Lloyd Pray, 1956.

Interview with Lloyd Pray

In the field, late 1950s.

conducted by John Fournelle on June 8, 2001
and transcribed by Anna Bradshaw

Born in 1919, I grew up in northern Wisconsin, on Lake Superior. From 1929 on, my family had a little cabin on a bay on the north side of the Bayfield Peninsula. My pre-college world was largely centered on the joys of hunting and fishing there. My nature-loving mother had some interest in rocks as well as wildlife and wildflowers. I now look back and realize that such, subliminally, may have been a part of my later latching onto geology as a career.

A recruiter from Carleton College came through my hometown, Ashland, and interviewed me at the lake. Shortly thereafter I joined the freshman class of '37. I was a chem major. Then I took a course in geology, and I was hooked by the most dynamic person I have met in the field of teaching—Larry Gould. He had been second in command of the Byrd expedition to Antarctica. I was hooked on geology. You could spend much of your time outdoors, you get paid for it, and you don't have to wait for your vacation. It's been a labor of love ever since.

After becoming geology majors, a buddy of mine and I spent the summer of 1940 touring the all western states via an old Buick. We went some 10,000 miles, visiting most national parks of the West as well as Caltech and Berkeley, as possible grad schools. We camped out, cooked for ourselves—all at a cost of only a couple hundred dollars each (in Missouri, thanks to a gas war, we got eight gallons for $1.00). We were nearly broke in the last couple of weeks and used our Carleton directory to look up people in towns to get free meals. At Carleton my senior research project was on a nearby esker, but I had little undergrad experience with pre-glacial geology, so after graduation I took the eight-week summer field course of the University of Iowa in the Black Hills. There I first met Lewis Cline, one of the field camp instructors, who later was recruited for his enthusiastic teaching of sedimentary geology to the UW-Madison. Lew was a fine friend whose untimely death came only three years after I came to Madison in 1968.

Larry Gould told me to "Go West" for graduate work and I chose Caltech where I fell under the spell of Prof. Ian Campbell, an igneous petrologist and a life-long personal role model. Ian had a fellowship for me to do granite petrography in the Sierra Nevadas, which started my "hard-rock" career. After my 1943 MS, I went to work for the Mineral Deposits Branch of the USGS, first on a tin deposit in California, then spent a year with the pegmatite studies, first in the Black Hills of South Dakota and in the Blue Ridge of Georgia and North Carolina. I mostly mapped and studied mica and beryl prospects—the mica needed for capacitors of the then secret radar program, beryllium for the also secret atom bomb program. It earned me a war-time deferment. Late in 1944 I received a Naval Commission (ensign) and spent a year at the U.S. Hydrographic Office in Maryland, learning more surveying and preparing coastal bombardment charts for islands under Japanese control. In spring of 1945 I luckily met an interesting Red Cross hospital worker named Carrel Myers on a train diner going to NYC from Washington. We hit it off well, have been married since 1946 and have had a fine large family. But on VJ day (Victory over Japan Day) in August 1945, I received my orders to be Hydrographic Officer on the USS Dutton, a small wooden-hulled survey ship, with

complement of five officers and 35 crew members. I spent three happy months in the SW Pacific and Japan before catching up to my ship in Haiphong, Indochina. By that time most of the experienced officers and crew had left or were about to go home, and I became Navigating Officer. Once aboard we sailed to Hong Kong, and then got orders to return the ship to the States. That took an extra six months—from Hawaii to San Pedro, CA, at two knots (the navigating officer was by then also engineering officer, who knew nothing about diesel engines nor their fuel consumption.)

> One of my jobs at Caltech was teaching a course in metamorphic and sedimentary geology...students included Jack Schmidt, who became the only geologist-astronaut, and Dallas Peck, later USGS chief geologist.

I left the Navy in the summer of 1946, by which time I had learned that Ian Campbell had gotten me a three-year National Research Council Fellowship to come back to Caltech for my PhD. I had said sure, I'll come back. Carrel and I planned a fall wedding, and my summer was mostly devoted to building a small house near Pasadena as rental units were unavailable. We lived in that house for the next 10 years, first while I was a graduate student and then, as a junior faculty member teaching mostly field geology and sedimentary geology. I also taught undergraduate metamorphic geology. The students were sharp and tolerant. My major PhD thesis was under Dick Jahns, an igneous petrologist—but a superb all-around geologist. He had maneuvered grad student funding of $3 a day for six months of each of two grad students to map the Sacramento Mountains escarpment in New Mexico. Its only geologic map had been made many decades earlier. We flew over in a light plane and I was excited as soon as we viewed the fine outcrops of the mountain escarpment. There was some interesting structure, minor intrusives were obvious, but it was mostly sedimentary stuff about which I knew little. How to split up major mapping was solved a couple of months later when the other student dropped out of Caltech. So I inherited mapping the whole Sacramento Mountain escarpment with 12 months of field support. Great good luck—it lead to shifting my career focus to sedimentary geology, which has been fun.

When I started mapping in 1946, there had only been a couple of papers on the major rock units in the previous three decades, the most useful being one on Mississippian bioherms by Lowell Laudon and his student, Art Bowsher. So I went to Kansas where Lowell was teaching before coming to Wisconsin, in order to swab him down about his Mississippian work. He generously shared his ideas and detailed sections. At the time, I didn't know anything about stratigraphic sectioning, but I found they were pretty darn good. Lowell never went into the field with me, but he also gave me a useful emerging manuscript. Lowell knew stratigraphy and fossils like the back of his hand, but he didn't know much of anything about carbonate lithologies, which later became my feed for the rest of my research years. So I did the detailed mapping at two inches to a mile, for 40 miles with 5000 feet of relief. Knowing little paleontology, I had to figure out facies and correlations on what I could see in the field. The mapping turned out to be pretty right. I believe in field observation and love it.

Carbonate sedimentology was just coming out of 30 years of heavy minerals and sieve analysis in the early 1950's. They were both passions in the early years of sedimentology, but they had almost nothing to do with interpreting depositional environments or predicting facies and porosity and the rest. Beginning in the early 1950's, much sedimentologic innovation was fostered by petroleum industry research laboratories. For two decades or so, persons entering academia from industry revitalized the teaching of sedimentary geology. When I was mapping the Sacramentos, I had no idea how to look at thin sections of carbonates. I had little sedimentary rock training, and even the basics of carbonate rock nomenclature were largely undeveloped.

I went back to economic geology for my minor thesis. There was a man named Foster Hewitt, a long-respected USGS economic geologist. He took up office space at Caltech, and he had a Mojave Desert field project. He needed somebody to go out there and map and interpret some newly discovered veins in the Mountain Pass region of bastnaesite, an unusual cerium group rare earth fluorocarbonate. This was appealing. Another USGS geologist and I did such—which proved an acceptable minor thesis. Shortly after our study of the uneconomic but fascinating veins, Foster Hewitt came rushing into my office and said, "Lloyd, look at this thin section from an abandoned gold claim right near the veins at Mountain Pass. I think it's half finely crystallized bastnaesite!" It was. Foster and I rushed to view the locality and found more similar rock on the pristine desert hillside of the old gold claim. We were the first to sense its economic potential for rare earths.

Lloyd Pray with sons Ken and John, 1965.

At the last GSA meeting, they asked the old timers, 50-year-members, to describe their most memorable moment. I sent in, in 33 words, that my most memorable moment was standing out in this valley looking at this hillside where there was possibly a world source in rare earths. It is now the world's major source of much used cerium group rare earths.

One of my jobs at Caltech was teaching a junior semester course in metamorphic and sedimentary geology following Ian Campbell's semester of igneous geology. Excellent students included Jack Schmidt, who became the only geologist-astronaut, and Dallas Peck, later USGS Chief Geologist. An ocean floor sedimentary geologist, Dana Russell, at the La Jolla, CA Naval Electronics Laboratory was hired in the early 1950's to teach a Caltech graduate sedimentary course and I was one of his students. He subsequently was hired to head up geology research at the newly established Marathon Oil Research Center in Littleton, CO. Each of five department heads had the right to hire one more experienced PhD, in addition to their other 10-15 newly hatched PhD research scientists. I was picked by Dana Russell to be his "lead man" at Marathon—a very big break for my sedimentary geology career.

At Marathon, I essentially flipped a coin between research on clastics or carbonates, and opted for forming a three-person team in carbonates, made up of Alan Horowitz, a paleontologist, John Wray, another paleo person with a background in calcareous algae, and myself. The first thing we did was to take a course of J. Harlan Johnson at the Colorado School of Mines. It opened our eyes and minds to the utility of looking at thin sections of carbonate rocks and identifying the fossil bits in them, including calcareous algae. Up to then, algae were a never-never land for most sedimentologists. We immediately took our new-found carbonate learning to training seminars for Marathon geologists, put together a training manual (it formed much of the later successful carbonate publication under the authorship of Horowitz and Paul Potter), and initiated research that brought our group into many ancient carbonates and for the first time, the wonders of modern carbonates in Florida and the Bahamas.

At that time, Marathon was in cahoots with Conoco and Amerada, in concessions in Libya and at that time oil in Libya was unknown. Oasis Oil was the triumvirate, had the most magnificent checkerboard you ever saw across Libya. The upshot was that Amerada was known for its geophysics, Conoco was known for its surface geology, and Marathon got known for its sedimentary petrology, and primarily for the carbonates. Very shortly, we were putting on schools in Libya for Oasis. The research director for Marathon was a geophysicist who was really a philosopher. The community built there under his leadership is one that still hangs together, even though the lab has now been disbanded. I stayed at Marathon for 12 years and it was just a golden time, just simply wonderful. I was able to formulate my own research projects. I was never good at picking up a detailed study, being involved with it, and writing it up. I hated to write things up, and I've never been known to write things up very much. When you get what you think are answers, it's time to go onto something new.

I had never been in the UW-Madison geology department before I came here in 1967 for an interview and talk, but I had known several of the faculty for years. I had collaborated with Lowell Laudon on Mississippian bioherms of the Sacramentos, and though I hadn't known him well, had been involved with Gene Cameron in the World War II USGS pegmatite group. After the war years, Gene got the position as head of the USGS Mineral Deposits Branch, but I don't think he liked that job and took the Wisconsin position, which he thrived in the rest of his life. I accepted the UW faculty appointment in 1968, during the heyday of campus Vietnam war unrest. My lab was teargassed, as the gas came through open windows during a protest march up Bascom Hill.

I never applied to NSF for funding. In my era (in the 1970's and 80's) I had contacts and support from the oil industry. I could pick up the phone and tell Exxon I wanted some support or a fellowship for a good student, and we'd get it. I never had to lean on NSF grants. It is sad that 80% of academic time is spent writing proposals that are going to be rejected. You almost have to do your research first to show that it's got to work. I've always admired the person who can think in other ways and has ideas. They may just have a little bit of evidence for them, and they think it might be true. That's maverick thinking. Science is questioning. It's learning to frame the questions more than getting the answers.

> You have to be a bit of a ham to teach...I'd come in with a straw hat and straw in my mouth and say, "Today we're going to talk about water witching."

The UW-Madison geology department has long had a very good reputation. In sedimentary geology, it has ranked high since the addition of William Twenhofel in 1916 (see Bob Dott's review of his career in the 2000 *Outcrop*). Laudon and Cline followed Twenhofel in the late 1940's, Dott joined in 1956, Maher and Clark in the 1960's. In the golden years that I experienced, there was particularly close interaction in the 1970's and 1980's among Byers, Dott, Clark and myself. In the last decade, sedimentary geology remains strong, with additions of Toni Simo, Alan Carroll, and more specialized paleontologic emphasis.

You have to be a bit of a ham to teach. I had different things I cooked up for my annual fall class in beginning geology (101 with lab). When we studied water, I'd come in with a straw hat and straw in my mouth and say, "Today we're going to talk about water witching." Almost everyone had a grandfather who had used water witching and swore by it, because it worked. I had five wastebaskets lined up in the front of the room with cardboard on top of them. Each one contained a bottle of beer. I had my stick and I would go over each one, and over only one would I make the forked stick wobble. And lo and behold, on lifting the cardboard cover there would be a bottle of beer! The moral: in southern Wisconsin, you can get water wherever you put your well. In those days, after Laudon, we no longer had 600 in general geology; we had 200 maybe. A lot them were taking it to get rid of the science requirement and they were prepared to hate it. It was fun to get them interested, or at least to see there was someone talking to them who displayed his love of the field. People would write to me years later and told me I got them involved in geology.

I've recently received the Twenhofel Award and the AAPG Distinguished Educator Award (old codgers sometimes get recognized just before they die off). But I was most pleased in 1998 to receive a new award on Guadalupe Mountains National Park, namely the Wallace Pratt Stewardship Award. I consider it a tribute to my 21 students doing theses there. You have to be in love with that country before you are worth much. It's rugged, it's physically demanding, and is especially hot and dry in summer months. My students often learned things that were complete surprises to me and we collectively were good for the science of geology of the Guadalupe Mountains. The mountains will long remain a site for geologists from around the world to come and pay homage to what these outcrops have to offer. In our work, we upset some classic carbonate "apple carts," especially some espoused by top carbonate experts, such as Bob Dunham, of Shell, who I much respected. We had different interpretations of the role of vadose and marine diagenesis in the Capitan Reef complex. He was world renowned for his interpretations of vadose effects on carbonates, but in our opinion, overstated the role of vadose effects in forming the back reef pisolites and interpreted the dominant cement of the Capitan Reef as vadose rather than marine. Our opinion now is largely accepted.

In 1977 SEPM held a three day conference in the Guadalupe Mountains. Half a dozen Wisconsin grad students were field locality guides, helping Mateo Esteban and myself, the trip leaders. I had invited Bob Dunham to participate in the trip. When we were visiting a Mateo key locality of indubitable modern caliche (vadose) pisolites to show their dissimilarity to the Permian back-reef pisolites, we were using a field microphone attached to a control box, to effectively reach the some 200 trip participants. When Mateo's pitch was finished, I asked Bob Dunham to explain how he thought these pisolites resembled the Permian ones he saw. Mateo handed the microphone to Bob for his story, but I held the control box—and I cut off power shortly after he started his presentation. Of course, I put the power back on, but there was a dramatic momentary gap. That antic made a hit with all, including Bob. At the end of the three day trip, we held a field summation before the busses scattered. Bob yielded on the pisolite interpretations, but said he was not ready to give up on the massive vadose cement in the Capitan Reef rock. Bob and I ended the trip by hugging each other, which all appreciated. So often, people in opposite camps can't or don't do that.

Photos courtesy of Lloyd Pray.