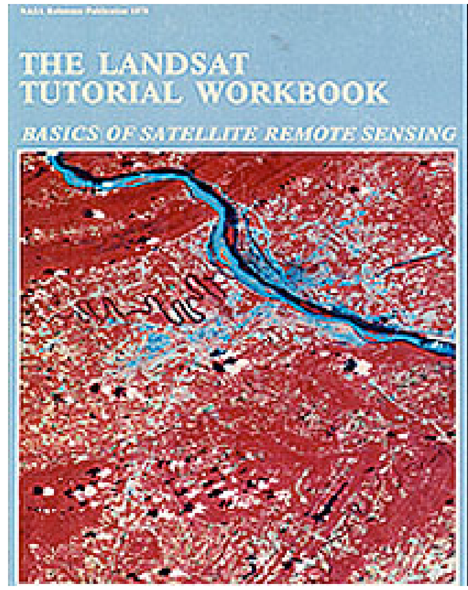
I decided to select a small part of the eastern U.S. for intensive study using various types of sensors and systems. This would normally be the way in which a state would go about using remote sensing products - working in an area of limited size (state or smaller). Thus, I chose as the basic scene Landsat images that included Harrisburg, PA near the center.



This would touch upon agriculture and forestry, land use, environmental concerns, and even geology (the folded Appalachians) and mineral resources (coal). To include more diversity, some study subjects required looking at images adjacent to the Harrisburg scene - from Washington, D.C. eastward to New York City. And to provide the user experience with other parts of the world, and the varieties of land use and environment in those regions, I had an Appendix that included 12 Landsat scenes from the U.S. and elsewhere. To give the user a strong feel for the role of computer processing, I included both a chapter that showed various ways in which the

Harrisburg scene could be processed and another Appendix which delved into image processing principles to considerable depth. To assure that the user was engaged in learning and not just reading the text and looking at pictures, I included many questions right in the body flow of the text which forced the user to think about what was just read or to extract information from the imagery tied to the text. But, contrary to most teaching texts, I had answers to all questions, located in the back of the book (which was page-large and had a paper cover).

To assure that this teaching approach would work, I tried out sections or chapters of the Landsat Tutorial Workbook (LTW), as I named it, on both fellow ERRSAC staff and, when possible, on outside training "students". As 1981 closed, the book reached final form and again was prepared in camera-ready format by Anne. The book, NASA RP 1078, came out from the Government Printing Office in early 1982. I sent copies to everyone who had earlier gone through ERRSAC training, to several Universities where I knew remote sensing education was in full swing, and to many professionals. The book's reception was almost universally favorable, to the extent that it became the prime text at a number of institutions and was referenced in nearly all other textbooks printed after '82. And, as we will see later, the LTW formed the basis for creating a much larger, more comprehensive Tutorial covering almost everything in the field.




**The Landsat Tutorial Workbook - a training manual**

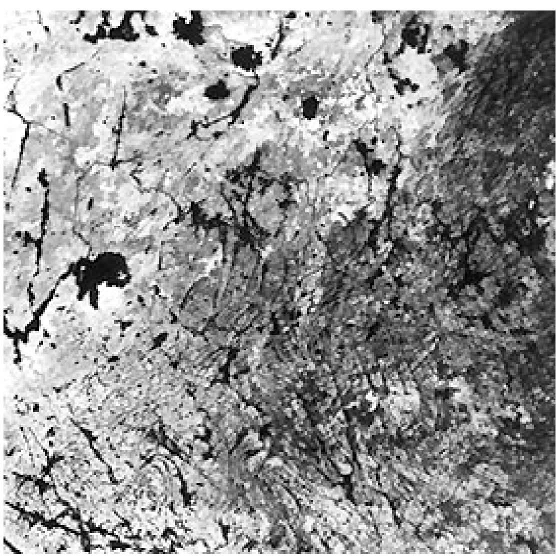
My ERRSAC years were marked by little research and few publications - I was in the "doing" rather than "thinking" mode. In 1977, I did co-author a paper with Bob Summers (ERDA) and Bill Smith (ERIM) on the role of remote sensing in Energy exploration, giving at the 11th International Symposium on Remote Sensing of Environment. Then, I received a request from NASA Headquarters to "ghost write" parts of a paper on "Energy and Aerospace" that R.C. Seamans, Jr (the Administrator of the U.S. Energy Research and Development Administration [ERDA]) had been asked to prepare for the Royal Aeronautical Society. In 1982, I gave a paper, with Bill Campbell as co-author, at the 3rd Conference on Economic Benefits of Remote Sensing on The Role and Cost Effectiveness of Satellite Remote Sensing Technology. Much of the ideas in that paper had been developed earlier when Lou Walter, following a request from NASA Headquarters, had me do a cost benefits study of remote sensing in general - putting me into an area of thought for which I had little background other than my one college Economics course, but by applying common sense and finding scattered information in the literature, I came up with a very reasonable and believable overview.)

I only traveled infrequently in this ERRSAC job - trips to State Agencies were made pretty much by Cressy and his cohorts to set up working arrangements to cooperate with them on specific projects and/or to send trainees to Goddard.

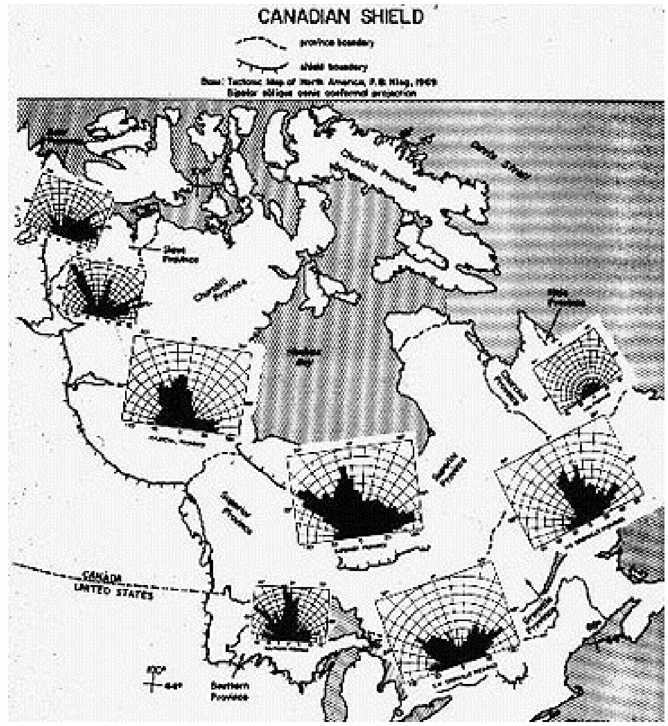
But there was this important exception: In October 1981, I was funded to attend the First Snowbird Conference (Snowbird is a ski resort in the Wasatch Mtns south of Salt Lake City) on the role of impact craters in modifying the Earth's Environment. This was prompted in large part by the claim of Luis Alvarez and his son, Walter, that a huge impact was responsible for wiping out the dinosaurs and much of other life forms 65 million years ago at the end of the Mesozoic Era (known now at the K-T boundary [age discontinuity]: K stands for the Cretaceous Period; T for the beginning of the younger Tertiary Period). Of the many meetings I've attended in my lifetime, none was more exciting and intense than this one. After each paper, 6 to 8 people would line up at the microphone to make comments, ask questions, or just dispute. Schedules quickly broke down, so by consent we continued the program into each night. The topics thus reviewed formed the basis for almost all conversation - breakfast, lunch, dinner, walks around the facility, all were almost exclusively devoted to give and take on the still revolutionary idea that an impact could have pro-found effect on the Earth's history and life. I'd say that this meeting was the turning point among scientists of various persuasions that has led to broad acceptance of the thesis that impact is one of the most fundamental processes in the formation and development of planetary bodies. Impact became fashionable and its proponents were treated as prophets rather than quacks. Although I had been largely out of the field for 12 years, my earlier accomplishments were recognized and I was treated as an expert at the Con-ference. On several subsequent occasions, Walter Alvarez sent me samples of material collected at the K-T boundary to examine for shock effects.

In the summer of 1981, ERRSAC got word that it would be disbanded after the new Fiscal Year. NASA Headquarters had decided, partly from program cutbacks and reduced funding, to cease this phase of its Technology Transfer program. All RAC's were thus closed out. A few ERRSAC people left Goddard but most were just reassigned. In early 1982, Herb Blodgett and I were brought into Code 622 (later 923), the Geophysics Branch. Charles Schnetzler was the Branch Chief at the time, and key personnel included Paul Lowman, Pat Taylor, Herm Thomas, and Bob Langel (he was NASA's expert on geomagnetics - MAGSAT was its prime data gatherer and much of the Branch's work centered on reducing the data to obtain a worldwide map of magnetic anomalies).

I decided at first to concentrate again on lineaments. I dreamed up a project with Paul and Herb to map the major lineaments detected in the Canadian Shield, grouping these by Age Province, and producing histograms of dominant orientations to see if these varied from any one province relative to the others. This was tedious work that occupied me off and on for more than two years. In June of '82, Paul and I managed to convince Schnetzler of the need for a field trip, mainly in the Sudbury area. There we field checked some of the lineaments recognized in Landsat images, verifying that most we had singled out were valid tectonic fracture systems. We concluded that the fractures were emphasized (in the imagery) by glacial erosion and that many were then occupied by lakes (one fracture near the Grenville Front had islands within a long lake that were composed entirely of tectonic quartzite breccias). After this project was finished, we had affirmed that fractures in one province had different orientation patterns (as discerned from rose diagrams) from those contiguous. This was the first ever study of a Precambrian Shield terrain in terms of its regional distributions of fractures. We prepared a summary paper but not until 1990 was it published in a GSA magazine. The Landsat image below shows the type of terrain we worked with. The fracture orientation plots for the entire Shield are a first of its kind at that scale.



**Landsat IR band image of fractures in the Superior (upper) and Grenville (lower) Provinces**




**Rose diagrams (azimuth orientations) of major fractures in the Canadian Shield**

In July of 1982 I was invited to be a key speaker at an Education Conference at Vandenberg Air Force Base, organized by Jim Taranik who was at this time the Geology Program Chief at Headquarters This was tied to the launch of Landsat 4 from a pad near the ocean. The weather was foggy at the time, with a ceiling of less than 500 feet. At the countdown I was quite calm but when the rocket disappeared into the fog bank, I was surprised to find tears rolling down my cheeks. By now, I realized, I was emotionally bonded with the Landsats.

**The launch of Landsat 4**

Lineaments analysis was a voluble topic to study but it didn't really occupy my full day. I needed to pursue research in other areas of geologic remote sensing. One question that concerned me about the value and validity of using such data for rock type identification and stratigraphic distinctions was the accuracy of the results. If a sequence of different rock types were mappable from Landsat or other space data sources at accuracies of less than about 70%, then most geologists would not warm to its usefulness. So, I began a detailed study of accuracy for map units (from published maps as the "ground truth") in the Utah test sites (see page 38) and in areas within California. Under ideal circumstances, units accuracy (correspondence between computer-based classifications of space data and the same units on a geologic map) could reach 90% or better. Under other conditions this could drop to less than 60%. My findings were published in two papers, one given at the ERIM Conference on Remote Sensing in Geology in 1983 in Colorado Springs and the other in the same conference in 1984 in San Francisco. The bottom line: Landsat type data were useful for reconnaissance mapping and could achieve high accuracies if rocks were not covered with vegetation or thick soils, i.e., were well exposed and in suitable structural arrangements (typically, folded units). With the advent of hyperspectral remote sensing in the 1990s, this accuracy has been greatly improved, provided the rocks meet the above suitability conditions.

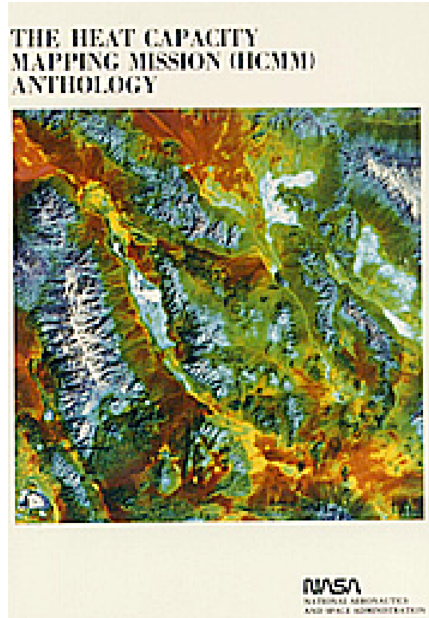
I had not been in the Geophysics very long before I received a request or commission from NASA Headquarters to salvage a major NASA Mission. As part of a program (Applications Explorer Mission series) to cut costs, NASA launched the low budget HCMM (Heat Capacity Mapping Mission) Satellite on April 26, 1978. Its single purpose was to demonstrate how thermal data could improve our ability to recognize materials and features of interest on the Earth's surface. The parameter it was designed to measure is Thermal Inertia, a property that determines how long it takes for a given material to cool under ambient conditions. The program was budgeted at $60 million dollars, with $3.5 million going to 35 Investigators. The worked perfectly and data flowed to the investigations.These groups had done excellently over the next two years in demonstrating the capabilities of this type of satellite (which had to repeat orbit such that it crosses the same ground area twice in a day-nite cycle). Their reports were turned in, and the usual procedure was to hold a conference or get-together to present results and exchange ideas. But, the program had run out of money, so the means to hold a conference was now unavailable.

This was unprecedented in NASA history - a mission without a follow-up conference. What to do? Someone at Headquarters familiar with my efforts with Mission to Earth and the Landsat Tutorial Workbook arrived at the idea of my putting thermal investigation results and a broad overview of thermal remote sensing into a book. I was thusly chosen - never asked, just told to do it. No matter that I had no background in thermal remote sensing. I proved I could write readable technical summaries - so let me learn basics and then be clever.

The proper procedure would be to involve the Program Scientist for HCMM. This was Dr. Robert Murphy, a physicist, who quickly convinced me that he was no longer committed to this satellite or its results. He was uncooperative and wouldn't even read and review the critical chapter on thermal principles. So, I resolved to go it alone. But I did enlist the involvement of Locke Stuart who was the investigations coordinator. He became co-author, in spite of having only written the 11 page Appendix on System Parameters and Performance

Within 6 months I had finished this 264 page book. It came out in late 1982, as NASA SP-465, in a folio sized version that permitted HCMM images (example below) to be printed in the size they were normally sent to the users.

**HCMM Night IR Image of British Columbia showing the different types of geomorphic terrains in the mountains**




**The oversized HCMM book**

My knowledge base in thermal remote sensing had greatly expanded. Perhaps the best compliment given me was by Anne Kahle of JPL, the NASA expert on thermal subjects: "I was pleasantly surprised. Nick did a better job than I had guessed. He has mastered the subject".

I was able to introduce HCMM's accomplishments in a poster session paper on Thermal Inertial Mapping at the Second International Symposium on Remote Sensing of Environment, in Fort Worth, TX in early 1982; a synopsis of this was included in the Symposium Proceedings that came out late in the year. I also had a poster session emphasizing the geologic findings with HCMM imagery at the 1983 GSA Meeting in Las Vegas.

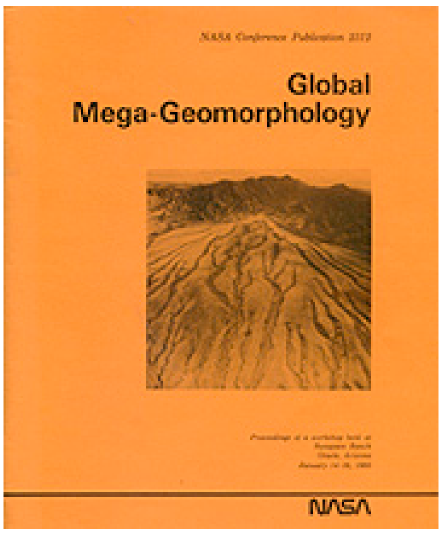
In 1984 I diverted my attention to another aspect of Landsat imagery that I had always felt was a primary payoff in its contribution to Geology and Geography: its ability to display in a regional context the various larger scale geomorphic features of the Earth (as had already been demonstrated and accepted as the principal means by which other planetary bodies were being described and analyzed from imagery acquired by space probes). This approach had been largely neglected by the geological user community which tended to focus on geological problems that had economic significance (as in finding oil or minerals). Here was a uniquely scientific utilization that warranted a proper place in the study of Earth from space.

This interest was rekindled when a summer intern was assigned to me in 1983. He was Prof. Robert S. Hayden of the Geography Program at George Mason University in Virginia. Bob had a remarkable background. A large, rather heavy, jolly individual, he was an ordained Episcopal priest who had done parish work for almost 20 years, then left the active church to go into teaching. Landform studies had become a specialty with him. We decided to devote our summer to collecting Landsat images that strikingly illustrated a variety of geomorphic attributes. By summer's end, this collection had become impressive. Because he came from nearby, we stayed in touch and he would frequently visit. He applied for a second summer, returning in 1984. By this time I had conceived of a new research program designed to call attention to the benefits of space imagery for landforms characterization and analysis. I found a sympathetic supporter in Dr. Mark Settle, the Geology Programs co-ordinator at NASA Headquarters who had similar feelings about this disregarded field that so obviously was a beneficiary of space imagery. Together, we inaugurated a master plan to raise the status of *Regional Geomorphology* among the geological community by showing how space imagery fostered a whole new look at the Earth's landforms hitherto not being addressed since only aerial photos and maps had been the customary data sources. The strategy, which he backed up with funds, would be 1) organize a conference or workshop on the subject we began to refer to as Global Mega-Geomorphology, and 2) produce and publish a picture book type of Atlas showing the different kinds of landforms that are well displayed at space image scales. Bob and I started right in to meet both objectives.

Item 1 culminated in a four day Workshop on Global Mega-Geomorphology at the Sunspace Ranch conference facility at Oracle, Arizona, about 30 miles north of Tucson. This took place on January 14-17, 1985 in ideal winter weather. The host coordinator was Dr. Victor Baker of the Geology Dept., U. of Arizona. The attendees (for all of whom we paid expenses) were a Who's-Who of Geomorphology. In total, there were 31 participants. As our keynote speaker, I chose a friend, Dr. Kathryn Sullivan, the first woman astronaut to walk in space, and a topflight geologist who photographed landforms during her two space missions.

The format involved presentations of 24 papers, each a half hour long in the 3 mornings at Oracle. In the afternoon the participants split into four Working Groups discussing these subjects: 1) Global Geomorphology; 2) Landscape Inheritance; 3) Process Thresholds; 4) Planetary Perspectives. A Panel Group at the end discussed a variety of topics. The fourth day was a field trip around the Santa Catalina and Santa Rita Mountains, led by Vic Baker. All presentations and the Working and Panel Group summaries were collected into NASA Conference Publication 2312, edited by Bob Hayden, and distributed widely to U.S. and International University

Geology/Geography Departments. Consensus as the meeting ended was that this Workshop was hugely successful and laid the groundwork for incorporating space data in studying landforms at the regional level. But, sadly, I have never since been shown evidence that this has happened. The Conference was summarized in a Proceedings volume, NASA CP-2312:




However, I also came away with a mandate, and dollar support, to put together a book length NASA Publication, to be entitled *Geomorphology from Space: A Global Overview of Regional Landforms.* This would take two years to complete, but once out would be highly praised for what it turned out to be: a pictorial atlas glamorizing landforms as conspicuous features that made Earth's surface as interesting as those of the other planets, and, because of the comprehensive captions written for the image plates, a synopsis of the Earth's global geology unlike any of the few such books previously written.

I was joined in this effort by Dr. Robert Blair, Jr., Chairman of the Dept. of Geology at Fort Lewis College, Durango, CO. Like Bob Hayden, who continued to participate in our endeavor, Rob Blair spent two summers with me as a NASA Summer Fellow. We decided that the best way to proceed would be to designate chapter themes, such as Tectonic Landforms, Aeolian Landforms, etc., and then enlist the professional geomorphologists who had been at the Workshop to write individual chapters based on images we would supply. We further decided to have each image from space supported by 3 or 4 ancillary images taken on the ground or from the air.

This last requirement proved to be the hard part in putting the book together. Most images were outside North America and supporting photos could be difficult to obtain. But a partial solution soon presented itself, and I got travel funds to take advantage of this.

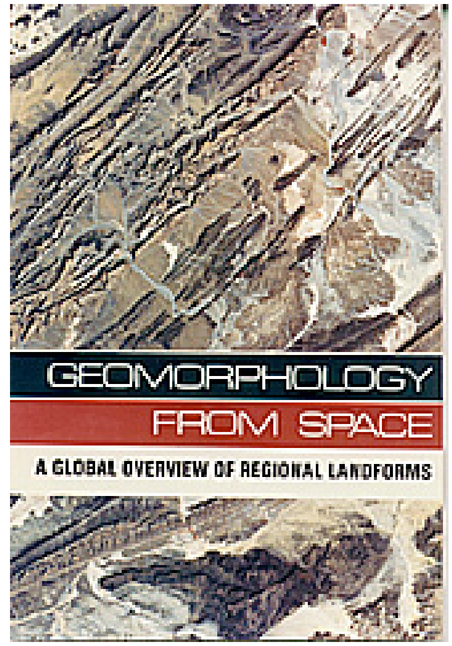
Two of the Workshop attendees, Cliff Embleton and Ian Douglas, were among the co-organizers of the First International Conference on Geomorphology, held in Sept. 1985 at the University of Manchester, England. More than 500 experts would be there. I was specifically invited and asked to review the results of the Workshop. I also asked for special time on stage in the Opening Ceremony to plead with the attendees for contributions to *Geomorphology from Space.*

That evening was the formal Opening Ceremony. I was told that I would be on stage with the notables. When I unpacked my clothes, and took out my dress sportscoat, its front buttons were missing (the cleaners forgot to re-attach them). Ellie was appalled by my dishevelment. And she really hit orbit when I was seated next to the famous Duke of Devonshire, whom I met and talked with. During the program, I was permitted to address the full gathering about the Geormorphology from Space project.

Vic Baker and Rob Blair were dumbfounded to see me at the podium. But my appeal got results, as more than a dozen attendees sent me ground photos later.

**Podium speakers (NMS on left; the Duke second from right)**

By late 1986, work was nearly done in getting Geomorphology from Space ready for the printer. The effort was behind schedule, largely because Art Bloom of Cornell and Oscar Huh of Louisiana State University had been remiss in meeting deadlines for their contributions. But, once again with Anne Schmidt's help (she really enjoyed working with me because the subject matter was so interesting), we had camera ready copy in the hands of the Government Printing Office by January '86. Even more than Mission to Earth, good reproduction of photos and images was critical. The job GPO did was OK, I guess, but not outstanding. Still, the book was a beauty - the best thing I had ever "honchoed". Reviews were all positive to glowing. The book eventually sold out its printing. When I inquired a few years later about a second printing, I was told that GPO had destroyed the plates.




**Geomorphology from Space: NASA SP-486**

But over the years demand for the book continued to besiege NASA. A very surprising solution was applied. Without any prompting on my part, the Jet Propulsion Laboratory decided to fund a conversion of the book into a CD-ROM. This was a first for them: never before had they gone outside of JPL to sponsor a CD. Efforts began in 1995 to develop the CD, with the bulk of the work being done by Carla Evans and Blanche Meeson of the Goddard DAAC facility (JPL's role was mainly funding). To improve quality, they scanned many of the original photos/images I had in my possession at home. The resulting CD is a remarkable reworking of the book. I'm told several thousand CDs have been sold since its first release in 1996. The book and its CD companion have become a classic! For example, this Landsat image of western South Africa shows in a regional panorama the famous pediplanes cited by Lester King in his theory of landforms development through successive uplift-erosion cycles.

**Landsat image of four erosion surfaces in South Africa**

Two changes at NASA happened in 1986; both would affect me over the next three years. First, the Geophysics Branch got a new Chief - Dr. James Hertzler, a world class scientist known for his work on geomagnetics applied to sea-floor spreading. Jim would be a much needed driving force that reactivated some flagging programs. However, his interest in remote sensing seemed rather lukewarm, so people like Paul Lowman, Herb Blodgett, and I didn't benefit much from his attention. Second, there was still another turnover at Headquarters, with a new Geology Program coordinator, Dr. Peter Moguinnes-Mark, who specialized in volcanology and did appreciate remote sensing. But he proved to be quite partial to the JPL program, giving it abundant support, while short shrifting us at Goddard. He stayed only two years before relocating at the University of Hawaii, being replaced by a lady geologist whose name escapes me.

But, before Mark Settle left, he encouraged me to follow up on the interest we'd bestowed on Geomorphology. He felt that our profession still had to demonstrate that useful information was extractable from imagery that would lead to new insights about landforms. He suggested I devise a test experiment which would show the way. I thought for a while, decided that Tectonic Geomorphology offered the best promise and was also in one of my fields of expertise. By simple pondering, I came up with a smashing idea: Various mountain belts of the world were even then being shown to consist of separate blocks or <u>terranes</u> - island arcs or detached continental pieces that would be carried by sea-floor spreading up against continents at subduction zones, but would not be carried under, instead being forced by obduction to weld onto the upper crust. Since each terrane had its own distinctive lithologies and point of origin, it seemed logical that grouped terranes the geomorphic expression of each (controlled by its structure and rock type) might well differ from its neighbors. From this hypothesis, I arrived at the concept of <u>Terranes as Terrains</u>. I started to read avariciously through the literature to see if anything had been reported along this line. Negative. Then, I identified two terrane clusters in North America accessible for onsite study: 1) the Appalachians, and 2) the Klamath Mountains of southwestern Oregon. I chose the latter because it was the "type locality" of the inception of the terrane concept and because it was confined to a much smaller area compared with the Appalachian Piedmont. I began to assemble Landsat imagery of that part of Oregon and adjacent northern California. I had already decided that there was strong evidence in this imagery that the landforms in various terranes had distinct signatures (elevations, ridge orientations and spacing, etc.) that were strongly different visually, so that a combination of field study and measurements of parameters in the imagery and topographic maps seemed likely to give me positive (and publishable) results.

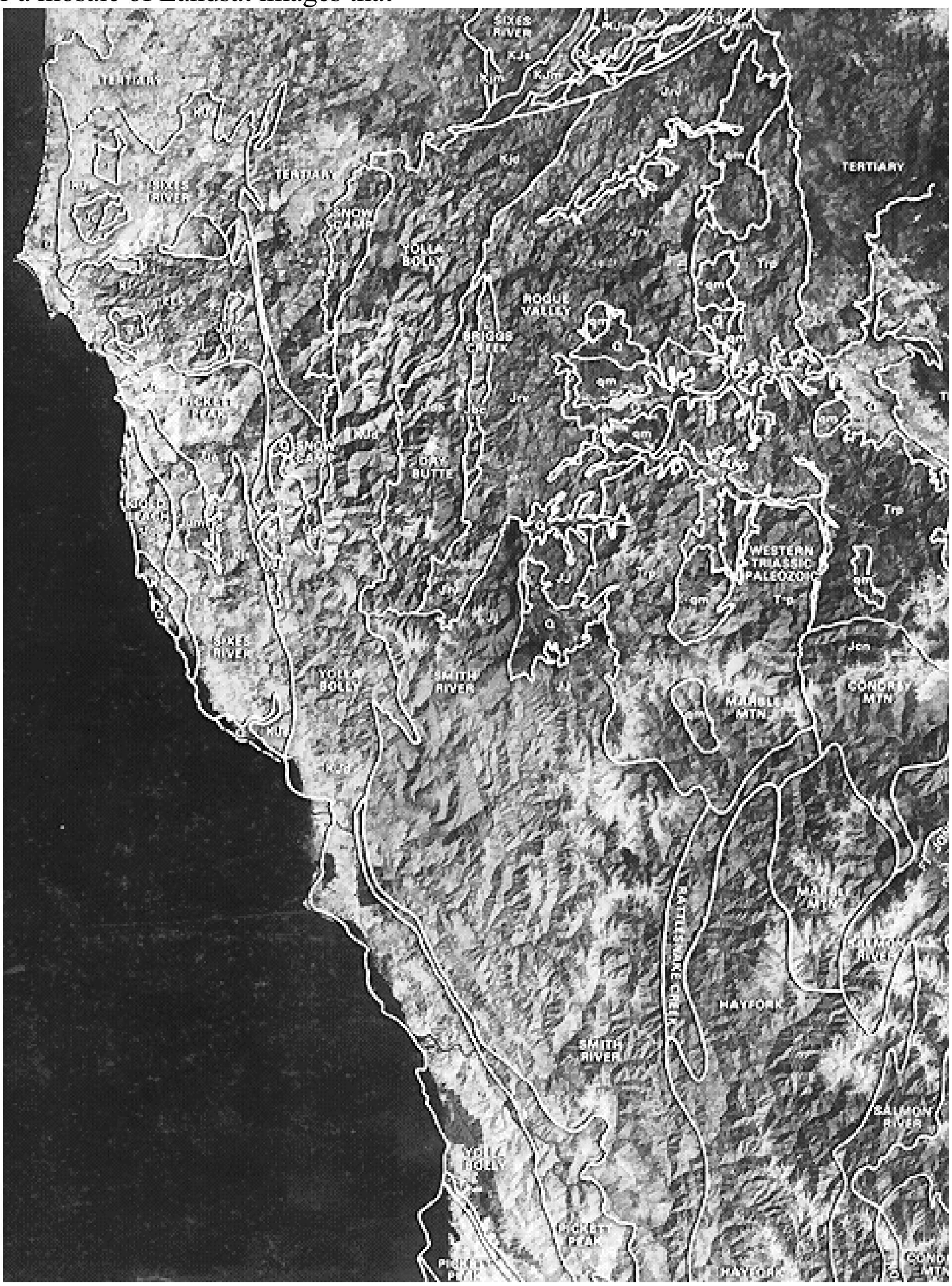
The project looked promising but to understand what was happening and make my results scientifically valid, I needed to visit the region and study it in detail. That was too much for a typical 1-2 weeks travel request. Charlie Schnetzler suggested that I apply for a sabbatical - as a senior scientist, I would likely qualify for one. I did, asking for 3 months (which meant that on return I was obligated to stay with NASA for 4 x 3 or 12 months). My choice as base of operations was between U. of Oregon and Oregon State. At the latter, I knew both the Chairman of the Geology Department, Bob Yeats, and Chuck Rosenfeld, the staff remote sensing specialist (officially, the State Remote Senser; he had done the supporting work for the guru known as the Bagwham who was eventually evicted from Oregon), who was associated with the Geography Department. We chose OSU in Corvallis; this proved just the right choice because Corvallis turned out to be a great town and we found a superb low-cost "rental", essentially house-sitting for a university family on a summer sabbatical.

For the first six weeks I put in a 5 full-day week in a small office in the Geography Department's floor. Much of my efforts were devoted to extracting various kinds of topographic information from published topographic maps. These were then investigated largely by statistical studies. Sometimes I used the facilities of Rosenfeld's Remote Sensing Laboratory which was quite sophisticated, but he had no Landsat computer tapes or other data/imagery of the Klamath Mountains that I could analyze by image processing. Still, by mid-summer I was picking up some meaningful information just by number crunching the topo data.

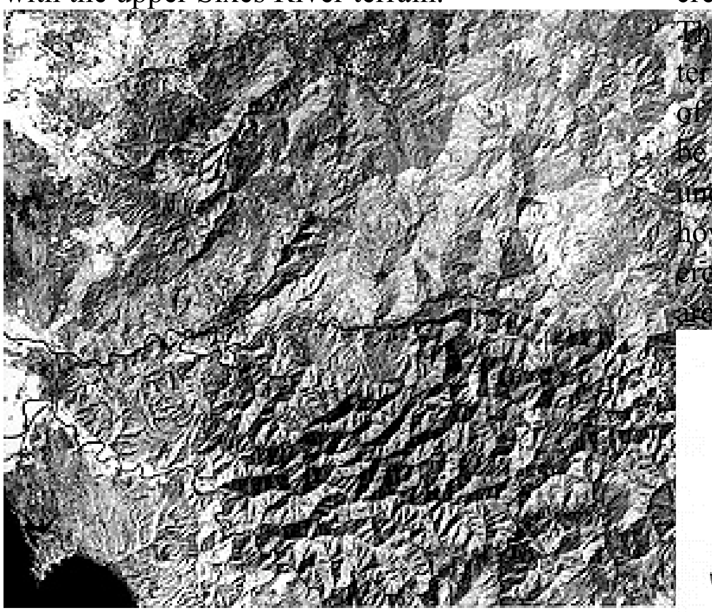
Towards the end of the summer, I was approached by the Geography Department Chairman, Tom Maresh, about the possibility of joining their staff. The arrangement would involve my being almost self-supporting through soft money from NASA and other sources. In time, a professorship might be offered. In 1987, I would have enough years at NASA to take early retirement which would supplement my remote sensing income. Needless to say, I found this offer tempting but my wife was concerned that the sum of dollars would be too small for us to live comfortably and securely. I politely rejected the offer. In retrospect, as Corvallis has grown into one of the prime living areas in the West, I now wish we had gambled, since both of us loved this Oregon life, and I always had landed on my feet in the past.

While in Corvallis I spent most of my time using topographic maps of the Klamath mountains to analyze different parameters. Many measurements suggested that there were statistically significant differences in mean elevation, density of ridges, orientation of ridges, and other indicators of geomorphic expression. While in Corvallis, the flight over a part of the Klamaths by the French SPOT satellite took place but I didn't receive the image pairs (for stereo analysis) I'd ordered until after I returned from Oregon. Probably the highlight of the summer was the week spent in the field, where I ground-truthed the general variations in landforms characteristics; I was accompanied by a graduate student from Afghanastan who had been a freedom fighter against the Soviets.

Most of my NASA time that first year back from Oregon involved doing more analysis of the Klamath terranes. I put together a mosaic of Landsat images that clearly indicated visually that the different terranes had distinctive geomorphic differences:

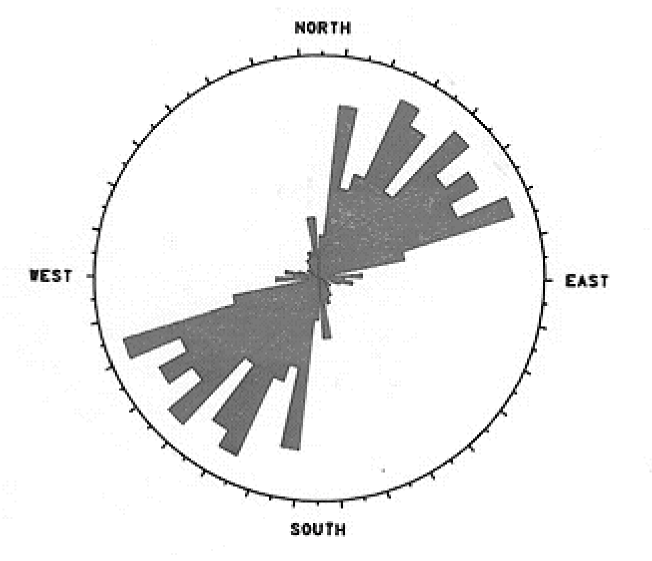



Two terranes - the Elk and the Sixes River - proved to be the most definitive evidence for differences which demonstrated adjacent land units had such significant variations in landforms that one could argue these had been added to the continent at different times. The top of the next page contains part of a single Landsat image in which the lower Elk terrane is clearly different in the nature of its terrain compared with the upper Sixes River terrain.



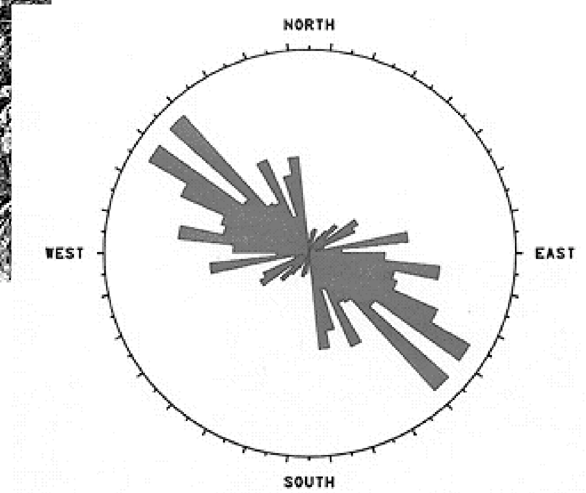
**The Elk Terrane (bottom) and Sixes River Terrane (top)**

Most striking is the differences in orientation of the ridges (which also varied in terms of average elevations and densities).




LINEAMENTS FROM TERRAIN: sixes river 6 degrees

This rose diagram showed a pronounced northeast trend of the lines tracing the crests of ridges. Now compare this with the diagram for the Elk terrane crests appearing at the top of the next page. That trend is strongly northwest. If the two terrane units had both been present as part of a single land mass, the orientations should be approximately the same. Instead, the underlying rock units fabric which controls how the terrains are developed during erosion are so different that the two terranes are dissimilar in time.

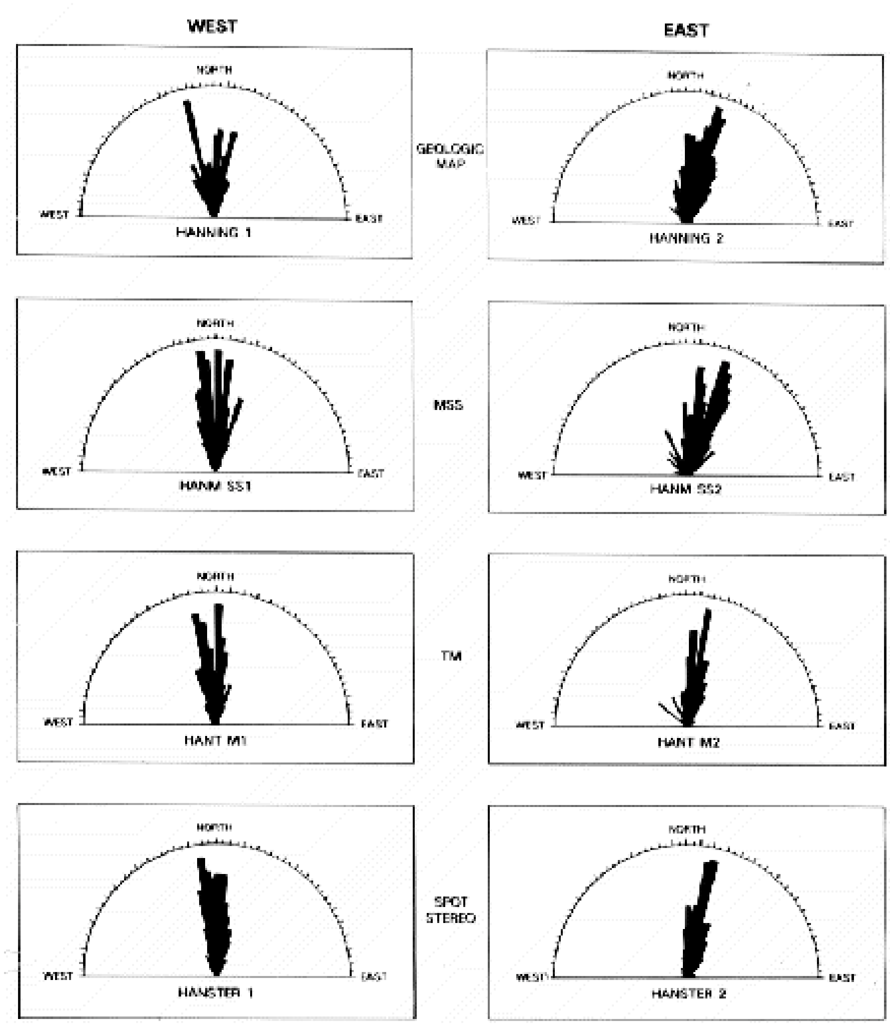



LINEAMENTS FROM TERRAIN: elk 2 6 degrees

By November of 1987, I had accumulated enough intriguing information to give the Klamath results as a poster session at the Annual GSA meeting. But I was unsatisfied with the breadth and scope of what I had learned, to the extent that I have never published the study in the literature. In particular, using the Landsat and SPOT images alone did not provide any startling new information about the terrane distribution or the geomorphic characteristics of each. To extract meaningful information required using the space imagery in stereo. We were not equipped to analyze stereo topography at Goddard, nor was there adequate funding to acquire a large number of SPOT stereo pairs. To this day, though, I think much more lies inherently in space imagery as a tool for terrane analysis.

Branch Chief Jim Hertzler was under pressure to get more remote sensing type work underway in the Geophysics Branch. He had always been interested in the East African Rift Zone. He devised a study that would involve Paul Lowman, Herb Blodgett, and me, and obtained funding and also brought in an expert who had worked on the Rift Zone for a year's Fellowship in the Branch. My enthusiasm was tempered by the realization that no matter what I found that was new or useful, travel money to visit sites in Africa simply wasn't there (this fundamental flaw in doing remote sensing science without going into the field to check out the study sites remained my major dissatisfaction with my NASA work; after I left NASA in 1988, beginning in the '90s, there was a major shift in NASA travel policy that allowed government employees to accept travel funding from private or other government organizations, provided that it did not come directly from funds supplied by the NASA individual through grants, etc.).

I decided my contribution to the Rift project would combine my geomorphic interests with my experiences in analyzing lineaments as guides to tectonic control. I conducted a survey of lineaments orientations determ-ined in Landsat and SPOT with studies previously made on the ground and with aerial photos. What I determined was that in the Rift, the space imag-ery produced orientation plots (rose diagrams) of accuracy comparable to these other sources of information.  These results were positive enough for me to summarize my studies on accuracy of mapping from space in a poster paper given at the ASPRS/ACSM annual meeting in Baltimore in 1989.




**Plots of ground mapped Rift Valley fault trends (top) (in Kenya) showing that almost almost identical orientations are identified in Landsat and Spot images**

In August of 1987, I went on a totally unexpected trip to Europe. The Austrian government was sponsoring an International Symposium on Remote Sensing in honor of their once-citizen Bill Nordberg. It was held in his home town of Graz. At the suggestion of Hugh Bloemer, I was invited (with all expenses paid) to be one of the keynote speakers (chosen, I think, because of my dedication of *Mission to Earth* to Nordberg). At Graz, I gave my speech, praising Nordberg and describing the achievements of Landsat, then listened to the other papers, fortunately most in English.

I had made arrangements to meet a professor at the University of Gottingen who was interested in my geomorphology work. While in Graz I learned that Bloemer was driving to visit relatives in Hamburg and would be glad to drop me off as Gottingen was enroute. We drove through northern Austria into Germany, past Regensberg, and then Hugh took a wrong turn going north. With the map on my lap, I soon realized we were heading directly for the Fulda Gap (heavily militarized) in East Germany. Got to within 10 miles of the border. We cut west across back roads until we encountered the Autobahn that passed Kassel, and then detoured to Gottingen, where I arrived around 8 PM. My one day meeting with the professor proved mutually enlightening. Had dinner at his house, spent the next morning there, then was put on a train that ultimately went to Munich. I was supposed to get off at Schweinfurt and catch another train west to Frankfurt, but no one told me that and I missed the conductor’s announcement (in German) about the transfer to Frankfurt (only a few got off - a tip). By the time I reached Wurzberg I knew I had made a mistake. Had to backtrack and catch a later train, so I didn’t reach Frankfurt and my hotel until late in the evening. The flight back was routine.

In March of 1988 I was contacted by a George Atamian of the Bushnell Corp. in San Dimas, CA (near LA) about preparing a “toy” or juvenile educational kit that would be a part of their Science series. These are boxes containing a pamphlet and various low cost items used to explore some subject, like astronomy or the environment. They are sold in Toys’R’Us and similar stores. He wanted to develop one called Satellite Science and I was recommended to produce the input. For this I received $5000, a pair of fine Bushnell binoculars, and a super telescope (which I still use for my birdwatching hobby). As I progressed, he asked me to come to their office for a conference. While there, it was decided that my writing was too technical, so they hired a grade school teacher to simplify it, which she did while screwing it up technically. Satellite Science was completed, released in 1989, did well in toy stores, and then Bushnell soon after pulled the plug on their entire educational project. It remains, however, unique - the only space imagery kit made exclusively for young people.


**The Bushnell Satellite Science “Toy”**

In early April of 1988, one of my colleagues in the Goddard Geophysical Branch, knowing that I had always wanted to go into teaching, came to my office with an issue of a Geophysics magazine that had a section on academic positions open for hire. There was an ad that fitted me perfectly. The Department wanted someone to start a remote sensing program; the position description also mentioned that knowing Planetology would be a plus. The school was one of the State Universities of Pennsylvania, Bloomsburg University on the Susquehanna River about 45 miles southwest of Wilkes-Barre. So, after getting a reluctant OK to apply from my wife, who really didn’t want to move, I sent my Vita to them.

What I didn’t know when I applied was how this position came about. Bloomsburg University’s (B.U.) Department of Geography and Geosciences (G&G) had lost a position through the retirement of its Astronomer; B.U.’s Physics Dept. had just hired an Astronomer. But, when the B .U. Vice President had gone to a professional meeting elsewhere, she saw a display of remote sensing technology that much impressed her. She decided this subject belonged in the B.U. curriculum and G&G was the logical place to locate the person they would hire. So she authorized the position and, though late in the academic year, the ad was inserted in several journals, including JGR where Taylor had found it.

My background was so much more complete and rounded than any other applicant that, on paper, I was far and away the leading candidate. My age however indicated that I would not stay long but could get the program underway, and probably could bring in outside money to build it up. So, they had me come to the camplus. My interview was during the last week of classes. I gave a stellar presentation which proved I was a top expert. I told the faculty in a joint meeting what the remote sensing course should be like. It needed to be organized around computer processing. As prequisites the students should be upperclass-persons, should have some math and definitely some physics, but could include people from beyond the G&G students, i.e., from anywhere in the University. They agreed to this. My interview with the A & S Dean, went smoothly and he concurred that I should get $25000 off the top for equipment, etc.

And sure enough, it was proffered just a few weeks later. The rank was at the Associate Professor level, definitely too low since several previous offers (e.g., Kent State) were at a Full Professor. Nor was tenure included. That alone should have tipped me off and elicited a rejection, but I suspected that the State System of Higher Education (SSHE) would not authorize a full professorship (that surmise proved wrong; if I had demanded it, they would have likely come up with it - again another mistake I made since I was overanxious to break back into teaching, realizing that my age would preclude almost all other opportunities). But I had tired of working at Goddard and the situation in 1988 there was grim in outlook, since geological remote sensing was not well supported (JPL "uber alles"). I wanted to cap my career by ending in the academic world which had been my original goal.

The Branch threw a quasi-retirement party for me (again - double jeopardy. I received several splendid gifts but none more so than two pictures. The first showed Mount St. Helens and had some of the ash beneath the glass. The second, put together by Malcolm Tarlton, the young giant of a man who had been my draftsman and illustrator on several of the NASA books, was a montage of miniature photos of the dust jackets of all 7 books for which I was responsible. A great going-away. But this time I would continue to stay closely in touch with most of my colleagues. And of course Nick, Jr. was working there, as a computer specialist in Bill Campbell's Information Sciences Branch.

It was really difficult for me to leave Goddard after 21 bountiful years. But at least I can gaze on it from above using this IKONOS image (at 4 meter resolution):


**Goddard as viewed by the IKONOS satellite**

Just before we were to leave for Bloomsburg, I learned that the promised $25K had been lost - the University had a half million dollar over-run. To offset the loss of the $25K, I wrote a proposal to NSF. It didn't get funded (largely, I think, because so much of it was for basic equipment; NSF reviewers usually discount any proposal that has to start from scratch when basic equipment is lacking). I tried to be given some support from the Bloomsburg University Foundation, but they failed to come through. Thus, I entered the second semester, during which I was to give the Remote Sensing course, with real concern because the crux of the subject was so heavily tied to computer-based analysis. I had 12 students in this first class. None, repeat none, came from the Geology program. I soon learned that not one student had ever taken a Physics course, and their mathematical backgrounds were fair to dismal. I taught the course using 100s of my 35 mm slides and abundant imagery from the extensive collection I brought with me from Goddard. That was a third rate remedy. Even after I wised up enough to "dumb down" the course, the student response to the tests was way below my expectations. In effect, I failed to reach most of the students (several were smart, and gained useful knowledge from the course). When I came to giving final grades, 5 students had such low exam scores that I had to give an "F". This had obvious repercussions: word got out that this was a hard course and one's GPA would be lowered. Thus, in the second year I had only 8 students.

The second regular academic year went quite smoothly in the Fall Semester, but things really went awry in the Spring of '91. After two years of promises, nothing had been done about getting me anything for the Remote Sensing course. I was getting disgusted just using 35 mm slides. I asked the Department Chairman to push the administration to fulfill their latest promise of funding, now much less than $25000 since computers has dropped way down in price and I had located an excellent computer software package, Idrisi, suited for training in image processing. The semester started with no sign of action. I commenced the course, promising the students we'd get at least one computer/software system fairly soon. Nothing through mid-March. I finally decided to take matters in my own hands - the Chairman wasn't making the effort to meet my deadline. I asked for an appointment with B.U.'s VP and Provost. Sat down with her, explained the problem, and received an immediate commitment for the money, using a discretionary fund. Go ahead and order, she said.

So, at last I got the money, placed the orders, and the computer arrived 10 days before the class ended. I spent all week end learning the image processing program, then gave a demonstration on the final Monday and set up times for each student to have 2 hours on the computer. That went fine until one student did something wrong and crashed the system for the rest of the week. This whole thing left me disgusted. But I am happy to report that my successor, Dr. Michael Sheperd, eventually got 10 computers and other equipment to set up the lab I had myself sought; the State Higher Education System had voted in 1994 to add an equipment fee to all student tuition, which now gave B.U. $500000 a year for such necessities.

In 1992 I elected to retire again - permanently. But, this did not mean I would terminate my professional activities. For about a year, I worked on petrographic studies of the Manson (Iowa) impact crater (32 miles wide; buried by glacial drift) which, in the 1960's, I had proved conclusively was caused by a collision with an asteroid - using shock metamorphism as the evidence. Manson was then a candidate as the "killer crater" because its 65 million year age coincided with the demise of the dinosaurs. Later it was redated at 73 million years and fell out of contention. I was able to produce a paper covering these shock features, published in a Geological Society of America Proceedings on the recent studies of the Manson structure.

From 1993 to Fall of 1994, I was given the honor of being a Sigma Xi (Science Fraternity) lecturer. Most of my talks (at 10 universities and several industries) were on topics related to remote sensing. At one, Northern Iowa University, I was a visiting resident scholar for a month.

In mid-1995 something happened that was to become my dominant activity for the next seven years - serving as a tonic or purgative for my health by keeping my mind so preoccupied that I wasted little time (with one exception in 1997) in thinking about my aches and pains. All this came about through an idea that Nick, Jr. came up with which has led to my "magnum opus" in my professional career.

Nick was still at Goddard in 1996 when he first thought of this: a number of times he, because his name was identical to mine (when he omitted the "Jr."), was contacted by outsiders asking for a copy of my out-of-print *Landsat Tutorial Workbook*. Naturally, he couldn't comply. But, after realizing that there must be some strong interest "out there" in obtaining NASA-sponsored training documents pertaining to remote sensing, he broached the subject to Bill Campbell and together they came up with the notion that I perhaps should revise and update that Tutorial and put it on the Internet. Thus was born the idea for **The Remote Sensing Tutorial (RST)**, a massive work with hundreds of online pages and more than 1000 illustrations, many in color (far more than could be practically included in a textbook on that subject which is usually limited in the number of color plates since the copies printed would be too small to carry the costs of color work. I decided as time went on to make the Tutorial far more comprehensive, with it embracing not only terrestrial applications but the planets and the Cosmos as well. It would eventually include a downloadable image processing program called PIT and would have many innovations, including links to other Web sites, that work only when online.

The address for the RST is *http://rst.gsfc.nasa.gov*. Going into the year 2003, I am still maintaining and updating the RST. Working through the contractor to Bill Campbell's Code 935, Global Science and Technology, Inc. (GST), I have earned more than $80000 in "consulting" fees since 1996. Work began in May of 1996 on the first parts of the Tutorial. Although not now being funded, I continuously update the RST.

Here is the Table of Contents, which amply describes the RST's subject matter and scope; below the T of C is the logo one sees when going to the Web site or using the CD-ROM:

**Foreword**
**Overview of this Remote Sensing Tutorial; "Getting Acquainted" Quiz**
**Introduction to Remote Sensing: Technical and Historical Perspectives; Special Applications such as Geophysical Satellites, Military Surveillance, and Medical Imaging**
**Section:**
**1. Image Processing and Interpretation: Morro Bay, California; First Exam**
**2. Geologic Applications: Stratigraphy; Structure; Landforms**
**3. Vegetation Applications: Agriculture; Forestry; Ecology**
**4. Urban and Land Use Applications**
**5. Mineral and Oil Resource Exploration:**
**6. Flight Across the United States: Boston to San Francisco; Quiz; World Tour**
**7. Regional Studies: Use of Mosaics from Landsat**
**8. Radar and Microwave Remote Sensing**
**9. The Warm Earth: Thermal Remote Sensing**
**10. Aerial Photography as Primary and Ancillary Data Sources**
**11. The Earth's Surface in 3-Dimensions: Stereo Systems and Topographic Mapping**
**12. The Human Remote Senser in Space: Astronaut Photography**
**13. Collecting Data at the Surface: Ground Truth; the "Multi" Concept; Hyperspectral Remote Sensing**
**14. The Water Planet: Meteorological, Oceanographic and Hydrologic Remote Sensing**
**15. Geographic Information Systems: The GIS Approach to Decision Making**
**16. Earth Systems Science; Earth Science Enterprise; and the EOS Program**
**17. Use of Remote Sensing in Basic Science Studies I: Mega-Geomorphology**
**18. Basic Science Studies II: Impact Cratering**
**19. Planetary Remote Sensing: The Exploration of Extraterrestrial Bodies**
**20. Astronomy and Cosmology: Remote Sensing Systems that provide observations on the Content, Origin, Development of the Universe**
**21. Remote Sensing into the 21st Century; Outlook for the Future; Final Exam**
**Appendix A: Modern History of Space**
**Appendix B: Interactive Image Processing**
**Appendix C: Principal Components Analysis**
**Appendix D: Glossary**


**The Remote Sensing Tutorial Logo**

As the reader may deduce, the contents above pretty much draw upon and summarize most of the subject matter in which I have specialized over the breadth of my career. It is thus a capstone project.

The RST has at least 15 mirror sites around the world, has an average of 5500 hits per month, is very frequently referenced and cited, and has been adopted and utilized by various universities and the Chinese Science community at Lanzou and the Ukrainian Space Program. It is also part of NASA's Education Initiative.

In 2000, it was nominated for the International Pirelli Award ($25000) for the best science or technology Web site. It didn't win, possibly because, unknown to me, about 40 illustrations had not been properly transmitted to Italy online (a condition I missed because my download capability is time-restricted by my slow modem, so I did not uncover those bad pages until too late). Meanwhile, it became available in a CD-ROM version, which has sold moderately well.

One of the benefits I obtained from Goddard and GST was a Gateway Computer that was state-of-the-art in 1996. This allowed me to construct the layout and image placement at home. A local computer specialist, Bob Rush, helped me for a year or so in this work. He taught me enough about using an ".html" marker language to construct the pages that I became self-sufficient in making new pages or additions. My problem (and it was often a frustrating one) was that Code 935 elected to have my inputs placed online from down there at Goddard. First, I had to rely on a computer specialist (with whom I had worked in my last years at GSFC) to get material up online. This could drag on for days, even weeks. Then, that task was shifted to GST. Finally, in July of 2001, I received the OK from Bill Campbell to directly access the Branch Server computer, using WS-FTP to transmit the changes onto the Web site. The RST currently is being managed by John Bolton, director of Goddard's Remote Sensing Educational Outreach Lab.

To recapitulate: the RST is the largest literary undertaking in my more than 5 decades of scientific endeavors, has been both satisfying and fun to create, and can justifiably be considered the quintessence of my long career. It has kept me very busy through much of my early retirement years. It has also wedded me to being an Internet jockey and has proven that, despite my initial fears in the '80s, I can master computer use.

So, to those readers who've gotten this far - welcome to the summation.

By the Lord's grace, at 75 I am still active professionally by virtue of the Remote Sensing Tutorial. We have now been in Bloomsburg for 14 years, the longest that either my wife and I have ever lived in the same house. Since coming up here from the Washington, D.C. area, the Landsat TM sensor has produced this lovely image (subset) of these Pennsylvania mountains and rolling farm lands (Bloomsburg in center):

**Bloomsburg on the Susquehanna River**

As I look back over my career, one conclusion is certain: My initial intentions for specialization that persisted through the Ph.D. did not follow the script that was presented when I entered the work force. I had expected to be a *conventional geologist* with my roots in academe. Instead, I moved first into applying geology to underground nuclear explosions, then turned my research towards impact craters (at that time not yet fashionable), followed by directing my studies into Planetology, and ultimately spending the bulk of my career in another new field: Remote Sensing.

When people ask what I am professionally, I often describe myself as an EXOTIC GEOSCIENTIST. But, that phrase might belie the fact that all along I've used those conventional teachings and skills inculcated during my college years and for a short time thereafter. The lesson I pass on is simple, just this: Become expert and diversified in as much of the "regular" subjects you're taught, for you never know if "exotica" is your future.

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