Paul Witherspoon Transcript

I graduated from Pitt (University of Pittsburgh) in June 1941, with a B.S. in petroleum engineering, and went right to work for Phillips Petroleum as an Apprentice Engineer in their Oklahoma City oil field. I remember that on Pearl Harbor Day, December 7, 1941, I was digging ditches to remove some old pipelines, when the gang pusher came over to tell us that the Japanese were bombing the naval shipyards in Hawaii. Fortunately for me, Phillips got a large defense plant project to design and construct a butadiene-styrene plant in Borger, Texas, to manufacture synthetic rubber that was badly needed in the war effort. I was first transferred to the main offices of Phillips in Bartlesville, Oklahoma, where I worked as a chemical process engineer in designing the plant. I was next transferred to Borger to assist in putting the plant into operation, and I remained there throughout the rest of the war.

I returned to Bartlesville with Phillips in 1945, where I met and married Elizabeth Talbott, October 26, 1946. Little did I know of the inner strengths of this young woman. Near the end of 1946, we were transferred to the thriving metropolis of Eureka, Kansas. I was still working for Phillips as a petroleum reservoir engineer, but after about a year and a half, I began to realize that I needed more education in engineering and geology. We were also starting a family with our first son, born in 1948.

I discussed my problem with Elizabeth, and when I told her that I felt the need to pursue some graduate studies in nearby Lawrence at the University of Kansas, her reaction was typical, “When can we leave?” So, with our sixteen-month-old son, we moved to Lawrence, Kansas, in the fall of 1949, and I started working on a MS degree. I was able to get a job as a Teaching Assistant that paid $1,100 over nine months.

Housing was a real problem for non-veteran married students, so the best we could do, at first, was to live with a bachelor farmer in an old farm house about five miles from town. Elizabeth had to learn how to pasteurize milk, separate off the cream to make butter, and prepare special meals for the farmer. That lasted for two semesters. Then we found an apartment-rooming house, just a half block from the campus that we could manage for the landlady for most of our rent on one of the apartments. Elizabeth had to work with the landlady in cleaning all the rooms where students stayed. This was much better and lasted until we departed from Lawrence.

I got my MS degree from Kansas in 1951 in petroleum engineering physics. Around that time I got a job with the Illinois State Geological Survey, in Urbana, Illinois, and also started working on a Ph.D. at the University of Illinois nearby. By then Elizabeth and I had three children, and we were both very busy people. I don’t know whether I had considered an academic career before this time, but I found the academic atmosphere at Illinois both challenging and exhilarating, and knew that I wanted this kind of intellectual stimulation to be a permanent part of my life.

I was not yet finished with my Ph.D. work when, in April 1956, U.C. Berkeley advertised an opening in their Department of Mineral Technology for a petroleum engineering professor. I called the contact person at Berkeley and was told to submit my application with an appropriate list of references. After a short interval, I received a letter telling me to come to Berkeley for an interview and to bring my wife. We arranged for friends to take care of the children, and Elizabeth and I had a very interesting and informative series of meetings in Berkeley. I got the job, but there was a catch: I had to finish my doctoral dissertation by the end of 1956 to be available at the beginning of 1957! I don't know how I did it—it was probably the worst six months of my life—but I did manage to finish and thus meet Cal's requirement. Nothing like a little motivation! So Elizabeth and I packed up the kids, packed up all our stuff, and headed west to California. It wasn't *The Grapes of Wrath*, but it was a difficult trip, with the car getting stuck in the snow in the mountains outside of Taos, New Mexico. Obviously, though, we did finally make it.

Needless to say, I enjoyed the intellectual life of Berkeley; there are certainly few places like it in the world. In 1965, I made something of a shift in my career focus, from being a petroleum engineer to a geological engineer. Along the way, I felt continually challenged in my work, and both my colleagues and students were a constant stimulation.

Late in the 1960s, I think, there was a growing interest within geology in energy and environmental problems, and like most people concerned with the world situation at the time, I wondered what sort of contribution I could make. My students and I, and some other faculty members, started to study geothermal systems, with the thought that this might be another source of energy for the U.S.

Along about that time, Lawrence Berkeley Lab, was also getting interested in these kind of problems and started an Energy and Environment Program, and (in 1971) put out a call to people at Cal Berkeley and other institutions for project ideas. I helped put together a proposal to start an engineering project on geothermal energy, which turned out to be the first funded project in the new E&E Program. Many other earth-sciences projects got started within the E&E program, which soon became its own Berkeley Lab division, first headed by Andy Sessler and Jack Hollander, and in the mid-70s by Bob Budnitz. Within this division, I led the Geothermal and Geosciences Program, and was able to bring a number of colleagues, like Tom McEvilly and Frank Morrison, into this program. They developed some really important, cutting-edge research.

The geothermal work we did included reservoir engineering, geochemical and production engineering, geophysical studies and land subsidence research (related to the impact of geothermal wells). Around the mid-70s there began to be concern, in the scientific community and in the general public, about how to safely isolate nuclear waste from mankind. There already seemed to be a general consensus that geologic burial within a rock repository would be the safest way to handle such waste. But how would the rock in which the waste was buried react to the waste? And what was the best host rock? These were still big questions. It was clear to many of us that much of the geological and engineering work we were doing in the geothermal program could be applied to nuclear waste isolation. At that time, in the 70s, there were a number of sites being contemplated for a nuclear waste repository, not just Yucca Mountain. I remember Hanford, in Washington and salt mines in Kansas.

In early 1977, a golden opportunity came our way. I organized and moderated a workshop— sponsored by ERDA [the U.S. Energy Research and Development Administration, precursor to DOE] in Austin, Texas—on the movement of fluids in largely impermeable rock. Fortunately, KBS, the Swedish Nuclear Fuel Safety Program, sent a representative to this workshop. This person mentioned to me that an old iron ore mine at Stripa (in central Sweden, about 90 miles west of Stockholm) was about to be shut down because it was approaching the end of its economic life. The mining operations had revealed the presence of an extensive body of granite lying just outside the underground workings of the mine. The operations had men and equipment that could easily excavate an experimental drift, or tunnel, about 5 meters in diameter at a depth of 300 meters. The drift could be driven far enough into the granite rock mass to establish that rock conditions there could accommodate a number of field investigations related to the general problem of underground nuclear waste isolation.

At this time there was no comparable location for such activity in the U.S. The level of investigation activities—rock characterization, stress measurements, rock properties, pressure and temperature effects on rock permeability, thermal stress effects on rock—planned for Stripa could be put into operation in a matter of a few months, much faster than anything we could have set up in the U.S. So we very enthusiastically accepted their offer of a cooperative work program with the U.S. (specifically us!). This cooperative program was initiated in the spring of 1977, and all tests were expected to be done by the end of 1977. Why? It turns out they had some new power plants that were about to go into operation in Sweden at that time, and by Swedish law, nuclear power plant operators had to describe how radioactive waste generated from their plants could be isolated in a completely safe way—*before* they would be given a license to operate. Our cooperative work would provide a great deal of important information to address that critical problem. People were waiting on us—there was not a moment to lose!

Meanwhile, on the basis of this work, and the ongoing geothermal work we were doing, the Geothermal and Sciences Program had enough long-term funding sources, enough potential scope, and more than enough potential for scientific progress, for it to become a division by itself. That's exactly what happened in July 1977, when our program was spun out of E&E and became the Earth Sciences Division. Andy Sessler, who was then the Lab Director, appointed me as Division Director, and I had to rearrange my teaching load on campus. I had a few misgivings about this, but on the other hand, I was very excited by the possibilities of all this basically new work at the Lab.

I was Division Director for 5 years. I guess the project I remember best from that time is the Stripa Project, which actually lasted beyond the point when I stepped down, in 1982. I believe we set the standards for conducting site investigations for nuclear waste repositories in that project, and were probably the first to comprehensively study flow and transport in fractured rocks at depth. With fracture systems, the strike and dip are often measured, but very little other data on fractures is recorded or studied. At Stripa, we fully characterized the fracture system, applying various rock mass characterization schemes to the drill core data to assess how much variation there was in the interconnected network of fractures. We also combined orientation data from fracture maps of the tunnel walls and floor, with the orientation data from drill cores, to delineate the main fracture sets in the Stripa granite. Such information, which can be used in numerical simulations of groundwater flow or rock mass stability that include orientation and trace-length distributions, would be invaluable in decisions about final emplacement of the waste. This kind of information is still finding usage in our present Yucca Mountain studies.

Of course, I stayed active within ESD after 1982, and after my retirement from teaching at Cal Berkeley in 1989. Capitalizing on all my international associations, from all my teaching and work at the Lab, I've been a consultant to various projects, most notably the Yucca Mountain Project. I've also been the prime mover in producing worldwide reviews on the progress in nuclear waste isolation every five years, since 1991. We put out the *Fourth Worldwide Review* early in 2006. This "ongoing" book, which documents how nations around the world have attempted to handle the issues in nuclear waste isolation, has been very helpful to the worldwide effort to solve this problem.

Looking back, I've found it highly gratifying that so many people I started out with at U.C. Berkeley, both colleagues and students, came onboard with me to the Lab to basically form the Earth Sciences Division, and help advance the earth sciences in the way they have. It's hard not to think of Bo in this context, but certainly he and many others have advanced upon what started out as rather stimulating but somewhat limited geothermal studies.