

Regional Oral History Office
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University of California
Berkeley, California

Western Mining in the Twentieth Century Series

Douglas W. Fuerstenau

MINERAL PROCESSING ENGINEER AND SCIENTIST: IN
EDUCATION, RESEARCH, INDUSTRY, AND INTERNATIONAL
COOPERATION

With an Introduction by Frank F. Aplan,
and Supplemental Interview with Klaus Schoenert

Interviews Conducted by
Eleanor Swent
in 2001

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Professor Douglas W. Fuerstenau

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PREFACE

The oral history series on Western Mining in the Twentieth Century documents the lives of leaders in mining, metallurgy, geology, education in the earth and materials sciences, mining law, and the pertinent government bodies. The field includes metal, non-metal, and industrial minerals. In its eighteenth year the series numbers sixty-five volumes completed and others in process.

Mining has changed greatly in this century: in the technology and technical education; in the organization of corporations; in the perception of the national strategic importance of minerals; in the labor movement; and in consideration of health and environmental effects of mining.

The idea of an oral history series to document these developments in twentieth century mining had been on the drawing board of the Regional Oral History Office for more than twenty years. The project finally got underway on January 25, 1986, when Mrs. Willa Baum, Mr. and Mrs. Philip Bradley, Professor and Mrs. Douglas Fuerstenau, Mr. and Mrs. Clifford Heimbucher, Mrs. Donald McLaughlin, and Mr. and Mrs. Langan Swent met at the Swent home to plan the project, and Professor Fuerstenau agreed to serve as Principal Investigator.

An advisory committee was selected which included representatives from the materials science and mineral engineering faculty and a professor of history of science at the University of California at Berkeley; a professor emeritus of history from the California Institute of Technology; and executives of mining companies. Langan Swent delighted in referring to himself as “technical advisor” to the series. He abetted the project from the beginning, directly with his wise counsel and store of information, and indirectly by his patience as the oral histories took more and more of his wife’s time and attention. He completed the review of his own oral history transcript when he was in the hospital just before his death in 1992. As some of the original advisors have died, others have been added to help in selecting interviewees, suggesting research topics, and securing funds.

The project was presented to the San Francisco section of the American Institute of Mining, Metallurgical, and Petroleum Engineers (AIME) on “Old-timers Night,” March 10, 1986, when Philip Read Bradley, Jr., was the speaker. This section and the Southern California section of AIME provided initial funding and organizational sponsorship.

The Northern and Southern California sections of the Woman’s Auxiliary to the AIME (WAAIME), the California Mining Association, and the Mining and Metallurgical Society of America (MMSA) were early supporters. Later the National Mining Association became a sponsor. The project was significantly advanced by a generous bequest received in November 1997 upon the death of J. Ward Downey, UC Berkeley alumnus and early member of the mining series advisory committee. His own oral history was completed in 1992. Other individual and corporate donors are listed in the volumes. Sponsors to date include nineteen corporations, four foundations, and 113 individuals. The project is ongoing, and funds continue to be sought.

The first five interviewees were all born in 1904 or earlier. Horace Albright, mining lawyer and president of United States Potash Company, was ninety-six years old when interviewed. Although brief, this interview adds another dimension to a man known primarily as a conservationist.

James Boyd was director of the industry division of the military government of Germany after World War II, director of the U.S. Bureau of Mines, dean of the Colorado School of Mines, vice president of Kennecott Copper Corporation, president of Copper Range, and executive director of the National Commission on Materials Policy. He had reviewed the transcript of his lengthy oral history just before his death in November, 1987. In 1990, he was inducted into the National Mining Hall of Fame, Leadville, Colorado.

Philip Bradley, Jr., mining engineer, was a member of the California Mining Board for thirty-two years, most of them as chairman. He also founded the parent organization of the California Mining Association, as well as the Western Governors Mining Advisory Council. His uncle, Frederick Worthen Bradley, who figures in the oral history, was in the first group inducted into the National Mining Hall of Fame in 1988.

Frank McQuiston, metallurgist for the Raw Materials Division of the Atomic Energy Commission and vice president of Newmont Mining Corporation, died before his oral history was complete; thirteen hours of taped interviews with him were supplemented by three hours with his friend and associate, Robert Shoemaker.

Gordon Oakeshott, geologist, was president of the National Association of Geology Teachers and chief of the California Division of Mines and Geology.

These oral histories establish the framework for the series; subsequent oral histories amplify the basic themes. After over thirty individual biographical oral histories were completed, a community oral history was undertaken, documenting the development of the McLaughlin gold mine in the Napa, Yolo, and Lake Counties of California (the historic Knoxville mercury mining district), and the resulting changes in the surrounding communities. This comprises twelve volumes, including eight Knoxville District McLaughlin Mine volumes with several interviews each. The remaining four volumes contain the interviews of William Humphrey, Hugh Ingle, Patrick Purtell, and James Wilder.

Future researchers will turn to these oral histories to learn how decisions were made which led to changes in mining engineering education, corporate structures, and technology, as well as public policy regarding minerals. In addition, the interviews stimulate the deposit, by interviewees and others, of a number of documents, photographs, memoirs, and other materials related to twentieth century mining in the West. This collection is being added to The Bancroft Library's extensive holdings. A list of completed and in process interviews for the mining series appears at the end of this volume.

Interviews were conducted by Malca Chall, Fredric L. Quivik, and Eleanor Swent.

Eleanor Swent, Project Director
Western Mining in the Twentieth Century

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Western Mining in the Twentieth Century Oral History Series

Interviews Completed, November 2009

Horace Albright, *Mining Lawyer and Executive, U.S. Potash Company, U.S. Borax, 1933-1962*, 1989

Frank F. Aplan, *Mineral Education Generalist, Professor of Metallurgy and Mineral Processing, 1951-1998*, 2003

Samuel S. Arentz, Jr., *Mining Engineer, Consultant, and Entrepreneur in Nevada and Utah, 1934-1992*, 1993

James Boyd, *Minerals and Critical Materials Management: Military and Government Administrator and Mining Executive, 1941-1987*, 1988

Philip Read Bradley, Jr., *A Mining Engineer in Alaska, Canada, the Western United States, Latin America, and Southeast Asia*, 1988

Catherine C. Campbell, *Ian and Catherine Campbell, Geologists: Teaching, Government Service, Editing*, 1989

William Clark, *Reporting on California's Gold Mines for the State Division of Mines and Geology, 1951- 1979*, 1993

John Robert Clarkson, *Building the Clarkson Company, Making Reagent Feeders and Valves for the Mineral Industry, 1935 to 1998*, 1999

Norman Cleaveland, *Dredge Mining for Gold, Malaysian Tin, Diamonds, 1921-1966; Exposing the 1883 Murder of William Raymond Morley*, 1995

William E. Colby, *Reminiscences* (California mining lawyer), 1954

Harry M. Conger, *Mining Career with ASARCO, Kaiser Steel, Consolidation Coal, Homestake, 1955 to 1995: Junior Engineer to Chairman of the Board*, 2001

James T. Curry, Sr., *Metallurgist for Empire Star Mine and Newmont Exploration, 1932-1955; Plant Manager for Calaveras Cement Company, 1956-1975*, 1990

Donald Dickey, *The Oriental Mine, 1938-1991*, 1996

J. Ward Downey, *Mining and Construction Engineer, Industrial Management Consultant, 1936 to the 1990s*, 1992

Warren Fenzi, *Junior Engineer to President, Director of Phelps Dodge, 1937 to 1984*, 1996

Hedley S. "Pete" Fowler, *Mining Engineer in the Americas, India, and Africa, 1933-1983*, 1992

Douglas W. Fuerstenau, *Mineral Processing Engineer and Scientist: In Education, Research, Industry, and International Cooperation*, 2009

James Mack Gerstley, *Executive, U.S. Borax & Chemical Corporation; Trustee, Pomona College; Civic Leader, San Francisco Asian Art Museum*, 1991

Robert M. Haldeman, *Managing Copper Mines in Chile: Braden, CODELCO, Minerec, Pudahuel; Developing Controlled Bacterial Leaching of Copper from Sulfide Ores; 1941-1993*, 1995

Guy Harris, *A Career in Mining Chemicals*, 2003

John F. Havard, *Mining Engineer and Executive, 1935-1981*, 1992

Wayne Hazen, *Plutonium Technology Applied to Mineral Processing; Solvent Extraction; Building Hazen Research; 1940-1993*, 1995

George Heikes, *Mining Geologist on Four Continents, 1924-1974*, 1992

Helen R. Henshaw, *Recollections of Life with Paul Henshaw: Latin America, Homestake Mining Company*, 1988

Homestake Mine Workers, Lead, South Dakota, 1929-1993, interviews with Clarence Kravig, Wayne Harford, and Kenneth Kinghorn, 1995

Lewis L. Huelsdonk, *Manager of Gold and Chrome Mines, Spokesman for Gold Mining, 1935-1974*, 1988

William Humphrey, *Mining Operations and Engineering Executive for Anaconda, Newmont, Homestake, 1950 to 1995*, 1996

Hugh C. Ingle, Jr., *Independent Small Mines Operator, 1948 to 1999; Corona Mine*, 2000

James Jensen, *Chemical and Metallurgical Process Engineer: Making Deuterium, Extracting Salines and Base and Heavy Metals, 1938-1990s*, 1993

Arthur I. Johnson, *Mining and Metallurgical Engineer in the Black Hills: Pegmatites and Rare Minerals, 1922 to the 1990s*, 1990

G. Frank Joklik, *Exploration Geologist, Developer of Mt. Newman, President and CEO of Kennecott, 1949-1996; Chairman, Salt Lake 2002 Olympic Winter Games Committee*, 1997

Evan Just, *Geologist: Engineering and Mining Journal, Marshall Plan, Cyprus Mines Corporation, and Stanford University, 1922-1980*, 1989

Robert Kendall, *Mining Borax, Shaft-Freezing in Potash Mines, U.S. Borax, Inc., 1954-1988*, 1994

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume I, 1998

Anderson, James, "Homestake Vice President-Exploration"
 Baker, Will, "Citizen Activist, Yolo County"
 Birdsey, Norman, "Metallurgical Technician, McLaughlin Process Plant"
 Bledsoe, Brice, "Director, Solano Irrigation District"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume II, 1998

Cerar, Anthony, "Mercury Miner, 1935-1995"
 Ceteras, John, "Organic Farmer, Yolo County"
 Conger, Harry, "President, Chairman, and CEO, Homestake Mining Company, 1977 to 1994"
 Corley, John Jay, "Chairman, Napa County Planning Commission, 1981 to 1985"
 Cornelison, William, "Superintendent of Schools, Lake County" (Includes an interview with John A. Drummond, Lake County Schools Attorney)

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume III, 1998

Crouch, David, "Homestake Corporate Manager-Environmental Affairs"
 Enderlin, Elmer, "Miner in Fifty-Eight Mines"
 Fuller, Claire, "Fuller's Superette Market, Lower Lake"
 Goldstein, Dennis, "Homestake Corporate Lawyer"
 Guinivere, Rex, "Homestake Vice President-Engineering"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume IV, 1998

Gustafson, Donald, "Homestake Exploration Geologist, 1975-1990"
 Hanchett, Bonny Jean, "Owner and Editor, Clearlake Observer, 1955-1986"
 Hickey, James, "Director of Conservation, Development, and Planning for Napa County, 1970 to 1990"
 Jago, Irene, "The Jagos of Jago Bay, Clear Lake"
 Jonas, James, "Lake County Fuel Distributor"
 Koontz, Dolora, "Environmental Engineer, McLaughlin Mine, 1988-1995"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume V, 1998

Kritikos, William, "Operator, Oat Hill Mine"
 Landman, John, "Rancher, Morgan Valley"
 Lyons, Roberta, "Journalist and Environmentalist"
 Madsen, Roger, "Homestake Mechanical Engineer"
 Magoon, Beverly, "Merchant and Craft Instructor, Lower Lake"
 McGinnis, Edward, "Worker at the Reed Mine"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume VI, 1999

Robert McKenzie, "McKenzies in Monticello, Berryessa Valley"
 Harold Moskowitz, "Napa County Supervisor"

Marion Onstad, "Neighbor and Employee of the McLaughlin Mine, 1980-1995"
 Ronald Parker, "Resident Manager of the McLaughlin Mine, 1988-1994"
 Richard Stoehr, "Homestake Engineer and Geologist to Senior Vice-President and Director"
 Joseph Strapko, "Exploration Geologist, McLaughlin Mine Discovery, 1978"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume VII, 2000

Jack Thompson, "General Manager, McLaughlin Mine, 1981-1988"
 Twyla Thompson, "County Supervisor, Yolo County, 1975-1985"
 Avery Tindell, "Capay Valley Environmentalist"
 John Turney, "McLaughlin Metallurgist: Pioneering Autoclaving for Gold"
 Della Underwood, "Knoxville Rancher, McLaughlin Mine Surveyor"
 Walter Wilcox, "County Supervisor, Lake County, 1979-1995"
 Peter Scribner, "Boyhood at the Knoxville Mine, 1941-1944"

The Knoxville Mining District, The McLaughlin Gold Mine, Northern California, Volume VIII, 2002

Dean Enderlin, "Mine Geologist, Reclamation Manager, McLaughlin Mine"
 Susan Harrison, "McLaughlin Natural Reserve"
 Raymond Krauss, "Environmental Manager, McLaughlin Mine"

Marian Lane, *Mine Doctor's Wife in Mexico During the 1920s*, 1996

John Sealy Livermore, *Prospector, Geologist, Public Resource Advocate: Carlin Mine Discovery, 1961; Nevada Gold Rush, 1970s*, 2000

J. David Lowell, *Using Applied Geology to Discover Large Copper and Gold Mines in Arizona, Chile, and Peru*, 1999

Plato Malozemoff, *A Life in Mining: Siberia to Chairman of Newmont Mining Corporation, 1909-1985*, 1990

Donald H. McLaughlin, *Careers in Mining Geology and Management, University Governance and Teaching*, 1975

James and Malcolm McPherson, *Brothers in Mining*, 1992

Frank Woods McQuiston, Jr., *Metallurgist for Newmont Mining Corporation and U.S. Atomic Energy Commission, 1934-1982*, 1989

Gordon B. Oakeshott, *The California Division of Mines and Geology, 1948-1974*, 1988

James H. Orr, *An Entrepreneur in Mining in North and South America, 1930s to 1990s*, 1995

Vincent D. Perry, *A Half Century as Mining and Exploration Geologist with the Anaconda Company*, 1991

Patrick Purtell, *Maintenance and Management at the McLaughlin Mine, 1985 to 1997*, 1999

Carl Randolph, *Research Manager to President, U.S. Borax & Chemical Corporation, 1957-1986*, 1992

John Reed, *Pioneer in Applied Rock Mechanics, Braden Mine, Chile, 1944-1950; St. Joseph Lead Company, 1955-1960; Colorado School of Mines, 1960-1972*, 1993

Joseph Rosenblatt, *EIMCO, Pioneer in Underground Mining Machinery and Process Equipment, 1926-1963*, 1992

Paul Schipke, *Mining and Environmental Engineer for Utah-BHP Company, 1972-1997*, 2004

Robert Shoemaker, *Metallurgical Engineer: Union Carbide, Bechtel, San Francisco Mining Associates; Metallurgical Consultant, 1953 to 2000*, 2001

Eugene David Smith, *Working on the Twenty-Mule Team: Laborer to Vice President, U.S. Borax & Chemical Corporation, 1941-1989*, 1993

Simon Strauss, *Market Analyst for Non-ferrous Metals and Non-metallic Minerals, Journalist, Mining Corporation Executive, 1927-1994*, 1995

Langan W. Swent, *Working for Safety and Health in Underground Mines: San Luis and Homestake Mining Companies, 1946-1988*, 1995

James V. Thompson, *Mining and Metallurgical Engineer: the Philippine Islands; Dorr, Humphreys, Kaiser Engineers Companies; 1940-1990s*, 1992

William Wilder, *Owner of One Shot Mining Company: Manhattan Mercury Mine, 1965-1981*, 1996

Alexander M. Wilson, *Leading a Changing Utah Construction and Mining Company: Utah International, GE-Utah, BHP-Utah, 1954 to 1987*, 2000

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INTRODUCTION by Frank F. Aplan

It is indeed a pleasure to write this foreword to the oral history of Douglas W. Fuerstenau. I have known Doug for sixty-two years as a professional colleague and as my oldest and dearest friend.

When I re-entered the South Dakota School of Mines and Technology (SDSM&T) in the fall of 1946, after nearly four years of service in the U.S. Army, I was soon to meet Doug Fuerstenau as a fellow student in metallurgical engineering. At that time I was five years older and a year ahead of him in the program. After graduation in 1948 I attended the Montana School of Mines, now Montana Tech, in Butte, MT as a graduate lab assistant in Professor Donald W. McGlashan's mineral processing group. During the summer of 1949, I had a job with Day Mines, Inc. at Wallace, ID in the Coeur d'Alene Mining District. One Saturday I was sitting in the Stein Club drinking beer, when quite unexpectedly I walked Doug who was working that summer at the lead smelter of Bunker Hill and Sullivan Mining Co. in the adjacent town of Kellogg, ID. I owned a '37 Olds automobile and we spent every weekend visiting the many mining operations in the district. We drove back to South Dakota together, and that September we drove to Butte, MT where Doug had a fellowship also in McGlashan's lab. McGlashan had a dynamic research group of a half-dozen students and we were fortunate to be there at that time. It was an excellent learning experience. Doug's research was on the use of chelation agents to float oxides of copper minerals, while I did studies on the effect of temperature on lead-zinc separations. All work and no play makes Jack a dull boy, and Butte was a wide open mining camp. One of our favorites was the C.O.D. Club. Kind of a rowdy place, but they had the good Dixieland band of Jake Flores from California. Again, we toured several Montana mining operations including Anaconda's Great Falls Reduction Works. Doug was a good artist and regularly entered the newspaper contest sponsored by Butte Beer Co. His reward for his winning cartoons was a case of their beer. He frequently won and he kept the dorm, where we both lived, well supplied with beer.

We graduated with MS degrees in 1950 with Donald McLaughlin as the commencement speaker. I went to work for the Climax Molybdenum Co., Climax, CO whereas Doug worked the summer for Howe Sound Mining Co., at the northern end of Lake Chelan, WA and that fall entered the doctoral program with Professor Gaudin in the Richards Mining Engineering Lab at MIT. A year or so later I became an assistant professor at the University of Washington (UW) in Seattle.

By mid-1953 Doug had completed his ScD and joined the MIT faculty as an assistant professor, and I entered MIT as a graduate student in the same group. Doug had married Peggy Pellett, a student at Radcliffe, and they were living in Cambridge. It was at MIT that Doug really came into his own. He had a head full of ideas which came from him like sparks from an anvil. He had the intelligence, creativity and drive, and Gaudin furnished the financial and organizational backing to forward Doug's ambitious research. This program included such things as saline mineral flotation, the influence of slimes on flotation, activation and depression mechanisms in sphalerite separations, flotation kinetic phenomena, shape and rheological aspects of bubbles, and the development of the electrostatic model of flotation. These concepts resulted in several publications which are today well known in the literature. He often used me as a sounding board for his ideas, and in a way I was sort of able to get inside his mind as he, stagewise, developed these and other concepts. All this time he had a substantial teaching load, was a good teacher,

and made wholesale revisions in course content. Just before Doug left for Union Carbide Corp. in Niagara Falls, NY in 1956, we did a “quickie” bit of research on the mercaptan flotation of chrysocolla. MIT was a tough task master, but all was not work and no play as there was a lot to do in the Boston area on the cheap (a necessity for those of us so impoverished). Durgin Park restaurant with large portions and low cost, Crane’s and other beaches, historical sites, scrumptious chocolate fudge sundaes at Brigham’s, and a trip to see New Hampshire fall foliage with Doug and Peggy, Jim and Dorothy Brown, my wife, Clare, and myself. Doug served as an usher at our wedding.

At this time I also learned, first hand, about one of Doug’s idiosyncracies. “Have you got a minute?” was a very time expansive statement. When Doug and Peggy moved from their apartment in Cambridge to a larger one in Watertown, MA, it was, “Do you have a minute?” Six hours later I completed the “minute” job!

I received my ScD degree in 1957 and went to work at the Kennecott Research Center in Salt Lake City, UT. In December of that year I took a job with Carbide in Niagara Falls, NY associated with Doug but working for Rush Spedden, research director for the Union Carbide Ore Co. Doug left Carbide in 1958 going to Kaiser Aluminum for a couple of years and then on to U. Cal at Berkeley while I stayed with Carbide at Niagara Falls and Tuxedo, NY for a decade. I have been a professor at Penn State since 1968. While at Carbide and at Penn State University I have made numerous trips to or through San Francisco where we had an opportunity to get together. The evenings were spent talking at school, at home, at restaurants, or at the jazz joints. Trips to Napa or Sonoma wine country were mandatory. We have met regularly at technical meetings, and the telephone lines are kept busy between us.

We had remarkably similar careers, especially early on. As a consequence I got to know Doug well. Close contact has continued regularly, and we have coauthored several technical papers of which the chapter on non-sulfide flotation in *Froth Flotation* is the best.

In my view Doug Fuerstenau is by far and away the best the world has to offer in the field of mineral processing. He is thus a natural successor to the triumvirate of Richards, Taggart, and Gaudin.

Frank F. Aplan
State College, PA
2008

INTERVIEW HISTORY by Eleanor Swent

Douglas Fuerstenau has profoundly influenced everyone living in the second half of the twentieth century, because, as I learned while interviewing him, everything that civilized society uses, from bread to paper to microchips, has to be made by grinding, dissolving, mixing, or separating particles. His research in the fundamental science of particles has earned him admission to the national scientific academies of the United States, Australia, Japan, Russia, and India, and he has received highest academic honors in Belgium, Sweden, Italy, China, Germany, and the United States. The University of California has given him its highest honors: the Berkeley Citation and Berkeley Fellow. In 2005 he was named to the South Dakota Hall of Fame.

For nearly twenty years Professor Fuerstenau has been principal investigator and faculty advisor to the Regional Oral History Office series on Western Mining in the Twentieth Century. It is appropriate that the series finally includes his own oral history. The project grew out of conversations we had while driving to the McLaughlin Mine inauguration in Napa County in October, 1985, and he and his wife Peggy firmly supported the concept from then on. He was a tireless and effective advocate and fundraiser for the series, urging expanding its breadth and depth to include not only academics and CEOs but also what he called “journeymen” of the field.

Professor Fuerstenau was a founder and for many years editor-in-chief of the International Journal of Mineral Processing, which dedicated a special issue to him in 2003. The preface to that issue lists his many professional accomplishments. It is said that the first half of the twentieth century was dominated by the metallurgical research of Antoine Gaudin, and the second half by his student, Douglas Fuerstenau.

He has been successful in academia and also in industry, working for Union Carbide, Kaiser Engineers, and serving as longtime director of Homestake Mining Company. This breadth of accomplishment recalls that of mining engineer Herbert Hoover, and more recently, two other interviewees in this series, James Boyd and Donald McLaughlin. Professor Fuerstenau turned down some attractive offers in other areas because his real love was research and teaching. He advised an astonishing number of graduate students: 60 PhD and 65 MS candidates; he has continued to guide many of them through their later careers, as a critic of science and also a stickler for proper English. At thirty-six, he was asked to serve on the University of California budget committee; he was very young to be given this responsibility. His South Dakota childhood, with early responsibilities and a strong moral and ethical upbringing, contribute to his outlook on life, his love of music, and insistence on correct English. He is also known for his great talent as a cartoonist.

The letter of invitation to participate in the series was sent on April 17, 2001. A planning session was held at the University of California Faculty Club and sixteen interviews were conducted beginning July 23 and October 26, 2001, in his office in 477 Evans Hall, a temporary location while the Hearst Mining Building was undergoing seismic retrofitting. Construction noises outside were a constant background accompaniment. One session was captured on videotape.

He came to each session well prepared, with a mental or actual outline of topics expected to cover. Used to writing and speaking to an audience, he is an ideal interviewee, relaxed and

articulate. Throughout the interviews, he was considerate about restricting interruptions and assuring that we were as comfortable as possible in crowded quarters. The recordings were transcribed and lightly edited and sent to him for review in installments during the spring of 2002. Despite some health setbacks, he continued diligently to review the transcript as well as carrying on his family responsibilities and international professional activities, receiving honors in South Africa and at Berkeley. In July 2005 he was the plenary speaker in Brisbane, Australia, for the celebration of the centennial of the cyanidation of ores.

The Fuerstenau interview is supplemented by a brief interview conducted on January 23, 2003 with his houseguest and colleague Klaus Schoenert, who is internationally recognized for his work in comminution, the breaking down of particles which consumes much of the world's energy. Schoenert's development of a high-pressure rolling mill significantly reduced the energy required in processing cement, as well as many other substances, such as diamonds; and has been hailed as the greatest contribution to mineral processing since the invention of flotation. This interview is of interest for its important technical information, and also for his account of service in the German army near the end of WWII, and imprisonment by U.S. forces in Austria for a brief time after the war ended. Schoenert was a visiting lecturer at UC-Berkeley in 1969. The final editing was done by Professor Fuerstenau, who devoted many hours to clarifying it while "trying to keep it Germanic."

The Regional Oral History Office was established in 1954 to record the lives of persons who have contributed significantly to the history of California and the West. A major focus of the office has been the history of mining in the western states. The list of completed oral histories documenting the history of mining is included in this volume. Many of the interviews in this and other subject areas can be found online at <http://bancroft.berkeley.edu/ROHO/>. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. Interview tapes are available for listening in The Bancroft Library. The Regional Oral History Office is a division of The Bancroft Library and is under the direction of Richard Cándida-Smith.

Eleanor Swent
Berkeley, 2005

I EARLY YEARS GROWING UP IN SOUTH DAKOTA, 1928 TO 1945

[Interview 1: July 23, 2001 in his office at 477 Evans Hall, UC Berkeley]

[Tape 1, Side A]

Parents, Grandparents, and Early Years on South Dakota Farms

Swent: The first thing is for you to tell us when and where you were born.

Fuerstenau: I was born December 6, 1928, in Hazel, South Dakota, in a farm area. Hazel is a little town in Hamlin County, South Dakota, about twenty miles southwest of Watertown, South Dakota, Watertown being one of the bigger cities of the state.

Swent: Was it flat prairie country?

Fuerstenau: It was east of the very flat farmland in the central part of the state that once was the ancient Lake Agassiz. It is actually very good farmland in the eastern part of the state that is not totally flat and has lots of sloughs and lakes which are remnants of the ice age. My ancestors homesteaded there. My grandfather on my father's side was born in Germany about 1860, and came from Posen, which is east of Berlin. He came to Minnesota after a brother had earlier come to the States. I guess he went back and convinced his other brother to come also. Then on returning from Germany, from what I read, in 1882 my grandfather homesteaded in what was then Dakota Territory.

Swent: What was his name?

Fuerstenau: Emil. Emil Fuerstenau. I think maybe his middle name was Adolph and I'm sure glad that I was not named after my grandfather. But the grandson of our very good friends, the Schoenerts, in Germany, who is only a few months old, has the name Emil. So Emil is apparently still a good German name.

My grandmother also came from Posen and she was born in 1872; I remember that from childhood. I think she came to this country when she was sixteen, and lived a couple of years in Chicago with an aunt, then married my grandfather and moved to the farm. They had six sons, of which my father, who was born in 1897, was the second oldest. That grandfather died of stomach cancer at age 55, and one of my dad's uncles also died of stomach cancer at that same age. Must have had too much *Schwarzwald schinken* or something.

Swent: What was your father's name?

- Fuerstenau: Erwin Arnold Fuerstenau. All of my uncles have good German names: Adolph, Martin, Walter, Elmer, Herbert. My father said he couldn't speak English until he went to school; they were raised on German.
- Swent: Was there a German community there?
- Fuerstenau: Oh yes. Nearly all of the farmers in the immediate vicinity were German.
- Swent: It wasn't just the one family?
- Fuerstenau: I know of three somewhat interrelated families. All of them moved from around Posen to that area near Watertown. My grandmother's maiden name was Stoltz, and there were a lot of Stoltzes who also lived in that general area. A third family that I guess had married some Fuerstenaus was named Krenz, and those three families owned a lot of farms in an area of several square miles. Obviously all homesteaded at about the same time.
- Swent: Had they been farmers in Germany?
- Fuerstenau: No, no. From what I heard from my father, his grandfather was a professional musician in Posen. It is said that he was born in 1825 and had a stipend from the state until 1870. That's when Bismarck came in and must have cut out such subsidies, the way I read it. Probably the thing for younger members of the families to do was to come to this country as homesteaders. So on that half of the family, I'm obviously pure German.
- The other half, on my mother's side, is Scandinavian. Her mother, my grandmother, was born in Iowa in 1874. Her name was Dagmar Almblom. Danish. She came as a little girl to what was then Dakota Territory also. That grandmother went through the tenth grade, and for a woman in the 1880s in Dakota Territory to go through tenth grade in school was obviously a rare thing.
- Swent: That was very well educated.
- Fuerstenau: Oh yes. She was a very bright woman. The grandfather on that side, who I never met, came from Norway. So my mix is half German, half Scandinavian. They all homesteaded in an area near Hayti, which is a small town south of Watertown. This is a localized area where numerous Norwegians, Swedes and Finns settled at that same time. My mother's father had emigrated from Norway. Interestingly, there was virtually no intermixing between the German and Scandinavian farming areas.
- My mother was born in 1901. Her name was Hazel Pauline Karterud. Typical Norwegian spelling of her last name. My mother graduated from high school and then apparently went a year or so to normal school in Madison to teach school. Her first job, at about age nineteen, I think, was a school teacher in the

farm area, at the school district where my dad was living. She taught in a one-room country school house, grades one to eight. I've seen this little old school house. It was still there in a field a few years ago. A cousin of my father's, an older cousin, was chairman of the school board, and I remember my mother telling me that he said that the school teacher can't be dating a Fuerstenau when a Fuerstenau is head of the school board, and so they didn't renew her contract and I'm glad of that because this was about 1920 or '21. About a year or two later my dad started making contact again with my mother, and after a couple years, they got married, in 1925. I'm certainly glad that they didn't get married in 1921. [Laughter]

Swent: She couldn't continue teaching then?

Fuerstenau: After that she moved to Herried, South Dakota, in the northern part of the state where her oldest married sister lived. My mother taught there a couple of years.

Swent: There was a famous blizzard in—wasn't it 1889?

Fuerstenau: Eighty-eight.

Swent: Eighty-eight. Did your family have recollections of this?

Fuerstenau: My Grandmother Karterud wrote down her remembrances of that. She said that that January morning was very mild so some people had gone to town and others had taken animals out to water. At about eleven in the morning, the blizzard arrived abruptly with blinding snow and the temperature dropped abruptly. Many animals and a number of people froze to death, or later died of pneumonia brought on by their exposure. Also, I have a coffee grinder that my grandmother had had, a little old coffee grinder that you see in antique shops. And there's a little note in there stating that this coffee grinder was shared by three, four families during the blizzard of '88 in which they roasted wheat to make it taste like coffee, and ground it. One of the family members was named Mellette, who later became the first governor of South Dakota. He had a farm near by, around Watertown. Yes, the blizzard of '88 was, is very infamous.

Swent: It was a terrible, terrible time.

Fuerstenau: And came on unexpectedly. Well, they had no way of predicting it in those days.

Swent: It must have been awfully hard homesteading.

Fuerstenau: Yes. They generally started with sod houses, and then a small house. My Grandmother Karterud said that the worst problem was getting fuel to burn for cooking and heating because there were no trees for miles around. Their fuel

consisted of fried cow pies, straw and hay. They were plagued with dry years when there was little rain and no snow. The shallow lakes dried up. The farm that my Fuerstenau grandparents had when my dad was raised had many large buildings and were all well kept—and a large house. It was probably built, my guess, is 1910-ish, or maybe even a little later. Since my grandfather had homesteaded there about 1880, his land must have been quite fruitful. There was a large cow barn and horse barn, a big granary and chicken coop.

I have a very good remembrance of when I was very young and, probably 1932 or 1933, a tornado came through. I remember going over to my grandmother's farm, fairly near by, just five miles away maybe. And this tornado absolutely smashed the windmill down, and the silo, which was adjacent to the cow barn. We're talking big buildings. The horse barn was absolutely smashed flat, and the granary flat, which showed you how narrow a tornado can cut through. It missed the house, missed the cow barn. And you know, every farm there had what I remember were storm cellars. Not part of the house, but out where you could go down in if a storm was coming. And obviously they were used primarily for storing vegetables and things for winter—cabbage, potatoes, et cetera.

Swent: And they called it a tornado?

Fuerstenau: Oh, it was a real tornado.

Swent: Not a cyclone?

Fuerstenau: No, it was a tornado.

Swent: My recollection is that in Iowa they called it a cyclone. They're the same, but I think it's just a matter of terminology.

Fuerstenau: Cyclones can be huge, or fast moving, slow moving. It's circular wind, of course.

Swent: Yes. They grew wheat?

Fuerstenau: Wheat, corn, rye, oats—until the dust storms came. After that, my mother said that one year all they got was Russian thistles. And she said that another year they had a pretty good crop of rye but it sold for only five cents a bushel.

I can remember these dust storms coming. One in particular that I remember, you could see on the horizon an orange-brown layer building up as this storm came closer. Whenever that happened my mother would wet sheets and put the sheets on the inside of the windows to collect the dust so it wouldn't get throughout the house. And I remember one particular storm when the wind really blew hard—it probably was 1932. My dad must have been somewhere else because my mother took us out from the house under a blanket. I don't

know why. I would have stayed in the house, but she took us outside the house and under a blanket. She, and my sister, and myself, and my one brother who was probably one or two years old. So we were out under a blanket until this dust storm blew by. And I can remember playing along the road, you know, where the dry dust collected along the fences in drifts. Along the fences, it looked just like snow drifts in South Dakota, only this was just fine dirt. So that's just what eastern South Dakota was like in the early 1930s. Now staying in the Black Hills, you didn't see any of that, did you?

Swent: Not in the Hills, no.

Fuerstenau: Would the sky ever be sort of—?

Swent: I don't recall seeing that at all.

Fuerstenau: That's because there would have been little farming in that part of the state. As you know, the dust bowl era started because they plowed Montana in World War I to grow wheat. That's what really was the later cause of the dust storms.

Swent: I don't remember the dust storms in the Hills. I remember forest fires in those dry years.

Fuerstenau: There it would have been bad.

Swent: But we drove across the state to visit my grandmother in Iowa, so I saw it driving across the state. And grasshoppers, too.

Fuerstenau: I'll tell you about grasshoppers later. But those dust storms were really, really bad. It just looked exactly like the pictures one sees of the dust bowl era.

Swent: You mentioned the windmill. This was for water. You had a well?

Fuerstenau: A well, yes. All water was well water. And I have two brothers. My brother Dick, who was born in 1931, caused a real scare climbing up the windmill before a storm.

Swent: Let's get their names.

Fuerstenau: My sister, Jean is a year older than I am. She was born in '27, I in '28, my brother, Dick, 1931, and Maurie, whom you know, in 1933. As a matter of fact, I can still remember the very day that Maurie was born. I was only four and a half. He was born in June; my birthday is December. But I still remember the very day.

Swent: Was he born at home?

Fuerstenau: My grandmother's house. I remember an uncle taking my sister and me to the park in Watertown, where my Grandmother Karterud lived. And we came back; there was Maurie. [laughs]

[Added by Douglas Fuerstenau during editing: My sister, whose full name is Shirley Jean, is Mrs. James Hadeen and currently lives in Laramie, Wyoming. My brother Dick, whose full name was Richard Kendall, had a 20-year career as a petroleum geophysicist with Mobil Oil Corporation in Texas. Tragically, he died at the age of 43 from lung cancer, leaving two young daughters, Julie and Kendall. My youngest brother, Maurice Clark, has had a long career in metallurgical education and currently is distinguished professor of metallurgical engineering at the University of Nevada in Reno.]

Anyway, when my brother Dick was only about two and a half, he climbed up the steps to the top of the windmill. And a storm was coming, and that was a great traumatic thing, you know, to get this little kid down without scaring him. You know that windmills have a ladder that goes clear to the top, and Dick was quite a way up it. I don't recall how they got him down with the storm coming. Whether my dad went up, gently to not scare the little kid, or whether they coaxed him down. But the event is still part of my many early memories of the farm.

But you talked about water, and of course on a farm the drinking water is in a pail with a dipper. Perhaps when I was about three, or a little more, one day in the summer my grandmother came out from Watertown, and she and my mother were cleaning something with white gasoline. And they put the white gasoline in the dipper, the drinking dipper, and I came in and took a drink of this thing. And I am lucky, I suppose, to be alive. I can still remember the horrendous shock of taking a big gulp of white gasoline. They must have gotten me to throw it right up. An awful dumb thing to do, using the dipper that way. Like I say, I still remember it, the shock of it.

Swent: You're lucky that you didn't get killed by that.

Fuerstenau: Oh yes, you're right.

Swent: There was no irrigation, of course?

Fuerstenau: No. In fact, I have seen some old pictures that my dad had. All the farmers tried to do the reverse, apparently, I would say around 1915. Eastern South Dakota has a lot of sloughs, as you know, because it was all glaciated. And they made an effort to drain the small sloughs by digging a rather big ditch that went for quite a distance. So of course, what they did is they lowered the water table, right? Each farm—as you've probably seen driving across the eastern part of South Dakota—may have several depressions that would have water in them in the spring and be dry later on in the summer. But you couldn't farm those areas, so they actually tried to do the reverse. I've never

seen those ditches, which meant that they obviously discovered what they had done. By lowering the water table, they had hurt the rest of the land. So there was no irrigation there.

Swent: Did you have electricity?

Fuerstenau: No. Kerosene lanterns and lamps with gas mantles which actually gave out a fairly bright light. Well, that went on until about 1940 or so, I think, in eastern South Dakota. Until the REA [Rural Electrification Act of 1936] came in. there was no electricity. A few farmers would—very few—have a windmill wind charger that would charge batteries to provide electricity for the house or barn. It was that sort of thing. Even churches had just kerosene lamps if there was an evening service.

Swent: Did you have plumbing in the house?

Fuerstenau: No, outdoor toilet. That must have been cold in the winter, that's all I can say. [laughter] By the way, there is a classic little book by [Charles] Chic Sale titled, *The Specialist*, which humorously describes the design and construction of outhouses. Chic Sale came from South Dakota. However, my grandmother's farm had indoor plumbing, which obviously they must have rigged up with a well system. I'm speaking of the bathroom.

Swent: They probably had a cistern of some kind.

Fuerstenau: Oh, there was a cistern. I know that the house had a cistern for collecting rain water, and that cistern water was used for washing because it would have been soft water. Not for drinking, but for washing.

Then, 1934, my parents lost the farm, which I always used to think of how traumatic that was. My mother one time said that they would have been able to hang on, but my father had co-signed a note for a younger brother who wanted to buy a farm. That younger brother couldn't meet his mortgage payment and both farms were lost. But a few years ago I mentioned to my mother that that probably was the best thing that ever happened at least for me. [chuckles]

Swent: In a way.

Fuerstenau: In a way. But to them it was obviously very tragic. And it happened to many people, of course.

**Living in Small Towns during Grade School Years, Particularly
Lemmon, SD**

Fuerstenau: My dad got some sort of job on leaving the farm, probably through a relative. I don't know any details. But we moved for a year to Hawarden, Iowa, which is just across the border from South Dakota, down in the southeast corner. It's north of Sioux City, right on the Sioux River, which comes through Sioux Falls, down to Sioux City where it flows into the Missouri.

So we lived there one year, and I started school there. My sister had started school the year before in the country school. It was about two miles away from the farm where we lived, and I remember walking to school in the fall, few times, and being there all day, and in the spring. If I did that in the fall, I would have only been about four and a half, and that spring I would have been a little over five. But I can still recall some days being there all day at school when my sister was in the first grade.

Swent: And walking two miles.

Fuerstenau: And walking that whole way. I have no recollection about winter time, but this was the fall and the spring.

Swent: Your mom didn't drive you to school.

Fuerstenau: I have no idea what happened in the winters; probably it was driving.

Swent: Did your mother know how to drive?

Fuerstenau: Oh yes. Anyway, when we went to Hawarden, Iowa, my mother said my dad didn't seem to know my age and when I started school in the fall I was put in kindergarten and spent that year in kindergarten. So I would have been five. I still have a lot of recollections of that year in school. And then in the summer of 1935, they moved to Lemmon, South Dakota. Have you ever been in Lemmon?

Swent: Not for a long time, but I have been there.

Fuerstenau: You know it's in the northwest part on the state, right on the border of North Dakota. One thing about it—I have a few other things to say about it—there is a petrified wood park occupying about three square blocks. Somebody who locally had a lot of money gathered in petrified wood, either from a ranch he had or something, and built this huge park, making huge conical monuments that might be twenty feet high of petrified wood logs. Lots of them. And he built a castle, things like that, all out of massive petrified wood. It's kind of an unbelievable thing. I did see that again a few years ago. I remember playing cops and robbers there as a little kid.

We moved there in the summer of 1935 and that fall, when I started school, I was put in the second grade so I skipped the first. Lemmon was then a town of about 1500 people; I think maybe it's double that now. About fifteen miles south was Grand River, one of the South Dakota rivers. I remember as a little kid, once in a while with family, going down there fishing and so on. But this town of Lemmon—

Swent: You lived in town?

Fuerstenau: Right in town, in about three different houses, as I remember. The small company for which my dad was working transferred him there to Lemmon. This town was right on the mainline of the Milwaukee Road, which came through Lemmon. And to tell you a little about what a small town is like, I can remember on Sunday evenings at six o'clock people would go down and line their cars up to watch the big passenger train come through town. [laughter] It stopped, of course, to load water and a few passengers, I suppose. That was the big event.

But something about Lemmon, that I just wanted to comment on: there's a book by a very good author, semi-religious, Kathleen Norris. She wrote this book called *Dakota*, which I bought and read. And you know, I knew who her grandfather was, because he was the local doctor; Totten was his name. You'll see, she mentions that in the book. And she writes about going to Lemmon and assimilating into the area after living in New York. I guess she inherited the family farm, or something, and moved back there. Anyway, that's Lemmon.

Swent: It's a wonderful book.

Fuerstenau: Oh yes. And she has done others, as you know.

I have a little note here. My Grandmother Fuerstenau, who was German as I have said, always had a flower garden, out on the farm. This quite large flower garden was fenced in, and all summer long she planted flowers and tended this flower garden. That was just left over from her German heritage. In fact, later on, in his last forty years, my dad always had the back yard completely full of flowers. He probably acquired the taste of my grandmother.

[Tape 1, Side B]

Swent: I think the flower garden on the farm was entirely the woman's domain.

Fuerstenau: Oh yes. It was fenced in so animals would not get in. I remember a gate on it, and so on, and all sorts of flowers that she tended, every day.

Swent: Did she grow vegetables as well?

Fuerstenau: Probably. Vegetables like carrots, peas and lettuce were tended by her. Rhubarb also. I think potatoes were farmed in a big way, but not necessarily for sale. Or maybe they did.

Swent: So they grew their own food.

Fuerstenau: Oh yes, sure. Chickens, eggs. Lots of chickens, turkeys, and even geese. I remember, even as a little kid, being chased by grandmother's turkey gobblers. Gobblers are mean. And when you're only three, four years old, they're mean and dangerous animals.

Swent: They had pigs and cows?

Fuerstenau: Oh yes. Pigs, cattle, so on.

Swent: Sheep?

Fuerstenau: No sheep. In eastern South Dakota they didn't raise sheep because that was farm land and not grassland like western South Dakota.

But you know, my mother talked about when Roosevelt became president they came in and took cattle, shot them and buried them. And I think you read about the idea that this would reduce the supply and raise the price. That obviously made a lot of farmers very unhappy, and I think that relatively poor people, like my relatives, became Republicans because of that. I think that issue was a major issue. My mother said they just came and said, "Okay, so many cows had to go. We'll pay you, but we'll take them." And they took them out and shot them and buried them.

Swent: That was terrible.

Fuerstenau: I have no recollection of that, although I do remember my dad butchering a pig one day.

Swent: What did your father do after he lost the farm?

Fuerstenau: I think he worked for two years or so for a wholesale liquor distributing company. That's what I think that was, but I don't remember any more about that. But that job disappeared.

Swent: Was that during Prohibition?

Fuerstenau: Was it? No. '35, '36. My own thoughts are that North Dakota may have been dry, and that is why the company had an outlet there in Lemmon right there on the border of North Dakota. That's my opinion, but I've never asked anybody about it.

Sometime in late 1936 that job disappeared. Then he taught woodworking, probably under one of those NRA [National Recovery Administration] programs for some time.

Now in 1936, it has to be that summer—you talked about grasshoppers. I can remember in that summer, the north side of the house would be coated with grasshoppers, a layer of grasshoppers three or four grasshoppers deep on the foundation, and probably working their way up the side of the house, staying out of the sun. I once commented on this some years ago to my mother, and she said the streets would be slippery driving because of squashed grasshoppers. That's how thick they were! So here you had the droughts plus this terrible plague of grasshoppers. Did you see them down in the Black Hills?

Swent: Well, not so much in the Hills. But driving across the state I certainly remember them. We couldn't open the car windows because of the grasshoppers, and we nearly suffocated in the car. I remember once in Chamberlain when it was 112 degrees.

Fuerstenau: An all-time record, almost.

Swent: Yes, and we stopped in Chamberlain, and that's what the thermometer was registering. It must have been 1936.

Fuerstenau: Yes, yes. Another thing about Lemmon, we didn't live near the railroad, maybe three or four blocks away. Actually, it was a nice house my parents had rented, right across from the school.

Often, my mother fed hobos—the guys that rode the rails. They would come by and she would have them perhaps do a little work or something. And we're talking about fairly clean-looking guys that were probably in their twenties.

Swent: A lot of them were farmers.

Fuerstenau: And she said that she thought that somewhere down by the railroad tracks there was probably written, "If you want a meal go to such and such an address." I still recall that quite often, she made this comment how they seemed to come to our house. And she was very kind.

Swent: I remember that too. I remember my mother giving fried eggs, and bacon, and bread, and coffee—feeding men who came to the door. But they were farmers who had been burned out, who came looking for work. I think they came to everybody's door, maybe. And some of them cried because they were so—they were desperate.

Fuerstenau: Oh yes.

Swent: It was a terrible time.

Fuerstenau: I mean, just for everybody, farmer or working somewhere.

Swent: You haven't mentioned church.

Fuerstenau: We were raised good old German Lutheran.

Swent: There wasn't much choice, was there?

Fuerstenau: Well, let me tell you, my mother, who was Norwegian, had gone to a Norwegian Lutheran church, and it blows my mind that the German Lutherans looked down on the Norwegian Lutherans, and the German Lutherans—

Swent: Luther was a German.

Fuerstenau: Yes, I know. But there was this pecking order. I sensed it, and my mother told me that because she was Norwegian my grandmother considered that she wasn't as good as the Germans.

I remember going to the country church when we lived on the farm when I was four or five years old. Then, either every Sunday, or every other Sunday, the sermons were in German. I can still remember some of the other old farmers coming to talk to my dad, and the first thing was, "Wie gehts." Then even in Lemmon, maybe every other or every third church service would be in German. This would be 1935, '36. I think all the ministers went to a Lutheran seminary and of course had to learn German. And maybe an awful lot of them had already learned it as kids anyway.

Sunday church was always a steady thing. Of course, later I went through the catechism and confirmation that takes place.

Swent: Did they do any farm work on Sunday?

Fuerstenau: No, no, they never did. Other than during harvest time, of course, when you really must complete the harvest in case of rain. Sundays were a time for people to call on friends.

Swent: You had to milk the cows.

Fuerstenau: Oh yes. Cows had to be milked, chickens fed, eggs gathered on Sundays, but there was no plowing or anything else.

Swent: What about the music in the church?

Fuerstenau: In the country church, my Uncle Martin was the organist. He was a farmer. He was several years younger than my mother, but she had taught him in grade

school that year, and she said that that was the brightest person she ever knew as a student.

Swent: This is your father's brother?

Fuerstenau: Yes, younger brother. And he started college for a year, but was enticed with a farm to become a farmer. And now this very bright guy's whole career was spent as a farmer. As I said, he was the church organist. And my dad and all my uncles were very good musicians. Another uncle, my dad said, was the best trombone player in the whole area. My dad was a very good violinist. If he hadn't been a farmer he might have been a musician. As a little kid I remember him practicing every night, playing the violin or clarinet.

Then when we lived in Lemmon, I started taking piano lessons, which I did for five years. Not that I ever got that good.

Swent: So music has always been a part of your life?

Fuerstenau: Oh yes. That started with me during the last two years there in Lemmon.

Out of all the relatives, only my dad's youngest brother, Adolph, graduated from college and he became a parochial school teacher in a Lutheran church in Wisconsin. He and his family visited us in Lemmon in the summer of '37, and I drove back with them to eastern South Dakota and spent the summer on the farms with relatives.

Summers with Cousins on Eastern South Dakota Farms

Fuerstenau: That summer I first spent about three weeks with my Grandmother Fuerstenau, and at that time, an unmarried uncle that she lived with. And then, with my Uncle Elmer and family, who was now living on the farm that my parents had lost. Obviously they were renting it.

It's funny, I can still recall seeing headlines of something that I knew nothing about at the time in the Watertown paper, "Amelia Earhart Lost," in great big letters. And I can still see that headline. Of course, at the time, I didn't know anything about her.

During that summer, I stayed with relatives at four different places, including my other grandmother who lived in Watertown. She came out to get me in her old Model-T [Ford]. Roads then, as you well know, were either just gravel or dirt. It had rained, and she decided to go down a steep hill alone in the car while two of us walked down. There was another lady with her. And the whole fields were just full of little frogs and little toads. Did you ever see a field like that?

Swent: No.

Fuerstenau: I remember the expression used to be, “It rained the toads.” Of course, the water brought them all up from under the dry soil—I mean they were just jumping all over. You never see that any more.

So I spent probably two or three weeks in Watertown with her, and after that some time at another aunt and uncle’s, which was my mother’s sister, Clara Roisum—farmers near the small town of Lake Norden, south of Watertown. At that time there were two girls, cousins of mine, one my age. These cousins made the newspapers in eastern South Dakota because they had tamed a steer that they could drive and ride, and they would be in parades with this steer. By that time it was the last part of the summer and that meant harvest time—and this uncle had one of these big old Avery tractors that you saw then, one that would go about one mile an hour, with huge wheels, and a canopy over the top—it ran the threshing machine. I still recall all that was involved in threshing at that time.

Swent: Did helpers come?

Fuerstenau: Oh yes. I’ll tell you later about threshing in general. But first I want to tell you about another problem. My aunt and uncle had gone to Watertown for the day, and the three of us were there at the farm, and a cow was in a little stream along the edge of the farm. It didn’t flow much; it was a remnant stream flowing between two small lakes. And then all of a sudden the cow just died standing up in the water. It turns out that it was anthrax. Several cows died during the next day or two. Because anthrax is a disease that is dangerous to humans too, these cows all had to be pulled out and then taken out into a field and burned under piles of straw. That was a traumatic experience, because it came on that quickly. I don’t know how many cows they lost, but I assume it was two or three. And probably more around the region. When I returned home in the fall, I would have been in fourth grade.

By the way, one thing that cold country like Lemmon is very good for is ice skating in the winter time. They would flood half a city block around which the fire department had built a low dirt retaining wall. And so ice skating was always the main thing to do in winter, although it was very cold. Your toes could get a little—

Swent: It would be awfully cold.

Fuerstenau: You’re right about that. Anyway, obviously life was not good for my parents. So in the summer of 1938, my mother went down to Black Hills Teachers College in Spearfish and took classes to renew her teaching certificate, which will account for the next year.

Early that summer, my folks drove from Lemmon back to Watertown for a visit, and I remember that around Aberdeen the whole place was stripped by Mormon crickets. They were those great big Mormon crickets. You could see them everywhere. They would strip the whole countryside, around Aberdeen. They weren't around Watertown.

Swent: And they made the highway very slick too; I remember still.

Fuerstenau: They must have. Those were big.

Swent: I remember there were warning signs.

Fuerstenau: Is that right?

Swent: At some point I remember seeing signs along the road that warned of crickets. You had to drive carefully, they were such a menace.

Fuerstenau: I believe it. Knowing about ordinary grasshoppers, and these were much bigger.

Swent: Terrible.

Fuerstenau: Yes, yes. It was around Aberdeen where the Mormon crickets were very bad, which is where our current Senate leader, Tom Daschle, comes from. The soil in that part of South Dakota has a high concentration of arsenic—and that comes from the poison used in the thirties to control the crickets and grasshoppers.

That summer I spent on the farm of my uncle Martin with two cousins, Norman of my age and his younger brother, Gilbert. I would have been nine at that time, so the summer consisted of half play, half work. Lots of play. I remember catching pigeons, and putting them in a big caged crate where I would fatten them with grain. Those were happy pigeons, being fed grain, but at the end of the summer I guess we ate them all.

Work involved cultivating corn with horses and plowing with a tractor. I actually got fairly good at cultivating and plowing in a straight row. Starting about then, there was a program of planting trees under a program of shelter belts to stop the wind from blowing dust. Every farm had maybe a 100 or 200-foot-wide band of trees of different kinds that went along the length of fields. Around the farm houses of eastern South Dakota, the homesteaders had planted trees on the north and west side of the farm buildings. So around my grandmother's farm house there already was a grove of trees. But the shelter belts were planted on a much larger scale, probably in 1938, '37. It was a major program that really changed the area. Today, eastern South Dakota has a lot of deer, foxes, and animals like that because of the shelter belts. There

were no deer, no foxes, in those earlier days. The trees allow that. But shelter belts do stop the wind from blowing dirt.

So anyway, that summer involved lots of work and lots of play and also maybe three weeks of summer Bible school, that the church always had each summer.

But then came fall, which was the harvest time. The same thing applied for the two years that I was there: namely a group of farmers who were all inter-related, either Fuerstenaus or married to Fuerstenaus harvested together. Two farmers that were cousins of my dad had adjacent farms across the road from each other, and these were fairly prosperous farmers. One named Hubert and the other Jake. They never married and they both died at about 95. My mother said they had all their marbles at 95, which is wonderful. This was the Jake Fuerstenau who was head of the school board that would not rehire my mother in her first teaching job.

Jake owned a huge steam engine tractor, the kind that you have seen in old pictures. Hubert owned the threshing machine, a big threshing machine. And so the threshing involved a coal-fired steam engine. In those days, the grain was first cut with a binder after which, as a kid, we picked up the bundles and stacked them into shocks. The crops then were wheat, and barley, and oats, and maybe flax. Those were the main ones. Millet, I guess, another one. The dust storms were gone by then.

Anyway, that fall in order to run the steam tractor, a trainload of coal had to come in on the railroad, and the coal then was hauled to each farm to run the steam engine. Getting the coal on time was a problem. The general procedure was that two days of threshing would be done at each farm. There may have been six or eight at most. As I said, all of the farmers were related to each other, in a three- or four-square mile area. The group would thresh at one farm for two days, then they would go to the next farm for two days, and then to the next one, two days, so that each one could get some money by taking grain down to the grain elevator. I remember my cousin and I would take those first loads of grain in a horse-driven grain wagon to the elevator at the railroad station. It was about five miles or so that we had to go. We also loaded grain bundles with pitch forks into a hay wagon and hauled them to the threshing machine. The time for threshing was always a huge occasion. All the wives did the cooking and there were, of course, really two dinners a day—a big dinner at mid-day and another big dinner in the evening for the threshing crew. And after they had made the two-day circuit, they would start over again and then thresh out everything for each farmer. So they made two circuits through the system.

Swent:

And prayed that it wouldn't rain.

- Fuerstenau: Also so that it wouldn't hail. The following year—I was there one more summer—was essentially all work. Summer of '39, so I was now ten. And that summer, at first it rained just as harvest was to begin, and as you know for harvest that's bad. But the second problem was that the load of coal for the steam tractor didn't come in from the railroad. And that held everything up for I don't know how many days, several days. Then it rained for a bit. And of course, for the farmers in that area, hail storms can wipe them out completely. There was real angst that they had at that time of year. By the way, threshing machines were used until the mid 1940s, when harvesting was done by combines, which cut, thresh and clean the grain in one operation. Today when you drive through eastern South Dakota, every little while in a field right along the highway you see a relic threshing machine, like a monument to times past.
- Swent: Did your mother stay there to help too?
- Fuerstenau: She wasn't there at all. No, the wives of the farmers took care of all the meals, et cetera. They would have been my aunts.
- Swent: I thought maybe your mother had to go back and help too.
- Fuerstenau: No, no, she had no relationship with that, nor my dad.
- I was there for three summers, and I consider that a good part of my growing up.
- Swent: Wonderful experience.
- Fuerstenau: As a young kid you have a lot of responsibility if you are on a farm. The last summer involved plowing a bit with a tractor, stuff like that. I was only ten then. And I'll tell you, it's not easy to drive a tractor in a straight line while you're plowing. Sounds easy, but—. [laughs]
- Swent: And there are an awful lot of hazards too.
- Fuerstenau: You know, there really are.
- Swent: They wouldn't let a child around there now.
- Fuerstenau: When you stop and think about a tractor with no cover over the wheels! Then that was a typical farm tractor. All of that machinery. When you cut corn and so on. At times people did fall in, and get chopped to bits. People lost arms, and stuff like that. Nobody I knew, but one read of them.
- Swent: Very dangerous.

Fuerstenau: Oh yes. Farm machinery was dangerous. And I suppose it's like all machinery that you use daily—you forget the dangers. For miners that's often—they talk about you have to be careful about that which is routine, you don't make the short cut and so on.

Those last years I was involved in milking, and maybe going out and getting the cows and bringing them in. Some of them were milked by milking machines and some were—I remember my uncle had an old—one of these one-lung motors, you know, you start and it goes, “chug, chug.” I would hear that and then I would wake up to hearing this chugging. It was supplying the vacuum line for the milking machine in the barn. You had to do a lot of the milking. Some cows wouldn't like the milking machine. You had to learn how to milk, which—once you learn it's easy, but it takes a while. [laughs]

[Tape 2, Side A]

A Year at Slick Creek Country School

Fuerstenau: In 1938, I told you my mother went to summer school at Black Hills Teachers College to renew her teaching credential and got a job teaching in a country school called Slick Creek School. [pronounces it “Slick Crick” and laughs, spells] And this was near Lodge Pole, South Dakota, south of Hettinger, North Dakota, which is about twenty-five or thirty miles west of Lemmon. And this was a country school, right on the Grand River. My dad came out on weekends for a while. Then they rented an old abandoned farm about two miles away where we lived in the farm house for that year.

Swent: Did you go to the Slick Creek School?

Fuerstenau: Oh yes. This was all eight grades, and I was then in the fifth grade. My mother was the teacher.

You know the Grand River, shortly before it flows into the Missouri, is where Sitting Bull lived. South of that there's the Cheyenne River, the White River, right? I don't remember all of them. About four or five rivers cross western South Dakota. The most northern one is the Grand River. It wasn't very grand. It was, you know, ten to thirty, fifty feet wide. That type of thing.

Swent: And four inches deep.

Fuerstenau: Right, most of the time. And anyway, we were there that year, and I remember the dirt and Russian thistles, and I always told my son how the ant hills near the Grand River were basically Indian beads—little tiny blue, and red, and white beads. Indians, of course, had camped along the river, and so the ant hills had a large percentage of Indian beads which the ants were using as sand grains. Everybody always had collections of arrowheads. One spring day

walking to school over the dirt where nothing was growing, I remember finding a great big Indian spearhead that I had for many years. Somewhere I lost it. But it was just lying on the top of the dirt in this field. We were there that entire year.

Swent: Were there Indian children in school with you?

Fuerstenau: No, none, not at all.

In the spring and the fall one could fish in the river for bullheads, the standard fish. But I do remember the ice breaking up in the spring. And of course, we ice-skated on the river in the winter, which was about our only entertainment. In the spring, the ice broke up, and it's amazing to see masses of ice chunks break up and crash. That took place in a single day. I think they let school out so people could go watch it. It would make big ice dams that would dam up the water for a while, but then it would break loose and the water would rush on.

Farmers, along that river irrigated by pumping from the river, so there were only a few farms there. I remember the farmer who was head of the school board, his name was Lingel, a German. One evening we were visiting Lingel, and he wanted to listen to Hitler talk on a shortwave radio. That was a speech Hitler was giving in Germany. But I didn't know any German then.

Swent: Do you have any recollection of what his attitude was towards this?

Fuerstenau: Not really. But anyway, they were all second-generation Germans. Of course, this was just at the time when all Europe was concerned with what Hitler wanted to do.

Swent: How was that, having your mother as the school teacher?

Fuerstenau: Probably made me work. [chuckles]

Swent: I'm sure.

Fuerstenau: My mother was a superb teacher. I remember almost nothing about the school except for taking the state performance tests. It was a one-room country school with a dozen or fifteen kids, at most.

Swent: Some of whom were your siblings.

Fuerstenau: Right. Maybe all of them. It must have been. Maurie could have been in the first grade, but maybe not.

Four Years in Mobridge, SD, Right on the Missouri River

Fuerstenau: When school was out that summer I went back again to the farm. This would be '39. Then my parents got what amounted to a dual job with a Lutheran academy in Mobridge, South Dakota, which is on the Missouri, west of Aberdeen. My mother took care of the dormitory, and my dad the grounds and buildings [supervisor] for this. I think this was probably a good school, which was closed a few years ago. But farmers from 100, 200 miles around who wanted to send their kids to this high school, could do so.

Swent: And you went to the same school?

Fuerstenau: For one year. This was a high school. I was now in sixth grade when we moved to Mobridge. So I did sixth, seventh, and eighth grades in the Mobridge schools and then the ninth grade at this Lutheran academy.

Swent: But some of them were boarding students.

Fuerstenau: Most of them. Most of them were. There might have been three or four that were local, or five or six. But it was a boarding school, and I think it served a very good purpose. A very solid education.

The summer of 1942, which I often think back on, I can hardly believe how much work I did for so little. There were three different truck gardeners right along the river—one big one owned by Steve Denoff right near where the highway bridge crossed the Missouri. He wanted to hire people to pull weeds. Some grade school friends suggested that we go down to see if we could get a job. So we went down to the river, were hired, and got paid ten cents a row, and I really pulled a lot of rows. He had a large truck farm where he grew all sorts of vegetables, melons, and possibly some fruit, and everyday he made a trip to town delivering fresh vegetables to the stores.

Anyway, he hired me and another high school kid after the few days of weed pulling, to continue working. He paid me eight cents an hour, and the high school kid twelve-and-a-half cents an hour. The high school kid, after about three weeks, finally quit. But I didn't, and you know, I worked all summer, six days a week, and it was from seven in the morning until six at night. I got up and left at six and either hitched a ride with a road grader, because there were still a lot of gravel roads—or with two Indians in an old car going to work somewhere. Either one or the other picked me up in the morning down along the road. I usually walked back. It was a mile and a half, maybe, cutting across the right-angle highway. By the way, we drove to South Dakota in 1975 and saw that all three ancient flood planes of the Missouri where the truck farms had been were under water because of the construction of the Oahe Dam near Pierre. By then, a lake about two or three miles wide came right up to the city limits of Mobridge.

Sometimes I stop to think about that dedication—five bucks a week. That experience may have entered into the dedication towards work that I’ve had. And of course, through all the years, I have told my own kids about my pulling weeds for eight cents an hour, especially when they were little. They heard “eight cents an hour” a lot.

But you know, the other day I looked up something because I wanted to know just how bad pay in general was then. I had just read an interesting book called *Freedom from Fear: The American People in Depression and War, 1925-1945* by David M. Kennedy, published 1999, Oxford Press—big volume. It got the Pulitzer Prize two years ago. And I had read this book and I remember—

Swent: He teaches at Stanford, doesn’t he?

Fuerstenau: He’s at Stanford, yes. In history there.

Anyway, I remembered seeing something about what pay was at one point, so the other day I looked it up. In 1940, the average industrial wage in the U.S. was seventy-four cents an hour. And he said retail clerks got thirty-five to fifty cents an hour. So an adult working for thirty-five cents an hour in a store was not that much better than the eight cents an hour I got as a kid, pulling weeds.

Swent: You weren’t being especially persecuted to get the eight cents an hour either; that was probably the going wage.

Fuerstenau: Must have been, when you were paying high school kids around that.

Swent: It was probably a fair wage.

Fuerstenau: I don’t call it child labor or anything, but I ended up doing a lot of things: driving a tractor, pulling wagons, cultivating, and things like that. Then in the fall at the time of the final harvest, he paid fifty cents for two hours’ work after school. That’s when you really have to get everything in before the first frost. In South Dakota frosts can come pretty early.

Swent: And that’s the end.

Fuerstenau: So that fall I started high school at the Lutheran academy.

Swent: Were your teachers there mostly men?

Fuerstenau: They were all men and they all were ordained ministers. Not that they were preaching. Well, they preached once in a while. They were addressed as professors. In fact, one of the ministers had married my parents in 1925. It was his first wedding back in a country church in eastern South Dakota near Hazel.

But Mobridge, again, was sort of like Lemmon in winter in that ice-skating was your main activity. There were several excellent ice-skating rinks.

Swent: What kind of skates did you have?

Fuerstenau: I think in Lemmon they were strap-on ones, but they were hockey skates by the time I was living in Mobridge. I was in sixth, seventh, eighth grade. Not that I ever did much hunting, but sometimes in winter I would go out rabbit hunting because you could get fifteen cents or something like that for a white jack rabbit.

Swent: Who bought them?

Fuerstenau: Somebody locally. I remember often coming back so cold. We would get what was called “chilblains.” Oh boy, did that hurt! And it takes a long time to get over it, an hour or two, it seemed.

Swent: On your face?

Fuerstenau: No, usually toes. Toes and maybe fingers. We would have earmuffs and stuff. Once your feet start getting cold, you can’t do much about it, right?

Swent: No. What sort of clothes did you wear to the Lutheran school? A uniform?

Fuerstenau: No uniform at all.

Actually, there was by definition a course in religion, but I must have taken at least five other courses. This was more than one would take in a high school. That came to my advantage later on. I took a course in German, and Latin, and I suppose algebra, and whatever you took in English, and history, and so on—obviously, solid, good courses.

Then that summer, or late spring, my dad had some kind of job offer in Provo, Utah. And he got as far as Rapid City where a second cousin of his, named Adolph Stoltz, owned a Coast-to-Coast store. Stoltz talked my dad into staying there and working for him. So that’s how we moved to Rapid City, summer of ‘43.

Swent: At this academy—this is the middle of the war—were there current events classes and was there any particular sensitivity about what was going on in Germany?

Fuerstenau: Well, I do know that German services stopped in church. By the way, my mother said that in World War I, in South Dakota, a lot of German Lutheran congregations found their churches painted yellow, and stuff like that. Then eastern South Dakota was a quarter or more German, and many were first

generation immigrants. That was World War I, but in World War II there was none of that really.

Swent: Of course, these people were all a couple of generations removed.

Fuerstenau: But it's interesting about Charley Lingel—that was his first name—wanting to listen to Hitler's speech on the shortwave radio. But that was in 1938 just before the war really started. So what his interests and leanings were, I—

Swent: I wonder if any of these people were still maintaining any contact with relatives back in Germany?

Fuerstenau: I knew none of that.

Swent: They really cut themselves off once they came here, didn't they? That was it.

Fuerstenau: Yes, yes. One of my mother's sisters had some interaction with cousins in Norway, for example, even up to modern times. That's the only interaction I ever heard. I've never bothered to look up anybody when I have been in Europe.

Swent: They didn't have telephones, and e-mail, and all those things.

Fuerstenau: Right, true. I did get a letter once asking for some reprints; this was twenty-five years ago when we were living in Germany—I'll be into that later. This professor wrote from what was then East Berlin, saying that he had a close friend, a Lieutenant Fuerstenau, who was killed in the war, wondering if that was a relative of mine. Who knows? I'm sure glad that my grandparents had left Germany.

My dad said the whole family had moved to this country. But obviously there might be secondary relatives that we all have that we don't—.

Swent: But they just completely cut loose from them.

Fuerstenau: I think the one thing that really stopped was anybody speaking German much. When we lived in Lemmon, that church really had a lot of old Germans—lots of first-generation people who spoke German. I remember one man named Wulff who said that he had homesteaded in 1909, about twenty miles west of Lemmon, and that when he got there, the whole countryside was grass three or four feet tall. Grass like that is gone. By the way, in 1878-79 eastern Dakota territory was open for homesteading, and at Wulff's time thirty years later land available for homesteads had moved on to western South Dakota.

I want to come back to the Slick Creek School. I always told my son Steve about the ant hills with the Indian beads. The only time we were back in that area was 1975, when he must have been about twelve. He wanted to find the

ant hills with the Indian beads, so we went looking. That whole area is now the National Grassland, and that summer everything was covered with green grass. So there were no Indian beads to be found. [laughs] No arrow heads, or Indian beads, or anything to be found, because of the grass.

But anyway, my parents became good friends of a family named Storm—and they were farmers—from church in Lemmon. They lived across the North Dakota state line—just north of Thunder Hawk, South Dakota. Wilhelm Storm had homesteaded there about 1906, and ended up with several sections of land. He was obviously quite astute.

You asked earlier about electricity. Well, when they first put electricity on their farm it was put in the barn. [laughter] The house didn't come for two to three years later, my mother said. A real old German.

Swent: First things first.

Fuerstenau: First things first.

A Summer Herding Sheep in North Dakota

Fuerstenau: Anyway, their son Walter had the farm next door, and I got a post card asking if would I come and herd sheep. So I did that for the summer of 1943. Walter Storm then had about 2,000 sheep. The sheep were in an area probably two square miles, or so, of rolling prairie land. Every night the sheep were put in a pen that was about a mile or two away from the farm house. I would head out by seven in the morning and put the sheep in at six, and then ride my horse back to the farm.

I did that seven days a week. And only a couple, three times in that whole summer did I have any time off, probably on a Saturday or a Sunday. I was now fourteen. And I think that was not good to be working seven days a week—. Once in a while, a young guy who was spending the summer at the father's farm came out, and he and I went swimming in the nearby Cedar River, a couple of times. But, basically I did that every single day and—

Swent: All alone.

Fuerstenau: All alone. I had a worthless dog. It's funny. The dog was so useless that I just never took it with me, only a horse. At first I just left the saddle tight on the horse all day. Walter Storm rather forcefully told me to loosen the saddle because the belt strap started to cut through the hide of the horse. I didn't know that. And he said, "You should know better than that." I didn't know better than that; I had never been told. Anyway, after that I then always loosened the saddle straps.

About noon the sheep, as you might know, all huddle together, until about three o'clock in the afternoon. You'll see big groups of them, a hundred, all together with their heads down. Finally I asked, "Why do they do that?" Well, apparently it keeps the bugs and the flies off their nose. Having all their heads down inside that mass of sheep in the hot summer must not have been too pleasant.

That summer I remember reading airplane magazines. It was in the war. I would buy those airplane magazines, and read about fighters, and bombers, and whatnot.

A lot of times I would wake up and the sheep were gone. I know one time I went up one draw, down this draw, because it was all hilly, water-cut, called draws. I finally found the horse. I could see the sheep way off, probably two miles away. Then I had to go and drive them back.

Another time was worse than that, because when I woke up—Little Bo Peep asleep under the haystack—I could see the sheep way off in another corner where there was a field of oats, and they were out in that oat field. You can imagine getting about 2,000 sheep out of a field of oats without a dog. I just had to ride back and forth on this horse. I got them out, finally. I imagine this was late in the summer, because the oats were already getting brown. So the farmer, whoever he was, must have been shocked at harvest time to see one end of the field trampled. That must have been manna from heaven for the sheep to get fresh oats like that.

So I did that the entire summer. I amused myself with things. By experiment I found a little rabbit can swim only three times across a small stream.
[laughter]

Swent: A little scientific research.

Fuerstenau: Right. It was terrible.

Swent: Then, at the end of the summer, I had to go to Rapid City. So how do you get from Lemmon to Rapid City when there was no train or bus—did you ever know about star mail routes?

[Tape 2, Side B]

Fuerstenau: You could make reservations with the star mailman. His route went from Lemmon to Faith, something like that.

Swent: Buffalo, I remember.

Fuerstenau: Well, then somewhere we changed to another mail man and went to Newell where I caught a bus down to Rapid.

Now that you mentioned Buffalo, I had made another note here. A few years ago, my friend Frank Aplan sent me a book. The book is entitled *Sheep*, and then in small letters it's called *Life on the South Dakota Range*. This book is written by Archer [B.] Gilfillan. It's republished by the Minnesota Historical Press—first published in 1929, and a new edition was published in '93. When Frank sent the book, he said, "This is no joke." And I read the book. Gilfillan had graduated from Penn, University of Pennsylvania, and had worked, like I did, on farms in the summer and had gone out to Buffalo, South Dakota, and herded sheep a little bit for somebody. Then he returned to the East somewhere, I think St. Paul, Minnesota or maybe Chicago, and entered a seminary to study for the ministry. Just the day before graduation, he decided not to graduate and went back out to Buffalo. He had an inheritance of a few thousand dollars and bought a farm that he lost. Then he spent the next sixteen years herding sheep, after which he wrote this book. Of course he lived in one of those sheep wagons that you've seen pictures of. I didn't. But he wrote beautifully about how the sheep reacted. I had observed many of these very same things about sheep.

Swent: I remember that name.

Fuerstenau: He died in Deadwood in '55. But he worked, after he left the sheep, in the '30s. He lived in Igloo, south of the Black Hills, and wrote once in a while for the newspapers. In the book, *Sheep*, he describes the lambing time. During my summer of sheepherding, two sheep had lambs. They were there for a few days and then unfortunately the coyotes got them. After that Walter Storm told me to find the ewes and milk them. So for a few days, I tried to recognize them; maybe they were bleating because they were not doing too well, and I would milk them for a bit.

Swent: Did they use the milk?

Fuerstenau: No, it was just to get rid of it because they lost their lambs to the coyotes. You would see coyotes around once in a while.

Swent: So you went in the morning and let them out of the pen, and then they just went—?

Fuerstenau: In an area that was probably two square miles or more that was available for grazing.

Swent: And the oat field was not fenced?

Fuerstenau: There was a fence around the oat field, but some section of it must have been down. I was the herder that was supposed to look after the sheep and keep them in the grass area. But when the sheep were spending about three hours clustered, I usually took a nap.

- Swent: Were you supposed to keep coyotes away?
- Fuerstenau: I didn't have a gun or anything. But by the way, when I first got there, the first day, they were shearing the sheep.
- Swent: By hand?
- Fuerstenau: Some of them were like a big hedge clipper and they also had heavy duty electric shears. They went fast, and of course they nipped the sheep a lot too.
- Swent: Local people, or did they bring in helpers?
- Fuerstenau: Oh, Walter Storm, and probably a couple of other helpers. The wool was put into bags that were as tall as the door, let's say. And I remember I was the person inside of the bag stomping the wool. I wasn't very happy about that. I thought, "What if there are ticks here?" But at least nothing happened. But that was my very first day on the job.
- By the way, I got paid thirty-five dollars a month that summer. And of course that involved room and board also. But still, that's just a little over a dollar a day, wasn't it? I was getting eighty cents a day pulling weeds, so I'm assuming I didn't get taken that badly.
- Swent: You took a lunch with you?
- Fuerstenau: Oh yes, sure. Of course, my worst experience was when the seven bucks got loose. They were several miles away, and when they found the sheep, I couldn't do anything. Finally, in about the middle of the afternoon, I put all the sheep in the pen with the bucks—they must have been in heaven—and went home to get Walter Storm, wherever he was, to come back and get those damn bucks out of there.
- Swent: They were not normally with the sheep?
- Fuerstenau: They came from several miles away. I don't know where they were kept. He may have had an awful lot of young lambs that winter; I have no idea. [laughter] That situation I just couldn't handle. Still, as a young kid, trying to solve problems like that was, again, a good part of one's developmental background. I was 14 years old that summer.
- Swent: I'm thinking, there must have been days when it rained?
- Fuerstenau: Only towards the late summer, when some days got grey. I remember two days then when there was a drizzle rain. I did crawl under a haystack, and slept under the haystack out in a field. I hollowed out a spot and took my nap under the haystack. I probably had a rain jacket on. I don't remember now

what the sheep did. But, as I recall, there were no thunderstorms. None that whole summer.

By the way, Wilhelm Storm, the father of Walter Storm, owned about 2000 acres, with much of it in wheat. I know that in one of those years he got something like 50,000 bushels of wheat. By then, harvesting in northwestern South Dakota was done by large crews traveling with several combines. They started in Kansas and worked their way north. They would come through and they would handle on a so-much-per-bushel basis, do all of the harvesting. So farmers such as the Storms didn't do any of their own harvesting. I have no recollection of that in the eastern part of the states where the farms were smaller. Harvesting was done by tin threshing the way I mentioned, but later eastern farmers then bought their own combines. But in the western part, it was done by these crews because acreage of farms was so much bigger.

Swent: That was a timing problem too, to get the combines.

Fuerstenau: Oh yes. But I have no idea how that was worked out. I imagine it was managed by contract with the same group each year.

Moving to Rapid City, South Dakota, and High School

Fuerstenau: Well, it was '43 when we moved to Rapid where I entered the tenth grade in Rapid City High School. My dad had been offered a job in Ogden, Utah, and stopped in Rapid City on his way to visit his second or third cousin, Adolph Stoltz. He was offered a job there and decided to stay in Rapid City instead of going on to Utah. Rapid City was and is the second largest city in South Dakota. Of course, the major thing of Rapid City was the air base. It was the final B-17 training base before they went to England. I recall seeing those massive flights congregate over Rapid City—where they were obviously practicing congregating into a great airwave. The air base dominated Rapid City at that time.

Swent: It was the Ellsworth Base, wasn't it?

Fuerstenau: It wasn't called that then.

Swent: What was it called?

Fuerstenau: I think it was simply called Rapid City [Army] Air Base. You see, Colonel [Richard E.] Ellsworth was the base commander, probably in the early fifties, when he was killed in a crash of a B-52 bomber that he was piloting over Greenland. That's when they named it Ellsworth Base.

You know about traffic jams. In '43, '44, '45 there was a steady stream of cars going to and from the air base at seven o'clock in the morning and five o'clock in the evening. It completely dominated Rapid City then.

Swent: Big change.

Fuerstenau: Yes. Well, you know, the weather was very good for flying because western South Dakota has so many days of sunshine.

[Interruption]

Fuerstenau: Before we were talking about Rapid City High School. I think public schools were very good in those days.

Early on, after starting at Rapid City High School, I saw some kind of ad about wanting a high school student to work for a couple, three hours after school in a grocery store. It was a semi-sized grocery store called the Spic 'n Span, and it was owned by somebody named John [T.] Vucurevich. I went down and talked to him, and he hired me. He owned this store, and with his wife, maybe three or four other people worked there. And Vucurevich, who was then about thirty-four, got drafted into the army sometime during that year, '43, '44. I remember seeing him wearing a private's uniform.

Anyway, I worked at the Spic 'n Span on Saturdays, and after school, all the way into the fall of '45 or a little later.

John Vucurevich is interesting to talk about because he came from Lead, and he and his wife had, maybe in Spearfish, a little restaurant or café before coming to Rapid City. John Vucurevich had only gone through the ninth or tenth grade in Lead. I think he was born in Yugoslavia, and came over as an infant with his parents.

Swent: I think his parents had a grocery store in Lead.

Fuerstenau: Anyway, he—and I have seen him in very recent years—he is a very well educated man. When he came back out of the army, he bought a state bank in Hill City. Then, he transferred that state bank to Rapid City. Eventually, he owned many banks in South Dakota, Iowa, and Wyoming, all through the state bank system. He was in the state legislature for a few years, and now is a wealthy individual who has set up foundations in Rapid City, and in Vermillion at the University of South Dakota. Amazing success story, and a very nice man. I've visited with him several times through the years when I've been in Rapid City. He's still alive, still sharp, eighty-five or more. [Note added by Douglas Fuerstenau: John Vucurevich died at age 92 in 2005.]

Swent: I think his mother was killed in that flood.

- Fuerstenau: She was. His mother was killed in the 1972 Rapid City flood, along with nearly three hundred others.
- Swent: Were there that many?
- Fuerstenau: Yes. But his mother was one. Those were all very nice houses, down along Little Rapid Creek, as you know. And obviously it overflowed, and she must have been in one of them. She had to be quite senior then.
- Swent: Yes. I may be mistaken. Maybe his father worked in the mine; I don't know.
- Fuerstenau: Must have. I vaguely remember reading somewhere that he did.
- Swent: What we called "Slavonians," in Lead—I think they were really Dalmatians, from Dalmatia—several of them did have grocery stores.
- Fuerstenau: Is Dalmatia Croatian?
- Swent: Some of them were Serbs and some of them were Croatian, I think.
- Fuerstenau: But John is such a self-educated person with only a ninth or tenth grade education in Lead.
- Swent: They were a very fine family. They've all done well. His brother, Alexander, was the one I knew better. The younger one.
- Fuerstenau: After Vucurevich was drafted, the store was managed by Bill Miller and I continued to work there. That really eliminated a lot of extracurricular activities that I would have had in high school.
- Swent: But you were probably learning other things.
- Fuerstenau: Certainly. I still recall prices then. For example: a pint of ice cream was fifteen cents; a loaf of white bread—from Swander's Bakery—cost fourteen cents, and whole wheat bread was eleven cents a loaf. I also learned a fair amount about meat. The butcher at the Spic 'n Span was Bob Henderson, a native Scotsman who must have been in his early sixties then. He used to take a lot of the credit for my success. A lot of people are always taking credit for my success. [laughter] Anyway, he not only was the butcher, but also he was a real fisherman who could always get trout in Rapid Creek. And I more than once went fishing with him and he would watch what the fish were biting and that would become our bait. When he was taking a break, or wasn't at the store, I would be the butcher. I'll tell you, it's not easy to slice a slice of ham on a bone with a saw. And those days, sliced ham was the most expensive meat you could get. It's hard to understand why.
- Swent: Well, meat was rationed.

Fuerstenau: Oh yes, completely. Everybody had the rationing books, as you remember. Gasoline especially. My brothers and my dad were all ardent hunters, but I never was. And I think that because there was no gasoline when I was in high school, I never went out hunting and never developed the kind of interest in hunting that my brother Maurie has.

[Interview 2: July 25, 2001 in Berkeley, CA]

[Tape 3, Side A]

- Swent: When we stopped on Monday, you had just gotten to Rapid City. You recalled some things about your grandmother's garden that you thought were worth mentioning.
- Fuerstenau: You had asked me about my grandmother's garden. I now remember, my Grandmother Karterud lived on the edge of Watertown. She had a huge garden. They grew all their vegetables, I think probably for the winter. My Grandmother Fuerstenau, I now recall, always grew a lot of nice fresh lettuce, carrots, peas, beans, and so forth, and I had completely forgotten about the main dessert which was rhubarb. Rhubarb pie and rhubarb sauce. And to this day, a great radio program is *Prairie Home Companion*, where Garrison Keillor is always talking about rhubarb pie. That's the only fruit-like thing that will grow in eastern South Dakota, and probably western Minnesota. No apples, but crab apples and choke cherries.
- Swent: And your grandmother probably made pie almost every day, didn't she?
- Fuerstenau: Well, it was part of life.
- Swent: You moved to Rapid City in 1943, and it was the middle of the war.
- Fuerstenau: Directly in the middle of the war. Rapid City, at that point, was one of the main air bases in this country for training B-17 bombers.
- Swent: What about Pearl Harbor, when that happened?
- Fuerstenau: Then we lived in Mobridge, and I can still remember that late Sunday afternoon, maybe at five or six o'clock in the evening, hearing on the radio about Pearl Harbor. Of course everybody was very concerned. Not like I understand how people were out here in California on the coast. But I remember that the next day—I was in eighth grade—there was a general assembly held in the school so that we could listen on the radio to Roosevelt's speech and declaration of war. I can still hear his voice.
- Swent: The South Dakota National Guard had already been activated at that time. Did you know anybody in the Guard?
- Fuerstenau: No, I really didn't. I was a little too young, fortunately.
- Swent: Right. I thought you might have had neighbors or uncles.
- Fuerstenau: Too small a town.

- Swent: Because they were activated, actually, before Pearl Harbor.
- Fuerstenau: Where we were living then was Mobridge, 3,000 people. For South Dakota a fair-sized town, but that's all it was then. Now it is considerably larger. I do recall someone whom we knew having one of the early draft numbers.
- Swent: But that was a significant day, even in Mobridge.
- Fuerstenau: Oh yes. When I first got to Rapid City I remember blackouts. Can you believe having blackouts in the middle of the country? Pulling your shades?
- Swent: Rapid City would have been a strategic target, wouldn't it?
- Fuerstenau: True.
- Swent: And gas rationing, and you had mentioned meat rationing.
- Fuerstenau: Sugar, as you well know. Gasoline, and meat were the—
- Swent: Tires.
- Fuerstenau: Tires, yes. Tires and gasoline probably went together. By rationing gasoline they may have saved the tires. I think that was what was behind it.
- Swent: Farmers were exempted from some of that.
- Fuerstenau: Completely. I know on my uncles' farms were tanks used as gas containers. Maybe they would hold several barrels of gasoline. Needless to say, they could fill not only tractors but also their cars or pick-ups or whatever right from the farm.
- Swent: But youngsters weren't driving around in cars at that point.
- Fuerstenau: Oh no, not I.
- Swent: And radio was the way you got your news?
- Fuerstenau: And all entertainment. Sunday night was Jack Benny, Fibber McGee, Fred Allen. You probably listened to them too.
- Swent: Absolutely. So you went to high school then, in Rapid City. Some of the teachers, you said, were very influential.
- Fuerstenau: Right from the very early days my mother always preached that we would go to college. In fact, she said, "Then you can get a job being a county agent." Maybe when you're a farmer, being a county agent meant you had a real job.

Fortunately, I never ever thought of going that route. But that dictated my program, because I then took a very academic high school program.

Swent: Was the pressure the same on your sister as on you?

Fuerstenau: My sister is a year older than I am, and today is her birthday, July 25. She went one year to Milwaukee to study nursing. I guess she got homesick and decided to come back to Rapid City, and did. I think that was probably the end of her advanced education. But I and my two brothers all graduated from the School of Mines.

Swent: But your mother wasn't as successful at pushing your sister?

Fuerstenau: Apparently not. Her problem was homesickness.

Swent: Was this part of your father's goal for you also?

Fuerstenau: No, not that I recall. But he may have a goal with relation to my middle brother, Dick, who went all four years to that Lutheran academy, and then one year to a Lutheran college in Minnesota. In my father's eyes, being a minister was probably the highest calling, and I think he wanted one of his sons to be a minister. But my brother got pneumonia very badly in high school. So badly that they called my folks and said that if his fever doesn't break by the morning he won't survive, just like in the movies. But he did. Then he got pneumonia again, that first year of college, and he quit, came back to Rapid and started at the School of Mines. This same brother died at age forty-two, forty-three of lung cancer. He smoked, but it's my personal opinion that the damage from two cases of pneumonia probably weakened his lungs, because forty-three is very young.

Swent: His lungs must have been vulnerable.

Fuerstenau: Right. I have always been a major photographer; that's one of my activities, and every picture I have of my brother Dick he always shows a cigarette in his hands. Fortunately, I never smoked a single cigarette in my entire life.

Swent: Does Maurie smoke?

Fuerstenau: My brother, Maurie, smoked fairly heavily, and when the surgeon general's report came out he stopped cold. Never touched another one.

Swent: Did your father smoke?

Fuerstenau: Pipe and cigars. I don't think I ever saw him smoke a cigarette.

Swent: And your mother?

- Fuerstenau: Never.
- Swent: Probably not. It wasn't something ladies did. What about drinking?
- Fuerstenau: About zero. I would say, to me it would be zero. Maybe a beer once or twice a year.
- Swent: You didn't have it in your home?
- Fuerstenau: Not at all.
- Swent: What did your family do for social life? Did your parents dance, or play cards?
- Fuerstenau: Yes, cards. I think church probably was a big part of their social life, and friends from church. It would not be what I would call an active social life.
- Swent: Lots of good eating but no drinking.
- Fuerstenau: Absolutely none. I usually now have wine at dinner, whereas they never had.
- Swent: Things have changed a lot. Well, let's talk about high school.
- Fuerstenau: Well, I really think one was blessed with some very good teachers. In high school I actually worked, and studied, and did the homework. In fact, in Rapid City High School every grade I had was an A, except one. And to this day it kind of rankles me a little bit that the Latin teacher wanted students to turn in a special project each six-week period. One six-week period I didn't do that and she gave me a B for the six weeks.
- For one of the special projects—I like to draw—so I made a poster of Uncle Sam. Remember the Uncle Sam poster, “I want you,” during the war? Only I put it in Latin. So that was my one six-week project, which is kind of silly. I could translate Latin faster than anybody else in the class could read it. Only two years of Latin were available, and that's all I would have taken anyway. That teacher was named Miss Sogn.
- I had a very good English teacher, Miss McCullough. I think she probably did her job well, because I think I've written a lot, and I think what I write reads fairly well. I imagine she had something to do with the beginning of it.
- The math that we had in my sophomore year was geometry. I went to my fiftieth class reunion—the only class reunion I ever attended—and my wife was astonished at how many different people came up and said, “You helped me every day with my geometry.” I had no recollection of that.
- Swent: You were an educator, even then.

- Fuerstenau: Maybe so.
- Swent: We should say, Rapid City was—I guess it was the second biggest city in the state, wasn't it? It had a wonderful school system. In the whole West River half of South Dakota, that was the big school.
- Fuerstenau: In those days Sioux Falls had maybe 75,000 people. Perhaps Rapid City 50,000-60,000, going up a little during the influx of people related to that air base.
- Swent: They did attract, certainly, the best teachers in the region.
- Fuerstenau: Then in my junior year I had two really outstanding teachers. One taught math, Florence Krieger. I looked in an old school book and saw she had real math degrees, such as a master's in mathematics from the University of Wisconsin. Her background wasn't a degree in education. She was not only an excellent teacher, but tough. I had advanced algebra and trigonometry from her, and a special course in solid geometry.
- Another teacher I enjoyed was Earl Prunty who taught chemistry. Even later when I was a couple years out at the School of Mines, I would go back once in a while after school and talk to him. He must have been an inspiration.
- Swent: That's probably the field that has changed the most since then. I'm thinking, maybe Latin and English and math haven't changed that much, but chemistry then was very different from what it is now.
- Fuerstenau: Totally. High school chemistry has to be roughly the same, but new directions in chemistry certainly—even of which I've had a small part—have really moved ahead.
- One thing I did in high school was play in the band, which really was virtually the only outside activity I participated in.
- Swent: What did you play?
- Fuerstenau: I played saxophone. I enjoyed it. But I'll tell you, playing a saxophone, marching at the last football game in November in South Dakota, really made your fingers stiff. You couldn't wear gloves. A trombone player or trumpet player could wear gloves. I still recall marching around and playing with fingers practically frozen.
- Swent: Did they have those spring music festivals still? When all the people got together from all over the state?
- Fuerstenau: Remember, this was during the war.

- Swent: Maybe they didn't have them during the war.
- Fuerstenau: Probably because of transportation problems then.
- Swent: They must not have had them then. Before, that was a big event in my high school life, the music festival in Rapid City.
- Fuerstenau: Is that right? I just don't recall.
- Swent: The high school bands and the glee clubs came from all over. It was a big social event as well as a pretty nice musical event. But I guess during the war they had to cancel those.
- Fuerstenau: Speaking of musical events, there used to be a series of community concerts. They even had them here, in Berkeley, when we first came. In Rapid City, I got tickets to these concerts and I remember—well—who's that super, excellent bass singer?
- Swent: Paul Robeson?
- Fuerstenau: Yes, Paul Robeson sang, and I remember that he sang fourteen encores. I counted them. And that was one of the highlight community concerts during my high school days. Another one that I recall was a concert given by the pianist Rudolf Serkin.
- Swent: Really top talent.
- Fuerstenau: And I got those tickets myself; but my folks didn't; I have no idea why or why not. It started my interest in music. It wasn't a performing interest, but a listening interest.
- Swent: There wasn't much exposure to opera.
- Fuerstenau: Zero. As I said, my real exposure to music was playing in the high school band. Because I worked after school and on Saturdays, there was little time for much else.
- Swent: There's a limit to how much you can do, and get straight A's too. And you were young.
- Fuerstenau: You know, at times I still think back that in the sixth and seventh grades there in Mobridge at the beginning of each year, your teacher always asked what your father did. Then, I don't think I ever got a grade above a B and I think that grading was determined by the stature of the family in the town—in a little town like that.
- Swent: Could be. But that didn't happen in Rapid City? Big city, didn't matter.

Fuerstenau: No, not at all. Then in January or February of 1945 somewhere in through there, one of my very good friends, Frank Marion, said that he was going to go to summer school and graduate. I looked at my total course load, and because of the extra course or two that I had taken at the Lutheran academy, plus credit for band, I found that if I went to summer school I would have enough credits to graduate. So in the summer of 1945 I took the three required senior subjects and finished in August.

So in the spring when I decided that I would start at the School of Mines in the fall, I needed solid geometry. So Florence Krieger, after class, taught me solid geometry, after four o'clock. With maybe one other person, I took this course in solid geometry for credit. Then, sometime in April, I remember somebody coming in and announcing that Roosevelt had just died. That event still sticks in my mind. So that was the way that I finished high school, really quite unplanned, in the fall of 1945. I was then sixteen.

That summer, the high school classes were in the morning, and in the afternoon I had a job in Swander's Bakery. My job was helping the man running the bread cutting and wrapping machine. I would have to go get the racks on which loaves of bread that had been baked were cooling, and bring them over to the bread slicing machine and wrapping. He would then take the loaves and put them into the machine, and take them off the other end of the machine and put them into boxes that I had gotten. I would carry the boxes to the half dozen different trucks that were going to deliver them in Rapid City and in various towns in the area, including Lead, Deadwood, and Sturgis.

I did that all summer, and of course I remember the big events of Hiroshima and Nagasaki, and much discussion over what that might mean. No one really, at that level, knew what atomic bombs were.

Swent: It was the end of the war.

Fuerstenau: It was the end of the war. Like I said, even all during early high school, I regularly read a lot about what went on in World War II. You would see big headlines of bombing Frankfurt and Mannheim, with five hundred bombers at once.

On my first trip to Germany, I got off the train and the sign at the station said, "Mannheim." What I knew about Mannheim was, of course, what we read on our side about those major bombing raids and so on. But all combat was over by the time that I enrolled at the School of Mines in the fall.

Swent: I wanted to ask you too if you have any recollection of Francis Case and Karl Mundt? Those were our senators for a long, long time.

Fuerstenau: Of course, what I remember was that they were actually two of the leading senators, and well known in the country. Case did a lot for South Dakota. He

was probably quite responsible for getting that air base there. He was very able, from what I saw later. A very modest man. Some lobbyist in Washington approached him about something that was on television in the early fifties and he really took affront over someone trying to influence him the wrong way. A very ethical man.

Mundt, of course, was very active in a lot of committees. You may know more about Karl Mundt than I do.

Swent: I remember getting letters from them about school events. They seemed to keep in touch. If you got an honor of some kind, you got a letter from the senator.

Fuerstenau: That was very nice.

Swent: But that must have been earlier when they were still trying harder. You didn't get letters?

Fuerstenau: No. Maybe they were busier, or maybe I didn't do anything that earned a letter.

Swent: No, you would have gotten them too. Maybe during the war they just got too busy. But it seemed to me that they kept in quite close touch with what was going on back home.

Fuerstenau: Which makes for a good senator. You know I read some years ago that Senator Frank Church, from Idaho, who got elected very young, and for four, five or six terms, never forgot an older person's birthday. He never forgot that he was from Idaho, and kept in touch with home, even though he was a national senator. In contrast, Fulbright, who was an international senator, forgot that he was a senator from Arkansas, and was eventually put to bed. So the good people realize, in smaller states, at least, to keep in touch with the people.

Swent: I think it made us feel closer to Washington than we would have otherwise. Looking back now, it seems to me that both Case and Mundt influenced the schools a lot. Karl Mundt established the National Forensic League. We had debate teams, and extemporaneous speaking contests, and things to encourage interest in current events. I think that was Mundt's baby. Or maybe Case, or maybe both of them.

Swent: So you finished high school in the summer of '45, and then entered School of Mines that fall.

II SOUTH DAKOTA SCHOOL OF MINES AND TECHNOLOGY, 1945-1949

Swent: Who was the president of the School of Mines then?

Fuerstenau: Joseph P. Connolly. You probably know his son, Tom Connolly, who became a VP of Homestake.

Swent: Right, and my parents knew Joe and Ann.

Fuerstenau: He died the second year I was there. I remember he had then two young boys, ten and twelve, and of course we now know them as Lewis and Thomas. They've got to be late sixties, now. I remember them as young boys. And Mrs. Connolly. All through the years I had a certain amount of contact with her because of the wonderful mineral museum at the School of Mines. She worked there at the front desk where people came to register or buy mementos or things. She was there for many years, and any time I came to Rapid City, which was fairly often, if I were out at the School of Mines, I would drop by and chat with her. Very friendly, wonderful lady.

Swent: A lot of teachers had been there for a long time too.

Fuerstenau: We had a couple of days of entrance exams to start with, and because Florence Krieger was such a good teacher, I did so well on the math exam that they tried to find a way that I could get into more advanced math. In those days, you started with algebra, then trigonometry, et cetera. Today, people start with calculus. But I apparently did so well that they tried to rearrange it so I could bypass all that. But the school at that time was so small that there was just no way to rearrange the courses.

[Tape 3, Side B]

Fuerstenau: The fall of 1945 was the beginning of the return of the veterans from World War II. Since discharges must have been in the order of seniority, the returning veterans ranged in age from mid-twenties into their early thirties. One freshman was 34 whereas I was 16. These veteran students were highly motivated, and many of them were married with families. Having a majority of the undergraduate students being returned veterans certainly made for a more mature undergraduate student body. They had exciting stories and attitudes that added to the education process of someone like myself.

For example, some memorable stories came from Pat Franke, who had been a major at about age 25, and whose fluency in German put him into a command situation with prisoners at the end of the war. He had been raised on a South Dakota farm with German parents. After the end of the war, he was in charge

of handling something like a million prisoners around Munich and had to make decisions as to what to do with them. He said they would put Russian prisoners on the train to ship them back to Russia and several of them just shot themselves. He said they had no idea why this was happening, but those Russian soldiers apparently knew that when they got back to Russia, what would happen to them for having surrendered. But he said, at the time, he had absolutely no idea of that and forced them onto trains. Needless to say, lots of war stories were exchanged over lunch by veterans.

Swent: That must have added to your education.

Fuerstenau: No doubt. In my freshman year, since I had not had physics in high school—physics was a senior course—I had to take a physics course designed for those who did not have high school physics. And I imagine that included a lot of the returned G.I.s. This class was taught by Professor Walter [A.] Rosenblith, who had joined the faculty in 1942 or '43. Rosenblith was a superb teacher, one of the very best that I had at the School of Mines. He was originally from Austria and his field was physics of the ear. He left in the spring of 1947, when I was taking the next level physics class, to join the research staff at Harvard and then moved on to MIT. He is one of the few persons—to show you what stature he has—that's a member of the National Academy of Sciences, National Academy of Engineering, and Institute of Medicine. Anyway it was from him that I learned my first physics. When he retired, he was provost of MIT. When I was there at MIT, his title was associate professor of communications biophysics. It was kind of a mouthful. His field was physics of the ear.

[Added by Douglas Fuerstenau during editing: Walter Rosenblith died at age 88 in 2002.]

Chemistry I liked. Every once in a while during lunch time, I would go to talk with the head of the Chemistry Department to find out more about chemistry and his background. This was Dr. Gerard [G.] Osterhof, who talked with me a lot. I then decided to major in chemistry, which I did. In my sophomore year I took a very heavy load, twenty-two units. One of them was a junior course in economics. Just out of interest, one of my classmates there was Bill Griffith, whom you might know. When he retired he was chairman of Hecla [Mining Company]. He and I took economics together. He was a senior then in metallurgy and had returned to college after several years in the army.

As I said, I had a heavy load, but all went well. I studied hard.

Swent: You were living at home.

Fuerstenau: I was living at home. I lived at home that whole time during my years at the Mines.

Swent: How did you get to school?

Fuerstenau: Walked.

Swent: Quite a ways.

Fuerstenau: Not really. Maybe a mile and a half. We were living in the northeast part of Rapid—two blocks east of East Boulevard. Later on my folks moved towards the center of Rapid City. So then I would ride with a friend of mine who had a car.

Swent: Did you continue to work?

Fuerstenau: A little bit. Just a little bit. On Saturdays I worked at Swander's Bakery, for a short while. And once in a while I would be the night watchman on Saturday night. The only other person there would be the baker, who would come in about midnight to start making the dough for baking bread on Sunday.

One thing I do remember, from earlier, was baking a whole load of dark rye bread that was shipped up to Fort Meade for German prisoners. It was baked specifically for them. Boy, from what one read or heard about American prisoners of war in Germany, and how they were treated, our treatment of German prisoners of war in this country was quite the opposite. Every little while that special load of dark rye bread would be baked and taken up there to Fort Meade in Sturgis.

In the summer of 1946, I took one or two classes during summer school and worked afternoons in the Chemistry Department storeroom. On occasion, I worked Saturdays in the College Library. At the end of the summer, I went back to visit my farmer relatives and helped with threshing for a couple of weeks.

Summer Work in the Rolling Mill of U.S. Steel in Chicago, 1947

Fuerstenau: One thing that I did do, which I consider a very high point, is working in industry during summers. The summer of 1947, U.S. Steel was recruiting for regular employees and for summer jobs in the steel mills in Chicago, at the South Works of Carnegie-Illinois Steel Corporation, a division of U.S. Steel Corporation. Three of us from the School of Mines were hired for summer work, and the three of us roomed together, in South Chicago. One of my colleagues was Ben Ennenga, who has given a lot to the School of Mines through the years. I saw that he died recently, after a long career as manager of an Amoco refinery in Casper, Wyoming. The other was Bob Lincoln, whose father was the old mining professor there, Francis Church Lincoln. Your father would have known him; I don't know whether you would have met him. Kind of a dignified gentleman.

Swent: I may well have met him on some occasion.

Fuerstenau: Anyway, that summer job was tremendous. The position was with the Metallurgical Department and was called metallurgical observer which was the kind of job that beginning metallurgists might have. College students filled in for regular employees while they were away on summer vacations. I worked in what was called the 54-inch Bloom Mill, which rolled the largest I-beams in the world. They were fifty-four inches, big I-beams. My job was to determine heating cycles of ingots in the soaking pits and take temperatures as the heating progresses. In steelmaking, the liquid steel would be cast into large ingots, which cooled, sometimes being outdoors several days. When the steel was to be rolled, the ingots had to be reheated over a period of several hours in what were called “soaking pits” so that their temperature was uniform throughout. There might be ten or a dozen of these huge 6- to 10-ton ingots in the soaking pits. With an optical pyrometer, I had to take temperatures hourly to determine when they were ready to be rolled. Temperatures were also taken during the first stages of rolling. To do this, I had to go to the control room directly over where the ingot was being rolled back and forth. This provided a record of procedures in case anything went wrong. Using an internal Bible, I had to determine and write out what the rolling procedure would be for the type of ingots being processed.

Swent: It wasn't the same every time?

Fuerstenau: Oh no. Different heats of steel with different compositions would need different lengths of heating, and be heated at different temperatures and rates. Some of them could be rolled only into a huge 54-inch wide slab or bloom that would be cut in two and sent back for reheating. Other ingots would be rolled continuously without stopping into I-beams—different procedures like that. Each piece of steel had its final destination. In some cases, a big ingot would be rolled back and forth a handful of times and then go on to the next stage, and be rolled back and forth a few times more. And finally billets or bars would keep going about a half a mile down, really roaring out of there because now they just have to keep going because the steel is steadily cooling.

Of course, in Chicago the summer was hot. The summer of 1947 was about the hottest that they ever had and the humidity was probably close to 100 percent. In the steel mill at work, we had to wear heavy wool suits because any splattering steel is hot and it burned. But the wool won't burn. Inside the mills, temperatures were about 130 or 140 degrees and fortunately our office was air conditioned.

It was a great experience. We worked shift work. Two weeks on days, and two weeks at night, and two weeks afternoon. That was, of course, the first time I had ever been in a major city so I spent a lot of time visiting museums, et cetera.

Swent: What were you paid?

Fuerstenau: About that time, a BS engineer was paid about \$245 per month, so I think that our pay was about \$225 per month. Whether I saved anything or not, I don't remember, but it was a good summer. One thing that I did do was buy a 35-mm camera, and that was the start of my long-time interest in color slide photography.

Swent: You had an apartment with your two friends?

Fuerstenau: We had a room, or maybe two. First we lived in an apartment right next to University of Chicago, 59th Street and Harper. But that entailed a long train ride to work. So after about a week or two, somebody working in the rolling mill where I was said his mother-in-law, Mrs. Kroeker, had a room for rent in her house a few blocks away. So the three of us moved south from 59th Street to 125th Street. We may have had two rooms. Since we didn't work together and had different shifts, the three of us were not together very often at the same time.

Swent: Where did you eat?

Fuerstenau: We may have made our own lunch before going to work and ate breakfast at Mrs. Kroeker's, probably with our own food kept in her refrigerator. We ate dinners at small local restaurants, as I recall.

We were about two, three, four blocks from Calumet Park, which is a fairly large park on the south end of Lake Michigan. Just north of that was the South Works where we worked. This was a huge steel plant, probably two square miles, if not more. At that time they dumped the slag right into the lake. You would see huge clouds of steam rising when they did that. From that part of Chicago going east until you reached Gary, Indiana, which is also dominated by a U.S. Steel huge plant, there was one steel mill after another. I recall that the first was Wisconsin Steel Works of International Harvester, then Republic Steel Works, then maybe Inland Steel. At night the whole sky was orange. I do mean orange, especially when they were pouring the heats from the Bessemer furnaces. I have pictures that I took at night; where the whole sky is orange. You would never see that today. I believe that all of the steel mills are now gone except for the one in Gary.

Swent: You said they were pouring the "heats"?

Fuerstenau: That's just a name. A batch of steel is called a "heat" of steel, probably because of all of the heat involved in producing it. It's just the jargon word. Each batch was called a heat, with a heat number.

Then the last end of that summer I spent two weeks where they were rolling stainless steel. That was very tricky in that stainless steel could only be rolled

a little bit, then had to be reheated, and then rolled a little bit, and reheated. And apparently, on some batch of ingots, I was to write down what the heating-rolling procedure should be and I forgot to write down the full procedure for the operator working the rolls. I learned the following summer that those ingots totally cracked because the roll operator tried to roll them straight through. They were useless and had to go back and be remade. I was still rehired the following year.

Swent: A lot of responsibility.

Fuerstenau: There really was. Come to think of it, I was only 18 years old at that point. That summer job really gave one a good insight into the steel industry. Several foremen there were graduates of the School of Mines. One was named Albert Gallo. You may know the family, because I think he had a brother that either worked at Homestake or ran the Bald Mountain mill. One or the other.

Swent: I remember the Gallos.

Fuerstenau: So this was one of them. I used to chat with him. Being foreman of a big operation such as that was quite a significant job. When our shifts overlapped I chatted with him.

Swent: How were you supervised in this job?

Fuerstenau: My supervisor was called a metallurgical supervisor from the Metallurgical Department

He came around daily during the day shift, and I suppose to talk a bit to see what had been going on and determine that there were no metallurgical problems caused by heating and rolling procedures. His responsibility was metallurgical control in the Rolling Division. I can picture him but can no longer recall his name.

Swent: You must have had to go through some training.

Fuerstenau: Probably fairly quickly. I remember that I worked about a week with a one of the regular senior metallurgical observers. He had graduated as an engineer from Armour Institute of Technology—which now is called the Illinois Institute of Technology. After that I was on my own since we were filling in for those on vacation.

By the way, workers within the steel mills in Chicago were fully integrated in 1947. Black and white workers shared the same change house, shower room, and in the mills themselves the same drinking fountain and dispenser of salt tablets. Mrs. Kroeker was quite outspoken, however, about neighborhood changes taking place around there.

Not all was work. Chicago had two very elegant large dance places: one called the Trianon, on the south side, and the Aragon, on the north side. One time my two roommates and I decided to go to the Trianon. I went back there quite a few times. At the Trianon, various big name bands would be there. Lawrence Welk was their main one. He, you know, came from North Dakota. This was the big band era: Charlie Spivak, Jimmy Dorsey, Tommy Dorsey, Claude Thornhill. I can vaguely remember some of those names. I think those places are long gone. But then they were large, elegant dance ballrooms.

Swent: Where did you get the girls to dance with you?

Fuerstenau: Single girls by the dozens were also there, many college students home for the summer. I dated some as I recall. But you know, when you live in a very large city like Chicago, distance is a problem. For example, the Trianon was at 63rd Street, on the South Side, and we lived at about 125th. That's sixty blocks away! I remember dating a girl a couple of times, who lived sixty blocks west on 63rd—you don't do that very often. [laughs]

Going to Chicago that first summer, Bob Lincoln and I got a ride with three physics students going home to New York for the summer: Norman Menyuk, Art Paskin, and Leo Weinreb. I believe that one of them had studied under Professor Rosenblith as a student when he was in the army, and that brought all three back to South Dakota to study physics. I remember when we got to Ames, Iowa, they hit a pothole and split a rim on the car. We were heavily loaded, right, with six of us in the car plus luggage. It was about four in the afternoon, took a while to get a replacement wheel, and we just drove straight through. At about two or three in the morning in Illinois, when all of a sudden we were half a mile past a crossroad, a red light came on behind us. A sheriff was sitting there at the crossroads where there was a stop sign, and whichever one was driving had gone through it and the sheriff said, "Did you see the sign?" And the driver said, "No." The sheriff then said, "Why don't we go back and look at it." Drove back, took a look at it, and he said, "Well, let's go talk to the judge about it." We drove about a half mile down the road, and with his spotlight he shined a light up into the second floor, obviously hit the mirror—that woke the judge up. I remember the \$13 fine. [laughter] That's like a story, isn't it?

Swent: In the middle of the night.

Fuerstenau: About two in the morning. Obviously the sheriff and the judge had a nice thing going.

So they dropped us off at the Morrison Hotel, which then was a major hotel in downtown Chicago. This was about five or six on Sunday morning.

Fuerstenau: The bellhop took us up to the room with the luggage, and Bob Lincoln reached in his pocket and told me, "I got eight cents." And I reached into my

pocket and said, “I got a nickel.” So Bob said, “Well, give it to me.” So Bob had the thirteen cents that he dropped in the open hand of the bellhop, who said, “Thank you,” and didn’t look at it. But he obviously glanced at it as he put it in his pocket and he said, “Well, I’ll be goddamned,” and walked out. [laughter] We must have had a dollar or two or something, but not much more. You’re not going to give, in 1947, a bellhop a dollar. That was the beginning of my stay in Chicago.

[Tape 4, Side A]

Changing Study Major from Chemistry to Metallurgical Engineering

So when we got back to South Dakota at the end of the summer, one of the courses that I started taking was physical chemistry. I didn’t like the lab which was required for chemistry majors, so I decided to switch my major to metallurgical engineering, which did not require the PChem laboratory. For somebody who has been heavily involved with chemistry and processing labs ever since, that is a little odd. Also I had just worked in the steel mills, so I had developed a metallurgical interest. The main metallurgy professor had been there for decades, Bancroft Gore, who you may have known and your father obviously knew well.

Swent: I certainly heard the name a lot.

Fuerstenau: Anyway, he died, in the fall, and two new metallurgy professors joined the faculty. One I used to talk to a lot named Alex McHugh—who was kind of an old farmer, but really I got to like him as time went by. And the other one, Gerald Van Duzee, taught physical metallurgy. But in the fall before that when Professor Gore died, the mineral processing course was taken over by Dr. Lincoln, who was the mining professor. This was the father of Bob Lincoln with whom I had worked in Chicago. Dr. Lincoln was already obviously approaching retirement age, and he lectured a lot about ancient methods of processing ores. You’ve probably seen the ancient devices for grinding ores called arrastras, which consist of a huge stone chained to beam that was dragged round and round by a mule. They were I think used in Mexico and Chile. I’ve seen them in Arizona. I still recall him sketching an arrastra on the blackboard, and drawing a mule. Then he stepped back and he looked at the mule, erased it, and redrew it a different size. [laughter] He really dated back. He told us about how you get a mule down a mine. Of course they had mules long ago down in the Homestake. Lincoln said that you put them in a cage, and turn the cage on end so they can’t kick, and that’s the best way to get them in the mine. [laughter]

He died at the end of that year, too. Also I took geology courses, mineralogy, and extra chemistry. As a sophomore I had taken organic chemistry from a professor named Carl [E.] Schilz. At least four or five times, he took three or

four of us on a Saturday trip to visit different places in the area. On one trip we went to Deadwood; where one of my fellow students had come from. We went into an old abandoned mine where ferrous sulfate, called melancthonite, made stalactites dripping from the ceiling, and stalagmites. Oxidizing pyrite formed the iron sulfate with water in the crystal structure. These were beautiful green stalactites and stalagmites. But on taking them out of the mine, the water of crystallization quite rapidly evaporated and they crumbled to a powder. We panned gold, went out to the Badlands, things like that. He had an interest in interacting with and furthering the experience of students, I guess.

Swent: A dedicated teacher.

Fuerstenau: Yes, yes. This was not related to the chemistry.

Swent: It was on his own time.

Fuerstenau: Yes, on his own time.

I also had some chemistry courses from John Willard, who I interacted with quite a bit through the years. Willard was one of these persons always working on some project or other. Some years back, I saw a program on *60 Minutes*, about Willard's Water. Did you ever read anything in the local paper? Apparently he did something with water, treating it with peat in some manner. It was purported to have all sorts of great properties. But anyway, this water was very good for cleaning metal, and they peddled this stuff as Willard's Water. Charcoal may also have been involved. But there was enough publicity about Willard's Water that it did make *60 Minutes*. It made quite a splash. It made local papers in South Dakota quite often.

Swent: It wasn't a hoax?

Fuerstenau: Oh, I think it must have been some sort of a hoax. They didn't think it was a hoax; they were believers. But Willard's Water didn't exist back in my days.

I did take several geology and mineralogy courses from professors Jim Bump, Ed Tullis, and John Paul Gries. I liked mineralogy, which was taught by Gries, a great teacher. Gries is still alive, and my brother has had a lot of interaction with him. I think he came to the School of Mines in 1936, after finishing his PhD in the University of Chicago. I think he lived in Rapid City until just maybe a year ago, and now moved to his son's since his wife died not too long ago. The last time I saw him he had to be obviously late eighties but still looked almost the same, and was still "with it" [mentally alert]. He had just written a book, or put together a book, called *Roadside Geology of South Dakota*. It's part of a series of books from a lot of states about how you can see the geology from roadcuts. I bought that book just very few years ago and found reading it and looking at the photographs and diagrams to be really interesting.

Summer Job in the Open Hearths at U.S. Steel in Chicago, 1948

Fuerstenau: After finishing my junior year, I worked again in the summer in Chicago.

Swent: At U.S. Steel again?

Fuerstenau: This was again at U.S. Steel, South Works of Carnegie-Illinois Steel in Chicago. And even though I had destroyed several ingots of stainless steel, [laughs] they hired me back. This time I worked in the open hearths. The open hearth is where they made steel at that time. Pig iron comes out of the blast furnaces at something like 4 percent carbon with a lot of silicon in it, and maybe phosphorous and manganese, and you have to get those impurities out of the steel. The way it was done in those days, was with large open hearth furnaces, eighty feet by twenty feet in plan view. These huge furnaces were charged with limestone for slag and scrap and maybe some iron ore, and then as the scrap is heated, it's oxidized to iron oxide. After some hours the molten pig iron from the blast furnace was poured into the open hearth from big ladles.

Swent: What was the fuel?

Fuerstenau: They were oil-fired, most of them, on the two sides, and as flames with the burnt CO came across the charge in the furnace, it would oxidize the iron to iron oxide. After that, the limestone was added to make a slag covering on the charge.

One job that the metallurgical observer had was to calculate out the amount of all the materials to be added to the furnace for that particular heat. How much lime—well, limestone would be one of the big ones that I remember. You would calculate out what the charge would be, how much scrap, and so on. Each heat of steel had its desired composition, eventual destination, and was clearly labeled. I remember heats for car fenders, beer can ends, springs, because each had to have a certain composition.

So to produce the final steel, these heats would be in the open hearth up to ten or twelve hours, maybe, and I would have to take temperatures, record the height of the slag, and observe whether it's foaming or not. Because the carbon being oxidized would bubble off CO₂, and you know that would cause the slag to foam. All this had to be watched and controlled.

Swent: I suppose all that is automated now?

Fuerstenau: Well, steel making has moved to different methods. Smaller, often electric, furnaces with scrap being the raw material—I haven't paid attention very much, but open-hearth steelmaking is now essentially obsolete. But anyway,

that was *the* way of making steel then. The other method then was the Bessemer process, where air actually was blown directly into the molten pig iron to burn out the carbon. That's what often lit up the sky at night that I told you about.

When the steel had the right composition, the open-hearth furnace was then tapped into huge ladles. Then the metallurgical observer had to go down to take the temperature of the steel as it's coming out of the ladle into these big ingot molds.

Swent: How did you do that?

Fuerstenau: Again, with an optical pyrometer.

Swent: You weren't sticking a thermometer in it?

Fuerstenau: No, no it was optical. You matched the red brightness of the liquid steel with an element in the instrument. When the color in the pyrometer was the same as the color of the steel, the system calibration would give the exact temperature. The optical pyrometer is the standard way to make high temperature measurements under those conditions. You've seen red hot steel. Take an electric stove burner, the hotter the burner, the brighter it gets. It goes from dull red, to orange, to yellow, and almost white as the temperature is increased.

Swent: Did you wear goggles of some kind?

Fuerstenau: You had to wear goggles, and you had to wear really heavy wool, and I do mean heavy, wool suits. I think the ambient temperature, some days, was 140 degrees inside those mill buildings. I remember the room where our desks were was air conditioned at 85 degrees. It felt like going into a refrigerator, at 85. On the outside it was 130 or 140.

So for a given pour from the open-hearth, there might be twenty of these ingots that would be 2 ft. x 2 ft., or 2 ft. x 3 ft., some 2 ft. x 4 ft, maybe 8 ft. high, and I had to take and record the temperature as each was being poured. For certain types of steel, alloys may have to be added as they are pouring. Some alloys might have been added in the open hearth, but others are added in the ladle. All of this you had to write down, and make sure it was done by informing the persons pouring as to what alloy and what amount should be added.

Some steel was still bubbling CO or CO₂ bubbles and foaming like Coca-Cola. For that, aluminum shot was thrown into the top of the mold to kill the bubbling. That's called "killed steel." It depends on what the steel is going to be used for as to whether you want it to be free of bubbles or leave the bubbles there when the ingot freezes. Of course, what would happen is the

aluminum would react immediately with the CO and be oxidized and get rid of all the gas that way. That entire summer was spent working in the open hearth floor. In one end of the open hearth building, they were doing some full-scale research on what was called “basic Bessemer” which—I think—is what has replaced the open hearth. That program was being carried out under the direction of a metallurgy graduate from the School of Mines, but I don’t recall his name.

That again was a summer job of great interest and responsibility, and so on. I have no recollection at all of who was in charge of the observers that summer. Again, that was shift work.

Swent: Did you stay with Mrs. Kroeker again?

Fuerstenau: I did, yes. And that summer there were two of us—Sam Adler, a physics major at the School of Mines and also a veteran. He told me that every time there was a Jewish holiday in the army they gave him the day off because they thought he was Jewish. Adler is the German word for eagle. But anyway, he said that he took the holidays.

Swent: He never complained. [laughter]

Fuerstenau: He didn’t complain. He took what they gave him. His job was in some other part of the steel works that summer, so we did not overlap very often.

Swent: Did you go dancing again?

Fuerstenau: Oh, probably. Without a doubt. But not with Sam Adler, because he was married.

You know, later I got very, very interested in baseball. And it’s funny, I never went to a single baseball game during those summers in Chicago, and that really surprises me, given how often I later went to ball games. But this was ‘48, just the beginning of television, and once in a while we would watch a game in a bar, but never go the ball park. I often went to, on a day off, to downtown movies that had entertainment. These sorts of fancy movie places would have a movie and then maybe a 45-minute show in between, such as the Harmonicats or a comedian. And I went to a couple of musicals, such as *Oklahoma* and also *Carousel* and so on when I had a Saturday free. It was a long ride from 125th South up to the Loop to do that.

Swent: You mentioned museums earlier.

Fuerstenau: The Museum of Science and Industry in Chicago is a tremendous place. They even have a mine in there. I went there several times. Then there’s another one called the Field Museum.

Swent: Natural History.

Fuerstenau: That's natural history. And then Art Institute is their art museum—funny name for it.

Swent: They're all fairly near each other.

Fuerstenau: Oh yes. That first week or so, a year earlier, we lived at 59th and Harper and that was about two blocks from the Museum of Science and Industry. They're all along the Lake.

So I used to spend days off like that—not wasting time—but doing things like that.

Swent: Took advantage of what Chicago offered.

Fuerstenau: Right; why not?

Swent: Did you ever take the train to Chicago?

Fuerstenau: The second summer I got a ride to Mobridge with a fellow student, and then stayed there with some old friends for a couple of days, after which I went by train, the Milwaukee Railroad, to Chicago. When I got on the train in Mobridge, sitting behind me were two guys from New York who had just finished their freshman year at Montana School of Mines. A very talkative one was name Herb Drechsler, with whom I may have talked all the way to Chicago. He was a senior at Montana School of Mines the year I went there as a grad student. And I have seen him on and off through the years. Later he received a PhD in mineral economics from Columbia, and the last time that I saw him he was a professor at the University of British Columbia.

One thing I remember on that trip was at that time passenger trains were run by streamlined steam engines. And going from Milwaukee to Chicago, I timed the train, and that steam engine was going about 100 miles an hour! Diesel locomotives eventually replaced all the steam engines in this country, but steam could keep up with them on speed. I have no recollection on the return, but I probably came back by train too.

Swent: Rapid City had several passenger trains at that time.

Fuerstenau: I now recall how I got back. I got off at Milbank in South Dakota, hitchhiked to Watertown, and stayed a couple of days with my grandmother. Then, a few days down on the farm for threshing time. We then went by car to Huron, where the state fair was being held. Did you ever go to the state fair in South Dakota?

Swent: Never did. I'm sorry I missed that.

Fuerstenau: So after spending the whole day at the state fair, I took the train to Rapid City. And that train took all night, and I think it averaged about 25 miles an hour, if it was moving at all. I think it was the Northwestern, wasn't it? The train went from Huron to Chamberlain where it crossed the river, and then to Rapid City. You probably took that on your trips East, so you know.

Swent: They didn't have a dining car in my day; they stopped for meals. I think they stopped at places like Kadoka for lunch; it was quite an adventure.

Fuerstenau: I now recall that's how I got back. And I remember hitchhiking.

Swent: And that was safe?

Fuerstenau: Oh yes. I was probably a tame-looking guy.

Swent: Well, those were learning experiences.

Fuerstenau: Oh yes. So that was now the fall of '48. I was now beginning my senior year.

Completing Undergraduate Studies at the South Dakota School of Mines in 1949

Swent: You went through in three years?

Fuerstenau: Four years. I went to summer school a bit—in '46 I went to summer school. I did take a course in speech and something else perhaps to make up for my three years of high school. And I worked part-time in a chemistry storeroom. Dr. Willard was teaching some first-year chemistry class at the time.

Swent: That was at the end of your second year that you went to Chicago for the first time.

Fuerstenau: End of the second and end of the third.

Swent: So then you're coming back to your fourth year.

Fuerstenau: Going clear back a little bit—I took this speech course and the teacher was somebody with a PhD in English and an All-American football player from UCLA.

Swent: Do you remember his name?

Fuerstenau: Hyman Palais. I had taken a course in English from him as a freshman, and in the summer he taught the speech course. Each talk was supposed to take so many minutes. I think I gave a talk on rocketry, and what it would take to go to the moon. But the one part I remember was one of my classmates, Bob

Olson, older than I, who had been a captain in the Engineers Corps during the war. He was talking about putting a pontoon bridge across the Rhine. He kept saying, in his talk, “Well, putting a bridge across the Rhine was like uh, uh, crossing the Delaware like, uh, crossing the Delaware, like, uh, crossing the Delaware—.” [laughter] He couldn’t come up with “Washington” on the spur of the moment. I still chuckle over that.

Swent: Panic at having to give a speech.

Fuerstenau: And then, all of a sudden, you get hit with a blank just like that—right?

I don’t remember much about what I did during my junior year, but I know I did a lot during the senior year. I had a good friend, Paul Fenske, whose father was president of that Lutheran academy in Mobridge. He was a two or three years older than I.

Fuerstenau: When I was in the sixth grade he may have been in the eighth grade. Before I started working in the summer, I remember his teaching me how to re-spoke a bike. And I took a bicycle I had and took all the spokes off and redid it, and redid the brakes on it, completely. I was maybe eleven or something. Years later, when Paul came out the army, he went to School of Mines, majoring in geology. Since he had a car, we drove out a lot of weekends, Sundays, hiking in the Hills. We visited a lot of those old mines, down around—what’s that little town next to Rushmore?

Swent: Keystone.

[Tape 4, Side B]

Fuerstenau: Keystone! Right around Keystone there are all sorts of small pegmatite and gold mines. The Holy Terror gold mine near Keystone was one. I remember climbing around the Etta Mine. That used to have the biggest spodumene crystals ever found in the world. There were a lot of pegmatite mines back there. And he and I would go into the abandoned mills, and I got a few minerals specimens that were lying around, in the dumps, and so on. You did the oral history of A. I. Johnson, who ran several of those old mines at one time or other.

Paul and I spent quite a bit of time out in the Black Hills on weekends, and I think that’s what moved me more towards the mineral processing side of metallurgy, rather than physical metallurgy, or high-temperature process metallurgy. This probably fostered my interest in minerals and ores.

In fact, I used to hike around, quite often, while still in high school, my last year there, with my friend Frank Marion. One time we went down to Mt. Rushmore in very early spring, and we hiked up alongside of Rushmore, and all of a sudden came around a rock, and there was the back of Washington’s

head! We had climbed up on top of Mt. Rushmore. I have a few pictures taken up there.

Anyway, one time later, Paul Fenske and I, and I think my brother Maurie who was still in high school, the three of us, hiked up the same route, coming up the front, on the right, and on up to the top of Mount Rushmore. Again I think that was very early spring; there were probably no tourists down there.

Swent: An exciting way to see Mt. Rushmore.

Fuerstenau: So then, as I recall, that year was more or less uneventful. In my sophomore year I really studied very hard and had all A's even though I was taking twenty-two units. When I went to Chicago I got a little more relaxed so I didn't work like that as much and spent Saturday nights enjoying myself a little more. So my grades weren't as good in junior and senior courses as they were as a sophomore. But I decided, as graduation approached— Without applying—I had an offer to come and work at U.S. Steel as a BS metallurgical engineer for \$245 per month—the going salary at that time. But I decided to go on to graduate school, and I applied to five different places, of which all accepted me and offered me fellowships.

Swent: Did South Dakota School of Mines have any graduate study?

Fuerstenau: I think there was no research going on anywhere at the South Dakota School of Mines at that time, except for the excellent group of nonfaculty paleontologists working on Bad Lands fossils. You could do a master's there, I assume, but that didn't occur to me. I applied to MIT. They wanted a course in petrography, which I hadn't had. So Professor Tullis in Geology taught this to me as a sort of one-on-one course. It was probably me more working with a microscope in the lab to get credit for this course in petrography.

I also applied to Montana School of Mines, because my friend Frank Aplan¹ who I didn't really know that well yet, had gone to Butte. Montana School of Mines was particularly interesting because it had a long history of outstanding faculty in metallurgy. So I applied to Montana School of Mines, University of Utah, Missouri School of Mines, and I think the University of Nevada. After considering the various places, I decided to go to Butte and turned all the others down, including MIT. I consider my year in Butte to have been a very good thing. My overall plans were to go to MIT for a doctorate after getting a master's degree from Butte. I didn't find out until years later, that apparently the letters to MIT somehow crossed in the mail, and Professor Rush Spedden, who was in charge of the mineral engineering graduate program at MIT, was not going to have anything to do with me when I reapplied.

¹ Frank F. Aplan, *Mineral Generalist, Professor of Metallurgy and Mineral Processing, 1951-1998*, 2003

Swent: Because you turned them down?

Fuerstenau: Because I turned them down the first time. I'll come back to that. Anyway, I do have some additional thoughts on making a decision to go to Butte first. For example, when you come from a small town, although Rapid City was a big city for South Dakota and I had worked two different summers in Chicago—you're kind of comfortable in the small place environment. I don't know whether that entered my decision not to go to MIT directly from South Dakota. But I've seen that in students. Quite a few of them that I've seen through the years came from a small place. A lot of them get a BS from South Dakota, go back and get a master's from there. Or get a BS from Butte and maybe stay and get a master's. They should go somewhere else.

Another example comes from my high school days. Every six weeks, the high school put out the list of who had five A's, four A's, three A's. I had five A's, always. And there was a student a year ahead of me, named Joe Szakacs, and he always had five A's, and he was very active in all sorts of extracurricular activities. The Rapid City paper made a big thing out of his getting a scholarship to go to Cornell. Our English lit. teacher was a Cornell graduate, and my guess is that she may have urged him to apply there and recommended him for a scholarship. But after his first year he quit and came back to Rapid City. I think that he was somewhat overwhelmed and did not adapt to Cornell. So anyway, I made the decision to go to Butte rather than MIT the next fall. So I graduated with a BS degree in metallurgical engineering from the South Dakota School of Mines and Technology in June 1949.

Summer Work at the Bunker Hill Lead Smelter in Kellogg, Idaho, 1949

Fuerstenau: Sometime during the spring, a letter came to the Mining and Metallurgy Departments from the personnel office of Bunker Hill stating that they had openings for about 250 summer students.

Swent: This was Bunker Hill Mining?

Fuerstenau: Bunker Hill and Sullivan Mining and Smelting Company in Kellogg, Idaho. I wrote to them and received back a letter with a job offer for the summer there. A couple of other students who had not applied and did not have a specific offer decided that they would drive out and see if they could get a job. I rode with them. We drove out through Yellowstone Park. We went there on the very first day that Yellowstone was open that spring. There were three or four of us. The owner of the car, Arnie Ulmer, had a '47 or '48 two-door Chevrolet. We slept that night in Yellowstone Park in the car, and I'll tell you, that was a hard go. I still remember waking up early in the morning, when it was just getting light, and of course the windows were kind of fogged over, and wiping off the window—there was a moose looking at me in the window! [laughter]

We drove on to Kellogg, and unfortunately those other students did not get a job since all of the 250 openings were filled. We were told that a lot of the miners and workers in the mills and smelters either lumbered or farmed in the summer. College students were hired to work as fill-ins.

My job was in the lead smelter. Now you read much about lead, and of course you know Bunker Hill always makes the press even today about their tailings and so on. But my job was in the lead smelter. In lead smelting, the galena concentrate, which is lead sulfide, is roasted, burned, in order to make lead oxide sinter. Then that lead oxide is charged to a blast furnace with coke, and the coke reacts with the oxygen in the lead oxide, producing liquid lead metal. So it's essentially the same thing as an iron blast furnace but it is carried out at a much lower temperature.

Let me back up one step, to the spring of '48 when Frank Aplan was still around. Talking about Idaho reminded me of my first excursion with Aplan because he and I made a lot of field trips that summer in Idaho. One time in the spring of 1948, Frank and I went up to Homestake to see the mill. The person whom we went to see was your father. I still remember that he first discussed the Homestake gold recovery flow sheet, talked to us a bit, and I assume passed us on to a junior engineer who then toured us through the mill. But I still remember your father in a white shirt and a necktie and I wouldn't be surprised if he also was wearing a black suit.

Swent: A suit for sure.

Fuerstenau: I know what they dress like today.

Swent: It was different.

Fuerstenau: Frank and I had a complete, thorough tour of the Homestake surface operations. That's when Homestake still had stamp mills running. That must have been the largest remnant stamp mill plant there was. Again, this was one of the first, if not the very first, of many mill tours that he and I went on.

Coming back to my job there at the Bunker Hill lead smelter, the sinter which came out of the roasting plant was loaded into charge cars that would then be transferred to the top of the blast furnaces, where they would be dumped into the top of the lead blast furnace. My job was to load those charge cars. You know, all that I had on was heavy work clothes of some sort, and a simple mask, and I mean simple. Also I had a rope tied around my gloves, and so on, to keep the lead sinter dust out. But for years the pores in my legs were black, and I think that was lead oxide. I'm still alive so—. [laughs] It amazes me what working around a lead smelter was like in 1949. Because when I was with Homestake [in later years], I visited the Herculaneum smelter south of St. Louis, and you would think we were landing on the moon. I mean we were

completely encased in helmets and in suits that looked like moon suits, compared to what it was in 1949.

Swent: You said your legs got the major exposure?

Fuerstenau: Well the pores in my legs—full of dust, and this was lead dust, lead oxide. Those pores were black for years. And I guess that was lead oxide. However, I think heredity made me lose my hair. [laughs]

Swent: Was there any awareness at that time of the dangers of lead?

Fuerstenau: Apparently not. I know that a lot of my time was just spent pulling a big lever to fill the charge cars about the size of small mine cars, and you know this lead oxide dust poured out all around and on me. And dumping those cars into the top of the lead blast furnace raised another cloud of dust. But later on, I was working in the floor of the lead smelter itself at times, where they were pouring the molten lead as it came out of the furnace. Fifty feet away were big drossing vats where they were removing impurities from liquid lead. So the fumes in there must have been very high—only big fans were ventilating the system. One would never see such a system today.

Later at the end of the summer there was a strike. And the strike shut down everything. Only a few of us had a job, which was going into the lead blast furnace with a jack hammer and digging out the solidified charge. And I was one of them. The furnace was cooled down, of course, and then two or three of us went into the furnace with jack hammers to dig out the material inside the furnace—something between lead and lead oxide, and coke.

Swent: What kind of protective clothing did you have then?

Fuerstenau: The same. I probably just had a simple, nothing dust mask. I mean nothing.

Swent: Hard hat?

Fuerstenau: One always would have a hard hat. Yes, I think even in the steel mills everybody always wore a hard hat and safety shoes. That was the one bit of safety that all of these companies seemed to have, namely steel-toed shoes, a hard hat, and safety glasses. But the real thing missing in the lead smelter, I think, related to this atmosphere business. Nothing more than a respiratory mask.

Swent: The Selby Smelter, I just happened to look up the other day, it was closed down in 1970, so by then there was real awareness of the hazards of lead.

Fuerstenau: Well, the mill tailings, and I'm not talking about smelter slag dumps, but the mill tailings at Bunker Hill have been an environmental problem. Dust from the mill tailings would blow, and the kids in a school yard, apparently right

near by, were getting affected by lead poisoning in Kellogg. That made the national papers some years ago. For someone having the kind of exposure to lead that I did, fortunately I assume had no lasting effect on me. Maybe it did, maybe it would have been better if it had. [laughs]

Swent: People who were working there in the thirties, forties, all those years, they must have had a lot of exposure.

Fuerstenau: True, true. And there were people who worked to retirement.

Swent: The mines were open long ago, early in the century, weren't they?

Fuerstenau: Oh yes. The Idaho lead district started either in 1900 or 1890. Two of the major early operations were Bunker Hill and the Morning Mine. My visiting some of these different mines and mills related to the people that I became acquainted with there. As I said, there were about 250 different summer students there, and I got to know several quite well. We lived in dormitories there for single workers—all big mining camps used to have housing for single workers, and we ate our meals there. I think that they must have provided box lunches for us when we were on shift work. At meals I think one met and interacted with other summer workers. One person whom I met and was friends with for quite a few years was Stair Dickerman, who came from Seattle. And because of my interaction with him, that fall he decided to go to Montana School of Mines as an undergrad.

One person whom I met there and with whom I had a close friendship for many, many years, was a young guy who had both arms completely wrapped up in slings. This was Jack Jordan, who had been a freshman at Montana School of Mines, and who while working underground in the mine had fallen down some kind of raise and broken both arms. My acquaintance with him went on for many years, including graduate student years at MIT. In the fall of 1950 he transferred as a junior to MIT.

One Saturday afternoon, and I think it was in the late afternoon, several of us drove over to Mullan, Idaho, and were in a bar having a beer, and sitting on the bar stool was Frank Aplan. He was working that summer for Hecla, I think at the Star Mine, there in Mullan. Frank had a car and practically every Saturday that each of us were free after that, we visited many of the mines, or actually the flotation mills, in the whole area. This included, for example, the Morning Mill. You know the Morning Mine was one of the famous old mines that dates back to the 1880s. And even when it quit there was still fairly high-grade lead in the ore body. But apparently it was deep—six, seven thousand feet, and “the ground wouldn't hold,” as the miners there would say. So they just couldn't go any deeper. But most interesting was that the mill had equipment in it that dated back to the turn of the century. They were using flotation machines that they had constructed about ninety years ago, their own classifiers and so on. But grades and recoveries of lead and zinc were still

quite good, even though they had to grind the ore very fine—to something like 50 percent finer than fifteen microns.

On another Saturday, we visited the Sunshine Mine. That's one of the major silver producers. The Sunshine ore assayed about thirty-five ounces of silver per ton. Since the silver was associated with sulfide minerals, a simple sulfide mineral concentrate was produced by flotation. This concentrate, which ran about 1,000 ounces of silver per ton, was shipped by truck and rail to the Tacoma smelter. I remember the mill superintendent telling us that earlier in the summer a freight train derailed on the way to Tacoma and spilled their carload of concentrates. Because of its very high value, he said that he told the railroad people that they would send their own crew to pick up all that "dirt." Is the Sunshine Mine still operating or did it close down?

Swent: Just recently, I think.

Fuerstenau: That was always one of *the* silver mines. Well, almost every Saturday we went to a different one, including the Bunker Hill mill which, for the region, was the big one. I still remember an old mill operator there. It was a little bit noisy. We were looking at the ball mill, and this operator came up to us and said, "This here's one of them there ball mills that grinds up that there ore." [laughter] That was a big operation. Bunker Hill also had an electrolytic zinc plant, too, in addition to the lead smelter there. I think the major owners were Bradleys², weren't they, of Bunker Hill?

Swent: Yes.

Fuerstenau: I didn't know any of that then, of course.

Swent: Fred Bradley made his name at that place.

Fuerstenau: Oh yes, and was president of it. They lived here. He and his wife were killed in a car accident on a Thanksgiving. It was in the newspaper.

Swent: That was his son.

Fuerstenau: But that son, again, was the one who, at that time, was head of Bunker Hill. You know more of that history than I do.

Swent: And Boston money, I think; that's why the Bunker Hill name.

Fuerstenau: I didn't know that. I knew there was Boston money in copper in Michigan.

² Philip Read Bradley, Jr., *A Mining Engineer in Alaska, Canada, the Western United States, Latin America, and Southeast Asia*, 1988

Fuerstenau: Since Frank Aplan had a car, at the end of the summer the two of us drove back to South Dakota for a week or so, stopping in Butte where I met Professor Donald McGlashan for the first time, and briefly toured the Montana School of Mines campus. A week or so later, we drove back to Butte to start the fall semester.

Some Additional Comments on Graduation from South Dakota School of Mines

Swent: I would like to just go back a little bit. You worked at Bunker Hill, then, the summer after you graduated. And you didn't mention your graduation at all. Was this a significant event?

Fuerstenau: [Laughs] That's right. Let us back up.

Swent: Yes. You haven't really finished Rapid City School of Mines.

Fuerstenau: I graduated in the spring of 1949. The commencement speaker was somebody named Potter, who was associate dean of engineering at Purdue. He was the father of a person who had been a physics professor for many years at South Dakota in the thirties. He gave the commencement address and it must have impressed me at the time since I recall his name. My senior year introduced me to more geology and some mining which probably oriented towards mineral processing, by the way.

Since I had been a chemistry major until my junior year, I had not taken the sophomore courses in physical and historical geology, so I had to take them as a senior to meet the metallurgical engineering curriculum requirements. As I had mentioned earlier, Dr. Lincoln had died, and a new mining professor came to South Dakota, named Lysle Shaffer. I really was impressed with Shaffer and I took a course that he taught on mineral economics, mining economics, which really impressed me. He had come from Missouri School of Mines, where he had been on the faculty, and made a big impression on me. I interacted with him a fair amount. And we'll get to him later, because he became well acquainted with Don McLaughlin³, probably through Homestake. He later came here to Berkeley and was responsible for my being invited to come here, I think.

I graduated with very high honors, not the highest. If you can believe it or not, I've done great things in the world of flotation, but I got a C in a course in flotation from McHugh. Aplan and others still chortle about my getting a C in flotation class. I don't know what happened, whether I didn't study, or I didn't

³ Donald H. McLaughlin, *Careers in Mining Geology and Management, University Governance and Teaching*, 1975

do something. I know that the diploma given then was an actual sheepskin. They are paper now. Graduation was just taken in the course of events. I had accepted the summer job as a laborer at Bunker Hill because I was planning on continuing my education as a graduate student.

Swent: But that was your first job in mining.

Fuerstenau: In that sense, right. Steel production is downstream in that the ore had already been mined, and concentrated, or whatever they needed to do. Of course, as I told you, I wandered around these old mines and mills, but had not worked in them.

Swent: You didn't go back to the farm that summer, then?

Fuerstenau: No, no. I didn't have time.

III MONTANA SCHOOL OF MINES, GRADUATE STUDIES, 1949-1950

Short History of Earlier Faculty and Students at Montana School of Mines

[Tape 5, Side A]

Fuerstenau: So I had decided to go to Montana School of Mines to get a master's degree. I would like to say that Montana School of Mines—in the metallurgy area—really had a fair amount of fame and that may have entered into my subconscious for going there. The president, who was there when I arrived, was Dr. Francis A. Thomson, an Englishman who had a little goatee, a very elegant looking man. He died in 1950 during my year there. One of the outstanding things that he did was to hire, in 1929, Professor A. M. Gaudin to come there, and I'll tell you more about Gaudin when we discuss MIT. Gaudin was there from 1929 to 1939 during which time he was becoming the great giant of flotation. He was still a young person then, in his thirties, born in 1900. He would have been twenty-nine when he got there. Starting in the middle twenties at the University of Utah, he did a tremendous amount of classic research on flotation, and in 1932 he published the first modern book on flotation. During that decade he had a lot of good grad students. Probably his most famous student was Plato Malozemoff⁴ who did his master's with Gaudin, in '32, after graduating from Berkeley. He worked a couple of years with Professor Gaudin. Butte had several significant people who came from there, in the metallurgy area, around the country. For example, two who later had productive academic careers were Reinhardt Schuhmann, Jr., and Albert Schlechten. Another was Oscar Tangel, who had an outstanding industrial career in extractive metallurgy.

The person who succeeded Gaudin at Montana School of Mines in 1939 was Strathmore Cooke, S.R.B. Cooke, originally a New Zealander. In 1946, Cooke went to the University of Minnesota, where he directed a lot of research on the flotation of iron ore. Many classic papers on iron ore flotation were done in Cooke's laboratories at Minnesota. H. R. Spedden, with whom I later had much interaction, had gone to Butte to study under Professor Gaudin in 1939, but with Gaudin having left for MIT, he did his master's degree under Cooke. So for a small institution, Butte had quite a history of good people.

As for my year at Butte, during my stay there two or three faculty really stand out. Frank Aplan, who was there two years, had different kinds of courses. I know he had a big interest in some that I had never had any contact with.

⁴ Plato Malozemoff, *A Life in Mining: Siberia to Chairman of Newmont Mining Corporation, 1909-1985*, 1990

Donald McGlashan was the mineral processing professor; well, in those days it was called mineral dressing. He had done his master's degree under Gaudin at Butte, in 1937, and had gone on to Penn State to work on a PhD which he never completed, because of going into the army. He said that after the war, he had a family and decided that he was too old to try to go back and finish the PhD program. So he joined the faculty at Montana School of Mines when Professor Cooke left for Minnesota. When I came to Butte, McGlashan had a good research group working on a range of mineral processing problems. There were maybe five of us in that group; Frank Aplan being one, myself, the oldest one being Bob Kupfer who was thirty-four and I was twenty. The other two were Chester Freshour and Wilbur Guay.

Swent: A lot of mines were operating at that time in Butte.

Fuerstenau: Oh yes, oh yes. When you approach Butte from the highway, there were and are huge signs stating, "The Richest Hill on Earth." Of course, I think that the profits from the riches were taken elsewhere. I want to tell you a little bit about the mining part. At that time, all of the mines were underground. Headframes appeared throughout a lot of the city. In 1950 almost all of them were operating mines. The School of Mines was located in the northwest part of Butte, on a hill that had been partially leveled off. Just below the metallurgy building a short way down the hill ran the railroad tracks that transported ore from Butte to Anaconda about twenty miles away, where the concentrating mill and the smelter were located. So one would hear and see the ore trains going by regularly. The train as I recall had the glorious name of Butte, Anaconda & Pacific Railroad. Today all facilities except for the smelter stack are gone from Anaconda. The Butte ore had a high arsenic content, and that created smelting problems. After much back and forth between Anaconda and the EPA over modernizing the smelter, the company, which was then owned by Arco, shut it down.

Graduate Student Research in Mineral Dressing

Fuerstenau: Professor McGlashan had one of his grad students in 1949, Bob Kupfer, working on a mining environmental problem early on. Kupfer was trying to extract the copper, iron, and zinc from Silver Bow Creek which flowed out of Butte. He was using ion exchange resins to do that, because naturally the runoff had these metal contaminants in it. There were a number of interesting projects like this that Professor McGlashan had his graduate students working on. But McGlashan, for some reason, had kind of a secrecy complex, and he actually locked up people's theses in the safe because he didn't want other people to steal the ideas. Papers should have been published but they weren't. He would have done the world a lot of good to publish some of this, the use of ion exchange to recover copper, and zinc, and maybe lead, from mine effluent streams would have been a real contribution.

My MS thesis research involved a study of trying to find universal reagents for floating copper minerals. You know, in copper ores there might be copper silicates, copper sulfides, oxides, native copper. So, can you find a compound that might interact with the surface of all of these minerals? The previous year, a graduate student named John Doherty had worked on this for his master's degree, and I carried that on. The technique that he and I used to assess the flotability of the copper minerals was measurement of contact angles, something that I will talk about later on. It really turned out to be a good piece of work, and his results were never published either. Two or three years ago, I finally wrote a paper that was published in the *International Journal of Mineral Processing* commemorating Frank Aplan's seventy-fifth birthday. How's that—publishing a paper fifty years later? But if we had done it earlier it really may have had some pioneering impact. The reagents which we tested as potential flotation collectors were chelating agents that were known to form strong complexes with copper ions in solution. I found one reagent that really was looking very good, phenylthiosemicarbazide, and McGlashan had another graduate student, Al Turk, follow up trying to make this reagent with various modifications in different ways, and that became his master's thesis. But nothing ever appeared in public about it. It's too bad. In contrast to Gaudin, who published a great deal that brought him much recognition, McGlashan did not publish. Even so, he was quite an inspiring person; as you may have gotten from Frank Aplan too.

Swent: Frank considered Professor McGlashan to be very inspirational to him.

Fuerstenau: I also took an advanced inorganic chemistry class from Professor Edwin Koch, and he was a very well organized lecturer. An organized lecture is far better than an off-the-cuff lecture. At MIT, for example, I had a lot of off-the-cuff lectures, and some organized, and because of this, I have always prepared and given organized lectures in classes. Every lecture I have ever given, I have always written out the notes, not that I read them. But I may have acquired giving organized lectures from Koch, who eventually was the president of Montana School of Mines, about two presidents later.

Earlier you had asked about graduate study at the South Dakota School of Mines and Technology, and I had commented that there was no research going on there at the time. At Montana School of Mines, the only professor who had a really active research program was McGlashan. There was a little graduate research in the metallurgy group but absolutely none in any of the other fields in Butte at the time. Having been a graduate student in Butte when Gaudin was very active there probably provided the insight and inspiration to McGlashan to develop his own active research group.

General Activities, Visiting Mines and Mills, and Winter in Butte

Fuerstenau: I was there just the one year. Twice that year it hit minus fifty-two degrees Fahrenheit. So cold! Unbelievable. Not two days in a row, but two different times. Butte is high, 5,700 feet; you've been there, so you know that it is in sort of a bowl. In my year there, all of the mines were in their heyday, and you could see the exhaust from the ventilation shafts rising straight up into the sky. That is, the white steam condensation. No wind. So with no wind that cold just settled in. They have never had winters there like that since, I have been told. Needless to say, I recall that intense cold very much.

Now I'm going to back up. My last year in South Dakota, just as the Christmas break was ending, that Sunday, late in the afternoon, it started to blizzard. That was the worse blizzard in umpteen years. It blizzarded from Sunday until Friday. You couldn't even see across a street in Rapid City. My friend, Paul Fenske, was driving back from Mobridge, and they got to Wall at about six o'clock by following right behind a big semi into town, and they spent the next five days in a booth in a café in Wall. Booths in all the cafes became hotels. When this blizzard was over with, the drifts on St. Joe and Main Street in Rapid City were clear up to the top of the second floor buildings. You weren't in South Dakota then, were you?

Swent: I was in Mexico, but I do remember hearing about it.

Fuerstenau: That was something. You would have heard about it. That went on for five days! You could not leave the house if you had to. If there was an emergency, I guess they came around in a big snow plow truck. But you literally couldn't see across the street. Also the ranchers all over western South Dakota had huge problems.

Swent: Lost a lot of cattle.

Fuerstenau: A tremendous amount. Another of my college friends who spent the five days in Wall said that he had a slight crack in the window of his car and when he went out on Friday, he found that his car had completely drifted full of snow. Well, that probably resulted from 90 mph winds driving the snow.

Coming back to Butte, when I was there that was the heyday of the mines; they were all going. Butte was a rough place, as I guess any major mining place used to be. One time Frank Aplan and I went down in the Travona Mine with a mining engineer from the Anaconda Company. This was a manganese mine right near the School of Mines campus. The headframe is now gone. But then they were mining manganese—and the mineral was rhodochrosite, which is manganese carbonate and is a pink mineral. You know, it was really something to see. The vein was essentially three feet wide and almost vertical on a steep slope. This was a massive vein of pink rhodochrosite. Beautiful! Of

course they had to mine it with wider stopes to be able to get in there—probably six feet wide to extract the vein that was about three feet wide.

And I remember we went from one level to the other on ladders, wooden ladders—you would never do that sort of thing today. And of course for the mining engineer, this was no problem to him. I'll tell you, at one point the ladder was leaning backwards, and you're kind of hanging on this as you're climbing up with water dripping on you. I'll tell you, I was really scared. Frightened, that's the word for it. As for our mining engineer guide, he's just climbing around like it's nothing.

Swent: He might have been showing off a little for you.

Fuerstenau: I don't know about that. But the thing is, it's probably second nature to him. The Butte ore body, historically had manganese around it, on the outside, and then lead-zinc next to the core. Then underneath there was, I suppose, silver-gold, which was what they first found and went after. And underneath that core was copper, for which Butte became world famous. The Travona mined what was left of the early manganese. In this case, as I said, the ore was manganese carbonate. Very beautiful, but I don't know what they did with it. They probably sold it to the operation in Phillipsburg—which we heard about from the small mines oral history. What was his name, the engineer from Reno?

Swent: Hugh Ingle?

Fuerstenau: Ingle⁵. Anaconda⁶ probably shipped their manganese concentrates there.

During that year, I visited a number of mills and smelters around Anaconda, Great Falls and Helena. Frank and I drove up to Great Falls and went through Anaconda's zinc plant there. It was built there because of the electric power needed in an electrolytic zinc plant. But I never went into any other underground mines, not even any of the famous huge copper mines, like the Mt. Con or the Kelly. A lot of the students worked weekends in the mines. For mining students, that was a good way to earn money and gain experience.

Swent: Being in Butte, Montana School of Mines must have had lots to offer in educating mining engineers at that time.

Fuerstenau: Indeed. It was interesting living in a city where there were so many miners and so much mining activity going on at the time. Butte had other things to

⁵ Hugh C. Ingle, *Independent Small Mines Operator, 1948-1999: Corona Mine*, 2000

⁶ Vincent D. Perry, *A Half Century As Mining and Exploration Geologist with the Anaconda Company*, 1991

offer too. For example, Frank Aplan really liked Dixieland [jazz], and I remember we used to go down to a place that I still chuckle over called the C.O.D. which had a great old Dixieland guy named Jake Flores who apparently—I'm talking now 1950—once upon a time was probably a main-line trombonist in New Orleans. He would be at the C.O.D. a month or two at a time. Frank would just go nuts over Dixie. I would sort of take it, but Frank really did.

Another thing I would like to mention was that now and then I went to church, a Lutheran church there, where I met somebody who had two young boys, sitting in a pew fairly near me. He introduced himself. It was Chuck Meyer. And Meyer then was a research geologist at Anaconda. Apparently, while working there, he did his Harvard thesis, which became a super famous document on the geology of Butte. He worked then with Reno Sales who was Anaconda's chief geologist. Meyer invited me to come see his lab, which I did visit. He was not an underground sampler, of which obviously there were probably dozens. Meyer had a research lab that he showed me, where he was making mineralogical and petrographic measurements. Of real interest is that I met Chuck at probably every Annual AIME [American Institute of Mining, Metallurgical, and Petroleum Engineers] Meeting that I went to on through the years. When I met him, I used to talk to him a bit. It was only a couple of years later that he came here to Berkeley in 1952, I think. Again, probably brought to Berkeley by Donald McLaughlin. I think Meyer may have started at Harvard graduate school while McLaughlin was still the mining geology professor there. It's kind of interesting how people overlap, isn't it?

Early Membership in the AIME as a Student

Swent: Yes. You mentioned AIME. When did you join AIME?

Fuerstenau: In 1946.

Swent: In Rapid City?

Fuerstenau: As a student. Several years ago I got my fifty-year Legion of Honor Certificate. I became a student member in 1946, and I guess those years count into total years of membership. My membership in AIME has always been of significance to me. For example, an early pleasure that happened was that when I was a junior, maybe after I switched to metallurgy, I got one of the AIME Woman's Auxiliary scholarships. At the time, I was happy and pleased with that. I remember even that the Dean of Students, Dr. Leighton Palmerton, came by to tell me about it on a Saturday, where I was working at Swander's Bakery. He apparently had gotten a letter saying that they were going to award me the scholarship. He lived fairly near and dropped by on his way home to tell me. That pleased me very much. Of course, those were considered scholarship loans that you paid half back. I remember that I finally paid the

whole thing back when I was working at Kaiser in '58. And the person who was then WAAIME treasurer, was Mrs. Evan Just. And that's when he was still editor of E&MJ [*Engineering and Mining Journal*] in New York before he joined Stanford's Mineral Engineering Department. But I remember it was to Mrs. Just that I had to send the last checks to.

Swent: I think my mother was on the WAAIME committee at that point.

Fuerstenau: I bet she would have been.

Swent: And Maurie also received a scholarship later, I think, didn't he?

Fuerstenau: He probably did. He probably would have, because he majored in geology. Your mother, without a doubt, could have been involved.

In my time as an undergrad I did go to local AIME meetings, Black Hills meetings, a couple of them in Rapid City. And I remember one time sitting directly across the table from—and I was just a young student—Guy Bjorge who was the general manager of Homestake up at Lead, right? He was the big man?

Swent: Right.

Fuerstenau: And at another one of those meetings I sat next to Homestake's chief geologist, Jim Noble. Shortly after, he left to be a professor at Caltech; I remember reading about that.

Swent: He was a Harvard graduate, also.

Fuerstenau: Things sort of tie together. Noble was at Caltech together with Ian Campbell at the time. And the person who was dean at Stanford for a long time, his name started with "J", was also there then. And then, I think what happened was that Caltech decided that they were going to get rid of their geologists and go just to geophysics and geochemistry. So all of these old-line geologist left or were retired. Ian Campbell became director of the California Division of Mines and Geology. The third person became dean of the School of Earth Sciences at Stanford—

Swent: Jahns, it was Jahns.

Fuerstenau: Jahns, Dick [Richard H.] Jahns. Some years ago I had a lot of interaction with him at Stanford. Anyway, the decision was made that they didn't need geologists but geochemists and geophysicists. And that took place here at Berkeley also about forty years ago. They didn't unload people, but they hired only geochemists and geophysicists. And then, all of a sudden, you don't have any faculty that knows the geology. Here at Berkeley is an outstanding person, namely [George] Brimhall, who is a very good mining geologist and also

knows geochemistry well. Both the sciences. He really, really knows field geology and ore deposit geology. It seems to me that a geology department needs those kinds of persons, in addition to geophysicists. I guess the world of biology is the same: you don't need classical biologists anymore; you just need a molecular biologist, and a biochemist, a biophysicist. Edward O. Wilson, in his autobiography, wrote of this taking place at Harvard in his early years.

Another memorable local Black Hills AIME meeting was held at Pactola, before the dam was built there. I remember sitting at the table that evening with J.V.N. Dorr, the famous Dorr who invented the classifier and thickener, around the turn of the century up near Lead. A dignified man with a little goatee beard. He was the speaker that night. He founded the Dorr Company, which existed as the Dorr Company, then Dorr-Oliver until taken over by a subsidiary of Kennecott.

[Tape 5, Side B]

- Fuerstenau: Something I learned about Dorr, either then or years later, that it was J.V.N. Dorr who first suggested painting the side of a highway with a white strip.
- Swent: I remember hearing that.
- Fuerstenau: That's kind of interesting, isn't it, that he came up with such a great suggestion, and so on. So this would be 1950. He invented the rake classifier and the thickener in 1900, 1901, or 1899, while working in the Black Hills at the Golden Reward Mine. I imagine your father knew him well, right?
- Swent: I'm not sure. I think he had already gone. My dad went there in 1911, and I think that Dorr had already gone.
- Fuerstenau: After his few years in the northern Hills when his processing machines proved to be so successful, he probably returned to Connecticut and started a company to make them.
- Swent: It was up around Trojan that he worked. I guess Golden Reward was the mine.
- Fuerstenau: The last time I was around Trojan you could still see where some of the old operations had been. I don't know whether that's all gone. It probably is.
- Swent: Probably. And [Charles W.] Merrill was up there at the same time, I think.
- Fuerstenau: From what I have read, Merrill spent about ten years at Homestake where he fully developed cyanidation as a process for the recovery of gold at that time. South Africans had discovered cyanidation, but it was Charles Merrill who made it practical. By the way, he graduated from the College of Mining here at Berkeley in 1894.

It is interesting that the local AIME Section in the Black Hills was very active in those days, whereas I have no recollection whatsoever of a local AIME Section meeting in Butte. Maybe that was because virtually everyone in the area worked for one company, Anaconda. Later, at MIT, I went quite often to Boston Section AIME meetings, and recall meeting the famous Harvard mining geologists, L.C. Graton and Hugh McKinstry.

More about Butte and Fellow Students at Montana School of Mines

Fuerstenau: Anyway, my year in Butte gave me a good flavor of mining.

Swent: A lot of fun?

Fuerstenau: Oh yes. I think that you heard a lot about Butte relaxation from Frank Aplan. I sort of relaxed every Saturday night and suffered every Sunday. Not every one.

Swent: What kind of financial support did you have at Montana School of Mines?

Fuerstenau: I had been awarded a fellowship by the Montana Bureau of Mines and Geology that paid me \$70 per month, as I recall, plus tuition. That allowed me to be a full-time student, and it was enough money to pay my costs and leave a little extra. Frank Aplan had a half-time teaching assistantship that paid him more but led to a two-year program.

Fuerstenau: Then, on graduation—

Swent: You got your master's, an MS?

Fuerstenau: Yes, this was my master of science degree. In late winter I concluded that I really had to lay into it if I was going to complete the research for my thesis in time, so I started working virtually day and night in the laboratory. Everything worked out all right. I submitted my thesis and had to pass a rather comprehensive oral examination before graduation. The commencement speaker was Don McLaughlin. He was president of AIME that year, and he was on the commencement circuit. He already had that nice head of white hair, that you know so well. I don't recall what his message was to the graduating class.

By the way, I had reapplied to go to MIT. Don McGlashan had gone back to the annual AIME meeting, which was in New York, and then had gone up to Boston. I guess he found out that Gaudin and Rush Spedden were a little unhappy at my turning them down the previous year. He said, "This is a very capable young guy; you ought to take him." It was only years later that I learned of these events related to letters crossing in the mail and Spedden's early negativity towards me.

In Butte I acquired some very good friends—first of all, my friend Stair Dickerman from Kellogg, came to Montana School of Mines as a freshman that fall. His roommate was another freshman named Don Cenis. Don was really one of the sharpest persons I ever met. He was an only son—his parents were a little bit older. His father, who, I think, had only gone through about the sixth grade, was general manager of a big coal mine near Bear Creek or Red Lodge, Montana. And that was the mine where sometime in the late forties there had been a big explosion and maybe one hundred, two hundred people died. You may remember that *Life* magazine had a big spread on this, showing pictures where somebody had painted last notes on barrels and things like that, while they were still alive.

Swent: The Sunshine Mine?

Fuerstenau: The Sunshine Mine disaster in Idaho was about 30 years ago. This was a coal mine near Red Lodge, Montana. His father was manager of that mine at the time. Anyway, Don Cenis was a tremendously sharp guy. Not only straight A's, but a very outgoing kind of person. Cenis graduated with straight A's in mining, and I think maybe also a degree in geology, and then did a master's in mining. After serving in the air force, at age twenty-nine he was the general manager over three uranium mines in Wyoming. The company was Western Nuclear and the president was named Adams. Langan [Swent] would have known him. During this time, Don's wife died of lupus.

Some time later, the personnel director of Kennecott in New York called me up and asked, "Do you think this person has the potential to become the president at Kennecott?" And I said, "I sure do." He worked a while for Kennecott at Ajo, Arizona, but his new wife did not like living in a small desert mining town, so he quit and came to Berkeley as a graduate student in mining. After one semester, he decided that he wanted to go back to industry and took a position with Texas Gulf Sulfur, directly as an assistant to the president of the company. T.G.S. was the big sulfur and potash company that also owned the Timmins Mine in Ontario, which I think is one of the biggest zinc deposits ever found. One thing that might be interesting, just to comment a little bit on, is that the chief geologist, and another geologist, and maybe some of their exploration people, went out and bought stock before they announced the discovery. That really made the national press. And of course they had to sell back their stock and everything else, because once that big deposit was announced the T.G.S. stock soared. That is the kind of thing, needless to say, that internal exploration people in mining companies cannot do.

Swent: In the old days they did. Hoover, in his day, that was the way they operated. It was accepted, wasn't it? I think the exploration people always got a piece of the action in those days. But it's frowned on now. Well, it's illegal now.

Fuerstenau: They pursue personal interest elsewhere now. Don Ceniz worked directly for Dr. Charles Fogarty, president of the company. Unfortunately, Fogarty and two vice presidents were killed when the company's private jet crashed while trying to land in the fog in Connecticut.

Swent: Wasn't that Kennecott?

Fuerstenau: No, Kennecott lost all of their new top management up in Canada in the late forties when someone had planted a bomb on a commercial airliner. After that most companies would not let all the top people ride on the same plane. Interestingly, I recall reading that the person that Kennecott then hired as their president was the president of Carnegie-Illinois Steel Corporation, Charles Cox.

[Added by Douglas Fuerstenau during editing: Charles Fogarty was elected to the National Academy of Engineering in the same year that I was, 1976, but I did not meet him at that inaugural function. Another mining executive who also was elected to NAE in 1976 and whom I had met earlier and I did talk with on that occasion was Charles Brinkerhoff, the retired CEO of Anaconda.]

Anyway, Don Ceniz was working as an assistant to Fogarty, who sent him out to Utah, because the production in their potash mine wasn't high enough. In six months he had the production back up to where it should be. And what angered Ceniz a little bit is that he was making less money than the general manager—and Ceniz was the person who brought the operation back to full capacity.

I think that this led to his becoming a vice president of Chase Manhattan Bank—he was only in his thirties then—for giving out finance loans from the bank to hard-rock mining companies. Ceniz was an extremely sharp and very capable person. Years later I recommended him to Harry Conger as a potential finance vice president, but Ceniz told Conger that he was not interested in being CFO of a company. I saw him every once in a while through the years, but do not know what kind of positions he held in later years.

I think his career just went downhill later on.

Swent: Because of personal things

Fuerstenau: Yes. I had did have contact with him for a long, long time. Ceniz was kind of an impulsive sort of soul and would move on rather abruptly.

[Added by Douglas Fuerstenau during editing: I looked at the 1950 Montana School of Mines college annual, the *Magma*, and noticed that the photograph of a freshman adjacent to that of Don Ceniz was Ray Beebe, whom I did not meet during my time in Butte. Beebe, who is my age, had gone into the navy directly after high school and started college in 1949. Ray had a distinguished

career with Marcona, as president of Carpcor, vice president of Newmont, and retired as senior vice president of Homestake. While at Homestake, he was also president of the Mining and Metallurgical Society of America, which contributed significantly to funding the ROHO mining series.]

Another long-time friend from Butte was Jack Jordan. I told you about him falling down the mine in Bunker Hill. He was a sophomore at Montana School of Mines that year I was a graduate student. And then he transferred to MIT in his junior year, which we will pick up on again later.

Swent: Where did you live in Butte?

Fuerstenau: In the dormitory. Three of us had a suite. One of the persons in the suite served as the proctor of the dormitory. His name was Chuck Arentzen. He then was twenty-nine or thirty, and he had been in the Bataan March in the Philippines and spent three years in a Japanese prison. He said that by the end of the war, he had beriberi so bad that he couldn't walk. He came back to Butte and started the metallurgical engineering program as a freshman. During my year there, he was a senior and graduated in 1950.

Swent: Had he been working in one of the mines in the Philippines?

Fuerstenau: No, I think that he was in the navy. After graduating in 1950, he joined Anaconda and spent his career with them, and mostly down in Tucson.

One time I was in a bar in Butte and I saw a poster about drawing doodles to win a case of Butte Beer. So I started drawing these doodles or cartoons, and I would send them to the paper. I don't know how many cases of beer I won, either in my name or using other students' names once in a while. I've got some of them framed now. With Butte being so cold in the winter, of course you could put bottles in the window and they would cool off rather quickly. The elderly woman, Minnie Tennis, who was house mother of the dormitory one time, jumped on me over cooling beer in the window, and said something to the effect: "And you, in the proctor's room!" Since I liked to draw, I also did all of the art work for the Class of 1950 college annual.

Over thirty years ago, I read quite a bit about Chuck Arentzen in another way. Some thirty years ago, a United Airlines 727 was landing in Salt Lake City, and it hit the runway so hard that it crashed. This incident was in all of the newspapers. About half of the people were killed. This occurred when the 727s were new, apparently on landing at the Salt Lake airport, the plane's rate of descent was faster than the pilots realized. After that, the procedures for landing 727s were changed, from what I remember reading.

Fire broke out in the middle of the airplane, but Chuck Arentzen was in the back row of the airplane. The back door cracked open, and they pushed in a fire hose. Because Chuck had been a fireman in the navy, he knew how to

handle the situation and saved himself, somebody else, a stewardess, and the director of research of Anaconda. This research director died four days later. But all the people in front of them were killed. And he got a presidential citation for this act. He's a guy who could use his head and knew how to handle himself. He saved that handful of people in the back of the plane. By the way, my friend Don Cenis had often talked about Bill Linderman from his home town of Bear Creek, Montana, who was the All-Around Rodeo Cowboy in the late 1940s or early fifties. Linderman was killed in that airplane crash.

Swent: Who was your other roommate? You said you had two roommates.

Fuerstenau: His name was Donald Levandowski, and a senior who was majoring in geology. He ran for being student president that fall, and he won. He ran on the premise that his name spelled backwards was, "Iksowadnavel." He prepared big signs stating this as his campaign slogan. It was just fun. And he won. Later he got a PhD from Michigan and became a geology professor at Purdue. I had had no contact with him for several decades until I did run into him under the Mineral Institute program, something that we will talk about later on. Unfortunately, he died of cancer just two or three years ago. It came on quickly, from what I understand. Nice, nice guy. And I went back for my 50th reunion, at Butte, last year, the year 2000, and Chuck Arentzen was there. He has to be eighty, more than eighty, and he's just one of those persons who looks and seems more like sixty. Levandowski was a very nice pleasant guy. In fact, there were only a handful of the undergraduates that were on the younger side in that class, because the bulk of them at Butte, as was the case in South Dakota, were returned vets. I happened to be a non-vet who was a graduate student working for a master's degree.

Swent: You must have been the youngest.

Fuerstenau: I was twenty-one then when I got—

Swent: You were far and away the youngest graduate student, I'm sure.

Fuerstenau: Yes, yes. So I was twenty-one then when I got an MS, master of science degree. So we'll carry on then. I would like to add that my going to Butte for a year had a profound influence on the personal and professional lives of two individuals—not only myself but I also think that of Frank Aplan. Our friendship that grew during my year at Butte eventually led to Frank's coming to MIT as graduate student. Without a doubt, my own later family life would also have been totally different if I had not gone to Butte for that year but had gone directly to MIT from South Dakota.

Comments on Some of the Early History of Flotation

[Interview 3: July 26, 2001 in Berkeley, CA, Evans Hall]

[Tape 6, Side A]

- Swent: I wanted to pick up on a couple of things that I learned in some reading yesterday. We were talking about Merrill and Dorr working early in the twentieth century in the Black Hills. I came across the name Elmore, as a person who was doing flotation research or development in 1905 in Australia, and I remembered that there was a Spearfish Canyon railway that operated in Spearfish Canyon in the Black Hills, long since defunct, but it was probably operating around 1900-1910. And one of the stops on that railway was named Elmore, and there's still a little community there of summer cabins. People have their summer cabins at Elmore in Spearfish Canyon. The Elmore brothers were credited with early development of bulk oil flotation in England—they were from England, and in 1898 they developed something called bulk oil flotation. And in 1904 they patented the introduction of bubbles. They had a company called Minerals Separation that they worked for, and they got the first commercial flotation there in 1905 in Broken Hill, Australia. So that probably is why that little stop in Spearfish Canyon is named Elmore.
- Fuerstenau: My guess is you're probably right. The Elmore process, which used large quantities of oil that would wet the sulfide minerals, was the predecessor, to froth flotation. The process worked by adding oil to the slurry and agitating it gently. In the original process, sulfide minerals would adhere to and even be entrained in the oil layer, which would be skimmed off so that the sulfides could be extracted from the oil. I read in a 1916 book, *The Flotation Process*, by T.A. Rickard, that a group of senior students at Berkeley wrote a paper on a senior research project on the Elmore process in which they violently agitated the oil-water-mineral pulp such that they made an oil foam that floated to the surface while loaded with the mineral particles. As discussed in Rickard's book, these students were on the verge of coming on to the fact that air bubbles assisted the process by creating an oil foam that would rise to the surface, and that way the sulfide minerals could more effectively be removed. In the Elmore process, the oil lake on top of the cell could become so loaded with particles that at times it would sink. The work of the Berkeley students was lost until 1916. An attribute of the Elmore process was that it consumed a large amount of oil in contrast to its successor, froth flotation. I have read a lot of this history and because I found it very fascinating. I presented some of this in a paper published in the SME symposium on behalf of my brother's sixty-fifth birthday.

Swent: The other thing that I thought we should give more than passing mention to: you had said that you visited the Morning Mine when you were at Butte, and I didn't pick up on the fact that the Morning Mine is one of the first places in North America that flotation was used.

Fuerstenau: Yes. On visiting the mill there in 1949 with Frank Aplan, which was a lead-zinc mill, we observed that much of the equipment was really ancient, and a lot of it was self-manufactured, I think. The flotation machines were sort of like old washing machines, had kind of a rotor that slopped the pulp or slurry, raised it up into the air a few inches, and when it fell back down onto the surface it created bubbles. This was their flotation machine. Another attribute of that operation was that they had to grind the ore very fine, something like 75% minus 15 microns to liberate it. As I remember, the grades of their products—I'm speaking of lead concentrate and the zinc concentrate—were adequate but were never very high because of that. An interesting old mill, a big one, and a major one in its day.

Swent: That wasn't your first contact with an old flotation device.

Fuerstenau: Maybe so. But that was just part of the large number that we visited in the Coeur d'Alene lead-zinc district of Idaho that summer. Other mills had homemade flotation cells.

Summer Work at Howe Sound Company, Lake Chelan, Washington, 1950

Swent: So now I guess we're ready to take you to Chelan.

Fuerstenau: So this is the summer of 1950, and I looked around at what might be another summer job. I contacted the mill superintendent at Howe Sound Company in Holden, Washington, up from Lake Chelan. The mill superintendent was Victor Zandon; originally it was Zanadvoroff, but he shortened it to Zandon. He wrote a number of papers about flotation mill operation. Langan might have met him on some occasion. I think he was probably a native Russian, sort of like Malozemoff. He looked a little like him. I may have gotten the idea to write to Zandon from a fellow grad student, Wilbur Guay, who had come from Holden. Anyway, I sort of implied that I would like to learn about how mills run from the bottom up, so they offered me a job and I went over there. The assistant mill superintendent was an engineer in his early thirties, named Steve Mitchell, a Washington State graduate.

That was my last summer job. The Howe Sound Company had a copper, zinc, and gold deposit in the Cascade Mountains, at a mining camp called Holden, Washington. One went about forty-five miles up Lake Chelan, by boat, to the dock there, and then, as I remember, it was maybe ten miles up a U-valley to the mine. And this was all beautiful country, and all had been glaciated, and

wherever you looked there were hanging U-valleys. Lake Chelan itself was a glacial valley, and the main valley going into Lake Chelan was also a U-valley, with numerous hanging U-valleys coming into the side of this main valley. The buildings at Holden really looked like a resort. The dormitories were very good, dining facilities and recreational facilities that they had for the workers were excellent. I think all of this has now been converted to some kind of resort.

But I worked in the mill, just as a laborer.

Swent: Can you remember what you were paid?

Fuerstenau: No. Again I don't. It's often that I recollect my childhood pay but not this. I assume that it was in the range of \$250 per month—mill laborer's pay at the time, maybe a little more.

Swent: And board and room included?

Fuerstenau: There was board and room there and probably at a nominal cost. In mining camps you always eat well. These dormitories were very nice, and I had a single room. I remember there was a stream flowing right behind, so if you're working in the night shift—one could hear the stream a lot of times when trying to sleep during the daytime.

The first job they gave me was picking sticks off the conveyor belt, up on the crushing level. This was an underground mine, producing between 2,000 and 2,500 tons of ore per day, and as a result there was a lot of broken timber pieces that came out with the ore that had to be removed before going into the gyratory crusher. The first stage of crushing was done underground with a jaw crusher that could handle wood pieces and scrap steel that could jam up a gyratory crusher. And I remember there were 385 steps that you had to climb from the bottom of the mill up to the crushers. For the first week, I could hardly do it. I counted those steps on one occasion.

A few years ago we were up in Vancouver, and Howe Sound Company had a famous old mine there called Britannia Beach. Well, the old mill is still there, and we went inside of it. It's built again up the side of a mountain. I looked in there and you could see about 400 steps going on up along the side. My guess is they used the same design plans from Britannia Beach when they built the Holden mill.

Swent: I think Pete Fowler⁷ worked at Britannia Beach. I believe he's the one who talked about it.

⁷ Hedley S. "Pete" Fowler, *Mining Engineer in the Americas, India, and Africa*, 1992

Fuerstenau: In fact, I know he did. Britannia Beach was a famous Howe Sound property. Why the company is called Howe Sound is that Britannia Beach is on Howe Sound, up there.

The object was to pick off the conveyor belt the chunks of wood that came out of the mine. I didn't do that for too long, but for a while. They strapped you in doing this job. I mean you were held with a strap or belt so you couldn't fall down onto the belt and then into the crusher. But what happens is, after a while, that the conveyor belt appears to stand still and the building moves, an odd sensation. That's why it's nice to be belted in. [chuckles] All of a sudden the belt appears to stop and the building appears to move. Crushing was done on a two-shift basis, whereas ball mill grinding and flotation ran on a three-shift basis. I was trying to recall what I did to take a break and to eat my lunch. I probably stopped the belt and the crusher since there would have been surge bins. The operator for the crushing section was near retirement age. I recall that every so often he and I would have to take a huge wrench to tighten bolts on the cone crushers that followed the huge gyratory crusher. This was because of steel wear in the crushers because of the hard ore. I did that job for three weeks, or something like that. A lot of intellect required for picking sticks off a conveyor belt. [chuckles]

Swent: A real entry-level job.

Fuerstenau: I did various jobs in the mill. One of them involved providing backfill material for the mine. To do this, the old tailings were hosed down with a hydraulic giant for repulping, reslurrying them. Then the slurry was pumped back up by the mill to a small thickener tank, where the fines were removed by an upward flow of water, and the underflow sands were taken back down into the mine as mine fill. At Homestake they also used the sand for mine fill. But I recall that one day I went down to move this hydraulic giant that was hosing down the old tailings—and I stayed down there a little too long and was a little aggressive in hosing the bank of tailings, and by the time I got back up the thickener was completely full of sand. The mill superintendent was a little unhappy about it. Everything related to the thickener had to be shut down, and hosed out, and started over again. Not major, but a bit of a problem.

Swent: A good learning experience.

Fuerstenau: Oh yes. And then later in the summer I was moved to running the zinc and copper flotation cells. Since this was before any automation, controlling product grades and tailings assays, and so on, took an operator with real experience. One would take samples and get the analysis back two hours later, in those days, so you can see that you're always way behind on making sure the grade and recoveries were achieved. Today, of course, in most operations, sampling is automatic, and analysis is done by x-ray fluorescence, and the answer comes very quickly. But then it took two hours. If I remember, I even

spent some time in the laboratory running these analyses myself, which were all, in those days, done by chemical analysis. The flotation performance was regulated mainly by controlling the level of the overflow weirs and some reagent adjustment.

The mill at Holden, again, used self-built flotation machines. And these were deep troughs, called Munroe-Pearse cells after the milling engineers who first built them. They were simply long tanks, about ten feet deep, and five feet wide, and maybe up to sixty feet long, with a series of one-inch vertical pipes introduced down the center line from a large manifold. These pipes were used to blow air into the bottom of the cell, agitating the slurry in the trough and providing bubbles for flotation. These very deep cells were claimed to have certain advantages, such as producing higher grade concentrates. This simple machine—not even a machine—was the type of flotation machine that the Howe Sound Company had at Britannia Beach, and also had there at Holden. The operation was not a high-grade mine, maybe running about 0.8 percent copper, only 1-2 percent zinc, and maybe one-tenth of an ounce of gold per ton. The final step was to recover the gold in a pyrite flotation concentrate by cyanidation followed by precipitating the dissolved gold with zinc dust. So there was a small gold plant, at the tail end of the mill.

All concentrates were shipped to the Tacoma smelter.

The mine operated on a five-day schedule, two shifts per day, but the mill six days per week on a 24-hour schedule. The operation was small enough so that they could shut it down and have it all running again within a half hour on starting up Monday mornings. So most Sundays were free to go hiking. You could hike three or four miles away up to cirque lakes from glaciers, and these lakes had colloidal particles that were produced by the grinding done by the small glaciers still there. This gave a milky green appearance to those lakes. So I did a lot of hiking with one or two of the other summer employees, whose names I simply don't recall. But it was a beautiful, beautiful area. I do recall that on some hikes, deer flies were a menace.

Swent: Did they have the same plan of hiring a lot of college kids in the summer?

Fuerstenau: There were quite a few. This was not as big an operation as Bunker Hill where there was a large mine, mill, lead smelter and zinc plant. But maybe they had a handful. One summer student taught me the rudiments of playing bridge, as a matter of fact. Because again, I'm assuming there were vacation replacements, and maybe some of the employees went out and lumbered in the summer or farmed if they had a farm down below.

It was just a good summer, and I had an opportunity to do a full range of mill operator jobs, including also looking after the ball mill grinding section. And as I said, I had been planning on going to MIT. I remember when I said that I was going to MIT, a laborer in the plant, a fairly young guy, said, "Oh yes,

they're the ones who just won the Henley Regatta," [laughter] and that made me chuckle. Here this laborer knew minute details about all sports. I don't know if I had even heard of the Henley Regatta then. But he knew MIT had won this that spring.

Also, of course in the middle of the summer, the Seattle paper had about three-inch headlines telling about the invasion of Korea. This was the summer of 1950 and that, of course, was of concern to everyone.

Swent: Was anything done about health care?

Fuerstenau: Oh yes. I had to take a physical to start with, and the company had a fairly young staff doctor there, and obviously good medical facilities, because you would have to take care of any injuries. There were no serious injuries that summer. I remember visiting him a couple of times, chatting with him for quite a while. I assume he wasn't overly busy.

But by the way, there was a regular problem, because of being up in the mountains with a long power-line coming along Lake Chelan. If there was a major thunderstorm, they lost electrical power. And that did happen, two, three, four times that summer, and that now means, in the mill, everything shuts down. We're talking about ball mills, flotation machines, thickeners and filters all start sanding up. This being a major problem, I suppose, for underground in the mine there was auxiliary power, but not for the mill. They obviously had procedures to get the mills restarted. But this power loss was something they had to live with, and basically every time there was a thunderstorm it would knock out the power on the grid somewhere.

So at the end of the summer, three or four us left by flying boat down Lake Chelan down to the town or city of Chelan, and then drove over to Seattle, where I spent the weekend with my friend, Stair Dickerman, who was a Seattle native. That was my first trip to Seattle, and experiencing there, in late August, that light drizzle rain that the Northwest is famous for. Having lived in arid country, I had never seen that faint, light drizzle before. From Seattle, I took the train all the way back, Milwaukee Road, through Butte, and on to Miles City, my brother-in-law met me and drove down to Rapid. I spent a few days there, of course, before starting a new venture, which was to go to MIT.

One last comment that I would like to make about living in the company dormitory that summer and also the previous one at Bunker Hill, during meals one talked with different workers, including underground miners. At that time I first learned about "tramp miners." Several times I heard a miner say that he had worked in the mines at Butte for a while, or maybe in Idaho, or at the Homestake Mine, or Climax or south in Arizona --- and was thinking of moving on again. An experienced miner could apply his skills in any underground mine and go elsewhere at will in those days.

IV MASSACHUSETTS INSTITUTE OF TECHNOLOGY, GRADUATE STUDIES, 1950–1953

Early Months at MIT and Meeting Faculty and Fellow Graduate Students

Swent: You had not visited there, or seen it at all?

Fuerstenau: I had never been east of Chicago. Chicago was my only time east of South Dakota, as a matter of fact. This was also my first commercial airplane flight out of Rapid City to Minneapolis, where I stayed for a couple of days with some friends, and then flew to New York. Those were the days of DC-4s, and they flew low. I remember, it was like flying through canyons, when you got into clouds, over New York State. Since the DC-4 could not go high, the plane sort of went around clouds, just like being in big canyons. I spent a few days with an aunt who lived in New York. Actually, they were living near Philadelphia, at the time, but this was my mother's younger sister who lived in New York for many years. So I had a great introduction to New York City, and the general area, for a few days. Of course, that's a great change from having spent a summer in the Cascade Mountains of Washington. Then I took the train, which as you already know takes about four hours, or something, to go from New York to Boston. Then I took the MTA over to Central Square in Cambridge and found my way to the MIT Graduate House, where I lived for the next three years as a graduate student working on my doctor's degree. My room there was already assigned. That was my first introduction to Cambridge.

Swent: It must have been pretty exciting.

Fuerstenau: Oh yes. The buildings and locale of MIT were totally different from South Dakota and Montana. A real change for me. MIT in those days was basically one big building facing the Charles River and looking at the Boston skyline. There was a dome at the Massachusetts Avenue entrance and a huge dome in the center, with several hallways that were sort of arms sticking out in a crab-like fashion. The Metallurgy Department, I still recall, was Building 8 down the far end after a long walk down the main hall. So my first morning there involved walking down that long hall to find the offices of the faculty.

Of course, I first met Professor Gaudin—Antoine M. Gaudin or always A.M. Gaudin—and Assistant Professor H. Rush Spedden, who remained there for a couple of years, and then Assistant Professor Phil deBruyn. Spedden was an assistant professor who served as executive officer of the large project that supported the research program in mineral engineering. Since deBruyn was still working on his thesis, he was both a faculty member and a grad student at the same time. Professor Gaudin was by far the foremost flotation expert in the world and it was with him that I came to MIT to study. When I arrived at

MIT, Gaudin was just fifty years old and already had contributed immensely to mineral processing research for a quarter of a century. I also met R. Schuhmann, Jr., associate professor of process metallurgy, who shared a secretary with Professor Gaudin. And of course, this was just about the time the term started, so we discussed a bit about what kind of courses one should take and so on, and met the various other students in the group, which may have been at least ten or a dozen graduate students.

Swent: Did you know any of the other students?

Fuerstenau: Not before. There was a variety of them.

[Tape 6, Side B]

Swent: You were just beginning to say that there were several Americans—

Fuerstenau: Oh yes. You asked the question about the students.

Swent: You had not known any of the other students before.

Fuerstenau: None at all, and they came a wide range of backgrounds, from all over. Names that I recall in our group were Gordon Bell, Dick Cole, Dick Charles, Bill Charles, Peter Laxen, Roshan Bhappu, Dwight Harris, Olav Mellgren, John Morrow, Jean Tournesac, Mel Chang and John Carr. There were two or three postdoctoral researchers also. Maybe I'll tell you more about some of them as we go along. In the Grad House, where I lived, my roommates were all fellow students for the very first term, and really most of the first year. The suites were such that there were two rooms with a bathroom in between, with two single beds, and desks, in each room. It was quite a nice set-up. Huge old building that must have dated back several decades. Right on the corner of Mass Ave. and Memorial Drive on the Charles River.

One roommate, Bill Charles, was from Pennsylvania, and his particular roommate was Peter Laxen from South Africa. Peter Laxen chose to return to South Africa directly after getting a master's degree and became one of the real giants of the carbon-in-pulp process. The first carbon-in-pulp process was installed at Homestake at Lead. Laxen's later career was as a researcher for what now is called Mintek, in South Africa, during which time he got a PhD from Witwatersrand University. I believe that he spent his whole career with Mintek and he really became Mr. Carbon-in-Pulp.

By the way, this Peter Laxen liked to play rugby, and there was another South African who was doing a doctorate in mechanical engineering. His name was Austin Whillier. I remember meeting him and going out to dinner a lot. He also returned to South Africa and won all sorts of awards and gold medals for his major contributions to cooling deep mines. If you pump cold air 10,000 feet down into a mine, the heat of compression is such that the air will become

just as hot as the rock, so it can't cool. So Austin Whillier came up with the idea of pumping crushed ice-water slurries down to cool the mines. I read about this and didn't realize that this was the person that I used to know when we were graduate students, until I saw a picture of him once. He was easy to identify because his ears stuck straight out. After he received his doctor's degree, he returned to South Africa and worked for the Chamber of Mines, where Neville Cook had been research director before he came to Berkeley.

My Grad House roommate was, for part of the first term, a French Canadian from Laval University in Quebec, Guy Powell. There was a Frenchman in our group, who came when I did, named Jean Tournesac. One time Tournesac told me that when he would speak in French to Guy Powell, Powell would answer him in English. Now this, remember, was 1950. Tournesac said then that the French Canadians were ashamed of their French in Quebec, so Guy wouldn't speak French back to him. That's obviously not the way it is in Quebec any more. But I do find that very interesting.

There were two people just finishing up. One, named Mel Chang, was just writing his doctor's thesis just as I was arriving, and for a few months I had a certain amount of interaction with him. He's kind of a live wire. For a few years, he worked at the University of Minnesota Mines Experiment Station on iron ore flotation problems. At one time, I know, he lived out here in Orinda and moved to Charlotte, North Carolina. We haven't crossed paths for years.

But the other person leaving had been there three years and they were flunking him out. And I'll tell you, when you just arrive and you learn that they had told someone who had been there three years that he will be given a master's degree and not allowed to continue for a doctor's degree, you do get quite concerned. I still recall Professor Gaudin later expressing distress that the head of the Metallurgy Department at Missouri School of Mines, whom he knew well since he had been a student of Gaudin's years earlier, had written an excellent recommendation about this candidate without pointing out obvious shortcomings. I sort of always have kept that in mind, namely that you don't whitewash, or say things that the person can't live up to. That can come back to haunt you. Today, most recommendations seem to be asked for on the telephone, perhaps so you would not be sued by negative comments in writing. [chuckles]

But anyway, that makes a new student a little nervous, when all of a sudden a fellow student had taken his main orals, and probably just flunked them cold, and was told that he could not continue. That's part of the trauma of doctoral programs everywhere, I think.

Swent: That's what makes the PhD valuable.

Fuerstenau: True. At least I think so anyway. In the middle of the semester, Guy Powell decided to live somewhere other than in the Grad House, so another grad

student who was part of our own group moved in, and he was my roommate for about the next half of this first semester. His name was Olav Mellgren, an excellent person who really did a lot of fine work later on in his career. For many years, later on, Mellgren was a senior lecturer at Imperial College in London and did some beautiful work on the thermochemistry of sulfide mineral flotation. But at the time Olav moved in as a roommate, I thought he was English, that he was an Englishman. He spoke English like an Englishman. And one day, quite a while after I had gotten there, he told me he was Norwegian. And every time I have seen him, which has been many times in the fifty years since, he reminds me of my comment. I said to the effect that, “You’re Norwegian? You know where I come from, we call a Norwegian nothing but a converted Swede.” [laughter] Eastern South Dakota, right? Of course my mother’s half Norwegian. Over that, Olav always laughs and repeats my comment to him. Olav turned out to be an extremely innovative researcher in flotation processing over the course of his career.

Some Comments on the MIT Doctoral Program

Fuerstenau: The doctoral program at MIT in those days, and it still may be that way, had no course requirements for a major. You had to have one minor, which in their system of counting, was twenty-four units. I took my minor in chemistry. Even though there were no requirements for the major, I either took, or audited, quite a few of the MIT metallurgy courses. The persons who had undergraduate degrees from MIT did have an advantage over those that didn’t, because they had already, let’s say, taken the courses on which written doctoral exams were based. So I took some of the courses for credit, and audited others, all during the academic part of my time there. The other requirements were written and oral examinations in your field, plus a thesis of course.

Swent: Was it just called “metallurgy”?

Fuerstenau: The department was metallurgy. MIT does everything by course numbers, and that was course three. I think civil engineering is Course I, but I know metallurgy is Course III, and Course V is chemistry. It may have been in the historical sequence in which departments were established. At that time the Metallurgy Department consisted of four main areas: namely physical metallurgy, process metallurgy, mineral engineering, and ceramics. Part of the doctoral requirements involved passing comprehensive written examinations in two of these fields. Today, the department is named the Department of Materials Science and Engineering, which now is quite a universal name.

A lot of the research in our group involved using radioactive tracers, so one course that everybody took first term was radiochemistry and it was a course all full of physics and chemistry majors. I remember the professor was a very famous person named Charles Coryell who gave open-book midterm exams. I

still remember one physics student having about eight, ten books on each side of him. He was so busy going through the books that it was almost laughable to be able to see him sitting there between two stacks of books. And this particular exam, the only reason I am commenting on it, is that one question asked to plot some kind of Z over A ratio versus something, and I hadn't the faintest idea so I just drew a line across the paper. Lo and behold, I got it right! And believe it or not, with all those physicists and chemists there, I got the highest grade on that exam. This was totally out of my field, but I studied and did all right.

The second course that first semester was metallurgical thermodynamics that was taught by Professor Michael Bever, and it was my introduction to Bostonian English. To this day, I can still hear Bever saying, "Delter H equals delter E plus P delter V."

But I will say this: we talked a little bit earlier about apprehension about coming from a small-town, small-school environment. I quickly found that I could do just as well as any other student, no matter where they came from. Maybe somebody who had his BS from MIT may have had the advantages, as I said, of already having had a lot of the course work. But my background was on a total par with anybody else's. I quickly found that out and never again had that as an apprehension. But being a professor, I have seen this attitude through the years of people coming from small schools being happier in their nest than getting out of it.

Life as a Graduate Student in the Boston Area

Swent: There's also, and I'm sure we'll mention this, the possible distraction of all those other things available around Boston, that you took advantage of.

Fuerstenau: My wife still makes comments about my grad student eating habits. For the first year there, my roommates and I ate every single night at a restaurant called the Honey Bee Café. Every night we would walk up to Central Square and eat there, every single night. Probably had either a pork chop or Salisbury steak. You know the Salisbury steak of New England, the hamburger mixed with egg and bread crumbs and a range of unknown spices and whatnot? And then the second year every night we went to a restaurant called Simone's and had spaghetti. I can't believe that one would get in such a rut and never change it. Spaghetti with tomato sauce, spaghetti with meat sauce. Friday they had fried clams, and I'll tell you, I sure wish I could have those fried clams today. They were marvelous. By the way, these cost only seventy-five or eighty cents for meals in those days. Another activity was every Saturday having lunch at Durgin Park. You surely must have been there?

Swent: I remember Durgin Park.

- Fuerstenau: And in those days, you got a big meal for ninety-five cents. That included dessert, and chowder, and like two big pieces of pork loin. That was what I usually had.
- Swent: So there was no food service at the Grad House?
- Fuerstenau: Oh, there was. I had breakfast there. Well, breakfast was very light. You could get lunch and dinner there, but I guess we must have quickly decided it was better to go out. We ate out every single dinner. I doubt if I ever had more than one dinner at the Grad House. Probably these roommates had been there already for a year, maybe over a year and that's probably how going out every night to eat got started.
- Swent: That wasn't included in the cost then?
- Fuerstenau: No, it was not. Undergraduate dormitory costs did include meals though, as was the case over at Harvard and Radcliffe.
- Swent: What did you do about laundry at all these places?
- Fuerstenau: Believe it or not, for the first couple of times, I would actually use one of these laundry cases and send it home. I quit that. But that used to be a standard kind of college thing.
- Swent: Sort of a brown, cardboard thing.
- Fuerstenau: Fifty years ago. But then, I decided that that was not the thing to do. I took care of that at a Chinese laundry down at Central Square. I remember shirts you could get done for fifteen cents. So I took all my laundry there. And now that you asked about it, I remember there was a fiber made by Union Carbide called Dynel. I think they still use it in blankets, but I had socks, maybe a half a dozen pairs, made of this fiber. I took them to the laundry, and when I got my laundry back these socks were like little doll socks. Obviously the heat in the dryer caused some kind of chemical reaction in the fiber.
- Swent: Let me just revert for a second. In Butte, and places like that, when you were off working in the summer, what did you do about laundry? Were there washwomen?
- Fuerstenau: Back in Chicago we could probably use the washing facilities at Mrs. Kroeker's house. Surely, at both of these summer places, either in Idaho or out at Lake Chelan, there must have been laundry facilities since we lived in rather large dormitory-like buildings.
- Swent: I'm trying to think when laundromats, and that kind of thing, came in. Were you doing your own washing or sending it out to somebody else?

- Fuerstenau: I took it to a laundry in Cambridge.
- Swent: You weren't running machines yourself, at that point?
- Fuerstenau: I don't think laundromats, like we know today, existed. That just draws a blank. I bet in Butte I was using the standard college send-it-home approach, using the expandable brown laundry box.
- Swent: With web straps. And a little window for the address card. You turned it over, slipped it in, and sent it home to mama. She would send it back with cookies.
- Fuerstenau: You hit it right on the head. I did that maybe once or twice, and then I quit that.
- Swent: Isn't that funny? Nobody in the world would do that now.
- Fuerstenau: But the laundromats didn't exist then.
- Swent: And boys didn't do laundry. As a young man it never occurred to you to run a washing machine, I'm sure.
- Fuerstenau: Moving on, a good friend all my years at MIT was Jack Jordan from Montana, the person who had fallen down a raise in the Bunker Hill mine that I told you about. He came to MIT as a junior, in chemistry. Actually, I took one or two courses with him later on.
- Swent: He was an undergraduate?
- Fuerstenau: He was a junior, undergraduate, yes. He did his first two years at Butte and then transferred to MIT as a junior, majoring in chemistry. We did a lot of things together, which I will get to shortly. He lived in an undergraduate dormitory, so he was never part of our regular evening meals. Later on, when he became a graduate student, he was part of the Saturday lunch at Durgin Park.
- Jack went with me to Philadelphia to visit my aunt for Christmas. Our transportation was provided by the loan of a car. This car belonged to another new graduate student who later who became very active in the AIME named Roshan Bhappu. He came to MIT in the fall of 1950 as a graduate student. Originally from Pakistan, he had studied at the Colorado School of Mines. He was going out to Colorado for Christmas to see his girlfriend. He was lonely for her. Carmen, you may have met Carmen.
- Swent: I remember Carmen.
- Fuerstenau: Lovely woman, who he later married. So he told me that I could use his car, and Jack Jordan and I drove down for Christmas to my aunt's, near

Philadelphia, in Bhappu's car. One thing I remember was visiting Longwood Gardens, which is a lovely garden on a DuPont estate directly west of Philadelphia. Anyway, on the way back, we had the car full of stuff. Maybe my aunt gave me a bunch of things. I remember some were paintings that she had painted. So the back seat of this car was full of stuff. We stopped to eat dinner on one of those two-lane highways, somewhere in Massachusetts at about 8:00 at night, and it was dark. Afterwards, we were only a short distance down the road when the cops pulled us over. They wanted to see my driver's license. I was from South Dakota and South Dakota at that time had no driver's license. The highway patrolman couldn't believe that there was a state that didn't require a driver's license. We weren't speeding, or anything, but he just wanted to check on this car loaded with stuff. Being a South Dakotan and not having a driver's license almost became a problem on the spot.

Swent: You couldn't rent a car.

Fuerstenau: Right. I got a license, needless to say, very soon thereafter. But it was a little traumatic.

Swent: Good thing you had an honest face.

Fuerstenau: I guess you're right. Of course Jack Jordan had a Montana license. Maybe the policeman made him drive the rest of the way back to Boston. We weren't too far out of Boston.

By the way, Roshan decided after one semester to go back to Colorado. He eventually did get a doctor's degree from Colorado School of Mines and got married to Carmen, who, as I said, was a very lovely lady. Roshan, as you know, became the president of SME [Society for Mining and Exploration] and also president of AIME, and I think Carmen was very active in the WAAIME, wasn't she?

Swent: Yes.

Fuerstenau: Extremely active, I think. Unfortunately, she got cancer and died, some years ago.

Then a new roommate moved in. He was my roommate for all the rest of my years there, and played a major role in my life, which we will get to eventually. This was Bud Samuel, Edmund Samuel, who was working on a PhD in biophysics. He had an undergraduate degree from Case Institute of Technology in electrical engineering and a master's degree in physics from there. And he had a friend who also had come from Case named Jim Tewkesbury, with whom I had a lot of interaction later on through the years, because Tewkesbury, after finishing his PhD worked out here for Chevron and we saw him many, many times after we moved here to Berkeley. But anyway, Bud Samuel and I lived together there for the next two and a half years, and

he and Jim Tewkesbury and I had the evening meals that never seemed to vary from then on.

But you were talking about activities. On Memorial Day in 1951, Jack Jordan said, "Let's go to the ball game." And at that time the Braves were still in Boston, and the Red Sox right near by. That day the New York Giants were playing the Boston Braves. It was a double header that went from about 1:00 in the afternoon to 9:00 in the evening. It was even drizzling towards the end. And you know, I got hooked on baseball. You would think one wouldn't with that beginning. I used to go to fifteen to twenty games a year after that. It cost sixty cents to sit in the bleachers, and you could walk in a half hour to Braves field, and maybe twenty minutes, or twenty-five minutes, to Fenway Park where the Red Sox played. So many an evening or afternoon, for sixty cents one could go sit in the bleachers to see a ball game. As I said, for years I got really hooked on baseball, but I haven't been to a baseball game in years. I knew all the statistics. We're talking the days of a lot of great players. The Red Sox had the great Ted Williams and Dom DiMaggio.

Swent: Vince DiMaggio.

Fuerstenau: Vince was the oldest and already retired, and he had played for the Braves, apparently, in the '30s. Dom DiMaggio played for the Red Sox and Joe DiMaggio for the Yankees. I really got more interested in National League games. Oh, I went to many of them. When I went to visit my aunt, in New York, I remember going twice to Ebbetts Field to see the Dodgers, and once to the Polo Grounds where the Giants played, and once to Yankee Stadium. So you can see, I really had become a baseball fan. There—

[Tape 7, Side A]

Fuerstenau: In one exciting game Casey Stengel benched Mickey Mantle.

Swent: Casey Stengel benched Mickey Mantle?

Fuerstenau: He benched Mickey Mantle. Again this was a Memorial Day double header, but this time at Fenway Park. It was Joe DiMaggio's last year and Mantle's first year. He was being groomed as DiMaggio's replacement. In the first game, Mantle struck out five times in a row and at that point Stengel benched him. I remember another time seeing Connie Mack in about his fiftieth year of managing the Philadelphia Athletics. He wore a business suit, and not a typical uniform. Going to so many games, one had a chance to see such great players of the 1940s and 50s as Jackie Robinson, Duke Snider, Pee Wee Reese, Stan Musial, Satchel Paige, Bob Feller, Warren Spahn, Willie Mays, Henry Aaron, Luke Appling, and many other great ball players of those years.

My roommate, Bud Samuel, had a car. I had no car. But with his car we went often to the beach, and so on, in the summer and out to see the leaves in the fall.

Swent: What about the music?

Fuerstenau: I recall that in the spring we went to the Boston Pops, several times. You could get a ticket that didn't cost very much to sit in the balcony when the Pops played in Symphony Hall, perhaps for a dollar or less up in the balcony. It cost money down on the main floor, where champagne was served.

Swent: Then there were the Esplanade concerts.

Fuerstenau: I was about to say: late in the summer the Pops also played in the Boston Common in an outdoor concert stand, called the Esplanade, as you know. We went out there quite a few times. In those summers Bud Samuel, Jim Tewkesbury, and sometimes Jack Jordan, and I spent a Sunday afternoon or early evening there.

There was another friend of mine from South Dakota, Bob Zimmerman, who graduated a year before I did. He was working for a PhD in physics, which he finished at age twenty-two, there at MIT. He used to sail a lot. He came from Philip [South Dakota], a farmer, who became an avid sailor. Remember they used to have the little dinghies in the Charles River? I sailed a lot with him. But I went as passenger. I didn't really have an interest in taking the lessons to learn how to sail, and pass the test, and so on. Zimmerman, as I said, was in physics working a high-energy physics research problem for Professor David Frisch using a Van de Graaff generator, as I recall. I saw him now and then as a friend. He lived with another physics student in an apartment near downtown Cambridge. In the summer of 1952 that roommate went somewhere to work that summer so I moved in with Bob for the summer.

Swent: I was going to ask, did you stay there for the summer?

Fuerstenau: Oh yes. You see, I was a research assistant, as would be all the grad students in our group. At that time I was paid as a half-time research assistant—and in fact I got a little upset at Spedden who was in charge of everything—because he kept my pay half-time in the summer. My stipend was \$90 per month, out of which I had to pay \$540 per year tuition. And now you subtract that and there wasn't much left. I learned that the grad students who were married were paid as full-time research assistants in the summer. And I remember going to Spedden and saying, "Look, I really can't live on this. X, Y, and Z are full-time." "Well, they're married." "Yes, they have wives that are working," which they all did. I don't recall Spedden increasing my status to full-time pay. By the way, ever since I have been here, students have complained about what research assistants get and what their costs are. And all I got was \$1,080 per year and paid \$540 tuition out of that.

Swent: Only \$540 a year.

Fuerstenau: Divide that by twelve! To make ends meet, I think I got money from home. I must have.

Swent: Or you maybe saved a little in the summer.

Fuerstenau: Maybe I had. I was able to stay there as a grad student working on research all summer without going elsewhere to earn extra money. I was then working full-time and getting paid half-time in the summer.

Selection of a Doctoral Research Topic

Fuerstenau: First of all, I only learned about the research that various other graduate students were doing by talking with them individually. During my days in the mineral engineering group at MIT, there never was a weekly seminar where grad students presented their research results. Maurie told me that during his years there, they had a spring seminar series where students presented their research results. At Berkeley, I always had a weekly seminar where students presented their research results along with thorough comments. I considered that to be very important.

Anyway, in the beginning I talked with Professor Gaudin about a research topic. Based on some of the findings of Mel Chang that couldn't be explained, Gaudin was very interested in measuring electrical properties of mineral surfaces under flotation conditions. So this sounded like a good topic for a thesis, and I took it on. If you put any mineral particle in water, it develops a charge. Everything is charged. Even air bubbles carry charges and set up potentials at the surface. You can measure these electrical potentials by moving the liquid past the solid particles. This field is called electrokinetics. You can stick two electrodes in the ground, and apply a current, and the water will move. Industrially clay has been dried this way. That technique is called electroosmosis. Well, I did the reverse, where water is forced to flow through a bed of mineral particles and this strips off ions at the mineral surface and sets up a potential that can be measured. This is called a streaming potential. From electrokinetic theory, one can calculate potentials at the surface called zeta potentials. If you measure changes in zeta potentials as you add different flotation reagents, you can follow what happens at the surface. So this is what I did for my thesis.

Swent: This is physics, isn't it?

Fuerstenau: Yes, but applied to understanding mineral surface chemistry. My interest was applying electrokinetics, not the phenomenon itself, but what you learn from how flotation reagents affect the potentials at a surface. Anyway yes, the measurement methods were physics. So I built the apparatus, designed it,

acquired everything. I did all this on my own. Sometimes, many of the students I've had here had to be spoon-fed. But I really had no input from Professor Gaudin on any of the details after he first suggested the topic. I just went ahead and did it on my own.

Swent: You were building on what Mel Chang had done? What was the relation with what Mel Chang had done?

Fuerstenau: He had studied the surface chemistry of quartz flotation using a soap as the collector. Flotation collectors are generally organic ions that have a hydrocarbon chain such that when they adsorb on a mineral, that mineral acquires a hydrophobic surface. In our discussions, I have mentioned soap ions are negatively charged anions and quartz is a negatively charged mineral. Because of this, soap ions do not adsorb on clean quartz. To make the soap ions adsorb onto the surface of the quartz, generally multivalent cations are added to the system. Industrially calcium ions are used although Chang used barium ions. These multivalent positively charged ions adsorb on the negatively charged quartz surface and act as a link for adsorbing the soap ions. But some of Mel Chang's results on the adsorption soap ions and activator ions could not be explained. The soap was adsorbing when it shouldn't. All of Chang's measurements were carried out with radioactive tracers to determine directly the amount adsorbed. So Gaudin wanted to know what might be happening by studying how these various species might be affecting electrical potentials at the surface.

First, I went to the library and read papers about electrokinetics and the surface potentials that you can calculate from such measurements, which were widely used in colloid science and some in biology. So I decided to undertake this study and proceeded to get whatever equipment I needed, and get the apparatus built that I would need to measure streaming potentials, and so on. I think Gaudin sort of thought, "Why don't you study all sorts of minerals?" But I finally settled on working only with quartz although I did carry out some measurements with a few other minerals. But the questions were what is the effect of reagents on the zeta potential? How does this relate to the flotation of quartz? I must say that I made some of the biggest headway the first full summer I was there, even though a lot of times in the evening we would go to ball games. When you're taking classes you don't have very many hours to work in a laboratory without interruption.

But I needed to work in an air-conditioned room in order to remove temperature as a variable. So I had one of the few air-conditioned labs around the Institute. That also gave me an incentive to work at night. I put in long hours because it was nice and cool; I didn't have to put up with that high humidity of Boston.

[break]

Foreign Language Requirements for the Doctoral Degree

Swent: I wanted to ask about the importance of knowing German for a PhD in science.

Fuerstenau: Well, early on, much of the research was published in German. In my day, you had to pass a written exam in two foreign languages in your field. In fact, when I came here to Berkeley, that was the same requirement, but now it has been dropped. In engineering they said computer language is a more useful language, and that became the engineering outlook. But I think chemical engineering, chemistry, may still require German. And I think that, for a while, even civil engineering here still required German. But foreign language requirements began to be looked at as an impediment and not as a necessity, so nationwide language exams in engineering have been dropped.

I had to pass an exam in French and German. And because I had started out majoring in chemistry at South Dakota School of Mines, I took one year of German there. Actually, I had a good teacher and I learned a lot. It was more reading, I think, than speaking. When I got to MIT they had special language courses directed at passing the doctoral exam requirements. I think that these courses included students from all technical fields, and the reading would be general technical papers that sort of covered general technical interests. So anyway, I studied German quite hard. The actual exam consisted of three or so paragraphs taken out of journals in the field, and in our case these were from metallurgical journals, such as *Stahl und Eisen*.

The first time I took the German exam, it was scheduled to be given at 2:00 on Saturday for all fields of study. But the Friday evening before, my friend Jack Jordan and I went out and had a few beers. In fact, quite a few beers. I stupidly forgot that the next day was this exam. When I got there at 2:00 and started the exam, I couldn't think very well. It was a disaster. And I'll tell you, when I went back to the class on Monday, I could read those exam paragraphs just like nothing flat. I never made that mistake again. I took the exam when it was given again in the spring. And I passed it with no trouble.

Now French. I thought they did an interesting thing with teaching French at MIT. I learned to read French, and I can still even read a little bit. I never learned to pronounce anything, although the French teacher was pronouncing words. But we were given a list of sixty-six French words that occur 44 percent of the time in French prose, common words, in their various forms. I used to have that list; but threw it away a while ago. So if you learn those words, you're half there. The technical French words are basically our words, whereas the Germans have their own words. Nitrogen is nitrogen in French, however you would say it. But in German it is *Stickstoff* for nitrogen and *Sauerstoff* for oxygen. And so on. So with all the technical words being the same in French as English, in six weeks I learned enough French to pass the doctor's exam. I suppose in six weeks I probably forgot it.

- Swent: That was going to be my next question. How much did this really come into play later? Was it useful to know the French? Of course, the German you followed up on.
- Fuerstenau: I would say, technically, probably not. Although now I can read German and speak German, and so on, reasonably well. I can pick up a German article and read it. French, I can make out what it's saying.
- Swent: Is it of technical importance to know these things?
- Fuerstenau: The answer to that is probably not really. I have made use of it reading some German theses, for example, for what I want to get out of them. And also German journals for papers on comminution, grinding. But basically, so much now is written in English. I know a very eminent Frenchman who says, "If we really want something to be known by the outside we write it in English."
- When I worked at Union Carbide, there was a Russian researcher there, and you know, everyone went to him with Russian papers to translate. What an expensive thing to do, right? This guy was spending half his time translating something for other people. I call that expensive since he was a PhD researcher. So the answer is, I think that it's not vital. I know that a few years ago, there were people who earned their living translating articles, and for my research projects a couple of times we paid to have important articles translated. My continuing to work on German is just my own interest, more than necessity. But foreign languages as a doctoral degree requirement was probably just a hurdle. And once it's just considered a hurdle, you should drop it.
- Swent: A screening device.
- Fuerstenau: You made a comment about English. I have made my own rule, and I'm probably the only one in this department who made such a rule for his graduate students. I have always made all my grad students take a course in technical writing, whether they're American or not American. And I've been able to see with many of them a real improvement in their writing, including style and so on. I'm a real believer in this. There have been two writing or technical communication courses given in the Civil Engineering Department here in Berkeley.

Recollections about Professor A. M. Gaudin, Teacher and Thesis Supervisor

- Swent: The main person you worked with was Professor Gaudin. What do you remember about him?

Fuerstenau: My first interactions with him were as a student but later on, of course, I got to know him well. Antoine M. Gaudin was born in 1900 in Turkey, where his father managed a French railroad. (His name was pronounced in the French way, GoDann. Often at technical meetings, by those who do not know, his name is pronounced in many different ways.) His father was sent to the United States in 1917 on some diplomatic post, and Gaudin entered Columbia—Columbia School of Mines—at that time. I think in 1918, he served briefly in the American army. He graduated in 1921 with an engineer of mines degree. Gaudin told me that in the early twenties he was selling garage doors down in Philadelphia. He got a letter from Professor Taggart, who wanted him to come back to Columbia University to work with him on some problem related to surface tension in flotation. Anyway, that brought Gaudin back into the mineral technology field where he was a researcher at Columbia until 1926, during which time he himself conducted what is really a masterful research project on grinding, comminution. He wrote a very long, thorough, paper which I still quote and still use some things out of it at times. That paper was published in the *AIME Transactions* in 1926. That must have been the basis on which he was invited to go to the University of Utah, where he was from '26 to '29, as an associate research professor at the Experiment Station of the University of Utah and also the Bureau of Mines. There he did a huge amount of classic pioneering work on the chemistry of flotation. That group designed and built a miniature flotation cell where they could float 5-gram samples of very pure minerals. These were the first flotation studies carried out on individual pure minerals. They delineated much of, let's say, early flotation chemistry. This was classic work that laid a lot of the foundation of how flotation reagents react with minerals. Much of that work is still useful and quoted today. That was the basis, obviously, for Gaudin's being invited to come to Butte in 1929. An excellent writer, during his ten years in Butte, he published a lot of papers on sulfide mineral flotation and fine-particle flotation and wrote two classic textbooks: *Flotation* in 1932 and *Principles of Mineral Dressing* in 1939. One of his prized students at Butte was Plato Malozemoff and another was Reinhardt Schuhmann, Jr.

Years ago, somebody told me the story of Gaudin's going to MIT in 1939. At that time, the mineral dressing professor at MIT, named Charles Locke, was retiring. There was assembled an outside committee to meet and recommend a successor. Gaudin was invited to be a member of the committee. And whoever was telling me the story said that of course Gaudin was chomping at the bit that he be the new professor. But here he was a member of the committee to select the individual. Whoever it was who was telling me the story said that the committee excused Gaudin from the room and named him to be the professor. Gaudin became the Richards Professor of Mineral Engineering. Robert H. Richards was the earliest major mineral processing person in the United States. He was a member of the first graduating class of MIT in mining and metallurgy and remained at MIT as professor of ore dressing his entire career. He had enrolled as a freshman at MIT when it was started in 1861 and

died in 1946 at age 100. The AIME Robert H. Richards Award was named after him.

Swent: Which you have won. That's the top award.

Fuerstenau: That's the top AIME award in mineral processing. After visiting every processing mill in the United States, Richards wrote a four-volume treatise entitled, *Ore Dressing*. I have a set of them, which I found in a bookstore in Palo Alto. Richards's successor at MIT was Charles Locke. When Locke retired, that's when Gaudin went to MIT. After Gaudin left the University of Utah, his published papers seldom involved direct flotation studies. He became interested in the nature of the adsorbed reagent films, and that constituted most of his later research. Schuhmann's MS thesis at Butte, published in 1935, is still classic.

Professor Gaudin's relationship with students was always very formal. His formality resulted from his French European background, I suppose. One needed to make an appointment with his secretary to see him in his office. I remember that every morning until about 10:00 he dictated correspondence to her—definitely more organized than I. Since he seldom taught courses, he had time to take care of administrative and consulting matters. For a person whose whole career was in academia, he did not seem to have an interest in lecture courses. He never advised any of the students on their academic program—I think that course programs were developed through conversations with the students who had begun their program a couple of years earlier. I don't recall his ever asking anything about how things in general were going. If you were writing a paper for publication with him, however, he went over everything in the minutest detail—in contrast to many faculty these days. Yet Professor Gaudin had a powerful influence on all of his students. Observing his thought process was a real part of the education process. I think that he was viewed with real respect by nearly all of the graduate students and provided real motivation to all of his students throughout his career.

Shortly after I arrived, he invited me to his home to dinner. Afterwards, he proposed that we play bridge, he and his wife and his daughter. Since I had only learned the rudiments of bridge that summer working for Howe Sound, you can imagine that that was quite some game—my bidding was unbelievable but I could play the hands. Generally, the Gaudins had all of the students as a group out once a year on a Sunday afternoon.

I never saw Gaudin lose his temper at MIT, although later I traveled with him on a couple of occasions and he would not tolerate shortcomings from ticket agents, hotel clerks, et cetera. Some years ago, someone who had been a student at Montana School of Mines in the late 1930s told me that Gaudin often had a short temper. That person said he was in Gaudin's office one time when Gaudin got so mad that he threw an ink bottle—I don't recall if it was at the person or not, but I hope that the cover was on the bottle.

Comments on Gaudin's and His Research Programs

Fuerstenau:

I would now like to tell you a something about a couple of other fellow grad students, and a little bit of what their influence might have been on me. After a class that Phil deBruyn was teaching and which those of us who were new students were auditing, several of us were standing around having coffee in the group. One of the students who spent one year in the group to get a master's, again older, because he was a returned GI, was Dick Cole, Richard E. Cole. I'll just tell you a couple of things about him. He came from South Dakota and he said he had six brothers, and his father named all seven sons R.E. Cole. The father wanted each son to be an R.E. Cole, and Dick was Richard E. Cole. He got a master's in that one year and rose to be production vice president and then senior vice president of Reynolds Metals, in the aluminum industry. When he got out of the army, since his wife was from Missouri, he did not return to South Dakota but went to Missouri School of Mines where he got his BS degree in metallurgical engineering. He was kind of forthright, and so on.

I remember him getting into an argument that day with Phil deBruyn, who as I said was assistant professor then. Dick Cole said, "I think Gaudin, as a researcher, pot shots," and deBruyn was arguing, "This isn't the case." I think, in retrospect, Dick Cole was more correct than deBruyn, because Gaudin would have somebody work on this topic for a thesis, and then the next person who joined the group would work on that topic. Gaudin had a fertile mind that was always generating ideas, and he would probably propose to the next new student that he work on some aspect of his latest idea. Gaudin published many excellent papers on flotation, comminution, and other topics through the years, but generally one paper did not follow on previous work. That had a big influence on me, because I decided after coming here that if I chose to work on any given topic, I would do so in depth. So at Berkeley over these many years, I worked in many areas, where I may have had four or five students consecutively conduct continuing research—I worked on agglomeration, pelletizing, flotation, applied surface chemistry, for example. I had several people do theses on pelletizing for a decade or more. I got interested in mixing solid particles; and several different grad students and postdocs worked on that for several years. Through all my years at Berkeley, I worked progressively on different areas of grinding. Stay on the same thing, and keep at it, because that way you keep moving ahead, instead of just looking at some small aspect of the field and then go on to another topic. It was that discussion that led me to my own personal philosophy. Years ago I had many people work on extracting metals from these deep sea manganese nodules; I stayed with that topic for a decade until we more or less wrapped up the topic. Looking back on it, Dick Cole, I think, had assessed Gaudin right. The memory of that coffee break discussion has remained with me throughout my entire career. It's influenced my approach to research ever since I got here to Berkeley.

[Tape 7, Side B]

Swent: Let's continue then. Were you through with Gaudin?

Fuerstenau: We will come back to Gaudin again as my career developed.

Analysis of Graduate Student Performance

Fuerstenau: Now I would like to comment about another fellow student, whose thoughts influenced my approach to assessing people. All during that period of my graduate student research, I shared a small office, a small lab that was probably no bigger than this office, with somebody else. We each had our own apparatus set up for running our experiments. I had my flotation surface chemistry apparatus in one end of the room, and he set his impact apparatus to study the breakage of glass at the other end of the room. My labmate was Dick Charles, Richard J. Charles, who was about three years older than I. He came from the University of British Columbia where he had gotten his bachelor's and master's degrees in mining engineering. I remember him telling me that he had worked for two, three of the gold mines in northern territories of British Columbia, et cetera. [phone interruption] So anyway, Dick Charles and I used to talk a lot, or at least at times since we had desks that faced each other. By the way, he was the only student who was not working on a flotation problem at the time.

One time I had a very interesting conversation with him about what makes an able person, about who were the most able people around us. Now, we were talking about graduate students. If A equals 4, at MIT you then could get a master's degree with a C average, which would be a 2.0. You could get a doctor's degree with a grade point average that was 2.5. You know, I think at MIT you could get a BS degree with either a 1.0 or 1.5. You could have all D's or half D's and still graduate. Of course, they had the cream of the crop in high quality students, but they just spread out the grades. Whereas here, in the Engineering College at Berkeley, before I came, they required, and still do, a 3.5 GPA.

Swent: For a bachelor's?

Fuerstenau: No, sorry, for a doctor's degree, a PhD. Early on, I used to argue against it here, because our students are very good. But all we're doing is compressing the evaluation—if you give a person a B it's nearly like giving a grad student a barely passing grade. I mean, a B-minus is a low grade as a result of this GPA requirement. I used to argue that this GPA requirement was too high because it just hurt the grading since you're just going to give virtually all A's to graduate students. Whereas at MIT you could spread grades out.

Well, anyway, Dick Charles said, in his opinion, that the best students, the most capable people in the department, seem to have a GPA of about 3.4. There was one person in the mineral engineering group who flunked his oral exam badly, but went to Gaudin's home in the evening and convinced Gaudin to let him have another chance. He told me that Gaudin asked him to play a game of chess that evening, and some years later Gaudin told me that he liked to play a game of chess to observe how that person thinks. This student did pass the oral exam on the next try. He could obviously reproduce well on course exams, but he couldn't put his background together to solve new problems. And I never knew he had this straight-A average until many years later when Frank Aplan mentioned that. So with a 4.0 GPA, he was above the 3.4 maximum that Dick Charles put those who could best apply their knowledge and use their ingenuity, drive, creativity, and so on. This student failed in his first industrial research job with a metals company but did have a long career with 3M, doing what I never did know.

By the way, I had about a 3.4, 3.5 maybe. Since I only needed a 2.5, I studied hard enough to get an A or B—if it was a difficult course I studied hard; if it was an easy course I didn't. I got half A's, half B's, and that's all I aimed at.

One other person, also a Canadian, was about thirty years old, thirty-one, when he came back to MIT for a doctor's degree and he had all A's. He and I were taking a course in chemical kinetics and we each got a B. I was pleased, but it was the only B that he got and he almost was in tears over it. He finished his thesis at the same time I did, and they almost failed him on his thesis defense because he had accomplished so little.

Swent: He got the A's.

Fuerstenau: Getting straight A's was his objective. But Charles's analysis, I thought, seemed indeed to be a very good index for assessing the potential of a person to be a good engineer. As to good, I mean outstanding.

Swent: An achiever.

Fuerstenau: Achiever, yes, that's a better word.

Defining the Field of Mineral Processing

[Interview 4: August 6, 2001 in Berkeley, CA, Evans Hall]

[Tape 8, Side A]

Swent: You've gotten to MIT and are starting to work with Gaudin. Before we get into more particulars today, let's have you discuss a little more generally the field of mineral processing, or metallurgy, at that time, and how you visualize it.

Fuerstenau: The field today is called mineral processing. Mineral processing plants are generally called mills or concentrators. Often the people who are involved with milling in mining companies are called metallurgists. I think your father was chief metallurgist of Homestake.

Swent: Before that they were even called assayer. Assaying covered the whole—

Fuerstenau: That was the most important thing, early on, that's true. Anyway, there is kind of a historical thing about the name of this general field. Richards, who was the earliest major person and actually was a member of the first class at MIT in 1861, called it ore dressing. Then it became mineral dressing. Somewhere in the late forties, I think, a kind of a foolish word got out there, called minerals "beneficiation." Sometimes you hear people not quite in the field call it mineral "benefication." I've even seen it spelled that way.

Swent: I have had lots of arguments with proofreaders and other editors in the office who come across this word "beneficiation," and they correct it, and say, "There is no such word." True, it isn't in most dictionaries. Not your ordinary little shelf dictionary that you have. It doesn't have "beneficiation."

Fuerstenau: In some AIME publication I read once that one of those persons behind this name said, "What do you do with the minerals in an ore? We beneficiate them." This name came into use in the late forties. By the way, I remember one time Gaudin said, "I just don't like that word. When I hear it, 'mineral beneficiation,' and often 'mineral benefication,' I think I have heard 'mineral defecation.'" [laughter] True story.

Anyway, at MIT Gaudin always called the whole field mineral engineering. He was called Richards Professor of Mineral Engineering. However, in my opinion, the whole field of mineral engineering is mining plus processing. But in AIME, for many years, these activities were in the Minerals Beneficiation Division, MBD. [looks at a plaque on the wall]

Swent: [Reading the plaque] "The Best Presentation Award for the Society of Mining Engineers."

Fuerstenau: Over the years, I was a big pusher behind changing the name of that division to Mineral Processing Division. Sometime in the late sixties it finally was changed.

Swent: It's easier to pronounce and spell.

Fuerstenau: True. I would just like to tell you how I visualize the field of mineral processing. It has two main steps. If you have an ore, you first have to crush and grind it fine enough to liberate the minerals from each other. This is determined by the nature of the ore itself.

Swent: Let me interrupt for just a moment. Crushing and grinding is always sort of one word, "crushingandgrinding." But do they ever only crush or only grind?

Fuerstenau: In the case of producing aggregate, they only crush the rock. Probably only crushing, of course, was used in early days of upgrading so-called wash iron ores, where you do not want to make fines. What is probably the correct technical term, and is now fairly widely used, is "comminution," which, if you look that up in a dictionary, says, "to reduce to minute particles." We now use the word comminution quite a bit. In chemical engineering, they often just use the expression 'size reduction,' which is self-descriptive.

What's interesting is Professor Hans Rumpf, in Germany, a really great man on particle technology, estimated 3 to 4 percent of the world's electrical energy goes for comminution. I was part of a study which the Bureau of Mines supported in 1978-79 where we did a complete survey of comminution technology, and 1.4 percent of all the electrical energy in the U.S. goes for comminution for ores, rocks, cement. That's really a big number. You can see the need for study in this general field.

After liberating the fine mineral particles, the object is to separate them. Since fine particles have a high amount of surface, separations are often based on surface control. The earliest separations were by gravity. You take a mineral like galena with a specific gravity of 7.5, and limestone with about 2.7, you can see it's easy to make gravity separations. In fact, ancient Greeks, in 500 BC, separated galena from limestone on big slab tables that you can still see. I haven't seen them.

Swent: That's Laurium?

Fuerstenau: Laurium. Yes, the mines of Laurium, where they washed the ground ore over sloping rock slabs with gently flowing water to separate this silver-containing galena from the waste rock. The water washed away the lighter limestone particles. The ore had been crushed by hand in stone mortars and ground with mill stones. The most well-known gravity separations, of course, were the placer mining of gold.

Separations can be made magnetically; something like magnetite you can separate with a magnet wet or dry from quartz. But the main separation method today is flotation. Something like two billion tons of ore per year, worldwide, are treated by flotation. An important aspect of the science and technology of flotation is finding ways to make one kind of mineral water-repellent or hydrophobic while leaving the others water-avid. If you have a slurry or pulp of ground ore in suspension in a tank and blow in air bubbles, the water-repellent minerals will attach to the bubbles, and rise to the surface forming a froth layer that can be skimmed off to make the separation. Separation with air bubbles is the first step in transforming a mined mineral to a useful material.

Swent: This is actually new, isn't it, in this century?

Fuerstenau: It started in Australia in 1905. The first froth flotation plant in the United States was built in 1911. I think it's called Timber Butte Mill in Montana. In fact, often one reads, from a raw materials point of view, that flotation is the most important discovery of the twentieth century. You can imagine what it would be like trying to recover metals without being able to readily separate the waste rock in which the desired minerals occur.

Swent: This doesn't go back to any earlier technique? It was completely new?

Fuerstenau: Essentially new. Although in Germany they claim the first flotation was in 1877, when two brothers, the Bessel brothers, separated graphite. Graphite is one of the few minerals that are hydrophobic naturally. Talc is another; you know how talc feels slippery. There are only three, four, five common minerals that are naturally hydrophobic. The Bessel brothers had a process where they ground graphite ore and then heated the pulp to near boiling, using dissolved air and steam as the bubbles. That really was true flotation but everything about their process was lost when they closed their mine in 1883. But the process of controlling the wettability of minerals by adding reagents and blowing in air came about shortly after the turn of the century. The Bessel brothers also patented adding small amounts of various oils to assist the graphite flotation. We talked a little bit about the use of bulk oil in the Elmore process. It's the same principles that allow making separations with air bubbles.

Swent: Still, even if you say 1870, that's still modern. Something really new on the technological side.

Fuerstenau: I believe it was about 1905, in Australia, where the first flotation plant as we know it today was put into operation on zinc tailings at Broken Hill.

Swent: Then they talk about some woman in Colorado who washed—?

Fuerstenau: Carrie Everson, I think, was her name. She would wash the miners' clothes and the sulfides would float to the top. That was, you might call, skin flotation. She devoted quite an effort towards developing her process, and took out several patents.

Swent: That was supposedly in the 1870s, or so.

Fuerstenau: Same principles of wettability. Another aspect of making separations, of course, is by leaching an ore, like in gold ore processing. There you grind the ore fine enough so that the gold particles are exposed to make them available to the leach solution. Again, that requires, for many ores, grinding the ore fairly fine. That's purely to expose the mineral, in this case gold or silver, such that it can make contact with the leach solution. In the case of precious metals, the leaching agent is cyanide. So again, processing involves first grinding and then separation, but the separation is done by chemical dissolution. There the problem is to separate the solution from the ground waste rock.

So a third major aspect of all this is carrying out solid-liquid separation, generally first by thickening or sedimentation in large tanks, and then by filtration. The first thickener, as we know it, was invented by J.V.N. Dorr, there in the Black Hills, in the early part of this century. The technical aspects of thickening also have a lot of surface interaction because it is controlled by the surface chemistry between the water and whatever minerals you're trying to flocculate, for example. While in the Black Hills, Dorr also invented the rake classifier used in conjunction with grinding mills to remove particles that have been ground fine enough and to return the coarse particles back to the ball mill. This led to closed-circuit grinding systems, which reduced overgrinding.

Finally, there's one other aspect: in some cases you want to reconstitute the fine particles. In the iron ore industry, the taconite industry, where you grind the material very fine to make the separations, maybe around fifty microns or so, you must agglomerate the fine magnetite because you don't want to blow the dust into a blast furnace. This operation is called pelletizing—a new operation, relatively speaking. Around 1950, the first pelletizing plant was constructed at Silver Bay on Lake Superior. This process was developed by Professor E. W. Davis at the University of Minnesota, and I like to cite this as an example of how devising new technology was able to make an ore out of a rock. We can come back to that later because it's an area that I've worked in extensively. All of these steps constitute the field of mineral processing.

Swent: It sounds much more simple than it really is.

Fuerstenau: Conducting these operations on a huge scale under optimal conditions with widely varying ore is challenging to industry. Research in the field is involved with, at least my approach to it, delineating the underlying principles of all of

these different operations so that you might improve the technology. Actually, this was the research approach that Professor Gaudin always took, which was to understand the basic science and engineering science that underlie the processes so that they could be extended.

Swent: It seems to me, and I'm not a scientist, that previously it was all mechanical, and then it gets to be chemical, and now you're getting more into physics actually, aren't you?

Fuerstenau: Right, perhaps. If you look in chemical engineering unit operations textbooks they always have a fairly large chapter called "mechanical separations." That's often the title of a chapter that will cover size reduction, and screening, and classifying, and filtration and even flotation. Chemical engineering texts will put all of this under one term: "mechanical." So in some sense, that's true. There is an awful lot of underlying physical chemistry in what really controls the ability to make complex separations.

Swent: That's what people weren't aware of. It didn't even exist one hundred years ago, the idea of the effects of electrical charge on minerals, did it?

Fuerstenau: Of course, most of the ores were much richer, and so on. With many ores simple separations could be made at coarse sizes. Even by hand-sorting. A lot of the ores that remain today are very complex, fine grained, so you have to know more about this because surface effects become dominant.

Swent: That is, again, a big change. They are able to process low-grade ores now.

Fuerstenau: That is the direction the world is going. Because you can see, as ores get lower in grade you just have to mine and grind more and more rock, and you produce more and more waste. Costs go up, and environmental problems go up, and so on.

Role of MIT Faculty in Tin and Uranium Ore Processing during World War II

Swent: Here we are at MIT, with Professor Gaudin. It's just after the war. Was there spinoff from wartime research that had been going on there?

Fuerstenau: During World War II, they had two major projects. Three persons, Rush Spedden, Reinhardt Schuhmann, and Professor Gaudin were actively involved with the processing of tin ores. This was Bolivian tin ore; the only source of tin for the U.S. during World War II was Bolivia. How to upgrade and concentrate that ore by developing a flotation procedure to recover cassiterite was apparently a major project they worked on at MIT. Spedden told me that before he went into the U.S. Army, he spent time in Bolivia at the tin mines. I remember one time Gaudin telling how they had gunny sacks filled with tin

ore, and they found in one case that all the ore floated; you couldn't make any separation. It turns out that Lever Brothers had a big fat rendering plant about two blocks away from MIT, and so the soap molecules that were floating through the air had got onto all of the ore and they couldn't make any separation. In floating nonmetallic ores, a soap is the collector reagent that they commonly use to make selected minerals water repellent.

The other project was a major effort on the recovery of the uranium from South African uranium ore. Even when I got to MIT, Gaudin still had, not at MIT but out at the Watertown Arsenal, a big effort of one hundred or more people working on this uranium recovery program. It was a big effort for which he was the director. That uranium recovery project was still going on in 1950, but maybe by '51 or more likely '52 that may have shut down. I think that their objectives were to improve solvent extraction and ion exchange methods to recover uranium from the leach solutions. Gaudin always considered the use of ion exchange resins for uranium extraction to be one of his big achievements, but it's something about which I personally know very little, and never bothered to look at in any detail. I remember Gaudin saying, one time, that the premier of South Africa, Jan Smuts, came to MIT incognito during the war to discuss what they were doing, what was going on, because in those days that was the only source of uranium ore available initially.

Swent: They were just beginning to open the Colorado Plateau at that time.

Fuerstenau: [Union] Carbide had uranium and vanadium plants there I know.

Swent: You weren't involved in that?

Fuerstenau: Not at all. An outgrowth of that was that the Atomic Energy Commission, the AEC, supported virtually all of the research in mineral engineering at MIT for two decades. Many of the projects of graduate students and postdoctoral researchers involved using radioactive tracers to carry out physical chemical kinds of studies on minerals. By marking a flotation reagent with a radioactive tracer, you can determine exactly how much reagent is on the surface under various conditions. That, I would say for more than a decade, some years before I came and some years after me, was a major research effort, a tool, I would say used extensively in the group at MIT. Measurement methods were often complex, and a different technique had to be developed for studying the adsorption of each different metallic ion or organic flotation reagent.

Swent: He assigned you, or together you decided on, the project you were to work on?

Fuerstenau: He made a suggestion which I thought was very good, which was to try to uncover some of the, I would say, physical chemistry basis of the flotation of quartz. A student, Mel Chang, who was just finishing his doctorate as I arrived, had some results that he couldn't explain. Gaudin thought that if we

could maybe make measurements of the electrical effects that might exist due to these reagents, one might be able to explain them. For example, most all of the reagents are charged, they're ions. Soap is an organic anion, so if a soap ion adsorbed, you're going to get a highly negative charged surface. If the mineral is negatively charged, it shouldn't adsorb the negative soap ion. I think Chang found some adsorption of soap anions on negatively charged quartz.

Swent: Anions?

Fuerstenau: Anions are negatively charged ions. And the positive ones are cations. Like anodes and cathodes; one is negative and one is positive. Another area that I got heavily involved with, related to that, was the adsorption of cationic flotation collectors, which are amines. Amines are organic cations. All of these are used in making flotation separations.

I'll just mention the definition of two types of reagents, because they will come into play. The common terminology for the reagent that makes a mineral hydrophobic is called a "collector." The other one that makes a good foaming agent is called a "frother," like alcohol; any short-chained polar compound will cause foaming. Soap solutions will foam too, as you well know from washing. Those are two reagents that have been studied a great deal. It's going to be reported, I trust, in Guy Harris's⁸ oral history. I trust there will be quite a lot of detail about some of the nature of these kinds of reagents.

My purpose was to study how reagents might adsorb on quartz. Quartz was taken as a model mineral because it is the most common gangue, or waste mineral, in an ore. You may want to float the quartz; or you may want to depress it from floating. The object, really, was to understand what the conditions are that are needed to make quartz either hydrophobic or hydrophilic. That was what I was doing, studying the basic surface chemistry involved in doing this.

Swent: This was your research project, but then you were also taking courses at the same time. What was in your courses? Would you like to talk about some of the teachers and the courses?

⁸ Guy Harris, *A Career in Mining Chemicals*, 2004

Additional Comments on Courses and Mineral Engineering Faculty at MIT

Fuerstenau:

I would like to talk a little bit about some of the courses. My minor was in chemistry, and we talked about the radiochemistry course. This was taught by Professor Charles Coryell, who was the first person to chemically identify element 61, promethium, and he was probably the most disorganized lecturer at MIT. I took two other courses in the Chemistry Department, one on advanced instrumental chemical analysis and one of chemical kinetics. Chemical kinetics was taught by Professor Isador Amdur, under whom my friend Jack Jordan later did his PhD research. I also took a course on crystal chemistry in the Geology Department by the famous crystallographer-mineralogist, Martin J. Buerger. The last course that constituted my minor was on electrochemistry taught by Professor Uhlig in the Metallurgy Department.

Three different times Professor Gaudin taught a course. I always thought of them as kind of unorganized. He never presented any set course from the point of view of principles, and to me I always considered that a bit of a shortcoming. For example, he was rewriting his flotation textbook, and one whole course was going through the chapters that he had completed, one by one—sort of discuss them, ask questions, and so on. In other words, it didn't take much preparation on his part. I personally believe in formal lectures. At other times he was heavily involved in consulting—for example, he was away from MIT a lot on a patent case related to potash flotation. He gave a whole course on discussing that patent situation, both patents in general, but also what were the arguments that went on as to how this flotation patent related to separating potash ores. That course actually was very instructive professionally.

It's interesting that in potash flotation you can separate potassium chloride from sodium chloride. When you think that these two are almost identical salts, it always intrigues me that they can be separated by flotation. In fact one of my first papers, which was coauthored with my brother Maurie, was trying to explain this. Anyway, that class was a bit interesting, but did not give you a foundation—unless you were fascinated by patent problems. I am a firm believer in courses that give the students a foundation.

I'll tell you a little aside: one time Gaudin said that—

[Tape 8, Side B]

Fuerstenau:

One time, Gaudin said that, in something like 1933, a German graduate student came to work with him in Butte, at Montana School of Mines. He said, "I would like to work on the flotation of potash ores." And Gaudin said, "You can't do that. You put the potash in water and it's going to dissolve." One year later a German patent came out on making the flotation separation out of a brine. You saturate the water, and now you do the flotation out of a

saturated brine. Gaudin said that was one major item that he missed by that off-the-cuff simple remark. [chuckles]

A person who was there at MIT during my first year and a half, two years almost, was Reinhardt Schuhmann. He always went by the name R. Schuhmann, Jr. He was a very bright, outstanding person who made a lot of important contributions during his lifetime—and a very modest individual. At age seventeen he went to Butte to do his master's with Gaudin. In 1935 he completed his master's thesis and published with Gaudin what is still a highly quoted paper on how xanthates interact with chalcocite. Then he went to MIT, and on his own devised and developed the first continuous laboratory flotation machine, small-scale like one liter or so, to study the kinetics of flotation. No one had ever done this before, and I consider that a real step forward.

There's a paper by a Bolivian engineer, García-Zúñiga, 1935-ish, that talked about the rate of flotation. As you can see, if you're blowing air bubbles into a pulp with particles present, what's the rate at which they attach, rise to the surface, and make a flotation separation? That's the mechanical kinetics of the process.

Schuhmann finished his doctorate in 1938 at MIT. Then he gave a paper at the AIME, and the chairman of the session was Professor Arthur Taggart from Columbia. Schuhmann said, which is correct, that the tailings, that is the discharge from the flotation machine, would have the same composition as the slurry, or the pulp, within the flotation machine. So if you have a suspension completely stirred, and you're letting it flow out, obviously if it's fully stirred what flows out has to be the same as what's in the vessel. Taggart said, "I don't believe it. You have to be wrong." When Schuhmann wrote this up as a paper Taggart turned it down and refused to let it be published in the AIME. Those papers from Schuhmann appeared in the *Journal of Physical Chemistry*, which is a very elitist chemical journal, and as a result really were sort of hidden from the world for many years, because that was just not the right place for that work. I don't think, at least in our area, that that goes on today. It turns out, as I said, Schuhmann was right, Taggart wrong, but Taggart was the big old man of his era and stomped on it.

Subsequently, Albert Schlechten, who did his BS at Montana School of Mines, went to MIT and carried on the next step of this continuous flotation kinetics where he studied the effects of particle size on flotation kinetics. Then, by the time that Schlechten got around to finishing his degree, he prepared a paper on his results, which also went into the *Journal of Physical Chemistry*. By then Professor Gaudin was at MIT, and Gaudin said that he should be an author. And Schuhmann said, "Yes, but you weren't here when it was done." "Everything that's done here, I should be an author." Gaudin always put his name first on all papers, and Schuhmann was really objecting to it. So the story was that Gaudin said, "Well, we'll make it alphabetical." Schuhmann's first paper was authored by himself, sole-authored, but Schlechten's was by

Gaudin-Schuhmann-Schlechten, something like that. All during my days Gaudin was almost always the lead author, and I decided later that I would not be the first author in most cases.

Swent: And the authors were not listed alphabetically. [laughter]

Fuerstenau: Maybe so. I would have to look up the reference to that paper. Schuhmann also made a lasting contribution to comminution. In the world of comminution, when you break a mineral in a crusher or a ball mill, the crushed or ground product has a complete distribution of sizes. You've probably seen that many times. If something comes out of crusher it's not broken in two but you get a complete size distribution from coarse particles clear down to sub-micron particles. In this country, in mineral processing, we always refer to the size distribution as a Gaudin-Schuhmann distribution. This is almost universal. Gaudin had presented the data in one way in his classic 1926 paper, and Schuhmann took that same approach and extended it to a cumulative distribution—in other words, adding up from the finest particles to the coarsest, you often get a pretty good straight line, and we call this a Gaudin-Schuhmann plot. It's the same Schuhmann.

Before I arrived at MIT, Schuhmann had switched into process metallurgy. By that I mean teaching and research in smelting, and so on. He gave a sequence of three very good undergraduate courses on process metallurgy, which I didn't take for credit but I audited them. These were outstanding courses, well-organized in contrast to Professor Gaudin's lectures. A couple of times, years later, I taught a process metallurgy course and really based it on Schuhmann's approach. He was an associate professor at MIT, and must have got a nice offer of full professor and left for Purdue at the end of 1952, where he spent his whole career from then on. He was head of his department, and I think they actually called it the School of Metallurgical Engineering.

I was a grad student and knew nothing about promotions, but they probably should have promoted him and kept him at MIT. As I mentioned earlier, the head of the department at MIT was John Chipman, who was often called the Father of Physical Chemistry of Steel Making. That, again, is process metallurgy, but all aimed at steelmaking. It was after I was here at Berkeley that I learned that Chipman himself had done his PhD here, in Berkeley, in chemistry, under G.N. Lewis, who was the great giant of American chemistry in the first 40 percent of this century. He was, by far, the major person in physical chemistry in those days. Chipman had done his PhD with him. I would have thought that Chipman might have worked on keeping Schuhmann there.

An enthusiastic lecturer who taught the introductory mineral processing, mineral engineering course was Rush Spedden. Spedden had a glad-handing, enthusiastic salesman way of talking, and he regularly taught the introductory mineral processing course, and obviously did it well. He was there at MIT for

maybe my first two years, or first year and a half. What I find kind of interesting is that Spedden, deBruyn and a fellow graduate student named Dwight Harris, all in their thirties lived together in an apartment in Cambridge. None were married, but within a year or so after I got to MIT, they all were—actually Phil deBruyn a little later. Dwight Harris had gotten his BS from Washington State College in 1937, so you can get his relative age.

Spedden left MIT around '52, because he had been there as an assistant professor as long as he could. He never finished a doctorate, and I assume that all of that entered into his career at MIT. He went to Union Carbide, the Electrometallurgical Company, in Niagara Falls, as group manager at the Metals Research Laboratory—and that will play a significant role in my life later on. Dwight Harris finished his doctorate in 1952 and went to the University of Wisconsin where he was on their faculty for a couple of years. I think he had been an assistant professor at Washington State for a while before he came to MIT as a grad student fairly late in life.

Swent: There had been a depression and also a war.

Fuerstenau: Right. Spedden, I know, was in the army. But I guess that Harris was involved with metallurgical research, being near the aluminum plants in Spokane. Anyway, that's my guess. Harris, at least for a couple of years or so, was on the faculty in Madison, Wisconsin. You did an oral history about Bob Shoemaker⁹ a while ago, and I remember Bob telling me he has a bachelor's and master's in chemistry from Oregon State, and that he went to Wisconsin to do a PhD in chemistry. He started that program, and for some reason he and the chemistry professor got at loggerheads, and the professor told him that they would be parting their ways. Bob went over and did a master's degree in metallurgy. My guess is he worked with Dwight Harris. When he finished, he naturally was looking for a job, and Dwight Harris, who had been the roommate of Spedden, probably told Rush Spedden, "Here's this good person, Shoemaker." Shoemaker then joined Carbide and worked for Spedden as a section leader there.

DeBruyn was an Afrikaner. He came from the University of Stellenbosch, in South Africa. By the way, three, four years ago I was in South Africa and went on a wine-tasting tour around Stellenbosch, which is just a few miles outside of Cape Town. It's really beautiful through that valley where Stellenbosch is. Apparently, that's a Dutch settlement in the southern part of South Africa which is more English, Cape Town, compared to Johannesburg.

⁹ Robert Shoemaker, *Metallurgical Engineer: Union Carbide, Bechtel, San Francisco Mining Associates; Metallurgical Consultant, 1953 to 2000*, 2001

Stellenbosch is an enclave of Dutch in that region—a lot of houses look Dutch-like.

DeBruyn graduated with a bachelor's and master's in geology from the University of Stellenbosch. Then I remember him telling me this: he wrote to Colorado School of Mines, stating that he wanted to switch to metallurgy. They said, "It will take you three years, and then you can get a bachelor's degree in metallurgy." When he wrote to MIT they said, "It will take you three years and we'll give you a doctor's degree in metallurgy."

Swent: Easy choice.

Fuerstenau: Isn't that right. So he went to MIT. When I got there, he was assistant professor, and he was teaching undergraduate mineral engineering courses, let's say one on processes like thickening and filtration and one on flotation, and maybe one grad course. His courses were very good, very well organized, and so on. Practically all during my time as a grad student he was working on his thesis, which related, again, to the surface chemistry involved in quartz flotation. DeBruyn's research involved using radioactive tracers to actually determine the amount of organic amines that adsorb on quartz. Whereas I was measuring zeta potentials to determine other aspects of how adsorption of such ions affect the quartz surface.

Professor J.Th.G. Overbeek's Spending a Year in Our Group at MIT

Fuerstenau: Gaudin, in writing his flotation book, got very interested in how the fine particles interact in water. If you have a suspension of fines, what makes them disperse or what makes them flocculate? It turns out he had come across a new book which was published in 1949, on the theory of stability of colloids written by Professor J.Th.G. Overbeek of the University of Utrecht. So Gaudin decided to invite Overbeek to come to MIT to spend the year 1952-53. Nicely, Overbeek did just that. It turns out, as a little bit of an aside, Overbeek and a colleague probably slightly senior to him, Dr. E.J.W. Verwey, were working for Philips during World War II, when the Germans were there. On their own, they spent quite a bit of time working on a very long detailed theory of what controls whether the particles stay dispersed or attract each other in suspensions. But because it was wartime, they could not publish their work. It was only published as a separate book in 1949.

Swent: This was in Holland?

Fuerstenau: Yes, published in Holland, but in English.

Swent: But Overbeek was from Holland?

Fuerstenau: Yes, and so was Verwey.

Swent: You said, “When the Germans were there.” You mean, “When the Germans were in Holland.”

Fuerstenau: World War II. Oh yes, Mrs. Overbeek was very upset that the Germans wouldn’t let her have milk to feed the babies, and things like that, during World War II. That was during the occupation by the Germans.

At the same time, in Russia, a very famous surface physical chemist named B.V. Derjaguin, and a theoretical physicist L.D. Landau—Landau got a Nobel Prize, as a physicist later for something else—did the same thing. Their work was published in Russia, apparently 1945-ish, and neither group knew of the work of the others, by definition, due to the times. Now, this theory is extremely widely used in all sorts of fields related to interaction of colloidal particles in suspensions, and is called the DLVO theory: Derjaguin, Landau, Verwey, and Overbeek. This DLVO theory is the standard basis for analyzing the stability of colloidal suspensions. Even looking at the interaction of an air bubble with a particle, we use this theory to explain what attraction there might be between the bubble and the particle.

Accepting Gaudin’s invitation, Overbeek came to MIT and brought his niece as a baby sitter. Subsequently, Phil deBruyn married the niece. [laughs] Overbeek gave two courses in the year he was there on the physical chemistry of colloid science. Really magnificent lectures. That was what, I think, really gave me a good foundation in the physical chemistry of surfaces and oriented me towards studying more in that direction. It turns out that everything that I had been doing on research was the right thing to do. I had planned it myself. In that last year, I had a lot of discussions with Professor Overbeek on my research, but everything I had been doing was correct.

Again, I really liked the formal lectures of Overbeek. Overbeek would have been about forty, forty or forty-one in age, and, of course, spoke with a good Dutch accent, but spoke English very well. A very able person. In fact, when I came here to Berkeley, I sort of patterned one main course I taught, at least the start of it, based on the kind of approach I learned from Overbeek.

Swent: Can you be more specific about that?

Fuerstenau: Well, a graduate course that I developed was concerned with the fundamentals of, let’s say, the physical chemistry of interfaces. Being in engineering, I want to look at something in a very fundamental way, but still direct it towards an application such as flotation, particle interactions, or mineral particle separations. I oriented this course so that it had fairly wide appeal and numerous students from other departments took the course, such as chemical engineers, water treatment civil engineers, soil scientists, ceramic engineers, and so on.

Comments on Various Doctoral Examinations at MIT

Swent: What kind of terms did they have at MIT? Were they quarters or semesters?

Fuerstenau: They were semesters. They had an interesting unit system: for every hour in lecture you were to spend two hours studying. So if you had what we could call a two-unit or two-credit course here at Berkeley, at MIT it would be called six units—two for the lecture and four for study. A full load was something like fifty units. In almost every other institution, if there's a three-hour laboratory, it's given one unit credit but at MIT, the three-hour lab carries three units. If you had two lectures and a lab, that would be a nine-unit course. I guess what they were trying to do was weight the effort that they expected a student to do in any course. This was then done on a semester basis. I was a half-time grad student, half-time research assistant, so maybe the course load would have been twenty-four units in each semester.

As I may have said earlier, the only course requirements were those for a minor. One minor, which I did in chemistry. No course requirements for the major, other than to pass the two very detailed written exams of three hours each. In the metallurgy field there were written exams in physical metallurgy, process metallurgy, and mineral engineering. You had to take two. Naturally, the grad students working in mineral engineering took the mineral engineering and the process metallurgy exams. A physical metallurgist would do the physical metallurgy and the process metallurgy exams. You had to know, in detail, two of the three fields. This is why I audited, for example, Schuhmann's courses. At that time the written exams were based on a complete knowledge of the undergraduate courses. So if somebody had a BS from MIT, life was a lot simpler than for those of us who came from the outside. I really studied hard before taking the written exam in process metallurgy, which involved problems on the thermodynamics of steelmaking and copper smelting, heat transfer in heating or cooling ingots, gas flow in furnaces, and so on. The mineral engineering exam included problems on comminution, thickener design, filtration, adsorption thermodynamics, mineral separations, and so on. Copies of previous exams were available, so one had an idea of the type of questions that might be asked.

Swent: At what point was this exam given?

Fuerstenau: I think I took them late in my second year. My total time for a doctorate was three years, including the last summer. My finishing was delayed a little bit because I had to postpone the oral qualifying examination, which was and is the major hurdle, because of having to have an appendicitis operation. To this day, I still remember that exam almost minute by minute. I think, actually, there were only three professors there for my oral exam. I don't remember whether Overbeek was in there or not, but Gaudin was, and Phil deBruyn. What I think was odd was that a fellow grad student who they made assistant

professor that third year, named Gordon Bell, was sitting in as one of my examiners, although he might not have asked very many questions.

To this day I can recall a lot of the questions. I remember at one point Professor Gaudin saying to me, “Can’t you learn to say ‘I don’t know’?” [laughter] But I was working my way through the many questions that were asked. One of them, I think the very one he asked me, why I can’t say I don’t know, ended up in his textbook later on. Maybe my thoughts gave him an idea, I don’t know

I know that this examination is always very traumatic for a lot of people. Around here, a typical thing is to start by asking them what’s their name and a little bit about their background, just to put people at ease. I remember, my roommate telling me that a fellow friend of his, who was a doctoral student in electrical engineering at MIT, choked up so much during his oral exam that he couldn’t answer anything. Some week or two or three later, the four professors that were involved each asked him individually to stop by their office, and each asked him a bunch of questions. Some days later, they told him that he had passed his doctor’s oral exam.

Swent: You had commented that your own oral examination was delayed by appendicitis.

[Tape 9, Side A]

Fuerstenau: About one week before my oral exam was scheduled, all of a sudden, in the middle of the night, I ended up having my appendix taken out. It was about two in the morning. When I told the medical people that I had a very bad pain, they thought I might have a burst appendix. This operation took place at about 2:00 a.m.

Swent: Where was it?

Fuerstenau: It was at a small hospital right adjacent to MIT, which I’m assuming was a small Catholic hospital. It was practically on MIT property, so they probably had an arrangement for hospitalization, I assume.

The next day the doctor told me that the problem was that my appendix just was stiff and attached to my stomach in some way so that it pulled, probably a genetic thing. But they thought that this might have been a burst appendix, so I underwent the emergency operation. I did chat with the surgeon later, and I remember his name was Chamberlain, and that his BS was in mining from MIT. He said, “I have a BS in mining engineering but I decided to go into medicine.” [laughs]

Swent: Quite a switch.

Fuerstenau: Yes. That then delayed my oral exam. I know it was in the wintertime, and probably delayed everything a full month into the spring. That held me back, and instead of being able to finish in June, I finally finished in August.

Swent: You stayed there all through the year?

Fuerstenau: Twelve months, maybe visited my aunt at Christmas, or I remember visiting friends one other time. I must have, for Christmas, gone down to my aunt's place in New York. I went home once only during the summer of 1952 for a trip. I remember, I got to Rapid City and it was 108 degrees, and you know, it didn't seem hot because western South Dakota doesn't have that Boston humidity.

Research in the Laboratory that Led to the Concept of Hemimicelles

Swent: Did you have other students working with you as helpers?

Fuerstenau: Not on the same project. There were about a dozen grad students in the group. As I mentioned earlier, I shared a lab with another person, Dick Charles, but his research was totally different from mine.

Swent: Were there undergraduates working there too?

Fuerstenau: Possibly working with other people, but not with me or Dick Charles. I got involved with some undergraduate research when I became assistant professor, but not while I was a student. The answer is that there was a lab technician that might help on maintaining laboratories and equipment, but he didn't help me at any time.

Coming back to what kind of help was available on my research. Professor Gaudin had said, "Why don't you look at measuring these streaming potentials?" As I have said, streaming potentials occur when a liquid is forced past a solid surface because ions are stripped away from the solid. I completely designed the apparatus to make the measurements, found the necessary equipment, and so on. There was a rather complicated bit of glassblowing for the cell that held the electrodes and the mineral particles. This would break quite often. There was a glassblower in the Physics Department, which was just down the hall. I can remember one day going in; you always had to coax the glassblower to do the job. I went in to his small shop and there was this little old man, the glassblower, sound asleep. I woke him up and said, "I need this urgent." He said, "Awful busy, awful busy; I just won't be able to get at it for two or three weeks." That's a true story. I woke him up. [laughs]

Anyway, I learned to blow glass. A fellow student, Dwight Harris, who I guess had known how to blow glass, showed me how. I practiced joining glass

tubing for quite a while, to where I became quite adept at glassblowing. When I would crack or break my glass equipment I eventually learned to remake it myself.

Swent: You didn't even have to wait for him to wake up.

Fuerstenau: No. Also, I needed very, very pure water. The metallurgy department had its own distilled water system. I remember once something wasn't quite right with the water, so I asked them to check the system and they found, in the holding tank, a dead mouse. [laughs] Then a distinguished senior professor, George Scatchard in the Physical Chemistry Department, which is again down the hall on another floor, had a huge and very good distilled water system. For several decades, Scatchard had done a great deal of thermodynamic work on aqueous solutions, and his graduate students needed high-quality water for experiments. I would make jugs of extremely high-purity water and carefully monitor his distilling system.

During the course of my research, I was measuring interfacial potentials, called zeta potentials, using the streaming potential apparatus that I had assembled. This is the potential that is very close to the surface due to the adsorption of ions. I found that the clean quartz particles that I prepared would reverse their charge at low pH, about 3. So at pHs higher than this, the surface of quartz is negatively charged. Probably the most interesting results were those that I obtained with the cationic amine collectors on quartz. In the way of explanation, organic compounds generally consist of chains of carbon-hydrogen groups and water does not like these organic chains. So a flotation collector consists of a molecule with a water-repellent hydrocarbon chain and a polar group that can attach the molecule to the mineral surface. In my case, I was working with an amine which adsorbs on negatively charged quartz because it is positively charged. Then the hydrocarbon chain is oriented away from the mineral, causing the mineral to become water-repellent so that it will attach to air bubbles in flotation.

From my measurements, I got plots or curves with sharp breaks instead of having something that would gently change with increasing reagent concentration. My studies showed that all of a sudden you would hit a certain concentration and there would be a very abrupt change in potential. I didn't know what that meant. I remember when—and there were often a lot of visitors—Gaudin would bring visitors around and you would have to show what you were doing to whoever that visitor was. I remember saying, "I've got these curves with these sharp kinks in them, these very sharp breaks, but I don't know what that means." Finally, after a while it hit me what must be happening. This is a very important phenomenon, and I actually was the first person to come upon it with dissolved surfactants in water adsorbing on solids.

Around 1913, J.W. McBain, who started in England but then spent his later years at Stanford, first postulated that these dissolved surfactant ions would aggregate at higher concentrations in solution forming what he called a micelle. By aggregating in solution as, say, spheres, the chains are removed from water and all the polar heads are oriented towards the water. This begins to occur at a certain critical concentration. So with soap or amines in solution, micelles begin to form and they just keep forming when more soap is added to the solution and that way the soap keeps on dissolving. That has a big effect on detergency. If you have oil on your clothes and you have a detergent or a soap, the oil will go inside those micelles. That's how a soap or a detergent works in washing clothes. This aggregation process takes place quite abruptly in a solution.

In one or two of his lectures, Professor Overbeek discussed the mechanism and thermodynamics of micelle formation in solution. All of a sudden it occurred to me one day that something similar must be happening at the quartz surface in my experiments. The chains of these adsorbed organic ions will all of a sudden aggregate at the surface like forming surface micelles out in solution. When surface aggregation starts, adsorption must increase sharply, and hence you get what I observed—namely these large potential changes. In my thesis, I called them something like surface aggregates or two-dimensional surface micelles. Later on when writing a paper with Gaudin, we decided to call these surface aggregates of adsorbed amine ions “hemimicelles.” Because the mineral quartz is negatively charged and the amine ions are positively charged cations, they would adsorb heads down to the surface such that there would be half of a micelle—a *hemimicelle*. The hydrocarbon chains would be oriented towards the water and make the surface water-repellent.

But I dreamt all this up, that what must be happening at the surface in these types of systems is that adsorbed organic ions associate at the surface. Being in the world of flotation, of course, that first paper was published in the flotation literature. I did publish a paper in the *Journal of Physical Chemistry* in '56 or '57. In about the last ten years people call all of these phenomena *self-assembly*, and it's now a big thing because in biology, surfactant behavior, and computer chip production, these same phenomena are important. Our cells are held together by the chains interacting; that makes the wall of the cell. This part of my thesis was the first time anybody made such observation of this adsorption phenomenon occurring at a solid-water surface. As I said, for a year or more I just used to call what I had found simply these sharp breaks or kinks until I postulated that surface aggregation must be occurring.

Phil deBruyn never believed these ideas at the time. In his thesis he developed some other kind of theory. But in the paper he wrote from his thesis he said that yours truly had an alternative explanation. If you calculate the concentration of adsorbed amine ions at the surface, it's roughly identical to the concentration to make the micelles in the bulk solution. As you approach

the surface, and if you look at the region where they are adsorbing as a small volume, the concentration is very high. That really was a major concept that came out of my doctoral research.

Swent: This was published where and when?

Fuerstenau: There were two papers out of my thesis coauthored with Professor Gaudin in 1955. They were both published in *Mining Engineering* and also in *AIME Transactions*. By the way, Phil deBruyn published his own thesis paper in the same volume of *AIME Transactions*. I remember deBruyn saying, "Why did you put Gaudin's name on there; he had nothing to do with that?" And I said, "I want my name associated, at least in my early stages, with Professor Gaudin." Whereas deBruyn did just the opposite. The paper on his thesis is authored just by himself. I think he would have had more stature if that publication would have had Gaudin's name as a coauthor. My first two papers were authored by A.M. Gaudin and D.W. Fuerstenau. At that time all of Professor Gaudin's papers were still published with him listed as the first author.

Swent: Even though he really didn't do the research.

Fuerstenau: He was my thesis supervisor and did suggest working on this research topic and of course provided the financial support. But I was very independent and did everything myself. I planned it, and I carried all of it out. But once in a while we discussed it. Probably he regularly learned what was going on when he brought a visitor around. By the way, you always knew if Professor Gaudin had visited the laboratory because of the aroma of cigars that he smoked. Also we had to write detailed quarterly reports for the AEC contract, and Gaudin thoroughly studied the initial drafts. As I said, I did the right things, which I was very pleased with by the time I was writing my thesis, which was then the summer of 1953. By the way, I typed it myself.

Swent: I was going to ask, did you type it?

Fuerstenau: Those were the days of new electric typewriters, but reproduction was done on a machine called a Multilith machine. You typed on a mat, and when mounted in the Multilith machine the mat picked up gooey ink, and you could run off hundreds of copies like that. That was multilithing, and it was kind of a fancy machine used for reproducing project reports, et cetera. I remember one time I did something wrong. I turned the thing on backwards and this gooey ink came up over my whole mat and up through the machine. Cleaning up that mess took hours. I was using the department secretary's typewriter in the evening, and this electric typewriter seemed to have such a sensitive keyboard that it seemed to type letters if you simply paused and they would have to be erased I did the whole thing myself, except I handwrote the equations in.

Swent: How long was the actual paper?

Fuerstenau: Professor Chipman, who was the department head, figured that a doctor's thesis ought to be no more than maybe seventy-five pages. He made that comment; seventy-five or so pages. My thesis is right here. [goes to get his thesis] One hundred, right on the head. As you can see, what I did was just handwrote in the equations. The body is only seventy-seven pages. I always sort of regretted that I picked a most sexless title called *Streaming Potential Studies on Quartz*. I should have had a title more like *Interfacial Phenomena in the Flotation of Quartz*. But, "streaming potential studies on quartz," of course was what it was about. That thesis title is on my MIT diploma. In later years here at Berkeley, I always make students give me about half a dozen titles for their thesis, and then we would discuss it and pick out what seems best.

Swent: ScD?

Fuerstenau: Oh yes. MIT did not give PhD degrees in engineering. It was the degree of Doctor of Science, or ScD. You can always tell when somebody has an MIT doctorate—or you could; now it's a little different—because the degree is ScD. Three of us in this department have ScDs but our department chairman's degree is PhD. I'm speaking from MIT. At that time the degrees given at MIT in science, math, physics, chemistry, biology were PhDs. George Parks, who did his BS and MS here at Berkeley, came to MIT, when I was on the faculty there, to do a doctorate in mineral engineering. When he finally graduated, he petitioned that his degree be called a PhD. I guess after that, it has gotten to be that you check the box whether you want your degree called doctor of science or doctor of philosophy. George Parks's whole career—he's retired now—has been at Stanford, by the way. It's due to him, apparently, that now I guess you check the box. My degree is doctor of science.

Remembering the Thesis Oral Examination

Fuerstenau: The final hurdle for the doctor's degree was an oral defense of your thesis. I just took this as more or less a formality. But the person following me the next day from our group almost didn't pass it. I often have said to other people that I'm glad my exam was on the first day and not the second, or I would have been in there sweating.

Swent: Who gave it to you?

Fuerstenau: Professor Gaudin was out of town, so I don't know if normally the thesis supervisor was there or not. He was not for mine. But the oral exam consisted of the chairman, Professor Chipman, who was the department head; Professor Overbeek; Professor deBruyn, who probably had submitted his thesis a year earlier, and Professor Carl Wagner. Wagner was a great giant of solid-state

electrochemistry, let's say, and mainly in metallurgical thermodynamics and solid-state kinetics. He wrote a paper published in German in 1933 about the motion of defects in oxides. That really was the first paper suggesting semiconductors, and I have often heard that they really should have included him in the Nobel Prize for the semiconductor. But it was published in Germany, in '33, fifteen years earlier than all that work at Bell Labs.

Wagner had gotten some sort of medal from Hitler, related to the thermodynamics on the rocket program in Germany during World War II, and was captured and brought by the American army to White Sands, New Mexico. Professor Chipman, who was well aware of Wagner's work on the thermodynamics of metallic solutions, somehow got him released to come to MIT. So he was at MIT; he must have gotten there 1950-ish, '51. He stayed there quite a few years, and then eventually went back to Germany to head the Max Planck Institute for Physical Chemistry in Göttingen.

I got to know him quite well, later on. I remember one time—I'm digressing a little bit. One time I went to talk to him. I had the results of some study, not related to my thesis; this was later. I said, "If you plot it this way you get a nice straight line." He said, "Well, you know, my observation of engineers is that when they plot their data and get a straight line, then they're content and happy and that's it." I fit that one. [laughs] As I say, I really got to know Carl Wagner, really quite well, later on.

Anyway, he was there [at the oral exam]. I still remember the opening question from John Chipman, which was, "Is this of any practical value?" My life's work! I suppose I mumbled about what kind of applications could come from it. The only other real part I still always chuckle over was that Professor Wagner asked me some very good, excellent question, and in all innocence I said to him, "You know, that really is a good question. I would like to know the answer to that myself. Maybe Professor Overbeek can go to the board and answer it for us." And Overbeek went to the board and went through the explanation of whatever this was. Sometime later that day, Overbeek said, "That really wasn't very cricket." It was not planned; I just innocently thought that that really is good and I would like to know the answer.

Swent: By then you had learned to say you didn't know.

Fuerstenau: I guess you're right. Anyway, I passed. I had to make a couple of revisions based on questions that came up about some physical assumptions that I had made.

Swent: You said you were talking to Wagner about something not related to your thesis; it was after that. Did you stay on after you finished your thesis?

Fuerstenau: Yes, yes. Let me back up. I then spent several years as an assistant professor at MIT.

Events Leading up to an Appointment as Assistant Professor

Fuerstenau: Let me back up to where I think Gaudin's thoughts on appointing me as an assistant professor might have come from. In my last semester, there was a Metallurgy Department colloquium that was run, that semester, by Professor Herbert Uhlig, who was probably the most well known person in the field of corrosion in this country at that time. Uhlig asked me to give one of the seminars towards the end of the colloquium on my thesis research, which I did. I was wise enough to plan out what I was going to say. Today, if I have a paper or seminar to give, I maybe put the slides together and that's about all of the planning that I do and make the presentation from the slides. But for that colloquium talk, I may even have written it out but without memorizing it. That would have been the spring of 1953.

[Tape 9, Side B]

Fuerstenau: That would have been spring of 1953, and I wisely invited Peg, who in a few months I was going to marry, to come over and listen to me. At ten o'clock that morning she came over to the MIT Grad House, and I presented that seminar to her. You don't want to hear your words out loud the first time when it's important. And I think I did a good job.

Swent: Did she give you any advice?

Fuerstenau: Probably. Without a doubt she may have.

Swent: A good run through, anyhow.

Fuerstenau: Oh yes. Like I say, I have often told people—if it's vital, hear your words yourself ahead of time. It was only a few days after that seminar that Professor Gaudin asked me whether I would be interested in staying on as an assistant professor. That indeed is what I did. I think it was late spring then that I knew that they were going to have me stay as an assistant professor, but I didn't tell anybody. I don't believe in passing on speculative things. You then may have to undo the bridge that you didn't cross. And that often happens. When there's a possibility I usually wait until it's a reality. The Friday evening after my thesis exam Professor Chipman invited me to his house for dinner, including another person, Tom King, who was coming from Glasgow University in Scotland to be an assistant professor in process metallurgy. That was the area that Schuhmann had been in. So the two of us were at Chipman's for dinner. Chipman originally came from South Carolina, so he was a maker of wicked mint juleps. It was the first time I ever had a mint julep. In the Boston summer that's a nice thing. King became assistant professor and many years later was the head of the Metallurgy Department there at MIT for quite a few years. That was the beginning of my teaching career there.

Swent: A couple of questions. You used the word thesis, and I used to hear thesis, and now people say “dissertation.” What’s the difference? Or is there any?

Fuerstenau: Around here, they use the word dissertation. I’ve always used the word thesis. [reading from his thesis] “This thesis is concerned with the electrokinetic property of quartz particles in solution.” “Thesis supervisor A.M. Gaudin.” At MIT that was called a thesis, so I generally use the word thesis. But around here at Berkeley, I think the word dissertation is the common one. You have a master’s thesis, but a PhD dissertation.

Swent: It used to be you had to defend your thesis.

Fuerstenau: That’s what I had to do. That was what I called “thesis exam.” At Berkeley, and I am pleased, we do not have an oral exam on PhD dissertations any more, at least in the sciences or engineering. I’m glad of it. This being such a big PhD place, you would be there half the time. I found an invitation to a final, formal thesis defense in the fifties, from our department, so they required it then. But once the committee signs off on the thesis here, that’s it. Sometimes people have a lot of revision work to do, once in a while, after they have turned it in to me. The ideal thing is to have the main supervisor go over it and get it into shape before it is submitted to other members of the committee. A poor thing to do, and it does happen once in a while, is for the student to give a preliminary copy to every member of the committee. Then everybody is making the same kinds of corrections. Anyway, at Berkeley we don’t have a public defense of dissertations.

Now my son Steve got a PhD from Yale a few years ago, and said that he first had to present a seminar, to anybody who wanted to come, and immediately after that the audience all departed, and then a committee of three or four faculty conducted detailed questioning of whatever they wanted to on his thesis. And that procedure, I think, exists in many places. In northern European universities, the thesis defense examination is very formal. Like I say, I’m glad that at Berkeley we don’t do that. But I had such an examination on my thesis, and as I said I thought it was just a pro forma thing. However, that is not always the case for every candidate.

On Meeting Margaret Ann Pellett

Swent: You mentioned Peggy. This is the first time she has come into the picture.

Fuerstenau: Yes; Margaret Pellett. My roommate, Bud Samuel, probably in the fall of ‘51, said that he and another friend were going to a mixer—in fact, I think it was the person who passed the electrical engineering oral exam by private examination. He said, “Do you want to go over to Radcliffe to a Jolly-Up?” Radcliffe mixers were called Jolly-Ups and were held at the beginning of the

term. You may have had them at Wellesley, for that matter. You probably didn't call them jolly-ups.

Swent: I think we called them mixers.

Fuerstenau: Well, it was a mixer. And I said, "I don't want to go over there. All those women wear fur coats. That's not in my league." Anyway, we went. I met two, three girls. One I may have dated once or twice. I can't even remember the name. My roommate, Bud, met and dated for many years a very close friend of Peg's named Carol MacLean. This was when they were sophomores. Bud dated Carol for quite a long time. I think even that Christmas I was invited up to Maine to Carol's home. I spent three days between Christmas and New Year's with her family because she thought I was a lonely soul with no family. So I did that. Maine is all full of snow, as you well know, in the winter. Very lovely up there, right near Portland, Maine.

Then, in the spring, I guess Carol and another friend, Joan Kennedy, went down to New York and Peg went home to New Jersey for Easter. Carol had arranged for Bud to pick them up on their return at the railroad station at about 5:00 a.m. The three met and took the overnight train from New York back to Boston. So I went along down to the train station. That was, I think, the first time I met Peg. Then, maybe, a couple of times we went out, six of us, for coffee or something. I didn't pay any attention, but somewhere along the line got interested and we went out to a dance on a Saturday night, one of these big dance places on the Charles River called the Totem Pole, out to the west of Cambridge somewhere. They still had big orchestras there in the 1950s. This was also a park where you could canoe in the spring. That may have been virtually our very first real date. So life grew.

Peg was majoring in American history at Radcliffe, which was really at Harvard. All of their classes were at Harvard, and some years ago Radcliffe was formally merged into Harvard University so Radcliffe College no longer exists as such. Once in a while she invited me to come and listen to lectures. I went to maybe a half dozen lectures and they were all great lectures. Harvard had a lot of great lecturers. There are a lot of great lecturers here at Berkeley, of course, too. She had one course from a professor named Frederick Merk, who was the successor of the Frederick Turner who had established the field of the role of the frontier in American history. Merk had written a book called *The History of the Westward Movement*. The Radcliffe women always had humorous names for different things, like jolly-up for the mixer. This course on the history of the western movement they called, "Cowboys and Indians."

Peg said that Merk started one lecture by saying, "I know none of you have ever heard of this, but this lecture is going to be on the role of flotation in developing the West." He talked about how when flotation came into being that it led to the development of Montana and Arizona. As you know, you

can't concentrate low-grade ores without flotation. One whole lecture in this course was devoted to this topic.

I went once or twice to hear Professor Davis, a very famous musicologist, lecture on music, and a couple others on history, just to hear them. At Harvard, undergraduates had to have a one-year course in a science—so she elected a course in geology as her science course. This was physical geology given by a famous humanist geologist named Kirtley Mather. Mather wrote little poems to remember geologic terms and names—she used to recite his little jingles that he gave for memorizing the geologic eras and names of fossils. I remember going to one of his lectures, which again were very good lectures. The geology course had something like two or three dozen students in it, but the other classes were huge. The music classes, the history classes, would occupy the whole auditorium kind of thing. You hear about big classes at Berkeley. Well, there are big, huge classes at Harvard too. But then again, there are obviously a lot of advanced courses that probably are on a smaller scale.

She wasn't totally ignorant of things related to mining and metallurgy. It turns out that her father and mother both came from Sussex County in New Jersey. Her grandfather was a country surgeon in Hamburg, New Jersey, which is right next to Franklin. Franklin was where the great zinc mine of New Jersey Zinc Company was located. In fact, when I first knew Peg, her uncle, her father's oldest brother, had been the manager of the mill for New Jersey Zinc in Franklin. His name was Jackson Pellett. In fact, if you look in the Taggart handbook, there is a description of a Pellett classifier, which he must have been the inventor of. I remember talking with him quite a bit. He said that in his younger days, a consultant to the company was Professor Robert H. Richards from MIT, after whom the AIME Richards Award is named. The ore body at Franklin mainly contained the oxide zinc minerals of zincite and franklinite, and the dumps today contain the greatest variety of minerals that can be found anywhere worldwide. Jack Pellett's son was a mill superintendent for New Jersey Zinc in Colorado and maybe in Tennessee. Again a Pellett in the metallurgical world. Another one of her cousins eventually became the finance vice president of New Jersey Zinc. I think that New Jersey Zinc was taken over by a merger some years back. They were also in the uranium business in Utah around 1960. Not where you were in New Mexico, though.

Swent: So she was not repelled by the idea of metallurgy?

Fuerstenau: Not at all, really. In fact, her father was an engineer, a sales engineer for Worthington Pumps in New York. Anybody who installed pumps in a major oil field would get them from Worthington and her father was the person they dealt with. He didn't go traveling, selling; he was always in New York City. He was, I suppose, what you would call a sales engineer. So being involved

with somebody, technically related to metallurgy, I guess was not strange for her.

We decided to get married at the end of August of 1953. She was just finishing her junior year. Her father kind of raised his eyebrows when we talked about getting married. But we were married August 29, 1953, the hottest August 29th in the history of New York City. We were in New Jersey right across the river, but it was 100 degrees and about 100 percent humidity.

The objective was that I was going to be an assistant professor at MIT while she completed her senior year at Radcliffe. Two of her friends had gotten married—one at Christmas in her junior year. This friend, Ruth Spindle, who lives in Denver, she sees regularly still. Another one who had been her freshman roommate got married also earlier that summer. As for that summer wedding, it was in the Harvard Memorial Church. I remember coming there at 11:30; Peg was a bridesmaid, only the wedding started at 11:00, so just as I arrived people were coming out—great timing.

Consulting on Papermaking for the P.H. Glatfelter Company

Fuerstenau:

With Professor Gaudin, I got involved with a very interesting consulting job in the spring and summer of 1953, which actually paid for the honeymoon. It's an interesting project which I can tell you about. Gaudin had gone to a technical meeting where he had met the plant manager of a paper mill located in Spring Grove, Pennsylvania. This was the P.H. Glatfelter Company, which makes the best high-grade paper in the U.S. Anything that is a museum-quality print, you'll often see acknowledging Glatfelter for the paper.

This was '53, and what was happening resulted from making paper half from scrap paper and half from new wood fibers. At that time the adhesive in Scotch tape was latex; today it's not. What happened was when they digested the scrap paper, cooked it up, the Scotch tape adhesive would make little rubber balls. Now the paper fibers, when you finally get to the last stage of papermaking, are layered onto big blankets that are something like four to six feet wide and twenty feet long, running as a continuous belt. Water in the paper fibers is sucked out on the blanket and the paper comes off as a large sheet. But these tiny rubber balls would start coating the wool blanket and after a while, the operation would have to be shut down to replace the blanket, and they're expensive. Also a lot of little rubber balls would get into the paper itself. I remember seeing high-quality typing paper that once in a while had transparent specks. Do you remember paper like that? This was the early fifties?

Swent:

Yes.

Fuerstenau: That's the latex that came off the Scotch tape in the recycled paper. Could we get that out? Gaudin suggested that I work with them on that, and I guess he was marginally involved. We went down together the first time, and afterwards I went down there to Spring Grove quite often, which was near York, Pennsylvania. I would take the overnight train. Using flotation and turpentine as a collector, we had good success in removing the rubber particles, since it turns out that the chemical structure of latex is kind of like the chemical structure of whatever molecule makes turpentine. I remember adding a little bit of turpentine, and then with a flotation machine we could remove these little rubber balls. The first thing that would come out would be the ink. Now, decades later, they do use flotation to de-ink waste paper—we weren't involved with that. Because the ink is comprised of carbon particles which are hydrophobic, flotation works nicely to de-ink pulped scrap paper. We ran a lot of flotation tests where a technician would collect the froth, and then count how many rubber balls would be collected on the filter paper, maybe look at the residue, and so on. I believe that they got to the point of applying for a patent for this process. But just then, 3-M changed the adhesive to a synthetic polymer, an acetate-polymer. Then the problem went away. But when you had the little latex balls, it really created this tremendous problem. I spent a lot of days on that in the summer of 1953 and for a year or more later.

You know, my pay as a consultant while I was a grad student was \$25 per day, and when I became an assistant professor, Gaudin suggested to them that my consulting pay be \$50 per day, which in those days was pretty good. I think at that time Gaudin's consulting fees were \$200 per day. For the summer, I got \$50 per day, which paid for the honeymoon expenses at the end of August.

Marriage on August 29, 1953

Swent: You were married, you said, in New Jersey?

Fuerstenau: In New Jersey, in a little church in Peg's home town called Haworth, which no one knows about. You may know, from New Jersey, you always have to give the next town. Haworth is about ten miles north of the George Washington Bridge. She went to Tenafly High School in the adjacent town.

Swent: Where did you go for your honeymoon?

Fuerstenau: We went to a mountain resort near Rutland, Vermont, which I discovered by reading something about it in a magazine or newspaper. This was near Plymouth Notch, Vermont, where President Coolidge came from. This was all lovely Vermont. Not a huge resort, but there were a lot of honeymoon couples there, all married on the same day. First we spent two days in West Point, which was only thirty miles north of Haworth. Then the second week we drove up to Canada and stayed in St. Croix, which is a short distance west of

Quebec City. We were in somebody's lovely old home, must have been kind of a small mansion. I recall that it included breakfast and dinner. They may have rented out three or four rooms max. It was so elegant, and I still chuckle about it, that I sort of couldn't enjoy it because I was afraid that the cost was far different from what we were paying for it, which was not the case. We saw a lot of Quebec City. There's a big island there in the middle of the St. Lawrence River, and we drove around the island one day. Took the ferry boat to the other side of the river, and things like that. Of course this was September so it was already getting frosty in Quebec.

Swent: Then you had to hunt for a place to live in Cambridge?

Fuerstenau: I had rented that in the summer, 1734 Cambridge Street, right across from Memorial Hall—that's that huge church-like auditorium at Harvard. It's kind of the symbol of Harvard. That summer, my roommate Bud and I, and my friend from Montana, Jack Jordan, all lived there together, because I had rented it. They moved out in the fall. So we had an apartment on the top floor of this old Cambridge house. It was really close in. Right smack on the end of Harvard, so Peg had it easy to go to her classes.

Swent: Peggy hadn't seen South Dakota yet?

Fuerstenau: No, no, that comes the following summer. I had gone to South Dakota in the summer of '52. I guess I had talked in pretty good terms about Peg to my mother, who began to conclude that something might happen there. Thanksgiving of '52, I was invited to Franklin for the Pellett family Thanksgiving, something like three turkeys were carved and served. This was at the home of her Uncle Jack, who had been in charge of the New Jersey Zinc mill. I think there were seven: three sisters and four brothers made the family. One was a doctor, and one was a dentist in town, and her uncle who had run the mill for New Jersey Zinc. Anyway, I had a huge dinner—at that stage in life I really ate a full dinner. I wasn't told that at four o'clock we would go to Peg's grandmother's house on her mother's side in Sussex, about ten miles away, where there was another full dinner. I guess that was the occasion of my meeting all the relatives for the first time.

Swent: Made it kind of official.

Fuerstenau: Yes. I guess that was really so.

[Interview 5: August 8, 2001 in Berkeley, CA, Evans Hall]

[Tape 10, Side A]

- Swent: We had to stop hurriedly last time, and when we stopped you had just told about your wedding. Did you have a best man at your wedding?
- Fuerstenau: Yes. My best man was my roommate Bud Samuel. It was really through him I met Peg. One of the ushers was Jack Jordan. The other was Peg's brother Pete.
- Swent: Did you have a car in those days?
- Fuerstenau: Yes. Peg's brother had graduated in 1952 from Columbia in business administration. He was about to get into the army, and his father bought a car for him so he could tour the U.S. to spend the two or three months that he would have before he would have to report to the army. This was a 1949 Chevy [Chevrolet]. When Pete was done with it, I then bought that car from Peg's dad. That was the first car I had. Before that any use of cars was that of my roommate Bud. Wherever we went on some sort of excursion, I went with him, and once in a while he loaned his car to me. I had no money as a student.
- Swent: You didn't need a car in Cambridge.
- Fuerstenau: No. Especially if your roommate had a car. Weekends or something like trips up into New Hampshire, et cetera, during the fall to see the colors or to go to the beach in the summer, he always had the car available. Anyway, I acquired a car at that point.
- Swent: You got settled down in Cambridge, in an apartment right across from the Memorial Auditorium.

V **MASSACHUSETTS INSTITUTE OF TECHNOLOGY, FACULTY MEMBER, 1953–1956**

First Teaching Experiences

Fuerstenau: Right, and then I started my first term at MIT as a member of the faculty.

Swent: This was your first time teaching, wasn't it?

Fuerstenau: Yes, right. In fact, that fall, if I remember, I filled in on some of the lectures for a few weeks for Phil deBruyn who went to Holland to get married to Joke, pronounced like Yokeh. I had mentioned that Overbeek's niece had come with them to look after their children, and I guess Phil deBruyn started to look after her. He was gone maybe about a month or six weeks, so my first teaching was really giving whatever lectures that were involved while he was away.

Swent: What was that class?

Fuerstenau: It was on flotation behavior of minerals, if I recall. It was one of the mineral engineering undergraduate senior courses.

Our first dinner guest, shortly after we moved in, was Frank Aplan. Frank, whom I had known from South Dakota and Montana, at that time was working at Climax. He was considering what he might do, and I suggested he come to MIT as a grad student. He arrived there in the fall of 1953 to start a doctoral program. Phil deBruyn had a certain research project related to adsorption through the vapor phase.

During the year that Overbeek was there, he, Schuhmann and deBruyn ate lunch together and worked out an analysis that combined the Gibbs adsorption equation with the Young equation on contact angles. Their concepts showed that in flotation systems, adsorption at all interfaces was important, and Aplan was first to attack the problem experimentally. This was to measure the adsorption of slightly volatile hexyl mercaptan on gold both from the vapor phase and from the aqueous phase. I suggested to Frank that this might be a good topic for a thesis, and it ended up with Aplan actually doing this research for his doctor's degree under deBruyn, although he was part of this whole Gaudin group.

Earlier, you asked about pay for different jobs. I can tell you a little bit about pay as a starting assistant professor. This was, as I say, the fall of 1953, and as an assistant professor I got \$4800 per year. At that time, MIT considered these appointments as being on an 11-month basis, so no summer pay. What really irritated me, and maybe other assistant professors too, was that postdoctoral researchers got \$6000 per year. That, to me, borders on something

ridiculous—even to this day, I don't understand the reasoning that the postdocs got 25 percent more than assistant professors.

Swent: They were doing just research?

Fuerstenau: Just research. Here, today, a postdoc gets paid a fraction of what an assistant professor gets. Back then, that always sort of rankled me a bit. Maybe they figured the fact that the faculty people could do some consulting justified the difference.

Swent: Were there many postdocs?

Fuerstenau: There were two or three in our group, or four or five maybe. In other words, through this major project supported by the Atomic Energy Commission, that's what it was called then and now is the Department of Energy, that Gaudin obviously could support a lot of people, and did so for quite a few years.

Swent: How many assistant professors were there?

Fuerstenau: Two of us—just speaking in the mineral engineering group—myself and Phil deBruyn, and a year later, Dick Charles was a third one, after he received his doctorate. Gordon Bell, who had been assistant professor during the latter stages of his graduate student days, left to work at Alcoa. I knew that a lot of the regular academic pay at MIT came out of contracts and grants. Years later, Professor Chipman, after he retired, used to spend January and February in our department here in Berkeley working with Professor Hultgren. I remember one time talking with him about MIT, and he said at that time—this might have been thirty-five, forty years ago—that at MIT there were fifteen FTE [full-time equivalents] in metallurgy, and thirty-three bodies. In other words, there's thirty-three faculty people on fifteen university slots, which meant that everybody had less than half of their pay coming from the university, and more than half was coming from research.

Swent: With outside funding?

Fuerstenau: Yes, with outside funding. I don't know what it's like today, but that's always been characteristic of a lot of private universities in this country. I know when I was first here, all of the engineering faculty at Stanford were 50 percent paid on the university funds and 50 percent from research projects. You can see what happens—money gets tight all of a sudden and you're going to break out in a cold sweat and wonder where's your pay going to be coming from. Here at Berkeley, all pay's 100 percent from the University for almost all the faculty. I'm not talking about summer pay. I think that was a policy that was set long ago, and actually a good one in some sense. I think at Stanford a real effort was made through the years to bring engineering faculty full-time on university funds.

In our group at MIT, we also had a lab technician, and his pay was also \$4,800 or \$6,000, probably the latter. I, as a poor young assistant professor, was in a bind. In those days, \$100 went for rent, \$100 for food, \$100 for the car, \$100 for taxes. There went \$400 per month. Peg's dad paid her Radcliffe tuition; she was a senior now at Radcliffe. That didn't leave a great deal. I remember gasoline, typically 17 cents a gallon, or something like that. Even once, a couple of years later in New Jersey, gasoline was 13 cents, compared to today!

Swent: Big difference.

First Supervision of Graduate Student Research

Fuerstenau: My research supervision started with one person doing a master's thesis with me, Marvin Turkanis, who did a nice piece of work on the flotation activation of sphalerite with silver ions. In addition, a very good person who had done his master's thesis with deBruyn started working with me for his doctor's research. What we wanted to do was continue the kind of work that I had done: namely how do surfactants, these organic reagents, adsorb at oxide-water interfaces in relation to flotation. I wanted him to work with a mineral that would change its surface charge at an intermediate pH range. You take something like quartz, it's always negatively charged until you get to a very, very low pH, but alumina, corundum, sapphire, at just a slightly alkaline pH of about 9 the surface changes from positive at lower pHs to negative. So we could study how the charge on the surface would affect reagent adsorption and flotation. This first doctoral student was Harsukh Modi, who had come from India, Bombay as I remember. He's about the same age as I am. In fact, five years ago I saw him in India. He's still not retired and really a pleasant, sharp individual. I still remember, he wrote English about as well as anyone I've ever seen. I think he told me that he had gone to a parochial school in India; and from childhood, apparently, studied most things in English. His writing was really, really excellent. All the time I was there as a faculty member, he was doing his research and finished his doctorate in three years.

Swent: How do you measure these charges? [Phone interruption] Can you give me a sense of how you were measuring charge? Did you design special equipment or was it already available?

Fuerstenau: We actually measured potentials, not charges although we could calculate surface charge. With some modification, he used the very same equipment that I had used to measure the potential that results when a solution flows through a bed of mineral particles when a solution is forced to flow through the particle bed. This strips off adsorbed ions and sets up a potential across the ends of the particle bed that can be measured and used for calculating zeta potentials. This phenomenon is called "streaming potentials." All of this part of the field called electrokinetics that I think I mentioned earlier.

Swent: It does have to be in solutions?

Fuerstenau: I always dealt with aqueous solutions. These phenomena exist with organic liquids, such as with gasoline flowing out of a hose in a filling station which caused many disasters years ago until they realized what was happening. Another important example is that when sap runs up a tree, there's capillary pressure that causes the sap to rise in the tree. There have been people who have actually measured the potentials that result from this. Just stick two electrodes up the tree, and as the sap rises, it sets up a potential that people have measured. Or you can do the reverse: supply the potential and make the liquid move. That's been used to dry clay. Engineers have used this to stabilize embankments when they're building a road through a cut because it pulls the water out, leaving a bank that is dry and won't slide.

Another widely used application of electrokinetics is called electrophoresis. This is if you have a suspension of very fine colloidal particles and apply a potential, then the particles will move. If the particles are negatively charged they will move towards the positive electrode, and from the speed at which they move, and so on, you can again calculate zeta potentials that result from ion adsorption. Changes in the zeta potential reflect surface effects due to the addition of reagents to the system.

This is what I had done for my doctoral research. Then, while Modi was doing his research, I all of a sudden got the idea, going back to the quartz system, to change the number of carbon atoms on the chain of the amines. All of my work had been done with dodecylamine, which has a 12-carbon hydrocarbon chain. So I got samples of extremely pure amines from Armour Chemical Company. Armour meat packing was a big supplier of amines, and soaps, and fatty acids, produced from tallow from beef; you know they manufactured soap. I was able to get extremely pure amines that had eight, ten, twelve, fourteen, sixteen, and eighteen carbon atoms. So when Modi was not using the apparatus, I made measurements myself and I did it quite quickly since I had had three years of experience. Now what happens is that the hydrocarbon chain wants to get out of the water, and the longer the chain, the greater is the driving force for it to get out of water. As the number of carbon atoms in the chain was increased, I got these very regular breaks in the zeta potential curves at systematically lower concentrations of the amine salt in solution. That was very definite proof of the idea that adsorbed organic ions are aggregating like micelles in solution, or as we called them hemimicelles, at the surface.

I published the paper in 1956 in the *Journal of Physical Chemistry*, just on that basis, rather quickly. I wanted to get these ideas into the chemical literature. In my youthful exuberance, I made the title of this paper about a paragraph long, "Streaming Potential Studies on Quartz in Solutions of Aminium Acetates in Relation to the Formation of Hemimicelles at the Quartz-Solution Interface." I had Modi work on the alumina, because there we

could work with a solid that we could readily make either positively or negatively charged by simply changing pH. In the case of quartz, under normal conditions, its surface always is negatively charged so I could only work with an organic cation. With alumina, Modi could work with an organic anion, like a sulfonate, when the alumina is positively charged, and he could also work with the cationic amine when it's oppositely charged. That was really the main purpose of Modi's thesis research.

In all my time at MIT, almost daily I went into the lab, and talked, and discussed, and suggested what next might be done on all the research of all the people working for me. Of course, I was young, and not tied up with a lot of things, but in other words, I was kind of spoonfeeding the people. When I came here to Berkeley I decided that this really isn't the best way to direct graduate student research, and that people should do much more of it on their own. Here at Berkeley, in my group, we would have regular weekly meetings for group discussions. Once in a while I spent a lot of time with a given person, but I decided the best way to develop somebody was if they put a lot of their own ideas into their research. When I was a graduate student, I did everything myself; I never went to Gaudin to say, "What should I do next? Where do I get this? How do I do it?"

Swent: Did he check on you though?

Fuerstenau: Most of it, I think our interaction on what I was doing was when a visitor came through and he was touring the lab; then I would tell the person what I was doing. And of course we were writing either monthly or quarterly reports, quarterly, I think, that went to the AEC.

Swent: That's not very often.

Fuerstenau: In contrast to what I did daily, which, as I say, is not the way I think research guidance should be done. It's pretty easy to produce a technician that way. To me it's much better that the person learn to get something done and accomplish it mostly on their own. I've had people here who regularly came by and; in fact, asked one everything: "Where do I get this? How do I do that?" That's not going to create a self-motivated person, I think. I've watched here how different people handle students, through the decades. Most let the student be quite independent. One of them, an outstanding professor here, I recall, even in his senior years, was spoonfeeding his grad students, more or less. But at MIT in my younger days, I really was constantly involved with them. Also, I had a couple of people that worked on projects that were not grad students in our department. Two, three of them were actually graduate students in geology, but they worked as research assistants with me with funding from the AEC contract. That major project supported all of the research in the mineral engineering group during my years as a graduate student and first years as an assistant professor. By the way, I never had any idea of what the total dollar amount of that was. I was not involved in the

management of that project. Rush Spedden was project manager during my first year, and maybe deBruyn became that person later on. It never even entered my mind to ask about funding in those years—actually a nice situation to be in.

I was always interested in doing research that related to flotation. I got interested in what might control the rate of flotation, so I wanted to look at what was the velocity with which air bubbles move. So with one of the geology grad students, named Cooper Wayman, we built a tank and a device to hold and release air bubbles. I had seen some publications and reports from the navy that air bubbles move in tap water slower than they move in distilled water. We decided to look at this rather closely. There was a famous physics professor at MIT named Harold Edgerton, who had built a stroboscopic camera to take super-high-speed photographs. You may have seen some of his photos—I'm speaking fifty years ago—like a bullet going through an apple, or a droplet splattering. He had a famous photo of a milk drop that showed little droplets sprayed out in a ring when it splashed.

Swent: I think I recall seeing photos like that in magazines.

Fuerstenau: We borrowed a strobe camera setup from him so we could take a series of rapid pictures as air bubbles were rising to measure their rise velocity and record their shape. What's interesting: when a bubble is about one millimeter in diameter it rises at a rate that's about twice as fast as it would if it were a solid. What happens is the gas starts to circulate inside the bubble and that reduces the drag on the bubble. If you add just a little bit of a frothing agent or an alcohol to the solution, those frother molecules adsorb at the bubble surface and act sort of like a sailboat. They're swept to the bottom of the bubble, and this creates a surface tension gradient that stops circulation inside the bubble and slows the bubble down. It makes the bubble now act like a solid. I'm speaking of extremely dilute solutions. I published that in *Mining Engineering* magazine, and yet that was the very first time a study was done to show how adsorption can affect bubble motion. If I had been thinking a little broader I would have continued this work by systematically studying a series of alcohols of different chain lengths, but I didn't. I was just aiming at finding out what a flotation frother would do to the system. When you make big bubbles, they start changing their shape, looking like an umbrella. You have seen this if you look at the bubbles rising in a bottled water dispenser. We got a very nice piece of work out of that, but I never—

[Tape 10, Side B]

Fuerstenau: I never continued it. A very good publication came out of it. Here was an interesting phenomenon that I should have pursued and published in a chemical engineering or fluid mechanics journal; other people have done it since.

- Swent: What sort of reaction do these papers get? Were you aware that these were important papers? Did people get in touch with you as a result?
- Fuerstenau: In the old days, before the Xerox machine, et cetera, people always wrote and asked for reprints. Once in a while people still do, but now much of the technical literature is available on the Internet. You could get an idea of the interest from who had written and asked for copies of the paper. I always had lots of requests for reprints of papers from all over the world. A lot of my papers got into AIME publications. Of course, they were picked up by *Chemical Abstracts*—which was published by the American Chemical Society. AIME papers are included, as are papers from a lot of other journals. Sometimes some of those professional journals may not be indexed this way, unfortunately. Of course in more recent years, some people give a lot of credence to the *Citation Index*. As you well know, people have tabulated how many times this author and this paper is cited by other people. The first time I heard about that I thought that was a little odd, but some agencies use that as their guide, I think. With the Internet, all of this is now readily available.
- Swent: It gives you a way of knowing how important your paper has been.
- Fuerstenau: The people working in the field would be reading them. These two things that I talked about were more broadly based research.
- Swent: It must have caused quite a little excitement.
- Fuerstenau: Yes, right. But like I said, I was always aiming at how to explain flotation phenomena. A little later I also supervised the BS thesis of an undergraduate, Joseph Scheller, who conducted an interesting detailed study of foaming or frothing phenomena. I had him investigate the growth and decay of foams of isoamyl alcohol in pure water. Gaudin included a couple of Scheller's figures in his *Flotation* textbook, but I never published a paper on that work—probably because of my leaving MIT. It's still interesting and maybe I'll write it up yet.
- But anyway, in the fall of '54 some new graduate students arrived—I suppose each year there were two, three, four new grad students. That fall, two very good people who did very well through the next decades arrived. One was George Parks, who had received his BS and MS degrees in metallurgy here at Berkeley and had come to MIT to do a doctorate. He had done an MS thesis with Professor Ravitz here. At MIT, Parks carried out his thesis research with deBruyn. Another very good person who arrived at the same time was Iwao Iwasaki. He's Japanese, did his BS and MS at the University of Minnesota. He told me that some family had a son who had come home to Minneapolis and was killed in a taxi accident on his way from the airport. With the insurance, the family set up a scholarship program to bring someone from Japan to the U.S. Iwasaki came as a junior from Japan to the University of Minnesota, and was undergraduate and master's student there. Then he came to MIT in the

fall of 1954. His nickname was, and still to this day, is Pete. A funny little story. He said when he came through immigration the immigration officer, American, said, “Your name is Iwao. Iwao means rock, so I’m going to call you Pete.”

Swent: Pretty learned.

Fuerstenau: Pete stuck. He’s always been Pete Iwasaki to us. By the way, he was another person whose writing of English was always excellent. He and Modi were there at MIT at the same time, and Pete Iwasaki’s written English was very, very good.

Swent: I think sometimes this happens to people who have learned English overseas, more academically, and they do learn it very well.

Fuerstenau: As you probably well know, the problem with Asians is mostly with articles—they leave them out. When they start putting them in, they know they have to go somewhere so they’re often a little bit random. [laughter] Iwasaki’s later whole career, or most of it, was at the University of Minnesota. He became very extremely well known for a lot of aspects of the surface chemistry of iron ore flotation. Actually he returned twice or thrice to Japan, but he had become so much of an American that he always returned. On the first occasion, he worked for Fuji Iron and Steel and believe it or not at Fuji they said, “You really need a Japanese doctorate.” He then wrote another thesis that gave him a Japanese doctor’s degree. That was more important to the Japanese than his MIT doctorate. He was there a few years, then came back, and rejoined the faculty at Minnesota, was there a long time, and when a well-known Japanese professor Wada retired from Tohoku University, he was invited there to be the professor. He was at Tohoku one month and the Japanese students paraded through his office protesting nuclear subs in Okinawa. He said he got on the telephone, called the people up at Minnesota again, and said, “Can I come back? Is my position still there?” He returned again to the U.S. When he roughly retired—he’s my age—he went to Japan, worked five years for Mitsubishi. Then when he hit seventy Mitsubishi retired him, and he has come back again to University of Minnesota but up at Duluth, where they have an iron ore research facility. His family—his wife and three children—were in Minneapolis for quite a while—but they didn’t like that and moved back to Japan. All these years, he makes the trip back to Japan fairly regularly. He’s a great, outstanding person in iron-ore flotation and in other methods for processing ores.

In the fall of 1954, Gaudin wanted me to develop a course on ion-exchange methods and ion-exchange phenomena, because ion-exchange resins were used to extract uranium from leach solutions. I developed the course and taught it at least once, maybe twice. It was an area that never really fascinated me, so I never pursued that.

Let me back up to the spring of '54. I was on the faculty and remember going to a faculty meeting where the faculty then voted on the award of the doctor's degrees for the coming commencement. So I voted on my own degree. [laughter] However, my doctoral diploma says August something 1953, but I attended the commencement in '54. MIT had a little enticement that if you attend commencement in person they give you the hood for free. So I participated in commencement. At that time Dick Charles, who I mentioned earlier, also got his doctorate. Then he too became an assistant professor. That would be June '54.

College Graduation of Peg Pellett Fuerstenau and Family Life

- Fuerstenau: Peg graduated from Radcliffe that spring. That was a great occasion. The commencement speaker was George Kennan, whose daughter, Grace, was also graduating at that time. Peg's father was there, but her mother was not well.
- Somewhat in the way of insurance, Peg and I decided that she should continue going to school. She decided to go to Simmons College to get a master's degree in library science. So that year of '54-'55 she was a grad student in library science. I remember her saying that there wasn't much science to it. Her one-year program at Simmons led to a master's degree. We thought, well, that would be a fallback if ever she needed to work.
- Swent: Let me ask a very personal question. Was contraception still banned in Massachusetts at that time?
- Fuerstenau: So many things were banned in Massachusetts then. In fact, in those days nothing could be sold for birth control in Massachusetts, only for the prevention of disease.
- Swent: You were obviously delaying your family.
- Fuerstenau: Yes, for several years.
- Swent: It might have been difficult in Massachusetts. Did you have to go to New York or something?
- Fuerstenau: We drove down to New Jersey fairly often, holidays, and so on. Massachusetts had so many negative laws; they were called Blue Laws. At that time, on Sunday the bars even had to take the seats off the bar stools.
- Swent: I'm surprised they were even open.
- Fuerstenau: They were open but the bar stool seats were off; you could only sit in the booth. Here would be the stand, sticking up like a post, and no bar stool seat. I

suppose it was all due to the Irish Catholic domination of Boston and Massachusetts in those days. Things like the pill didn't come in until we were long gone. However, condoms were legal—to prevent the spread of disease.

Swent: I think contraceptives were not available for sale. I don't know, because this was a few years after I was there.

Fuerstenau: It would have been the most rigorous enforcement of any state in the country.

Swent: Connecticut too.

Fuerstenau: Probably not as bad as Mass[achusetts]. Our first offspring wasn't born until '57.

By the way, one of my first duties in the fall of '53 was making an undergraduate student recruiting trip. MIT did a lot of freshmen student recruiting then. I was sent for a week, Monday through Friday, visiting three prep schools or high schools around Philadelphia suburbs for a whole week. I remember there was a Germantown Academy, Haverford Academy, and I think there's a Haverford Union College that's well known there. I would give the spiel about MIT to a whole room full of—

Swent: Boys.

Fuerstenau: Boys. There were girls there too. A lot of those prep schools were just boy's schools, weren't they? The general high school, of course, had both boys and girls.

Swent: Did MIT have any women at that time?

Fuerstenau: There must have been, but I just have no recollection of any women in any class I ever had. Anyway, I spent this whole week driving a rental car around the northwest suburbs of Philadelphia, finding the schools, and getting there on time. There would be two in the morning, one in the afternoon, or one in the morning, two in the afternoon. I visited fifteen places that week. At that time, MIT did the same kind of recruiting visits at many of the major cities around the country. I doubt if they do that kind of recruiting today.

Swent: You mentioned renting a car. South Dakota residents couldn't rent cars at that time. Were you a Massachusetts resident then?

Fuerstenau: I told you how driving back as a grad student when South Dakota did not require a driving license, and how that was so incredulous to the highway patrolman in Mass[achusetts]. I, needless to say, by then had a driver's license.

Swent: You got one in Massachusetts?

Fuerstenau: I had to and did. You were asking about postdocs. There was an Egyptian postdoc who was probably thirty-five-ish then, Mohammed Halfawy, who was conducting an investigation of the copper activation of sphalerite with Professor Gaudin. He drove a car like he was driving a wild camel. He came back and told us about how three times he had totally flunked the driving test. Obviously it's a good thing he flunked.

Once new freshmen students arrived, MIT took an interest in acclimating them socially. A couple of different times, another assistant professor named Al Backoffen and his wife and Peg and I hosted some kind of evening dinner at MIT for new metallurgy students, in the laboratory building where there was a foundry research facility. Tables were set up there and the dinner catered.

Swent: This was a social event?

Fuerstenau: Social event. I must have been assigned by the department head, two assistant professors and their wives, to do that. By the way, that first fall when we lived at 1734 Cambridge Street, the administrative assistant of the Metallurgy Department lived about a block away in a large apartment building. She must have been a woman then about fifty and I was twenty-four. She handed out the parking stickers, and I went to her to get a parking sticker. Of course she knew where I lived, which was one block from where she lived. You know what she said? "I take the bus; you take the bus!" End of my request for a parking sticker. [laughs] Shows you who had the power. I have never forgotten that.

Swent: You were in walking distance, weren't you?

Fuerstenau: Sure, a mile and a half, maybe a mile and a quarter. Then it turns out another slightly older grad student who had worked earlier for this Watertown Arsenal uranium project of Gaudin, named Will Freyberger, lived in Melrose, a Boston suburb. Well, by mistake, he got two parking stickers. He gave me one of them, and that was my parking sticker. But the ad[min.] assistant wouldn't give it to me. Quite often, we played bridge with Freyberger and his wife, who also had worked on Gaudin's uranium project at Watertown Arsenal.

Swent: How did Peggy get to Simmons?

Fuerstenau: She drove our car, first dropping me off at MIT on the way to the Simmons campus along the Fens in Boston.

Swent: And to Radcliffe first?

Fuerstenau: We were right on the very edge of the Harvard campus. Some of the lecture halls were only a block or so away. At Radcliffe the dorms are about six blocks up Mass Avenue from Harvard Square. Of course you could go by

streetcar, but that was an easy walk. The Radcliffe library, and two, three other Radcliffe buildings are near Harvard Square, but no classes were there. I think it was just a library, and maybe Radcliffe offices. All classes were at Harvard. By the way, I don't recall ever being at the Simmons campus.

Swent: Life was pretty hectic. You mentioned entertaining. What sort of social life were you having with all of this? Did you have parties?

Fuerstenau: With some of the other assistant professors we got together for dinner. I remember a Halloween party at one of them—that sort of thing—plus playing bridge. People don't play bridge out here very much, whereas around Boston in winter, having dinner and playing bridge was quite common. That first year, two of Peg's best friends were also newly married, so we would be back and forth for dinner once in a while.

Swent: What did you eat?

Fuerstenau: I'll tell you one thing we ate. One of her friends, Ronnie Strauss, Schwab was her married name, served tunafish casserole. The next day at home Peg served tunafish casserole, and I said, "Never again will there ever be tunafish casserole in our house." There has never ever been tunafish casserole. I think that was probably the typical guest-type of meal for student guests. [laughter]

Swent: This was before pizza. How about drinking?

Fuerstenau: Plenty of beer—little hard liquor. The first time we ever bought a bottle of wine was after we moved to Buffalo. I do remember playing golf one day with Will Freyberger, which I did with him a few times. When we came back his wife mixed some red wine with soda water, very dilute, which is a very nice drink on a hot and humid Boston summer afternoon. We played bridge with the Freybergers quite a bit, and might have had a little bourbon or something. I don't think we ever owned any bourbon. I remember having bourbon with Freybergers because one time Will commented on the fact that ice cubes in bourbon and water do not fuse together. Ice cubes always sinter together in a glass of plain water.

Swent: We all have noticed how ice cubes behave in a glass of water.

Fuerstenau: Freyberger commented that a ceramics prof at MIT—much of ceramics involves the sintering of particles at high temperature—discussed this in a lecture. The alcohol molecules adsorb on the surface of the ice cubes and that prevents their sintering. Anyway, at 1734 Cambridge Street, I tried making—letting the cider ferment. I set it up in the manner that Rush Spedden had described. I had it arranged so that I could put the apparatus in the bath tub and let it gurgle away when we were out. We came home late one afternoon and the whole thing had blown up, exploded to the ceiling. [laughter] Never tried that one again.

Swent: You mentioned dropping in for a beer.

Fuerstenau: Must have a bit. I used to go out drinking beer with Jack Jordan and so on when I was a grad student. It was just minor. It just wasn't part of our existence, I guess.

Swent: Did the professors entertain you? Did they invite you to their house?

Fuerstenau: Yes. We were several times at Professor's Gaudin's house. He lived out in Newtonville. Once or twice a year he would invite all of the group, grad students and postdocs, to his home on a Sunday afternoon. Later on when I was an assistant professor, on several occasions he might invite distinguished visitors to his home for dinner, and I remember more than one time we picked up the visitors and drove them to Gaudin's home and brought them back to Cambridge. Four of the younger faculty and their wives seemed to get together every little while to play bridge. Several times, small groups or the entire departmental faculty were invited to the home of John Chipman, head of the department. He had a garden about a half city block or more in size. One other occasion was a cocktail reception at the elegant home of John Norton, filled with a number of Greek statues and other forms of art. I think he probably married well. He lived right near Harvard campus in a beautiful house.

[Tape 11, Side A]

Fuerstenau: I got started going to concerts a bit. In the spring of my last year as a grad student, the Met [Metropolitan Opera] came to Boston and I remember going with Peg to see *Carmen*. That was the first opera I had ever been to and of course it was magnificent. I still remember Carmen and all her cohorts instead of being dressed in red they were dressed in yellow, bright yellow and white dresses. This left an impression on me, I guess. Then, in the first year that we were married, we got tickets to a series of operas produced in Boston by Boris Goldovsky. Often you heard him on the Met broadcasts. That one year he produced four operas in English. I suppose that got me interested in opera. We didn't have much money, but we could afford to buy the tickets for the rehearsals of the Boston Symphony. I bet one didn't pay very much for them. Charles Munch was the symphony director at that time. For a couple of years, we went regularly to those final rehearsal concerts. I remember Professor Gaudin regularly attended the Saturday night symphony concerts. Then Phil deBruyn, after he got married, followed Gaudin, and they too had Saturday symphony tickets. We never went to a regular Boston Symphony concert, but we did go to the rehearsals, which led to a continuing interest.

Swent: That's one of the nice things about Boston and Cambridge, isn't it?

Fuerstenau: Oh yes. Of course the Bay Area's equal to that, if not more so. The San Francisco Opera is probably number two in the country, and there is no opera

in Boston. Boston Symphony, as you know, has always been almost number one in the country. Of course in the summer, there was the Boston Pops. Peg had an uncle who lived in Newtonville, and her aunt was the youngest sister of her dad. The Eades took us to Boston Pops concerts in Symphony Hall a couple of times. Her uncle, Fletcher Eades, was an obstetrician who delivered all the offspring of Arthur Fiedler, the founding director of the Pops. I believe that he had been a resident or young doctor up in Hamburg, New Jersey, where Peg's grandfather was a retired doctor. What I find interesting is that her grandfather got his MD from Columbia University in 1872.

Swent: That's early.

Fuerstenau: He was born in 1849. Her father was sort of the tail end of a large family. It almost seems like a missing generation that he did his MD in 1872. Anyway, the Eades had a place down at Buzzards Bay on the shore, so we were down there every once in a while, visiting them for a weekend or so on several occasions. That was another nice type of social activity.

Swent: What did you do in the summer?

Fuerstenau: I mentioned that MIT appointments then were called eleven-month appointments. You were expected to be there in the summer with one month vacation.

Visit to South Dakota in the Summer of 1954

Fuerstenau: In the summer of '54 we drove out to South Dakota.

Swent: That's a long drive.

Fuerstenau: Right. But that gave us a chance to see the country. On the way out, we spent one night in Chicago with Peg's cousin, Linda and Larry Ziff. Larry was working on his PhD in English at the University of Chicago. For many years he was on the faculty here at Berkeley in the English Department, later going to Oxford and then to Johns Hopkins. We stopped and spent about three days on the farm where my grandmother and one of my dad's brothers who at that stage in life still hadn't gotten married; he later did. That gave Peg a chance to see farmland. I think she had never been west of the Delaware Water Gap until that trip.

Swent: Imagine that. It must have been a very new experience for her.

Fuerstenau: She had never been west of New Jersey. I remember we were in Blue Earth, Minnesota. As you come across southern Minnesota, you go through Blue Earth and Austin, one of which we stayed in overnight. In the middle of the night there was the biggest banger—absolutely frightening thunder-crash-

bang-lightning thunder storm that I've ever been in. When we drove away the next day about a half mile down the road, there was a big tree that had been split by the lightning and was halfway across the road. It was not too many miles to Watertown, in the vicinity of where all my farmer relatives were; and both grandmothers were still alive.

Swent: You hadn't been home for a long time.

Fuerstenau: Not really, right. We drove around the Black Hills a lot. It was a nice vacation. My brother Maurie had gotten married only two weeks after we did. So we met Joyce, his wife. We were probably in South Dakota for two, three weeks. For some days, with Maurie and Joyce, we drove out to Yellowstone Park and camped in a tent. We spent a few days in Yellowstone Park, and then back to Rapid City. We were there in Rapid, had a nice time, and started the trip back east again. On the way back we went by Spring Grove, Pennsylvania, where I spent a few days again with P.H. Glatfelter working on this problem of getting the Scotch tape out of waste paper pulp. That project was still going. So we were probably there three or four days. By the way, this was the time when environmental pollution things weren't an issue. The smoke, not real smoke, but a white vapor, poured out of that plant. You've been around paper mills, maybe, and they just plain stink. That white cloud went to the southeast which was up on the hillside area where all the fancy houses of the managers were, including Glatfelter's. They picked the direction where the prevailing wind went. Before we got married, I spent a full week there or more, working there with the research group, and stayed in a room at the home of one of the former managers—at the house of the widow of a manager, as I recall. Anyway, that nice home was right in a direct line of this awful foul white cloud. Just a couple of years ago we drove over there to Spring Grove and this Glatfelter plant is huge now and you hardly smell that there is a paper mill and there's no fumes or vapors.

Swent: There's some progress then.

Fuerstenau: There's progress. When we got back to Cambridge, we looked for and rented another apartment late that summer, 11 Ellery Street. It was sort of half-way between Harvard Square and Central Square, and of course Peg went to Simmons; we were still close in.

Swent: How did she choose library study?

Fuerstenau: At Radcliffe she had majored in American history. She had served on a library committee and enjoyed talking with the librarian. She did not want to join a company and start that sort of professional career and she wasn't interested in teaching. However, her classmate, Ruth Spindle, wanted to teach and was taking a few education courses at Harvard.

Progression of Events at MIT

Fuerstenau: Getting back to life at MIT, one time, for example, Gaudin asked me to do something—some task related to the research program; I’m not talking about detail, but maybe administrative or something related to the group; I forget what. DeBruyn went running into him, “Why did you ask D.W.F. to do that? I should be the one to do it!” Obviously, deBruyn was looking at me as competition. At that time, he could be a very dour person. As time went on, it got so that if I would just see him in the hallway my stomach started to turn. Beginning about in ‘54 I started to eat Tums, about like people eat candy; I probably went through two or three rolls of Tums per day, and it was all over deBruyn. If I would just see him walking, a knot would get in my stomach and I would grab the Tums. That was not creating a very good situation. I began to think that if there were room for one person in the department, deBruyn was here first, he was doing good work, very competent and dedicated, so he would be the one to stay. So I never looked at him as competition, but he considered me competition.

Another opportunity arose for some diversion. In the fall of 1954, some visitors from Austria came to our group in mineral engineering. I was invited to apply, by them, for a Fulbright [fellowship] to go spend the year in Vienna. Peg, to this day, says we should have done it. I was just getting started, and Vienna then still was occupied by the Russians, so I didn’t think this was the safest place.

Swent: It wasn’t all just waltzes.

Fuerstenau: Right. I thought about it not very long, but I decided that I really wanted to get my show going at MIT. It could have been interesting, but I decided not to do it and stay there at Cambridge. Like I say, Peg, even to this day, says we should have done it, but I didn’t.

Early Interviews for a Faculty Position at Berkeley

Fuerstenau: My fellow graduate student, Dick Charles, graduated in June 1954 and then was appointed as an assistant professor. Because I was already an assistant professor, I had to serve on Charles’s thesis oral. So I studied his thesis so that I could ask intelligent questions. His thesis was an innovative study of the efficiency of transferring energy by impact to break brittle materials. He continued a research program on comminution in the area of fracture and on grinding in ball mills and also developed an interesting graduate course on comminution. Then in February of 1955 both he and I got invited to come out here on an interview trip to Berkeley.

Swent: You mean to recruit students.

Fuerstenau: No—to interview for a faculty position in this department. I went to an AIME meeting which may have been in New York or may have been in Chicago, and then flew out here at that time with Lysle Shaffer who had been the mining prof[essor] at South Dakota that I told you about. He, in 1952, moved here to Berkeley as professor of mining. Dick Charles, but I didn't know it at the time, had also come out interviewing here, and Charles who's three years older than I am—said, "I'll come, but I'll have to be associate professor. I won't come as assistant professor." He was just in his first year of being assistant professor, but he had worked in gold mines for two or three years up in Canada before he came to MIT. They wouldn't make him an associate professor and he turned it down. I learned all this later. I was here roughly the same time. Earl Parker was the department chairman, and he was the person who saved this department in the early fifties. There was an associate professor here who had tenure that he wanted to squeeze out of here.

Swent: "He" meaning Parker?

Fuerstenau: Yes. Parker was the chairman. They were recruiting for a person in the area, mineral processing, but at the same time squeezing out somebody else. At Berkeley if you're an associate professor you have tenure; so this person had tenure and they wanted to get rid of him. He and Professor Ravitz and Hultgren sort of made a kind of triumvirate. I gave a seminar, talked to various faculty, and all that stuff you do on an interview.

Swent: They were trying to recruit you to come here?

Fuerstenau: Me! To become a junior member of a faculty group that contained a tenured faculty member that they were trying to get to leave. I decided that this was just a can of worms. When I left here—those were the days of those propeller Convair airplanes—I flew to Rapid City and got as far as Casper, Wyoming, and a blizzard hit and we had to land. Because of the snowstorm we had to stay overnight in Casper and went the next day to Rapid. Here I got stuck in the snow there, and you know what it's like here in February, lovely, and so on. When I got back to Boston, you know the weather aspect of California never even entered my mind. I decided that I didn't want to get into any situation like what I just told you about. I wrote and said, "I don't want to be considered for this position." That was a very wise thing. If I had entered into that environment as a young person, I don't think my career would have ever developed the way that it did.

Swent: There wouldn't have been enough Tums anyway.

Fuerstenau: Oh yes; right. The next year my pay was raised to \$5,000 from \$4,800. [laughs] Hurrah! Dick Charles, whose wife had had a baby, said, "I can't live on this." So he went out to interview a few places. Dick is a very spirited person. I remember he interviewed at Union Carbide. He said that one part of the interview was at Niagara Falls, but he also went to New York and met Gus

Kinzel, corporate vice president of research for Union Carbide. Kinzel was a great giant in AIME and one of the founders of the Engineering Academy. He was a metallurgist. I still remember when Dick said, "I went into his office and in contrast to my office here, absolutely clean and the telephone was in the drawer." There indeed are some clean-desk people. Dick also interviewed at DuPont and General Electric, and he went to work for GE, and left his role as assistant professor at MIT, just after a year. By the way, he had a very good long career in the Research Lab of GE. At MIT, he was breaking things, so he, at GE, did a lot of work on the strength of glass, and crack propagation in glass, and the electrical properties of glass, and the conductivity of glass. He stayed in the GE research lab his whole career, in Schenectady, and I think was eventually the manager of ceramic research for GE. That brings us, I guess, to—

Swent: You decided then to stay on?

Fuerstenau: I was still there, yes. That now is bringing us through the summer of '55.

Research That Led to the Electrostatic Theory of Flotation

Fuerstenau: Then, as I said, I was always out in the lab a lot. My first graduate student, Harsukh Modi, had been working for almost two years on the surface behavior of aluminum oxide, corundum. This was synthetic sapphire because I wanted the oxide to be as pure as possible. Hydrogen and hydroxyl ions provide the surface charges on oxides. Corundum was interesting because it is positively charged below pH 9 and above pH 9 the adsorption of hydroxyl ions causes the surface charge to become negative. So one day I went into the lab and said to Modi and Pete Iwasaki, "I want you to add some particles of this very pure corundum to water in two small flasks at pH 7, (you know water's neutral at pH 7) and in one of them you put in an amine (that's the organic cation) and the other you put in a sulfonate (that's the organic anion)." I also told them to make two more suspensions of the corundum and adjust the pH to 11 and again add the amine to one flask and the sulfonate to the other. I said to them, "At pH 7 because the corundum's positively charged it is not going to float with the amine and you're going to see it float with the sulfonate. At pH 11 you'll see it will not float with the sulfonate but will float with the amine." Both Iwasaki and Modi said, "No, no, that's not going to be the case at all." Well, that's exactly what happened. I had conceived of this idea as a way to make flotation separations of minerals by regulating their surface charge. If the mineral's positively charged, it will absorb a collector that's an anion; if it's negative it will absorb a collector that's a cation. That's how that concept first came about. In fact, of all flotation systems, that's the easiest one to explain—namely what is happening during the flotation of oxide and silicate minerals. In retrospect it is so simple; yet it was not until about 1955 that the idea hit me as to this must be what is happening. It's very nice to use physical chemistry to understand why a mineral separation is

possible. A mineral like quartz changes its charge at maybe pH 2, we'll say, and iron oxide maybe around pH 7. The pH at which the surface charge reverses is called the zero point of charge, the pzc. You can see just by regulating pH you can make the two minerals negative, or make one negative and one positive, or if you go to very low pH, both are positive. That's why by flotation, we can nicely separate iron oxide from quartz. Sulfides are very complex; that's another story. Anyway, years later—

[Tape 11, Side B]

Fuerstenau: Years later, when Frank Aplan nominated me for the Gaudin Award, he wanted to stress that this concept was my specific achievement—namely that conceiving the idea that controlling surface charge and using an oppositely charged collector can lead to successful flotation separations. In his nomination, he called this the electrostatic theory of flotation. All of this came out of working with very pure systems and realizing that a flotation collector will go to the surface of an oppositely charged mineral due to electrostatic attraction for systems without chemical bond formation.

Swent: When did you have this idea? Did it come to you in the night or in the bathtub?

Fuerstenau: That one probably evolved slowly. It may have been lurking in the background of my mind when I suggested to Modi that he conduct his research with corundum where you can readily control the sign of the surface charge. Anyway, sometimes we do think of things at night, or often things hit me while shaving, that's true, or they may slowly evolve. I suppose this is what creativity is all about. In my dealings with researchers in my career in education and graduate research, you really can see whether somebody is creative or not. I've seen some very, very bright people who are not creative—they can't nucleate an idea. I've got some very specific examples that I can think of, extremely bright people: if somebody suggests something to them they can work that to an exotic peak. I've always been a person who can just read a paper or listen to a seminar and come up with a lot of things that might be done. I've never had a shortcoming of new ideas. I've already told you two, three, four different things that I worked on at MIT that first year or two that all of a sudden hit me to work on. In retrospect I wish I had pursued some of them a little further.

When Pete Iwasaki returned to the University of Minnesota as an assistant professor, he used those concepts and really did a lot with this approach in his research there. He and Professor S.R.B. Cooke and their graduate students wrote many papers on the surface chemistry of floating iron minerals, since iron ore is important in that state.

[Added by Douglas Fuerstenau during editing: One of those graduate students whose thesis was concerned with the surface chemistry and flotation of a

number of pure iron oxide minerals was H. S. Choi, who returned to Korea and has been recognized as the person who changed Korea from an agrarian country to an industrial country. Dr. Choi founded the Korea Institute for Science and Technology; he was behind founding the Korean National Science and Engineering Foundation. For eight years he was minister of science and technology for Korea under President Park. He spearheaded the establishment of a steel industry in Korea, amongst other things. I had met him when he was a graduate student at the University of Minnesota, but I don't recall that. But I did see him on three different trips to Korea during the last twenty-five years. The last time I saw Dr. Choi in Korea, he talked about his thesis studies that had led to those several papers on iron ore flotation—and some later ones on kyanite flotation—and he said to me, “All I did was follow your ideas.”]

Our paper containing Modi's first experiments carried out in 1955 and '56 on these concepts was not published until 1960, the same year that Cooke, Iwasaki and their various coworkers published their own results. Thinking back on it, between June 30, 1956 and about September 1, 1959, I had four different jobs at four different locations, so I guess my writing that work up got pushed into the background because of those job moves. By the way, in several review papers over the years, I used to use Iwasaki and coworkers' figure for the effect of pH on the flotation of goethite with anionic sulfonate and cationic amine collectors because the behavior was so perfect—never being concerned then about credit for the origin of that flotation model.

Discussions with John Elliott Leading to an Industrial Job with Union Carbide

Fuerstenau: As I said earlier, deBruyn was always upsetting my stomach. In the fall of 1955, a new faculty member came by to MIT as an associate professor. I'm speaking of someone who became a very big giant in process metallurgy named John Elliott. Elliott had been a protégé of Professor Chipman. After he finished his doctorate, Elliott had worked, I think, a total of seven years for Inland Steel Company in their research department, and I assume probably had published papers on steelmaking while there. I went to talk to Elliott a lot because I was interested in an academic career but I wanted to know his thoughts of going to industry for a period before continuing in an academic career. That idea began to jell with me. In fact, all through the many following decades, whenever I would see Elliott at a meeting or conference, I always had much interaction with him. I don't have any idea how old he—he's probably at least ten years older than I am. I used him as a big sounding-board.

Swent: Having someone like that for counsel was very valuable.

Fuerstenau: Yes. The fall of '55 had another memorable event. First Hurricane Carol came through and then a week later Hurricane Diane. The eye went right over Boston. Have you ever been in a hurricane where the eye—?

Swent: No.

Fuerstenau: It really hit. I remember looking out of the window from a lab and a row of tall poplar trees just folded over onto the cars. After about an hour, hour and a half, all of a sudden the sky was blue and perfectly quiet; the eye was right there. Then a half hour later it hit again but in the other direction as it passed over. That really messed up Boston and the whole region. Trees were down all over. Peg's aunt and uncle had gone for a month's vacation to Europe; so we were living at the Eades's house for that month or six weeks in Newtonville looking after their teenage daughter. I remember it was a tough thing to get out there that evening; of course no power. Diane hit a week later and it was bad there also, but the eye of the storm did not come directly over.

This hurricane did a lot of damage on the shore, though. Eades had a two-story house down on the Cape, Buzzards Bay, and it just disintegrated. It was blown or floated about half or three quarters of a mile inland. You always hear about the 1938 hurricane that had hit there. Apparently, all the nails holding the house together had rusted after being washed with sea water by the '38 hurricane, so when it got knocked by the waves in '55 it just disintegrated the lower floor of their house. Rusting occurs when you get an oxygen deficiency and is greatly enhanced by salt water. That was quite a memorable experience. Eades rebuilt their house farther back from the shore and on somewhat of a higher rock ledge. All of this is part of living there on the seashore.

It was that same fall that John Elliott had arrived and I talked with him fairly regularly about going into industry. As I said, Rush Spedden had gone to Carbide, and it must have been January or February that I received a letter from the research director of the Metals Research Laboratories of the Electrometallurgical Company inviting me to come out if I was interested in interviewing, which I did.

[Tape pause]

Fuerstenau: This was Union Carbide. The metals division was called Electrometallurgical Company. Only much later was it called Union Carbide Metals Company. Their research labs were in Niagara Falls. This was a very large research lab built next to their huge ferroalloy plants; they made ferrochrome, ferronickel, ferromanganese using electric furnaces. Of course the reason for that was the electric power from Niagara Falls. That's why the whole region consisted of just one electrochemical plant after another. Just a year before we moved there, the cliff below the American side of Niagara Falls caved in and took out a large part of a huge power plant, so they lost a large part of their power generation. Of course these were plants that were built at the turn of the

century, so the cost of their investment was nothing. Power was very, very cheap. Interestingly, the old power plant generated 25-cycle current, so lights would flicker in the lab— Once the power plant had its problem—[phone interruption]

I was invited to come out on an interview trip and decided to do that. This was probably February of '56 and we decided to drive out to Niagara Falls. Peg was working as a reference librarian in the Watertown, Mass[achusetts] library. I remember when she was looking for a job she went over to Harvard and they had a fancy brochure about working at Harvard. She was offered, at Harvard, a position as a librarian at \$2,900 a year, and this brochure said, "Harvard means more than money!" [laughter]

So anyway, she took off a few days, and we drove to Niagara Falls. Along the New York Thruway, we started to hit a heavy, heavy snow storm. Finally we left the freeway and were able to get a few miles down to a place called Canandaigua and stayed overnight in a hotel. The next day we had a nice simple straightforward trip to Niagara Falls—clear sky and sunny. My recollection is more how dreary it looked around Niagara Falls. I really don't remember much about the lab interview with people, but I do remember dreary small, flat, gray houses that seemed simply gray; Buffalo is gray and flat. We were there a couple of days. I remember Rush Spedden driving us around, showing us where people lived, the Falls, and that sort of thing. Then I went to New York and had an interview with the vice president of research of Electromet, Art Lytle. In fact he was the VP of research all the time I was with Electromet. Then I was offered a job to be section leader of the Mineral Processing Section in the Minerals and Chemical Engineering Group. Spedden was group manager. My pay at MIT was \$5,200 and Carbide offered me \$9,600. It was a good, big change.

Swent: Not a hard decision to make.

Fuerstenau: No. After all my discussions with John Elliott I decided that I would accept that and go there, which I did, without the idea of that ever becoming a permanent career route. Not once, not even once, did I ever discuss with Professor Gaudin anything about my staying at MIT. Thinking back on that seems odd to me now. I had drawn my own conclusions in relation to my career and proceeded accordingly. Of course, Rush Spedden would have cleared with Professor Gaudin their inviting me to Niagara Falls for an interview, but that thought never occurred to me for many years.

Summing up Research Accomplishments at MIT

Fuerstenau: Before moving on to my years with Electromet, I want to talk some more about some of my research accomplishments at MIT. Towards the end of the spring, all of a sudden I got an idea about what might make an interesting

flotation reagent for chrysocolla; that's copper silicate, and it's very difficult to float—almost impossible. My idea was to use a chemical called a mercaptan, which is like an alcohol, but where a sulfur atom is substituted for the oxygen atom in an alcohol. Mercaptans will chemically bond with metal ions in the mineral surface. This, of course, is totally different from flotation of quartz with amines where the collector adsorption is purely electrostatic to start with. Aplan and I, about the last day or two I was at MIT, including the last night, were working in the lab, running experiments on the flotation of chrysocolla with hexyl mercaptan. This was the same chemical that he was using for his research on mercaptan adsorption on gold. I had a flask that I was pushing a stopper into it and it broke and I carved out a piece of my finger, bad carving, just sliced it; it wasn't a slice, it scooped it out. I ended up getting into emergency and having it sewed up my very last night at MIT. Aplan continued to work on this flotation idea for some time more. I guess Gaudin got a little bit involved, thinking that it might be patentable. Eventually they decided not to pursue a patent because it worked some of the time and didn't some of the time. Actually, only a few years ago, Aplan and I finally wrote a paper on this, and published the results. We should have done that earlier. I had seen some infrared spectroscopy work where this reagent interacted with zinc silicate—willemite—that Milton Wadsworth infrared had done at Utah. I thought to myself, "That should also adsorb on copper silicate." So that's when we went to work on it. It's just one of those ideas you get, and then to work on it right at the very end. By the way, the reason why the process worked some of the time and not others with chrysocolla is that copper ions are readily dissolved from the surface region, and so sometimes there would be no surface copper ions for the mercaptan to adsorb or react with.

July 1, 1956, was going to be my first day at Union Carbide. By the way, starting in the fall of 1956, MIT was changing their pay policy from an eleven-month appointment to a nine-month appointment, so the pay would be the same but on a nine month's basis; and then people could pay themselves two extra months in the summer from research grants. So there would have been a little bit of a pay raise, but I didn't take the Carbide job for the pay. Dick Charles said he had to leave because he couldn't support a family on what he was making at MIT. I had decided to go to broaden my background, but a lot of it had to do with how deBruyn upset my stomach. You know, the first day in Niagara Falls my stomach pains disappeared and never came back, whereas I had had about two years or more of constant stomach pains. I would like to add that through the years after my leaving MIT, we have been good friends with the deBruyns, who moved to the Netherlands about thirty years ago. We still keep in contact.

By the way, coming back just a little bit, when Spedden was at MIT, he had a very nice office that he shared with a retired metallurgy prof, Sylvan Williams. I acquired that same office. I think maybe a secretary was there for a while. It was a long room that could hold one desk at one end and one at the

other. Maybe I had that office for three years. At the beginning of my last year there Professor Chipman came in and said, "You know, I would really, if you wouldn't mind, like to have you share this office." He said there was somebody named Walter Owen coming from England as a super-postdoc to work on a project related to steel strength. Chipman said, "You know what? For an Englishman he's really a hell of a nice guy." Eventually Owen, who was a physical metallurgist, went back as professor at Liverpool, and later came back to this country as department head at Cornell, research vice president at Northwestern, and then went to MIT where he was a longtime department head. So I had an office mate during my last year there. We'll pick up on what happened.

Swent: This probably is a good place to stop, and we'll start then with Union Carbide.

[Interview 6: August 10, 2001 in Berkeley, CA, Evans Hall]

[Tape 12, Side A]

Swent: You had said that there were a few things that we needed to catch up on from the MIT period.

Fuerstenau: Yes, I would like to just tell you about two or three things. Two are the result of a couple of visitors. One came during my first year as an assistant professor, Jack Schulman, who was reader in colloid science at Cambridge. He had been on a seven-month tour around the world and came to MIT, gave a seminar, and that evening Peg and I picked him and his wife up to go to Gaudin's home, where he had a fairly large dinner party for Schulman.

Schulman was a big expert, had published dozens or a hundred papers on what are called monolayers on water. If you put a long-chained alcohol, or fatty acid, or soap on the surface of water the hydrocarbon chain makes it insoluble, but the polar group orients it to the water. You can measure many properties of surfaces this way. He had used this technique for trying to correlate flotation behavior with insoluble monolayers on water, and he had written a couple of very interesting papers on being able to separate a cobalt sulfide mineral from other sulfide minerals by having a side chain on the collector molecule of the exact dimension so that it would fit on lattice cobalt ions. Trying to design a flotation chemical that just fits on mineral surface sites continues to receive attention. Anyway, Schulman wasn't an engineer; he was a surface chemist. I think INCO supported his around-the-world trip, and INCO, shortly thereafter, in honor of two previous presidents of INCO established the Stanley-Thompson Chair of Chemical Metallurgy at Columbia University in New York. Schulman became the first Stanley-Thompson Professor of Chemical Metallurgy at Columbia. I think he did some work on minerals, but most of his efforts were straight surface chemistry.

Just a little bit of an aside: practically the world's first surface chemist was Benjamin Franklin who poured a liquid fatty acid on a big pond and then measured the area that it spread over. He came very close to calculating, in the ballpark, the area of a molecule and so on. This was old Ben Franklin. He was a genius, as we know, in many ways. In fact, every surface chemistry book always describes Franklin as being the first person to do that. Anyway, several times I visited Jack Schulman at Columbia.

Developing the Modified Hallimond Cell

Another important visitor was W. E. Ewers, a researcher for his entire career with C.S.I.R.O. [the Commonwealth Scientific and Industrial Research

Organization] in Australia. He gave a seminar to the mineral engineering group at MIT on, let's say, the fundamental aspects of flotation that they were working on at the time. One of his slides showed a little glass flotation apparatus, called a Hallimond tube after an English mineralogist who designed and used it in the field to separate mineral grains. I listened rather attentively to his presentation of the results of his flotation experiments. This little device, which held about 100 milliliters of water, was a glass tube about eight inches long and one inch in diameter. It is vertical for about the first two inches and then bends over at a 45-degree angle. Above the bend, a vertical tube is incorporated to recover the particles that had attached to bubbles. Ewers used a little squeegee bulb to blow air bubbles into the bed of mineral particles in the bottom of the main tube lying on a porous frit. Bubbles and attached particles rise to the surface, and when the bubbles break, the floated particles drop back down into the vertical tube and you can readily determine the extent of flotation. What I did is I took this idea and added very accurate flow meters and stirring, control of the size of the bubbles, and so on. One need use, say, about one gram or so of pure mineral particles and conduct a very precise study of a flotation response in relation to solution chemistry. You could now correlate flotation with electrical effects, adsorption effects, etc. In a normal flotation cell one skims off the froth, and so that changes the solution conditions, whereas no liquid is removed from the Hallimond tube during a flotation experiment.

With a couple of graduate students, Paul Metzger and Gordon Seele, who I had conduct the experiments, we published a paper in *E&MJ*, [*Engineering and Mining Journal*] and to this day, worldwide, this so called modified—that's what I called it—modified Hallimond cell, or Hallimond tube, is almost universally used by those doing microscale flotation research. It all started out with Ewers visiting MIT, and my immediately seeing what might be done with this technique. Our first device used a capillary for nitrogen introduction—as probably was the case with Ewers—but because a mineral particle could plug the capillary, we later changed to a porous glass frit. It was our work that led the way because it showed how to make very precise correlations of flotation with both solution and surface chemistry. Ever since, most of our flotation papers all include Hallimond tube experimental results.

By the way, these were the only two seminar speakers inside the mineral engineering group during my six years at MIT. I recall going to only five other seminars or talks during my student days: Alan Cottrell from Cambridge, Earl Parker from Berkeley, Peter Debye from Cornell, Linus Pauling from Caltech, and Al Capp the cartoonist who drew "Li'l Abner" and lived nearby. This is in great contrast to the large number of speakers that I brought to Berkeley over the years.

Correlating Flotation Response with Zeta Potentials

Fuerstenau: There were a couple of other things I just want to pass on. I told you, you know, for my own thesis I measured zeta potentials. An earlier researcher, S.C. Sun, had done some work on this with Gaudin during the war in the forties. He was from China and afterwards had a long career at Penn State working on coal flotation. When Sun retired, Frank Aplan went to Penn State as his replacement. Sun had done some interesting work on measuring zeta potentials, but that never really took off. When I was able to show how you could correlate zeta potentials with Hallimond tube flotation response, adsorption, et cetera, to this day, worldwide, fundamental flotation research generally includes measurements of zeta potentials.

Swent: The formula that you developed?

Fuerstenau: No, I didn't develop it; that is old. But clearly I showed the application to understanding flotation, and that is why it now is quite standard. You still see many, many papers that include measurements of zeta potentials in flotation research.

The Role of Electrical Double Layer Phenomena in Flotation

Fuerstenau: One other topic that I wanted to comment on is using the concept of electrical double layers in flotation—that's become a very common thing. That grew out of Professor Overbeek being there at MIT, and then by my taking the concept of the double layer and showing how it fits flotation systems. Let me just explain

Swent: As opposed to monolayers, which Schulman was talking about.

Fuerstenau: Right on. A monolayer of the type that Schulman worked on consisted of a layer of insoluble long-chained alcohols or fatty acids on water one molecule deep. The so-called electrical double layer results from the fact that nearly every material in a liquid develops a charge on its surface due to the adsorption of ions. These may be some ions that are part of the surface of the solid, or they're hydrogen and hydroxyls for oxides. Most things are negatively charged. The whole system must be electrically neutral. To keep it neutral there's a second layer of positive ions called counter ions. That layer, because of the thermal energy of the ions, is somewhat diffuse. So this is what's called the electrical double layer. My contribution was realizing and showing that in simple systems of oxide and silicate mineral flotation, the collector functions as a counter ion in the double layer. This is what can be termed the electrostatic model of flotation.

I remember one time meeting a very well-known clay surface chemist who was originally a Dutchman, Henry van Oelphen. He studied in Holland in the

1930s at about the time when Overbeek also was a student. Van Oelphen worked for Shell in Houston. He wrote a couple of books on clay colloid chemistry—a good applied colloid chemist. I remember him saying one time when talking about double layers—he said, “You know, these double layers have got real power. A good illustration is how the buildings in Mexico City rise and fall with the double layers pushing the clay layers apart, depending on the season. It’s the double layers on the clay particles that cause the unevenness of all the buildings on the fill that Mexico City is on.”

In the late 1920s and in the 1930s, Gaudin proposed that flotation collectors function because they are ions that adsorb at the mineral surface. Likewise, Ian Wark in Australia in the 1930s was a strong proponent of collector ion adsorption. But they didn’t say why they adsorbed. During this same time, Taggart at Columbia strongly considered that collectors function by chemically reacting with metal ions at the surface. None of these ever considered double layer phenomena. In 1950, Professor Melvin A. Cook at the University of Utah, who was actively involved with flotation chemistry research at the time and a later developer of slurry explosives used by the mining industry, postulated that flotation collectors would have to function as a neutral molecule, such as xanthic acid and not xanthate ions.

[Added by Douglas Fuerstenau during editing: Cook wrote in a *Journal of Physical Chemistry* paper, “Assuming a complete or nearly complete monolayer of ions on the mineral particles, one would obtain a bulk concentrate with so much charge that it would explode with greater violence than an equal weight of nitroglycerine.” Cook, a physical chemist, obviously did not think in terms of electrical double layers.]

Out of my work, and the papers that I had in those early days that showed how electrical double layer effects have been used in interpreting flotation behavior, ever since—we’re now talking approaching fifty years—this approach has been quite standard. Prior to that, there was never any interpretation of flotation phenomena in terms of double layer phenomena. So those were the three things that really came as an outgrowth of my three years on the faculty there at MIT.

Swent: The Hallimond tube adaptation, the double layer—what was the third?

Fuerstenau: The interpretation of double layers in flotation and the electrostatic model of flotation were interrelated. The third one went back to my own thesis, namely that ions of adsorbed long-chained surfactants or flotation collectors begin to associate at the mineral-water interface when the surface concentration reaches a certain point. This is what Gaudin and I termed *hemimicelles* and since about 1990 has been called *self-assembly*. With streaming potential and flotation measurements, Modi investigated details of this for 12-carbon anionic and cationic surfactants on corundum; and, as I had told you earlier, I verified these concepts with quartz and amines of different hydrocarbon chain

lengths. I consider that to be a major contribution, not only to flotation but also to aqueous surfactant surface chemistry broadly.

Swent: So this was research that you were able to do at the same time that you were teaching?

Fuerstenau: Yes. Another part of teaching was whenever I had new thoughts, I passed them on to graduate students—that is part of teaching—although the actual lab work was done by the graduate students working with me. There were not many; I just had a small group, three or four maybe. We actually had a couple of additional real innovations in research. Earlier I had mentioned our pioneering work on how surfactant adsorption can slow down the rising velocity of air bubbles. Also, three persons worked with me on the activation of sphalerite with metal ions. Sphalerite will not float with xanthate as a collector unless a metal ion that makes a more insoluble sulfide than zinc sulfide is added to the system. Three graduate students—George Mao, Marvin Turkanis, and Paul Metzger—worked on different aspects of this problem. Turkanis investigated the interaction of silver ions with sphalerite. Mao's doctoral thesis showed how the uptake of copper ions by sphalerite is controlled by diffusion and that the activation can be reversed by adding a reagent that complexes copper ions. Mao had started his research with Gaudin, who later asked me to help direct the work. Mao worked with radioactive Cu^{64} prepared in the MIT cyclotron and that required rapid careful work to carry out his activation experiments. Metzger studied the activation of sphalerite with lead ions and demonstrated clearly for the first time that the reason that flotation mills added zinc sulfate to flotation circuits was to prevent activation of sphalerite from lead ions derived from galena in the ore. Nice work.

Swent: It was a very fruitful time then.

Fuerstenau: It was. Those are the days when I wasn't heavily involved in lots of other things, so it was daily—

Swent: Ideas were bubbling.

Fuerstenau: Bubbling, going in and discussing them with the students, and bringing them to fruition. At this same time, Phil deBruyn was directing several excellent research investigations on the electrical double layer at solid-water interfaces. In particular, first Freyberger and then Iwasaki completed excellent doctoral theses on the double layer and surface chemistry of silver sulfide. This sulfide was chosen because you can make a very reversible electrode of silver sulfide. Their theses involved elegant studies of the actual measurement of the surface charge on silver sulfide, but they did not relate any of this to flotation. Subsequently, deBruyn directed several interesting similar theses on the fundamental surface chemistry of oxides in water. All during this time, Professor Overbeek had some input on that since he continued with a

consulting arrangement on the project for a number of years after he had gone back to Utrecht.

[Added by Douglas Fuerstenau during editing: When Dick Charles and I became assistant professors, Gaudin took on none of the new students. Gaudin only had about one or at most two graduate students working directly with him during those particular years. In his year on the faculty, Charles built up a nice group working on comminution problems, and when he left for GE, I looked after them until they finished their degrees. During my years on the faculty, I think that Professor Gaudin spent most of his time writing his text on *Flotation* which was published in 1957—that plus consulting. My last few months there, I was involved with a consulting project that Professor Gaudin had with IRSID in France, where we were attempting to upgrade a low-grade French limonitic iron ore. We had some success but I do not recall much about the project. One time someone was carrying out a flotation test on this consulting project and Gaudin came into the lab smoking a cigar, looked at the froth in the flotation machine, flicked some cigar ashes onto the froth, and turned around with a big grin, and said, “A few ashes might actually help.”]

Maurice Fuerstenau Plus Some Other Final Aspects of the Years at MIT

Fuerstenau: I had mentioned my brother, Maurie. Maurice Clark Fuerstenau. Maurie had graduated in 1955 from South Dakota School of Mines in geological engineering. He had decided that he really didn't want to pursue a career in geology, so I said, “I'll see if I can get you admitted to come to MIT as a grad student,” which he was. Probably things were a little more informal. I assume his grades, of course, were fine. He came there in the fall of '55, so he was there during my last year. He eventually did his master's and doctorate with Gaudin. My guess is it may have been about 1960, '61 when he finally—

Swent: He was already married?

Fuerstenau: He and Joyce were married two weeks after we were. He's five years younger than I am, so they got married fairly young in life. He would have been about a sophomore, junior, something like that, as an undergrad. [laughs] To each his own, I guess. So anyway, we had a little family that last year there.

Swent: It must have been very nice.

Fuerstenau: It was.

Swent: Did they live near you?

Fuerstenau: I think at first they rented an apartment in Arlington, next to Cambridge, and then later near the Aplans. Frank Aplan got married in the summer of 1955, and I think all of them lived in the MIT married student housing which was

about like most married student housing is on campus: it's not that great. I was one of the ushers at Frank's wedding. I still recall it as one of the most excruciating experiences I ever had. They had a High Mass, and I didn't know the details—everybody up front in the wedding party kneeled for about forty-five minutes. I'll tell you, my knees were hurting so badly, and I would move from one to the other and the pain was excruciating. It was only later Frank told me that since I'm not a Catholic, I needn't have had been up there kneeling. Excruciating is the way I still recall it.

Swent: You had Protestant knees.

Fuerstenau: Right. Another thing that I did while a student was go to the annual AIME meeting. Years ago, every other year, the annual meeting of the AIME was in New York—that went on for a long, long time. Now they never meet in New York. While I was a student I remember going to my first meeting in 1952 and staying in the Y[MCA], down near Penn Station. I was very impressed to meet, and/or listen to some of these old giants who wrote papers in the thirties and the forties who were still active.

In those days a lot of the mill superintendents, mill managers, gave technical papers. You just don't see that much any more. For them to prepare technical papers meant they did it on their own. If they're running a mill, that's their main object. A very well known milling engineer was Jack Meyers who was a mill superintendent of Tennessee Copper, and he wrote many, many papers through the years, and good ones, innovative, almost research-type papers related to plant performance. In fact, a very famous thing in flotation was the discovery of using copper sulfate to activate sphalerite for making lead-zinc flotation separations. He wrote a paper, just as an aside, I think around 1913, commenting that copper sulfate would make sphalerite (zinc sulfide) float with the xanthate—while somebody else in Australia named Bradford always is given credit for the discovery in the same year, 1913. So forty years later Jack Myers was still involved with milling. Later I got to know very well his assistant superintendent, F. M. (Doc) Lewis with whom he coauthored many of his papers. There were a few others like that there. To attend was great experience.

That year Professor Taggart, who had already retired, got the Richards award. He was the very first recipient of the Richards award. I listened to his talk; but he really was an extra dull speaker. Gaudin introduced me to Taggart after his Richards Award talk, so I got to meet him and that was just about all.

Swent: Was there an AIME section at MIT that you joined?

Fuerstenau: It was called the Boston Section of AIME. By the way, they always had good attendance—all the Harvard mining geology people came to those meetings. There's a device called Gratton-Vanderwilt polisher for polishing mineral

specimens. Gratton was a retired mining geology professor at Harvard and the current one then was somebody named McKinstry.

Swent: Hugh McKinstry.

Fuerstenau: You know him?

Swent: Yes, I know him.

Fuerstenau: I do remember what he looked like. They came regularly, at least when the meeting was mineral oriented, but sometimes it was oriented to physical metallurgy. I must have gone fairly regularly because I remember different kinds of meetings. One was a very famous physical metallurgist from University of Chicago, named Cyril Stanley Smith (he always went by the full name). He was working on the packing of crystals in a solid. You've seen pictures of how all metals crystallize—how they pack? He was using soap foams and taking pictures as a model of the packing of crystals in a solid metal. If you're washing dishes and you watch the soap stay in a glass, as the soap starts to get old you see flat-sided bubbles; he was studying these.

By the way, Smith got interested—after he went back to MIT—in the Drake plate. He became an archaeological metallurgist and some years ago did the study on the Drake plate that showed that it couldn't have been made at that time. It's actually in the Bancroft, isn't it?

Swent: It was a hoax. Yes.

Fuerstenau: He's the one that did the metallography work, let's say, after we were here in Berkeley.

Swent: This was supposed to have been left here by Drake.

Fuerstenau: That was not a huge section, but obviously had MIT and Harvard people attending—

Swent: Were there still industries in Boston at that time that would have been connected to mining?

Fuerstenau: Copper Range Company headquartered there. You know Jim Boyd? He probably was the president then. I never met him then, but I did meet in Boston the vice president of production for Copper Range who was a mining graduate of South Dakota School of Mines—Harold Ewoldt. Calumet and Hecla must have originated there. That may have evolved into Copper Range. I don't know whether the name Hecla was tied to Calumet and Hecla or not.

Swent: It went back to Louis Agassiz. I was thinking of metalworks there, like Revere. Did they participate in AIME at all?

Fuerstenau: I have no recollection of that. Later on, Kennecott put their fundamental laboratory in Lexington, Mass. I was gone by then. That was not established until maybe 1960, which I'll just pull out of the air. I suppose the backbone for locating there was to be near some of the meccas of education in geology and metallurgy, which basically was MIT and Harvard. The president of Kennecott then was Frank Milliken, an MIT graduate, and that may have entered into their locating that research lab there.

Swent: It was mostly academic then?

Fuerstenau: Oh yes. That's why a lot of the papers that were given—

Just one other thing about AIME meetings back then, particularly the 1956 AIME Annual Meeting in New York. Maurie had to do a little project for his senior thesis at South Dakota, and at that time, I got interested in what's the mechanism by which sodium chloride and potassium chloride can be separated by flotation. So he did some work on that for his BS thesis at South Dakota, and I did a certain amount of work, actually myself, at MIT. My concepts then were that ionic size determined why a collector adsorbed on sylvite, potassium chloride, and not on sodium chloride. We wrote a paper that was published. At the meeting, Maurie spotted this: in the program they had a title in front of every name except his, and it just had M.C. Fuerstenau. So it had either Professor or Doctor D.W. Fuerstenau, and M.C. Fuerstenau. Maurie spotted that his was the only one that didn't have a title in the whole program. He says, "You know what? I think they think I'm your wife."

Then—this was after I was gone—the main Russian flotation scientist like Gaudin, Professor Igor Plaksin, visited MIT. This must have been late fifties. Maurie was there and Gaudin brought Plaksin around and introduced him to Maurie. Plaksin said, "We've read your papers." And Maurie said, "No, no, those are my brother's." And he said, "I am the M.C. Fuerstenau." And Plaksin said, "Oh, you're his brother? Well, we always thought you were his wife." [laughter] Maurie told me that story—

[Tape 12, Side B]

Swent: So Plaksin had also thought he was your wife.

Fuerstenau: Plaksin actually published papers with his own wife in Russia. Practically as many women there hold technical jobs as men; that's still the case. Like I say, Maurie—

Swent: Twice got relegated to a lower position.

Fuerstenau: He's a little sensitive, I guess. Let's see, we've got Union Carbide and Niagara Falls.

**VI UNION CARBIDE AND CARBON CORPORATION, THE
ELECTROMETALLURGICAL COMPANY, 1956-1958**

The Move to Niagara Falls and Living in the Buffalo Suburbs

Swent: When did you go there?

Fuerstenau: July 1, 1956. If July 1 was Monday, then that was my first day, or if it was Tuesday that was my first day, and got involved quite quickly—probably spent a little bit of time, not long, finding a place to live. We moved into a nice apartment in an area called Snyder, New York. Do you know where that is?

Swent: No.

Fuerstenau: Sort of the northeast corner of Buffalo. I had twenty-five miles, or something, to drive to Niagara Falls.

Swent: I think you said how much you were paid.

Fuerstenau: Starting pay was \$9,600, which was very good pay then. About a year later, Ernie Peters said, "I've concluded that everyone in this laboratory," speaking now of the professional PhDs, "all make \$10,000 plus/minus." Everybody was virtually paid the same, and very, very close together. That was almost double of what I had been getting at MIT.

Swent: Let's talk about the commute a minute. You had a car—was parking or driving an issue at that time?

Fuerstenau: For a while two of us drove—Walter Zwicker who was working for me—he and I drove, and then unfortunately he moved to Niagara Falls. It was a problem for a bit, at least that first year, because Bell Aircraft had a huge facility in Niagara Falls, so when you hit the edge of Niagara Falls it would be stop and go for about a half mile because of people going to work, and again in the afternoon, with those exiting from Bell Aircraft.

A few times snow would be a problem. Buffalo is one of the snow capitals of the world. In fact, if you flew into Buffalo, for about thirty miles east, it was all white off the eastern edge of the lake. Buffalo was just on the edge of Lake Erie, whereas Niagara Falls is on the Niagara River, right between Lake Ontario and Lake Erie. In Niagara, probably, we would have no snow but sometimes as you drove south to Buffalo during a snowstorm, every mile added another inch or half-inch of snow. That didn't happen very often so the drive wasn't as bad as I thought it might be.

Swent: That's a rather long commute.

Fuerstenau: I suppose we could have found a place in Niagara. But the air in Niagara Falls smelled terrible because in those days there was simply one electrochemical or electrometallurgical plant after another—and with no pollution control.

Swent: What determined your choice of Snyder as a place to live?

Fuerstenau: Maybe somebody suggested this apartment complex, which was a nice place. The University of Buffalo, which later became part of the SUNY system (State University of New York), was in Snyder—maybe a mile away from where we lived. That first year Peg took a couple of different courses as a student there, in things like child psychology. We will come back to some aspects of living in the Buffalo area.

Position as Section Leader in the Metals Research Laboratories

Fuerstenau: The job I had in Niagara Falls was as section leader in the Chemical and Mineral Engineering Group; this was the section for mineral processing research, let's say. The person who had that job just prior to me was Bob Shoemaker who transferred from the research lab to the development department. He probably told you about his work in both places. The Metals Research Laboratory had a lot of very outstanding people, both older and younger. At the time, there probably was no other industrial company that had ever assembled such an outstanding group of metallurgical researchers. Some few years after I left, they really downsized the lab—I guess companies sometimes are unwilling to wait for new developments to come out of such a laboratory and are unwilling to continue to spend the money to keep it going. Anyway, this was the research arm of the Electrometallurgical Company, which was a division of Union Carbide and Carbon Corporation. Buildings for both the research and development activities were located right beside the huge production plant. The main products they produced were calcium carbide and ferroalloys, mainly alloying elements for producing stainless steel: ferronickel, ferrochromium, ferromanganese, ferrosilicon. They also were a producer of titanium (which they didn't make there). The ferroalloys were produced in a huge plant using electric furnaces, located in Niagara Falls due to the electric power.

In those days, Union Carbide was larger than Dow Chemical Company, second only to DuPont. The corporation was comprised of several different well-known companies in addition to Electromet, such as Linde, National Carbon, Union Carbide Chemical Company, Union Carbide Nuclear Company, et cetera. Today Union Carbide has disappeared—taken over by Dow. This is an example of what management can do or not do.

Art Lytle was the VP of research of Electromet in New York, and Dave Swan was director of research in Niagara Falls. He was there about a little over a year during my time there when he was transferred to New York. Dave Swan

eventually became the vice president of technology for Kennecott, a job that he held for the whole latter part of his career. I think he was really quite young as research director, thirty-five-ish or less.

Under the research director then were several group managers. Rush Spedden, who had come from MIT, was the group manager for chemical and mineral engineering. There would have been maybe a half dozen groups. There was a group concerned with physical metallurgy, the properties of alloys. Another one was fundamental group, with persons working on long-range fundamental problems. Within each group were sections. Mine might have been called the mineral engineering section or mineral processing section. There was another on hydrometallurgy under Ernie Peters, one on solvent extraction, one was the fused salt section, and probably others.

Swent: Fused salt?

Fuerstenau: Yes. You melt the salt so that you can do processing in molten salts. Some metal compounds dissolve in molten salts, just like sugar or table salt does in water. There also were service groups, such as analytical, X-ray, library, and so on. So overall that laboratory was large and a very organized organization.

Then there was a section leader for each of these; they had some very good people in that capacity. For example, somebody named Tom Henrie was there for most of my first year, and he headed the work in the section on fused salts. Henrie left to go to join the Bureau of Mines. He had come from Salt Lake, University of Utah, and I think, went to head the Bureau Center at Boulder City near Henderson, Nevada. Then I think he headed the Bureau of Mines station in Reno, and then eventually—I always kept fair contact with him—he became Chief Scientist of the Bureau of Mines in Washington for a long time, in the latter stages of his career.

Swent: These section leaders would be all PhDs?

Fuerstenau: All but maybe one or two; one I recall was someone who was in his early fifties. The management didn't. Dave Swan had, I think, a BS in metallurgy from RPI [Rensselaer Polytechnic Institute], and Rush Spedden had a BS from Washington and MS from Montana School of Mines. Some of those who headed the other sections I can picture, but just don't recall all their names—Ray Foos, Don Hansen, Bill Krivsky, Ernie Peters, Don Weishart were names I do recall. Nearly everybody was on the younger side—and by younger I mean thirty-five or less.

Swent: And the people who were working for you?

Fuerstenau: There were several. A couple of them would be BS-engineer types who would be sort of in charge of running experiments and tests. Another was a PhD geochemist named Walter Zwicker. Another person that was there for most of

that year was Bob Carpenter, who had a BS and maybe also an MS from the University of Idaho and had worked at Uravan and then for quite a few years until his retirement at Bishop, California. Union Carbide had a big tungsten mine at Bishop that just a while ago closed permanently. There at Niagara Falls a lot of the research was done in developing a process to make ammonium paratungstate that kept that operation at Bishop going for many additional years.

Swent: Pine Creek.

Fuerstenau: Pine Creek, right. Anyway, Carpenter was working on trying to improve recoveries and grades of flotation concentrates. Their main product was scheelite, calcium tungstate, a very soft mineral that is major source of tungsten. The other valuable mineral was molybdenite, molybdenum sulfide. Since scheelite is very soft and easy to grind, the problem was that the grinding circuit produced too many fine particles. Bob Carpenter was working on ways of changing the circuit that would improve recovery. Using samples from Bishop, Carpenter did a lot of work in Niagara Falls to characterize the performance of the plant and try to find better conditions for grinding and flotation.

Zwicker was originally from Austria and had an Austrian PhD in geochemistry and mineralogy. He did our section work on mineralogy but also had a research program on trying to improve surface reactions that were involved with minerals. He told me he had spent the war in the German army on the Russian front. He said his job was being a spotter right up at the front for guns on the ground shooting at airplanes. He said that at the very beginning of the war the Russian airplanes were very easy to shoot down because they were made of wood. Then, after a while, they got American planes; I think, P-39s produced by Bell Aircraft. I know all that from my high school days when I used to be interested in that stuff. He said it completely changed his welfare, being up at the front, but he survived. I never did ask him how he avoided becoming a Russian prisoner.

He apparently did not get along with my predecessor. I remember one time he told me that his previous supervisor in the lab had given him a sample to identify and it turned out to be ground glass. I said, "You really should have taken that to the manager."

Swent: It was a practical joke?

Fuerstenau: Yes, but at company expense. He said that the magazine *Nature* would come around and on the routing slip it would say "Dr. Zwicker see page so and so." There were all these ads in the back: "Wanted: mineralogist in Mauritius" circled. He figured this also came from his supervisor. Dr. Zwicker said that he always felt that his supervisor seemed to have something against anyone with a PhD.

Our section also had a large pilot plant, and a very good person, Gene Lauer, was in charge of it—not an engineer but an excellent man—an exceptional person for the job. So our section had pilot plant-scale equipment that was used to test possible flowsheets for new mineral separation processes and to prepare larger scale samples for those doing metallurgical test runs. This pilot plant was staffed by a few people who were hourly laborers.

The union in the lab was extremely strong. Let's say if an engineer picked up a bucket, he would get a grievance. There were people spending much of their time fighting grievances over something or other—fortunately I never had a grievance filed against me. It really blows your mind to think that so much time and money was wasted like that. You had to be careful that you didn't go do something that somebody on the labor force thought he should be doing. I recall that weekly group meetings always had some discussion about what grievances were going on within some of the research sections within the group.

Swent: Which union was it?

Fuerstenau: Gee, I have no idea. Whether it was part of the same union that was in the plant or not, I don't know. The plant had a major strike, I think, the year before I came, and all the professional people were sent from the lab to run the plant, and they stayed there day and night. Bob Shoemaker told me about that and he also probably described that to you. He was quite gung-ho about having to run a furnace or something.

One person who had been a grad student working with Schuhmann at MIT when I was a student was Bill Krivsky. At Carbide, he did some superb work on the thermodynamics of the process for making ferroalloys—or one of some of the alloy materials that they were making—and they wouldn't let him publish it. As I understood it, Carbide operated a lot by, "It's better to keep things secret and that way other people won't use your technology." He told me that their refusal to let him publish that work made him decide to switch from research to management, and then at that point he became a group manager. He said it was that incident that made him decide that he would make that switch. Just to proceed a little further: he later became a vice president of Brush Beryllium. He eventually became president of some fairly large steel-alloy company. We're talking about somebody with good management skills. What moved him out of research into management was what I just told you.

Swent: He was never able to publish that?

Fuerstenau: Whatever that effort was. It was very fundamental work explaining conditions to run this process under, let's say, optimizing thermodynamic conditions.

By the way, another person who had an office right next to me was someone named Milton Stern. He had been a metallurgy grad student at MIT when I was there but working in the corrosion area. We had taken thermodynamics together during my first semester at MIT. Then when I got to Carbide his office was right next door to mine. While he was at the Metallurgical Laboratories, he did some classic fundamental research on the corrosion of titanium alloys. Since he was in the fundamental group, he could publish his work after patents had been applied for. If I remember, just adding a very minute amount of precious metals, such as platinum, you can completely inhibit the corrosion of titanium. He won some awards for this and so on. What I found interesting is he decided that he wanted to go into management. He left the laboratory at Niagara Falls to head the Speedway Lab of Linde, another division then of Union Carbide. In addition to producing gases, Linde was the maker of synthetic sapphires. That was a division of Carbide, and he went to work for them as head of a Linde lab—directly from conducting fundamental research he just pulled the shade down and said, “I’ve shown myself that I can do this great fundamental work and now I will devote my efforts to management.” As I said, he won awards from the Corrosion Society for this work. He eventually became vice president of Union Carbide Ore Company, which Aplan worked for later. Eventually he became senior vice president of Kennecott. My guess is Dave Swan may have made the suggestion about bringing Milton Stern to Kennecott. Later I recall reading that he was executive vice president of Stauffer Chemical Company.

You know that Kennecott—you probably got some detail from your interview of Frank Joklik¹⁰—had to divest themselves of Peabody Coal, which they had bought with money from Braden Copper or El Teniente when they got kicked out of Chile. So after having to divest Peabody Coal, Kennecott had all this money. Milt Stern was at that point senior vice president and proposed that Kennecott buy the Carborundum Company, at least that is what I heard. I gather what happened a short while later is that a lot of the Carborundum management eventually took over the management of Kennecott, and you know that Kennecott eventually evaporated from the earth by a chain of takeovers. Of course the name Kennecott is still there, but the corporation is not. This is just an example of another super-outstanding person who started his career as one of the lab researchers and then changed course.

Swent: Did you have a choice as to which section you would take?

Fuerstenau: No. Spedden, being the group manager, had brought me there to head the mineral engineering section. This was to replace Shoemaker who was moving

¹⁰ G. Frank Joklik, *Exploration Geologist, Developer of Mt. Newman, President and CEO of Kennecott, 1949-1996; Chairman, Salt Lake 2002 Olympic Winter Games Committee, 1997*

over to the development department. Development took over new projects and worked up potential processes to plant design and then on to construction.

Swent: Was the job as you had expected it to be?

Fuerstenau: Probably. Yes in that I was able to gain experience working with a range of ores. No matter what job I've taken on I've decided when you do it, you do it well, whether you like it or not. If I haven't already said so, you can gather I had no intention of making a career living in Niagara Falls. But you have to look at it as if that's going to be it. If you take on something and are unhappy, you're not going to do a good job and then you may not get a good opportunity for doing something else. I'm going to tell you a little bit about making use of what opportunities were there.

Coming back to personnel, somebody that I got to know very closely was an engineer who started there one month before I did, Ernest Peters, who later became one of the two big giants in the field of hydrometallurgy—I am speaking about the fundamental side. I would put him on a par with Milton Wadsworth at the University of Utah. Anyway, Ernie had done his PhD at the University of British Columbia with Jack Halpern, then an associate professor of metallurgy there, who later became one of this country's top inorganic chemists at the University of Chicago. Every day about 10 in the morning and 2:30 in the afternoon, Ernie came around for coffee. Only during that time at Union Carbide was I a regular morning and afternoon coffee drinker. Ernie had lots of explanations for everything. He was the person who I probably really interacted with most there. Ernie was there for maybe a year and a half, leaving at the end of 1957. He was enticed to come back to join the faculty at the University of British Columbia, where he had come from, and probably when Halpern left for the University of Chicago. Peters stayed at UBC for his whole career.

At the University of British Columbia on later trips, I met the head of the metallurgy department, Professor Frank Forward. Forward was inventor of what's called the Sherritt-Gordon process for the high-pressure leaching with ammonia of nickel-copper-cobalt-sulfide concentrates. The Sherritt-Gordon plant became very famous for the success of that operation up there, a Newmont subsidiary.

Swent: That was in Canada also.

Fuerstenau: In Canada, near Edmonton. In fact—we're getting a little bit ahead—when we were involved with the McLaughlin¹¹ process, I went up there to Sherritt-Gordon. For about six months, minimum, Sherritt-Gordon ran pilot plant tests

¹¹ The Knoxville Mining District, *The McLaughlin Gold Mine, Northern California, Volume VII*, 2001

on developing the McLaughlin autoclave process. Frank Forward was the inventor of this, and later on—

[Tape 13, Side A]

Fuerstenau: By the way, what's interesting is that Forward's only degree was a BS in chemical engineering, but—like Ernie Peters said—Forward really knew inorganic chemical reactions and was able to devise innovative hydrometallurgical schemes for new processes in hydrometallurgy.

Swent: You met Forward through him?

Fuerstenau: Oh yes, but only after I had come here to Berkeley. I went up to UBC and maybe gave seminars there three or four times through the years. Anyway, later on Forward became the science minister of all Canada for about a three-year period. I remember running into him one time when he had that role. That gives you an idea of his stature. I remember Ernie saying that Forward got the INCO Medal, which was a major medal in Canada, and he said it was one pound of platinum plus a one-pound nickel replica, something like that. I guess the actual medal went into a bank vault.

Some of the Social Aspects of Working for Union Carbide

Fuerstenau: Anyway, coming back to Carbide, we played bridge a lot in the winter at Niagara Falls; you can well imagine that was one thing to do. Eight of us played—this went on for two years—duplicate bridge. Every other Wednesday, I think it was—

Swent: Who were the eight?

Fuerstenau: One was Bill Krivsky, another Jim Downing, one was a technician, and I can't quite remember any of the others. But all were serious bridge players. What reminded me of that was that I was going to get back to Carbide's managerial social structure. The first year I only just played bridge for social bridge, so the first year I was probably groveling down towards the bottom. We kept a running score. Nothing was ever done with it but take note where everyone stood at the end of the year. Then I bought Goren's big, fat bible of bridge, and I used to lie in a bathtub and study a chapter, like you're taking a course. The interesting thing was that in the second year, I would follow Goren's finesses and all that and, having actually studied this bible, I was the person at the top of the list. I couldn't do that anymore; since we hardly play bridge. That was every other Wednesday—and my wife still comments once in a while about when it was at our house, which would happen say twice in a winter, there was a pot of coffee, and it was pretty quiet; it was serious stuff. No beer, or anything but coffee. Of course, as you know, in duplicate bridge you're playing against the opposite table.

Swent: I have heard that that's the one game that doesn't really involve luck; it's only skill.

Fuerstenau: It's how you play compared to your counterpart—one has to play the same hand that was played at the other table. Peg and I also played social bridge with friends.

Now I remember the social structural thing. Dave Swan got moved to New York and the group manager in physical metallurgy, Red Shortsleeves, became the research director. Bill Krivsky was moved up to being group manager of that metallurgical group. Being Carbide, they told him, "You now can no longer socialize with your friends who work under you in the laboratory." We played bridge with Bill Krivsky and his wife, with Milt Stern and his wife, and Don Wisehart and his wife. We all lived about a couple of blocks apart. When Krivsky got promoted he was told, "You don't socialize anymore with anybody underneath you." They just pulled the curtain down on all previous social interaction. I guess I picked that up directly from Milt Stern who said that in Carbide's structured world: "You do not socialize with people below you."

Swent: It was told right out?

Fuerstenau: Apparently they were told outright how they should function.

Swent: Were you ever given any such instruction?

Fuerstenau: No, but they did have something which we called "charm school." They started this course with people at the top—something called Sales Analysis Institute from Chicago. That was a two-week all-day thing, with about twenty people from all parts of the Niagara Falls organization, which would include management from the plant, development, research, and so on. Eventually it got down to my level, so I spent two weeks at "charm school," which was basically sort of how to sell—not sell by being a salesman, but it really helped in writing proposals and in making presentations. I never did finish the homework, so I don't have a fancy diploma from the Sales Analysis Institute.

I do remember when you write a sentence, or say something, they had jargon for things such as: "What's the good benefit?" I still remember, you always must put right up front what's the good benefit so the reader sees what's the benefit for himself in what you're asking for. I know that I have kept this in mind all these years and used it in writing proposals; I forget what some of the other things they taught, but I still remember "the good benefit." You always want to let people know what's the benefit they're going to get.

One person I recall from my "charm school" session had been a Spitfire pilot in World War II. He said he had been shot down three times during the Battle of Britain. On one of those occasions he landed on a ship, aircraft carrier,

somehow; the others he ditched and was picked up. Boy, three times and he still survived.

Swent: I sort of diverted you, and you were telling me about your friend who was told he could no longer socialize.

Fuerstenau: Somewhere along the line—just to show you what kind of pay did exist back then—Krivsky told me at the time I was leaving Carbide that as group manager, it's almost unbelievable, he was making \$14,000, and Red Shortsleeves as research director was making \$17,000. Now we're talking '58, which really, today, sounds incredibly low for a fairly large organization. When I left I was making \$12,000.

Swent: How much were you paying in rent?

Fuerstenau: Probably not much more than \$100 per month, probably. We bought a house the second year, the first house we owned, so our payments were for the mortgage.

Swent: How much did you pay for your house?

Fuerstenau: I still remember—\$19,600. It was a very nice tract area in Tonawanda, which is on the north side of Buffalo. A lot of people lived there who worked in the labs. One of our best friends from that era lived nearby—Art and Barb Pavlovic. Art was a solid-state physicist who spent his entire career at West Virginia University after leaving Carbide. We still keep in contact with each other.

I used to look forward to going home after work—that house was so pleasant—not a huge place, but it was quite fine.

Swent: Could you find a house for \$19,000 today that you would like to go back to? [laughter]

Fuerstenau: Good point. Then and now.

The Committee Structure in the Laboratory Management

Fuerstenau: Another aspect of the operation of the Metals Research Laboratories in Niagara Falls was the large number of committees, like the titanium committee, and maybe a fused salt committee, a chromium committee—I can't remember what others might have been. They were set up along commodity lines and possibly where new processes were trying to be developed. I recall that the different committees met weekly to discuss progress, what was needed, and what directions they were going, and so on. It was obvious to me that the very top people in the laboratory, like the research

director, and the assistant director, laboratory manager, group managers, and so on, spent a lot of their time in these committee meetings.

I still believe what was important was that there were two people, very senior people probably already in their sixties, who had important roles in such meetings. One was Walter Crafts, who was recognized externally as a distinguished physical metallurgist. Carbide developed a lot of the stainless steel alloys, but instead of going into the business of making them, there had been a corporate decision that Carbide would produce and sell the ferroalloy materials and not compete with their customers. Eventually, probably around 1970 or something, Carbide completely exited the ferroalloy business. Walter Crafts had published a lot of technical papers. With his extensive background in the early development of stainless steel alloys, he was a valuable member of the research staff and counselor to others.

The second person was Henry Erasmus, who had a bit of a Dutch accent. He knew virtually everything that Carbide had worked on with regard to processing and so on—he was just an encyclopedia of electric furnace processing.

At any internal meeting these two people were the encyclopedia of what Carbide had done in the past, what worked, what didn't, and they could suggest ideas. The new young guys weren't out there reinventing the wheel because these senior staff researchers could tell you what had been done twenty, thirty years earlier, whereas today such people usually don't exist in companies; they're retired at fifty something—and people with long experience are generally gone. I think you'll find that today in any company—mining company or chemical company—the same thing.

Swent: People don't stay for forty years.

Fuerstenau: It's not by choice. These people who retire at fifty-five aren't doing it actively—it's passive retirement in my viewpoint.

There was not a mineral processing committee because during my days no major new orebody was being considered. Just before I came there were major ones on processing ilmenite and also manganese ores. The mineral processing laboratory facilities for making small-scale separations and for characterizing mineral samples and making accurate size analyses of ground minerals and ores were excellent—probably set up by Rush Spedden. One part of what our section did was to look at the different ore sources and some of it reached pilot-plant stage. Any ore samples that came in from the exploration geologists were sent to this section. There was a very good scheme of how to characterize the ore samples and determine what the capability of making mineral separation might be. This was already put in place by Rush Spedden and involved a procedure where samples were separated by particle size, by specific gravity, and by electrostatics and magnetism. With this procedure, you

can now spread the minerals out and determine how effective you might be able to make a mineral processing separation. Spedden had trained exceptionally well a couple of older technicians who did these tasks very reliably.

Manganese ore was always the major thing—looking for manganese minerals suitable for making batteries. Sources of titanium were big during my time there also. Rutile and ilmenite were still the two sources of titanium. We also worked on ores for columbium and tantalum, which were other metals produced by Union Carbide. One of our important efforts was to try to develop a flotation scheme to concentrate pyrochlore, a columbium or niobium mineral.

Swent: Were these from Carbide's own mines or were they doing this—

Fuerstenau: Yes. The mines were operated by Union Carbide Ore Company; that was the official name. The Ore Company had a fairly large exploration staff. Then there also was Union Carbide Nuclear Company which ran Uravan there on the Colorado Plateau and the Pine Creek tungsten mine at Bishop, California. The Nuclear Company also managed Oak Ridge National Laboratory, too, like UC runs Los Alamos. Oak Ridge was run by Union Carbide for many, many years. No longer; I think Martin-Marietta manages Oak Ridge now, but for all those decades it was Union Carbide running Oak Ridge. All these companies were part of Union Carbide Corporation.

The chief geologist of Carbide, who I got to know quite a bit, was very active in AIME, named Olaf Rove. You'll see papers by him in the fifties, late forties, often about beach sands which is a major source of rutile. There was significant exploration and production from Florida beaches. I vaguely think that Rove worked for DuPont previously because DuPont is a major producer of titanium dioxide for paint.

Union Carbide was always looking at new methods for producing the metal. They had one whole section that used what one would call solid-state reduction: instead of melting it—smelting where you melt it—can you do the same reaction without melting? In other words, can you take an oxide and react it with carbon or with CO at a temperature below melting and now make the metal? I remember they had a large, big program on processing, making chromium out of chromite. Carbide mined their chromite in Northern Rhodesia. Then came the situation, after I had long gone, where Carbide couldn't import anything from Rhodesia or whatever country that is now—is that Zimbabwe?

Swent: I think it's Zimbabwe.

Fuerstenau: They sold the chromite to Russia. We bought the same chromite from Russia—the Rhodesian chromite from Russia—as I understand it, at a certain

factor of two or three in the price—but we worked on this. On one occasion we had to grind a large amount of chromite to be used by another section in the lab for their program, and we looked at doing it in our pilot plant with what is called a vibrating ball mill. Most ball mills are big round rotating cylinders that you've seen many times—many feet in diameter. A smaller-scale device that never really took off in this country: instead of rotating the mill, you shake it; it's like taking a bottle of ketchup and you shake it. The balls are rapidly shaken back and forth, and you can put intense energy into that small volume. It has a lot of pluses for it, but a lot of wear, and so forth. Now these are produced in Germany, but Allis-Chalmers had built them here. Ours was fifteen inches in diameter—that's really quite big—for that type of mill, quite a big mill.

We had to grind something like five tons of chromite for the solid-state reduction experiments. I decided well, we have to do it as a service; so while doing it, why not study the mechanisms of what is going on in the grinding action that takes place in vibratory ball mills? I had the lab technicians and workers that were in the pilot plant do this in various ways, and making measurements on what happened at this feed rate, and that feed rate, and so on. This included such things as determining power, material holdup in the mill, and making size distribution measurements on the product.

Eventually I wrote a paper, and did this purposely: there's an award in the AIME called Rossiter Raymond Award, which is for the best paper by an author under age thirty-three. I actually set about to get that award, and I wrote this paper which I think I called "Retention Time in Vibratory Ball Milling," and it was published in the *AIME Transactions*. I completely forgot about it until I think it was 1960 when in November I got a nice letter saying that I was going to be the recipient of the Rossiter Raymond Award. I actually set out to do it and did it by making engineering measurements while grinding these five tons as a service in our pilot plant. My idea was to learn something more than just doing it as a service.

Another interesting grinding problem, but much smaller scale, involved producing tantalum particles for making capacitors. At low temperature tantalum soaks up hydrogen, making a tantalum hydride that is brittle. We made tantalum hydride, and ground the material in a tantalum-lined mill with tantalum balls to produce the tantalum hydride particles. Then simple heating expelled the hydrogen and one had the tantalum particles. This was an interesting and really unique way of preparing metal particles.

Another area where much work was done was with manganese ores. Who's the small-mine man from Reno who did the oral history?

Swent:

Hugh Ingle?

Fuerstenau: Ingle. Ingle did work for Carbide as a consultant, and that's after me, but when he was in Brazil and in Africa he sent samples back which obviously got to Niagara Falls, and probably Aplan may have looked after them at that point.

Manganese ores were a major raw material, and Union Carbide put a huge effort in finding suitable manganese ore sources because you know there is virtually none in this country. There was a little bit in Montana as you know from Hugh Ingle, at Phillipsburg. I think during World War II they looked at getting the manganese from those Chamberlain manganese nodules along the Missouri River—I never saw them in place, but I have seen a couple. They are now covered by the dams on the Missouri. Anyway, the lack of manganese was always a tough thing in this country.

Carbide spent a lot of time, and they had two reasons for it: one was making ferromanganese, and the other was dry-cell batteries had manganese oxide in them as a component of the battery, and Carbide, National Carbon, made Ever-Ready flashlight batteries. I don't know if you knew that Ever-Ready used to be Union Carbide.

Swent: I associated Union Carbide with batteries.

Fuerstenau: Yes, that's National Carbon, and National Carbon also made the big carbon electrodes that were used in the electric furnaces in Niagara Falls.

By the way, Carbide got its name—this is one thing I wanted to comment on—they produced calcium carbide—that's how Carbide got its start. I suppose they took lime and reacted it in an electric furnace with carbon, and that made calcium carbide. They started that around the turn of the century, and the electricity from Niagara Falls was what they needed to make calcium carbide economically. They were still producing it when I came to Niagara Falls, in that huge plant.

Of course, calcium carbide ran the acetylene lamps in mining, as you know. You mix that with water and it generates acetylene. Sometimes one came out in the parking lot where all the cars were parked at the research lab and they were just coated with white dust, and this white dust was calcium carbide. Sometimes it rained—it may have been more lime than calcium carbide—but if it rained then these reactions occurred. Your car, during the day, would get literally coated with this white powder. You would never see any of that happen anywhere today, but that was 1956.

Swent: It was coming out of their smokestacks.

Fuerstenau: Out of the stacks. This plant was probably several buildings wide and three quarters of a mile long. It was as big as a steel plant and did have some pollution then.

Coming back to manganese ores, they had found that the best batteries could be made from ore from a mine in Ghana. The sad thing was that they were shipping the ore from Ghana up to Norway, making ferromanganese, and it turns out that the crystal structure of this Ghana ore was almost unique in the world. I'm not a geologist, but underneath the oxide ore was manganese carbonate, and when that carbonate got oxidized it formed manganese dioxide with a certain crystal structure that was the most reactive form for batteries. Common manganese minerals in most ores are pyrolusite and psilomelane, and—I just don't remember anymore the names of all of these manganese minerals, but there are dozens of them, almost, and the mineralogy depends on the crystal structure, impurities, and the valence state of the manganese, and so on. It turns out that that deposit in Ghana was very unique, similar to synthetic gamma-manganese dioxide produced electrolytically. I remember Rush Spedden telling that the geologists found not much of it but an identical deposit on the horn of Brazil. This was before plate tectonics ever really came out in the sixties, but in 1957 Spedden had commented that Africa and Brazil must have once been hooked together at that spot because of the manganese ore that had identical geology and mineralogy. Of course this now is explained by plate tectonics.

Walt Zwicker and two from the X-ray characterization lab, Gloria Faulring and Bill Forgang, worked extensively together on this manganese ore from Ghana that contained the manganese dioxide mineral that was so good for batteries. What is it in this Ghana ore? What mineral is it? They discovered, finally, a new mineral which was a certain crystal form of MnO_2 (manganese dioxide). Historically, a lot of minerals were named after a person, some by themselves and some by others, but they named this mineral nsutite—

[Tape 13, Side B]

Fuerstenau: They named the mineral nsutite, after Nsuta, which was the town in Ghana where the manganese mine was located. I remember Zwicker saying it took them a year or two to go through whatever the world mineralogical organization requires to get approval that this indeed is a new mineral, an unknown mineral, and to get the name approved. Anyway, it was not called zwickerite but nsutite.

A lot of the effort was directed towards manganese ores. There was a geologist in New York named Virginia Mee. I never met her but talked to her a lot on the phone because she forwarded samples that would come to her from field geologists. Eventually, I guess at some AIME meeting after I had left Union Carbide, Frank Aplan introduced me to her. After I had been here at Berkeley, maybe two or three years or something, she came by and visited me and she told me that Olaf Rove suggested that maybe she ought to go back to graduate school because in the New York office of Union Carbide she was never going to meet any young man. She later applied and came here to Berkeley as a graduate student in geology, and that fall took a look at all the

incoming new PhD students and latched onto one, later telling me, “Roger really would like to get married.” And I said, “What the hell are you waiting for?” because she was not a young kid. She married this fellow student who became a great giant in geochemistry.

Swent: What was his name?

Fuerstenau: Roger Burns. Just digressing a little bit, he worked for me as a postdoc for almost a year—a postdoc while writing his thesis. Using an electron probe, he developed techniques to determine the distribution of copper, nickel and cobalt in deep sea manganese nodules, something that we will talk about later. Roger went to England, was a New Zealander who had a degree in chemistry, did a PhD in geology here, to do a postdoc for two years at the University of Cambridge, where he wanted to really learn more petrology, petrography, and so on. They then went back to New Zealand. He wrote me, because I had got to know him well, that he had gotten permission and money from the university to buy some fairly expensive equipment, but the government wouldn’t give an import license to buy it. That really discouraged him.

There was an opening at Oxford and they asked him to apply, and the day it closed they offered him the lectureship at Oxford. When Peg and I visited them there about 1970, Virginia said, “You know, Roger made more as a postdoc for you than he’s making here on the faculty at Oxford.” She told me that there was an opening in geochemistry at MIT, and I said, “Roger really ought to take that,” and I urged him to apply for it. He went there and became a big giant in geochemistry at MIT.

Unfortunately, at the age 53, just a handful of years ago, he got stomach cancer and died. A very outstanding person; it was an unfortunate set of circumstances. So going back to my Carbide days, I had a lot of geologic interaction with Virginia by letter and by telephone.

An Eventful AIME Annual Meeting in New Orleans, 1957

Fuerstenau: A couple of times you asked about AIME. I assume you were always involved with AIME and WAAIME stuff, or not?

Swent: Not always, but just about. I heard about it from infancy, surely.

Fuerstenau: I’ll tell you about the ‘57 Annual AIME Meeting in February in New Orleans. A fairly large number from Niagara Falls went to New Orleans for that meeting. My hotel roommate was Ernie Peters. At that meeting, Ernie Peters and two others from Comminco received the Extractive Metallurgy Award for the most outstanding research paper in extractive metallurgy, work that he did at Trail [B.C.] before he even came to Union Carbide, and actually before he went back to graduate school. This was a detailed analysis of the

thermodynamics and kinetics of zinc slag fuming. The slag from a lead smelter runs 10 to 12 percent zinc, maybe, and by putting it through a rotary kiln, the zinc oxide can be vaporized. So Trail constructed a zinc vaporizing plant based on their analysis. He and his two associates at Trail wrote a paper on this, and that led to their receiving that major award there in New Orleans.

Another incident took place at that New Orleans meeting that I would just like to tell you about. Dick Charles—R.J. Charles—who I shared a lab with at MIT and who was now at GE, presented a paper at that meeting on work that he had done the year that he stayed on at MIT. This was an innovative approach to the theory of comminution: what's the relation between product size and energy in grinding? I don't know if I mentioned earlier that an Austrian named Rittinger had postulated in 1867 that energy for comminution is proportional to the new surface produced. Later a German researcher named Kick said that the energy for comminution is proportional to the strain energy in the volume of the particle required to break it. So there was always this technical argument in comminution: was it Kick or Rittinger's law that described the energy required for crushing and grinding?

About 1952, Fred Bond from Allis-Chalmers published a paper that he called, in a modest way, "The Third Law of Comminution," and it turns out that Allis-Chalmers used this to design ball mills and it's a kind of straightforward, quite easy to understand—not necessarily too accurate. Bond devised an experimental way to determine the energy for grinding minerals and ores plus a way to express the results in terms of a single number that is called the "Bond work index." It turns out that really if the energy relationship is proportional to the surface produced, you've got an equation where the energy for comminution is inversely proportional to the square of the particle size, and if it's proportional to the volume of the particle being broken it's just got an exponent of one, and Bond gave it one and a half. He's in between, an average of the two, and his analysis leads to a square root relationship. People still, to this day, quite widely use Bond's methodology.

Charles came up with a more correct approach. [telephone interruption]. What Charles showed was how comminution energy relates to the slope of the size distribution on a Gaudin-Schuhmann plot, which then said that the size distribution as affected by the material properties would then affect the energy relationship. He had conducted a lot of grinding experiments at MIT and written a paper on this, which I guess had already been published the year before, and he was presenting it orally at one of the sessions. I happened to be there. Whoever was the chairman of the meeting introduced Charles, and just as he started to speak—the room was a big room and was full of people, and a lot of fairly senior people, which probably were the kind that wouldn't necessarily be at a technical session anyway—all stood up in unison and marched out. Obviously it was a protest in relation to Bond because Charles was saying, "Bond's approach is wrong, not really right." They weren't going to listen to the ideas that Charles was proposing.

But all of these old-timers obviously had planned this protest and stood up like they were soldiers and marched out of the hall together. As a fairly young guy presenting one of his first papers, it must have taken him aback a bit. You would never see that today, knowing the way people are, but for some reason they weren't going to let this young guy show up their old friend Fred Bond. We're talking about maybe two hundred people getting up and walking out.

Swent: Refusing to listen to a new idea.

Fuerstenau: They were trying to make their statement of some sort—

Swent: And you said it wouldn't happen today. Why? What's the difference?

Fuerstenau: Today, even in applied fields, research and engineering analysis is based on more scientific and mathematical foundations and less on emotion, as was the case in many ideas put forth fifty years ago. By the way, Fred Bond had published many papers on comminution in the thirties, forties, and fifties during his career with Allis-Chalmers, which was a major manufacturer of crushers and ball mills. After Bond retired, the research division of Allis-Chalmers had several other research engineers who carried out some excellent studies of comminution fundamentals and published numerous papers, such as Bert Bergstrom, John Gilvarry, and Tom Meloy. Unfortunately, we seldom see industrial publications of the kind that came out of Allis-Chalmers when that company was a major equipment supplier.

Swent: This has nothing to do with comminution, but talking of changing times: I was at that same meeting in New Orleans, and it was my first really painful experience with Jim Crow. I had had a previous experience in Washington, DC, in '45, trying to meet up with my African-American college friend in Washington D.C., and we couldn't eat together. In '57, at that meeting in New Orleans, I was pregnant and went with Lang to that meeting and wanted to get on a streetcar to go from something to our hotel. The front part of the streetcar was full and the back part was completely empty, and I was very uncomfortable and I wanted to sit down where there were lots of empty seats and I was not allowed to sit there because it was reserved for black people. I was really furious and also kind of ill—I wasn't feeling good, and that's why I had left the meeting to go back to the hotel, because I wasn't feeling very good. I was forced to stand rather than take a seat.

Fuerstenau: Did they have it sectioned off, or was it just—?

Swent: I don't think there was a physical section but there was a conductor, and he refused to let me sit down in those seats because I was white. I was just about as angry as a person can be. It gave me a real feeling of that stupid senseless—and how it hurt people. I mean I wasn't hurt badly, but I was angered, and that was '57 in New Orleans.

Fuerstenau: Going to New Orleans, our plane went to Atlanta and it was quite shocking to me to see the restroom situation in the Atlanta airport where there were four signs: white male, colored male, white female, colored female—all four right in a row—the first I had ever seen any of that.

Helping with Reorganizing the Minerals Beneficiation Division of AIME

Fuerstenau: By the way, for at least half my time at Carbide, Spedden was there at Niagara Falls before he went to Union Carbide Ore Company in New York City. He was very active in the National AIME, and at that time was the Secretary-Treasurer of MBD (Minerals Beneficiation Division). They had a newsletter that was published probably three or four times a year and sent out to MBD members—a little newsletter with a dozen pages. There were cartoons in it. I remember drawing some of them.

Anyway, Rush Spedden was involved with plans for the reorganization of the Minerals Beneficiation Division and so I ended up having a fair amount of discussion with him, suggestions, helping rewrite bylaws, and so on. I proposed that there be established what I called then a Basic Science Committee—those were the words we used then—which would have papers on processing fundamentals. The MBD committee structure would be the same as before, with such committees as Crushing and Grinding, Concentration, and so forth. There may have been eight or nine MBD program committees. I thought what was needed was one where papers on fundamentals would be presented. That got established and was put into the system.

Some few years later I thought, “Well, about basic science; it’s not the science we’re talking about but processing fundamentals.” We changed the name to Fundamentals Committee, so now it’s called either as the Fundamentals Committee, or Mineral Processing Fundamentals. It turns out today that the Fundamentals Committee may have almost as many sessions as all the rest added together. There’s really a lot of interest in the more fundamental papers, and researchers who are interested in presenting and discussing their results should have a forum to do so.

Swent: It’s really needed.

Fuerstenau: Oh yes. At the time I looked at it as a way to encourage younger people. Early on I also said, “Younger people in companies often are only able to go for a couple of days to meetings, so why not have the fundamentals meetings always the first couple of days, at minimum. So that even became part of the meeting structure some years back. As I remember, even Carbide may have restricted how many days you might attend meetings, because it costs money to be there five days versus two days.

[Added by Douglas Fuerstenau during editing: In 2003 or thereabouts, the handful of the members of the Minerals and Metallurgical Processing Division of SME who were present at the business meeting voted to adopt a change in the organization of the technical committee structure of MPD, which included eliminating the Fundamentals Committee. I was there and did not fully realize what this might mean. The latest MPD sessions at the Annual SME Meeting have really been going downhill and seem to have little significance, with only a few papers of any note, and in my opinion it is because of this change in committee structure. I don't recall many, if any, of those concerned with mineral processing fundamentals staying for the business meeting, by the way. Only the future will see whether mineral processing research sessions in SME survive or regrettably go down the tubes.]

Planning for the Fiftieth Anniversary of Froth Flotation

Fuerstenau: What I consider to be the major AIME event of 1957 was the beginning of the planning for what was called the fiftieth anniversary of flotation in the United States. A committee was set up, and I suppose Spedden, in his role, suggested that yours truly be on this committee. I remember flying out to Salt Lake City for the first planning session. That would have been my very first trip to Salt Lake.

I assume that the person who had the idea for this was somebody who was a big man in the world of industrial mineral processing named Norman Weiss. He, I think, graduated from MIT in metallurgy about 1922. Only many years later did I learn that he had graduated from MIT, but during the time that I knew him he had worked all over the world, in gold plants, flotation mills, was with Dow Chemical, AS&R [American Smelting and Refining], and so forth. He was sort of like their chief corporate mill man. Somewhere—I have a book that's probably packed away in a box which is his own personal biography that he had published. He sent me a copy of this book which I read completely. He spent a lot of years in Mexico and South America involved with the design and performance of numerous milling operations, and so on. You just don't see people with his interest and his great technical competence around operations today—and who participate in technical meetings.

An engineer who worked for him named Dick Vincent, who, I think, was at Silver Bell in Arizona in charge of the mill there for AS&R, was named general chairman of the Fiftieth Anniversary of Flotation Symposium. I am assuming that the whole idea came from Weiss but that he wanted somebody a little younger than him to take on the top job. I was named chairman of the editorial committee. You may know, the first flotation plant in the U.S. was at Timber Butte in Montana, right outside of Butte, put into operation in 1911. This symposium was to take place in 1961, with a four-year lead time, to commemorate that historical date. By the way, Dick Vincent was a student of Gaudin's in the late thirties at Montana School of Mines.

I took it upon myself that we should publish a volume separate from the symposium proceedings. I first proposed that X, Y, and Z be made members of the editorial committee, and I would suggest who they might be, and then I proposed that we would have chapters in this volume on different subjects ranging from fundamentals to plant practice, et cetera. Of course the general committee all agreed on the membership of the editorial committee, and the editorial committee members subsequently agreed to serve and then I went down the line and suggested that we ask A, B, C to be the authors of these various chapters. Everybody agreed and so on, and then I approached the various people and the eventual result was the book, *Froth Flotation—50th Anniversary Volume*, which, if the AIME still had it in print today, it would be a very valuable book.

In the front of it, for example, is a picture of the Timber Butte Mill taken by a person who worked in that first mill, and I think there's a page—his name was Pierre Hines—and regrettably I can't find that photograph any more. I must have used it or loaned it to someone. I had a copy of this photograph which he took through the window, because that operation was all supposed to be a big secret in 1911 according to Mr. Hines.

Anyway, what I did then was approach all the prospective authors, and got virtually 100 percent acceptance. A couple of the early chapters cover the early historical aspects of flotation. An important chapter is one that I thought would be extremely valuable, namely a total survey of the magnitude of the flotation industry in the United States, including tonnages of all types of ores, types and consumption of reagents, energy and water consumption, and so forth. I wrote to Charles Merrill who was head of the Division of Information in the Bureau of Mines—I don't know if he was any relation to Merrill of cyanide fame—and he agreed to do this. He and his colleague, James Pennington, prepared a survey form that they sent out to all flotation plant operators, and they got complete reagent usage in the United States for every single flotation plant, including copper, lead, zinc, iron, phosphate, et cetera. Later, Bill Bloecher, a middle executive whom I knew at American Cyanamid and who had received his MS degree under Gaudin at MIT, jumped me and said, "That was terrible what you did, to display our business like that!" because I dreamt up the idea. The Bureau of Mines conducted these surveys every five years after that until they were abolished.

Swent: It had not been done before?

Fuerstenau: No, although only a while ago I saw that the Bureau of Mines together with someone at Colorado School of Mines had published similar but less detailed surveys annually in the 1920s. I told Guy [Harris] to make sure that one of those five-year summaries be put into the appendix of his oral history, because it clearly shows reagent usage.

There is an excellent chapter on the thermodynamics of flotation by Phil deBruyn, and one on the fundamentals of nonmetallic mineral flotation authored by Frank Aplan and myself. The volume has a classic chapter on flotation kinetics by Nat Arbiter—one on statistical methods in testing which was written by Bill Griffith who later became head of Hecla before he retired. He had published several papers on the application of statistics to mineral processing—

[Tape 14, Side A]

Fuerstenau: For sulfide mineral flotation plants, with a certain amount of advice, I went to two people. One was Elmer Tveter, who was at Dow and was a very important part of their reagent business, as would have been mentioned in Guy Harris's oral history. The co-author was Frank McQuiston¹² from Newmont. The way they assembled their chapter is really extremely useful—to see the flowsheets and operating data of the major sulfide plants operating around the world in the late 1950s.

A similar chapter on nonmetallic mineral plants was written by Bob Baarson of Armour Industrial Chemical Company. Bob received an MS degree from Montana School of Mines in 1949 and was quite a good friend of Frank Aplan. His career was with Armour, who was the major producer of amines and fatty acids as flotation reagents. I visited him there in Chicago a couple of times and used to see him at AIME meetings quite regularly.

One chapter that was truly a major contribution involved the economics of flotation plants. To write that chapter, I got Norman Weiss and Stan Michaelson. Michaelson was the chief engineer of Kennecott in Salt Lake—great engineer. They wrote about the operating economics and then they prepared detailed capital costs for three scales of plants, like a five hundred and a two thousand ton-per-day lead-zinc mill and a ten thousand ton-a-day copper concentrating mill, and I mean right down to the cost of all equipment and installation, and so on. They were using actual operating and design data, I am sure. An amazingly useful chapter because it was dated and construction cost indices given. Although it is out of print by AIME, you can find it on the Internet. It still should be useful by updating using the engineering cost index.

The chapters in the volume are totally separate from papers given at the Flotation Symposium held in 1961. There were two or three papers at the symposium that we should have incorporated into the volume, but it only occurred to me years later. They would have been rich additions.

¹² Frank Woods McQuiston, Jr., *Metallurgist for Newmont Mining Corporation and U.S. Atomic Energy Commission, 1934-1982*, 1989

I did all of this groundwork and then the editing took place later on. I worked until 11:00 night after night, on the editing of those papers. Somebody I'll talk about later, Fred Ravitz, who was a professor here, also did a lot of editing, and I know with one of them he made the comment after rewriting their chapter—that the authors would be embarrassed if we hadn't rewritten it. [laughs]

When the book finally was ready to come out, Spedden told me that the Executive Committee in the AIME had decided to list me as editor of this volume, which, of course, I edited far more than most edited volumes that you have ever seen, starting with the planning of it.

This project was underwritten by the Rocky Mountain Fund. [Gets the book, and reads from it] We called it the *Froth Flotation—50th Anniversary Volume* published 1962. The original costs came from AIME's endowed Rocky Mountain Fund; and it turns out that they made a lot of money from it because any profit goes back to the fund. I'm called the editor, but because my name is not on the outside cover, I was told by the Secretary of the IMM that Brits don't give me credit as editor.

Swent: It's on the title page but not on the spine.

Fuerstenau: Right. Here you can see all these flow sheets of Tveter and McQuiston. And here's this detailed chapter on economics by Weiss and Michaelson. I hope Joklik told you a little bit about Michaelson. I was told that, in an indirect way, he was sort of responsible for the breakup of Kennecott. Stan Michaelson simply was just an outstanding engineer doing his job. Because the power consumption there at Bingham was obviously huge and had to be reliable, I was told that he suggested, "Why don't we buy a small coal mine there in Carbon County, in Utah, so that if the electric company ever gives us trouble," I suppose Utah Power and Light, "we have our own coal mine that we can use to generate our own power." So Kennecott bought this coal mine—I assume it was not a huge coal mine—and that was the end of it. Whether they mined it or sold the coal I don't know. After Kennecott had bought Peabody Coal, owning this coal mine led to their being a victim of anti-trust action that was active in those days in Washington, and they had to spin off Peabody Coal. Anyway, Stan Michaelson was the person that ran all the engineering, and design, and so on for Kennecott in Salt Lake.

Swent: There were two Michaelsons.

Fuerstenau: There was Big Mike and Little Mike, and Stan Michaelson was Little Mike, and Big Mike was C.D. Michaelson who had come up from Braden Copper. I never met him. I think he was probably executive vice president, or that sort of level, because Frank Milliken was president for a long, long time. Did you ever meet Milliken?

Swent: No.

Fuerstenau: I did once. During a very long Kennecott strike, Milliken visited a number of different mining colleges around the country and spent some time with me.

The Robert Lansing Hardy Gold Medal

Fuerstenau: Another AIME activity that got started during my time in Niagara Falls was the establishment of the Robert Lansing Hardy gold medal. This award was established to recognize the most outstanding metallurgist under the age of thirty in the United States. Professor Arthur Hardy—a physics prof at MIT who had an office next door to Gaudin—had a son who was a metallurgist. At age twenty-five his son, Robert Lansing Hardy, had a brain tumor and died. The father, Professor Hardy, was very distressed and wanted to do something with the insurance, so Gaudin said, “Why don’t you establish an award to recognize a young metallurgist?” Gaudin, I guess, helped work out the details of this award which is now handled through the Metallurgical Society of AIME—the Robert Lansing Hardy Gold Medal Award.

It turns out that I was the first recipient. Looking back on it, it’s really the only award that I got that probably came through Gaudin. Except for the Raymond Rossiter Award, all other recognition has come to me after he had already left this earth. I was named the recipient for 1956 and was given the award at an early 1957 meeting of the Niagara Frontier Section of AIME. I remember I gave a talk that I prepared on mineral separation starting from ancient times all the way up to new developments. Back then, the medal was a two-ounce piece of gold—a nice medal. Today, I think due to the price of gold, it is gold-plated. We’re talking about an original endowment, maybe, of five thousand dollars, and that probably could not keep up with the increase in the price of gold.

I remember the day after I got that medal, one of the laborers, kind of a pleasant and humorous guy, who worked for me in the pilot plant there at Carbide, Jim Sullivan, said, “That medal and a dime will get you a beer anywhere.” [laughter] Which, of course, will tell you something about the price of beer then. He put it in the right perspective. That was a nice occasion that I never had any idea that it would come about. Through the years I have seen the list of who has been a recipient of the Hardy Award. I think we have—actually, five faculty members in our department who are recipients.

Swent: Is it given every year?

Fuerstenau: Annually by TMS/AIME to a person who has not reached his or her thirtieth birthday at the time of the annual meeting. In the 1960s, I was on the selection committee for four years, and one of the times during that period Craig Barrett got the award; he was then at Stanford. He’s now chairman of Intel. He was

an academic who decided to move to the world of industry some years ago, joined Intel, and moved up to CEO.

In all these years, there has been only one other processing person who received the Hardy Gold Medal. That was a former student of mine, John Herbst, who has become a giant in mineral processing control. Other than that, they've all been more or less physical metallurgists or materials-property-types of people.

I'll just comment briefly about a situation that occurred when I was on the selection committee. One year the best candidate was twenty-eight and another very good candidate was twenty-nine. I said that the twenty-eight-year-old candidate was better but since he had another year of eligibility, I would go along with selecting the older candidate. That is what happened, but we should have gone with the younger candidate since someone the following year could have been a super nominee and beat him out. I made a kind of a mental note of this kind of action.

Swent: You changed your mind on that.

Fuerstenau: Yes. And by the way, that person did get the Hardy Award a year later in 1965 and whose field was fracture mechanics, was first at Stanford and then at UCLA. One morning he flew from L.A. down to San Diego with regard to some problem related to an airplane crash. He was on that PSA jet that was landing in San Diego and was struck by a small plane with a student pilot at the controls. All were killed.

I have seen the full list of the Hardy recipients and it really is quite an outstanding group of metallurgists and materials scientists—several of the earlier recipients were later elected to the National Academy of Engineering. Quite a number are graduates of this department or are faculty members here.

Swent: It would be a good club to belong to.

Fuerstenau: Yes, yes. And I was the initial Hardy Award recipient.

Living in the Buffalo—Niagara Falls Area

[Interview 7: August 13, 2001 in Berkeley, CA, Evans Hall]

[Tape 15, Side A]

Swent: We have been talking about your time in Buffalo at Union Carbide, but we didn't get into any of your personal or private life. Is there anything you wanted to pick up there on living in Buffalo?

Fuerstenau: Sure. I think I mentioned earlier that Peg had taken a couple of classes on child psychology at the University of Buffalo, when we first got there. As a student, she was able to get student tickets to the Buffalo Symphony, and the seats were chairs at floor level in the orchestra pit, right in front of the stage. The orchestra was up on the stage. I suppose if they were performing an opera, we were where the orchestra would have been.

Well, anyway, the conductor was Josef Krips, a very outstanding conductor from Austria. At the time I think he was considered the world's foremost Beethoven conductor. So it was really something to sit there about ten feet from the conductor. It kind of surprised me how much singing and sort of talking he did during the concert, or almost yelling at some of the players. There was another time when Zino Francescatti was playing the Mendelssohn Violin Concerto and you could see a bowstring let go on his bow, and it was flying around in front of him, and you could see him watching it, and so on, but of course, he couldn't stop until there was a pause in the music. Then he very abruptly jerked it off. Later, as you know, Krips conducted the San Francisco Symphony for many years, or quite a number of years, after leaving Buffalo.

After arriving in Buffalo we decided to start a family and our first offspring was born April 28, 1957, Linda Margaret. So that was a very pleasant change in our life, and of course kept Peg fully occupied.

Life around Buffalo: Buffalo's pretty flat, absolutely flat like a table-top. My wife said that in her recollection Buffalo was ten months of gray sky. I don't have that recollection because I was then inside a building at work all day long, and again I was in Niagara Falls which might have had slightly different weather.

I would like to just comment on Niagara Falls. When we first came there for an interview, Rush Spedden took us over to the Canadian side to see the falls. It was winter. Snow piled up about eight feet, or something, and Spedden parked the car and said, "You can get out and go over there across the snow, and then you can see the falls," while he sat in his car. [laughs] I later knew what he meant. Every time there was a visitor we always went to the falls, and

I enjoyed it very much, summer or winter, because in the winter there would be a lot of frozen spray.

Swent: Did you reach a point where you had seen it enough?

Fuerstenau: Oh yes, but it's very spectacular, as you know. One thing we did in the summer a lot was go swimming in Lake Erie, on the north shore, which meant going into Canada, particularly with the Pavlovics. Water was fairly warm but waves could get fairly high, because Lake Erie's not very deep, sixty feet or something. In contrast Lake Ontario is several hundred feet deep.

So we regularly crossed the border at the Rainbow Bridge. In those days crossing the border only involved saying where you were born, and you were on your way. One thing I remember was \$1.05 U.S. per Canadian dollar. Now I think it's \$0.60 U.S. for a Canadian dollar.

Swent: They were almost the same then.

Fuerstenau: Well, maybe they were even a little bit ahead. That was a typical summer activity—we did that a lot of Sunday afternoons with our friends.

Swent: You had wanted to say something about Columbia University also, I think.

Fuerstenau: Yes, my wife had an uncle, Bob Watt, who was married to one of her father's sisters, and Bob Watt was a trustee of Columbia. I gather there's now a building named after him, or residence hall at Columbia, Watt Hall. Anyway, I had lunch with him one time in New York, and he told me that they had just gotten the Henry Krumb money—Krumb having made his fortune in mining.

There are the Krumb Lectures, you know, in SME or AIME today, and they renamed the School of Mines at Columbia the Henry Krumb School of Mines. So this would have been, I'll just say 1957-ish, that they had gotten \$8 million which was what Krumb left to the School of Mines. What always just made me chuckle a little bit is Bob Watt commented that the Krumbs had no children and his wife also had \$8 million, and he said, "And now we're out courting the old lady so we can get her \$8 million too." [laughs] And that was his inspiration.

My former student, Ponisseril Somasundaran, who's a professor at Columbia and holds the LaVon Duddleson Krumb Chair in Mineral Engineering, has always said that Henry Krumb's will is very tight, that it can only be used for the School of Mines. But her will is a little bit looser, so that if it's not used for that by Columbia it can go to hospitals and medical schools somewhere. But anyway, as I say, I chuckled at the time.

Swent: They did end up getting the whole thing.

Fuerstenau: They ended up getting it, and that has kept them in business ever since. Although I've always learned from others at Columbia that other departments, people, keep trying to nip away at it, and I have heard that Columbia is using some of the corpus for paying bills, which is not a good thing.

Sometime later Bob Watt told me that he had actually approached—this is while I was at Carbide—Professor Hassialis of the School of Mines, whom he knew well and whom I had met through the years, and told them that they ought to hire me. I didn't know any of that at the time, but apparently they had no real opening then until after Arbiter left for Anaconda.

Swent: Would you have liked to go to Columbia?

Fuerstenau: Well, I don't know. At that time I was used to being in the East, so it might have been okay. But I wouldn't have wanted to do so now.

Another change took place. My friend Frank Aplan, after he finished his degree at MIT, went to work in research for Kennecott, but apparently they were not happy living in Salt Lake City, neither Frank nor Clare, his wife. You may have learned more about that from his oral history, but Frank said he was interested in looking for another job, so I said, "Let me check out here. I think there would be a good opening back here in Niagara Falls." So eventually Frank joined the Mineral Processing Section during my last few months there, so once again our paths crossed. He eventually worked—as you probably know from the record—ten years for Union Carbide, on their mineral and ore problems. First at Niagara Falls and then down in Sterling Forest, near New York.

By the way, for the concentration of manganese ores, Carbide was using heavy media separation methods that had been worked out when Bob Shoemaker was in the Metals Research Labs. While I was there, a German company was producing spherical ferrosilicon that could be used to produce suspensions with a specific gravity of about 3.3 instead of about 2.9, if I remember correctly. One problem was that some of the ferrosilicon particles had a low density and we quickly found out that the reason was that some of them had bubbles in them. All of this had become important about the time that Frank arrived so I had him initiate an investigation on the viscosity of ferrosilicon suspensions used in heavy media separations, which was very important in upgrading manganese ores. Frank maintained an interest in the rheology of ferrosilicon suspensions for quite a few years after that, not only during his time at Union Carbide but later at Penn State.

Career Structure in Union Carbide at the Metals Research Laboratories

- Swent: What sort of career structure did they have at Union Carbide?
- Fuerstenau: What I find interesting is: not very long after I got there they started asking me, "Are you interested in a management ladder or a scientific ladder?" Literally this was just within months of my first joining them. The research director said, "Well, the pay structure is the same whether somebody wants to advance through the scientific ranks or through the management ranks." I noticed that the people in the scientific ladder were about twenty years older than those in the management ladder. They may have had the same pay, but it must have come at a different rate. But still, I was approached several times on this.
- Swent: What was the significance of that?
- Fuerstenau: They tried to say there was no significant difference. I'm saying a little bit that there was.
- Swent: In what way?
- Fuerstenau: People moved up faster through the management ladder, whether they became group manager, research director. But what they finally did was to originate a group, or a structure, that they called Basic Oriented Research. The object was to be able to work on basic research but still applied to the company problems. So I was classified in this in-between category that didn't change my job but put me administratively in a structure called the Basic Oriented Group.
- I did do, with Walt Zwicker and a technician, one or two pieces of fairly fundamental but applied research that eventually ended up as part of a publication, so I had a couple of papers that were based on work done there at Union Carbide, which I suppose is basic-oriented.
- Swent: What were they?
- Fuerstenau: One was this vibratory ball milling that I mentioned the other day, which was totally applied engineering science. The second one had to do with so-called activation phenomena in sulfide mineral flotation. I was really completing some work done at MIT. Actually, I always kept publishing about two papers per year, a lot of that based on what was in the bank from MIT. But I did do these couple of things that Carbide allowed me to work on, because if I were going to pursue an academic career I needed to have that on my record. With Walt Zwicker, we worked on reacting alcohols with the surface of oxide minerals, but none of that ever came to completion. He had already constructed an elaborate apparatus to do this before I had arrived.

Coming back a little bit to management attitudes, I do remember several times in, let's say, group meetings where the section leaders would be meeting with the group manager, and hearing about some researcher who was not in the management structure being referred to as quote-unquote: "the man on the bench." Quite often I heard, "Well, he's just a man on the bench." That was after Spedden had gone to New York. I took that in a little bit of a derogatory way, and it really helped me decide what I ought to do was really return to the academic world, because if you're in a research university you can be involved with very detailed science, engineering science, or engineering, and have full respect stature-wise, and not be this "man on the bench."

In many respects that sort of cemented my ideas that I wanted an academic career. I always considered as ideal, and still do, the kind of career that Professor Gaudin had as a professor in a research university at the leading edge of new engineering science in his chosen field. Gaudin didn't get involved with being a department head, or dean, that sort of thing, although he did spend a lot of time on consulting with industry. Overall, he achieved great stature while being involved on technical details his whole career. That's what I decided I wanted.

Swent: Research and the teaching.

Fuerstenau: Oh yes, because that's all part of it: dealing with young people and new ideas.

Visiting Monthly Seminar Speakers

Swent: Were you getting information at the same time from outside Union Carbide? Were there other contacts?

Fuerstenau: Oh yes. Carbide had a monthly seminar speaker. They were outsiders, but I do remember, still chuckle over, one inside metallurgist from another Carbide company. He was an older metallurgist—remember, I was only twenty-seven, twenty-eight, so when I speak of older, they're younger than I am now. He came from Haynes-Stellite, which was a Carbide division that produced cobalt alloys called stellite. He talked about the properties and uses of stellite. The only thing that sticks in my mind was that he said, "You know one of our products is that we make about four hundred testicles per year out of stellite." He said there's about four hundred men annually who want these for cosmetic reasons.

Swent: Interesting product by Union Carbide.

Fuerstenau: Right on. But I would like to comment on two or three major speakers. One was Henry Eyring, who was one of the major physical chemists of the middle part of the last century and who is known for his absolute reaction rate theory, predicting the rates of chemical reactions. He had been a long-time professor

at Princeton and then went to University of Utah where he was for the latter part of his life. Eyring was a very exciting, animated lecturer, who was another one who did his PhD at Berkeley in physical chemistry under G.N. Lewis. I recently read a book on the history of physical chemistry and the author mentions two people who should have been Nobel Prize winners but were not: one was G.N. Lewis and the other was Henry Eyring.

What I find interesting about Henry Eyring is that he received his BS degree from the University of Arizona in mining engineering. As I just told you, he was one of the giant physical chemists of the past century, and he went to the University of Utah and got a master's degree in metallurgical engineering. Milt Wadsworth there, who worked closely for years with Eyring, said that Eyring once told him that when he got his master's degree he went out to the copper smelter at Kennecott to work, and the smelter superintendent took him out in the plant where it was full of blue and white smoke and SO₂, and they had to put wet handkerchiefs over their nose and mouth to walk through the plant. When they came out the superintendent according to Eyring said, "Henry, you do this job well and in a few years this will all be yours." [laughter] And Wadsworth said Eyring decided on the spot that he was going to switch to chemistry and came here to Berkeley to study for the PhD with Lewis.

Swent: He didn't want it.

Fuerstenau: That's right. Anyway, Eyring did a lot of work on modeling chemical phenomena, rates of reactions, solution viscosities, and so on. I'm sure that his background in engineering flavored his approach to chemistry. As I say, he gave a very animated lecture.

Wadsworth one time said that Eyring was talking to him a little bit about genetics and brains. His father, obviously a strict Mormon, had two wives and had to move to Mexico, and he had about a half-a-dozen offspring from one wife that were just ordinary souls. But Henry and his brother were the offspring of the other wife, and they're both very sharp, which is a little bit of an indication that it's not just environment that determines our intelligence. Direct proof of that.

Swent: Had he been born in Mexico?

Fuerstenau: He was born in Mexico, which probably explained why he went to the University of Arizona, which is right across the border.

Swent: There was a big Mormon exodus there at the time that polygamy was outlawed in the States. A lot of Mormons went to Mexico.

Fuerstenau: Yes, because Eyring must have been born around the turn of the century.

- Swent: Remember when Romney ran for president? There was a big issue of that because he was actually born in Chihuahua.
- Fuerstenau: Oh, is that right? He probably had multiple mothers too. I mean he had one, but in the family.
- Swent: But then the question was whether having been born in Mexico he could run for president. That was quite an issue.
- Fuerstenau: For some reason that eluded me. I never picked that up. That's interesting.
- Swent: There were a lot of them that went at that time to continue polygamy. There are still a lot of Mormons in Chihuahua who are descended from those.
- Fuerstenau: Whether Eyring was in Chihuahua or not, I don't know—but I was there one time. It looks a lot like, as you well know, like Wyoming, Utah. The area is called the Chihuahuan Desert. There's less desert there than around Hermosillo, that's called, I guess, the Sonoran Desert. That's much more of a desert than around Chihuahua.
- Swent: So Eyring might have been part of that.
- Fuerstenau: I bet he was. That's why the father apparently moved to Mexico. As I said, it was either late eighteenth century or early nineteenth century—around the turn anyway.

Another interesting speaker was Norman Hackerman, a professor of chemistry at the University of Texas, who was an excellent surface chemist and one of the leading authorities on corrosion chemistry. I had some time that day to discuss some of our surface research with him that day. Hackerman later became president of the University of Texas and served for some years as chairman of the National Science Foundation Board in Washington.

We had one more speaker of note to me. That was Professor Earl Parker from Berkeley—a physical/mechanical metallurgist—who gave an interesting seminar. Then after lunch he spent the afternoon in my office telling me all the great pluses about why I should come to Berkeley. So that visit started the renewal of my interest in coming here. In the morning he was tied up with giving a talk and having discussions with the physical metallurgists in the lab, but in the afternoon he spent the time talking with me about how I ought to consider coming here. As I told you, I had come out here to Berkeley three years or so earlier and decided this was not the place to come, but by then the person that they wanted to leave had left and so there was a possible opening here under different circumstances. That started me thinking strongly about coming back here the following year.

VII KAISER ALUMINUM AND CHEMICAL CORPORATION, MINERAL ENGINEERING LABORATORY, 1958-1959

Interviews and the Decision to Join Kaiser Aluminum in California

Fuerstenau: One day, I had a call from somebody named Albert Copp from Kaiser Aluminum. He asked if he could come and talk to me about joining Kaiser, and so I said, "Well, yes, you can come," and so he flew to Niagara Falls. I remember him taking us out to dinner in the hotel across the river in Canada which has a great dining room overlooking the falls, and discussed coming here, or out to California for Kaiser. One part I remember of our dinner discussions, which didn't impress Peg too much, was he said that the previous year he had been away from home 54 or 59 percent of the time. [laughs]

Swent: He thought that was a selling point?

Fuerstenau: He thought that was a selling point whereas any wife obviously did not. Kaiser had a laboratory that they called their Mineral Engineering Laboratory, which was mainly concerned with the processing of various bauxite ores as potential sources for alumina, and they needed a person to head this laboratory. There were a dozen people, or something like that, in the laboratory. It was not part of the Kaiser Aluminum Research Division, but part of the Mineral Resources Division of Kaiser Aluminum and Chemical Corporation.

Swent: Where was that?

Fuerstenau: Permanente, California. Permanente, which was obviously the source of the name Kaiser Permanente. It was on Permanente Creek, which is just on the south side of Los Altos Hills. Kaiser Cement has a huge cement plant there. At that time I think it was still the largest cement plant in the world. There were buildings there that Kaiser used for the research laboratories of Kaiser Aluminum, and there was a new building nearby to house the Mineral Engineering Laboratory. There were several old, shut down facilities where they produced magnesium powder, and things like that during World War II. In one of the older buildings was a rolling facility where Kaiser produced aluminum foil. I believe that Kaiser was the original producer of foil for kitchens.

Kaiser personnel asked me then if I would come out and take a look. This was now in late spring 1958, or early summer. So I flew out on a TWA flight from New York to San Francisco. These were propeller days and TWA Constellation planes had a sleeper section, so I had a berth, sleeping berth, in the plane. I got on the plane; there were five or six other people who had just come back from Saudi Arabia: Steve Bechtel, Sr. and his wife, and three or four engineers from Bechtel's Petroleum Division. So we shared this sleeping

section of this Constellation. I remember Steve Bechtel, who was obviously senior at that point, he and his wife—

[Tape 15, Side B]

Fuerstenau: Steve Bechtel was obviously senior. Even Steve Bechtel, Jr. is now retired as head of Bechtel. I guess this was, what, forty-three years ago. They then went to bed, and I think the Bechtel engineers played poker, and I finally went to bed, which at night, as I say, is a beautiful way to fly. We had to land in Chicago or Kansas City in the middle of the night. We were all strapped in so that one kind of knew that the plane had landed, but it took nine or ten hours just to cross the country in those days.

I spent a day or so visiting Kaiser facilities at Permanente, seeing what went on, meeting people there, and so on. Then I guess I stayed in the Claremont Hotel, here in Berkeley, and had breakfast the day I was going back with Joe Pask, who then was the chairman of the Department of Mineral Technology, as it was called, at UC Berkeley.

Swent: Kaiser's headquarters office was at that time in Oakland.

Fuerstenau: Oh yes, Kaiser Center was where all of Kaiser Industries headquartered—Kaiser Aluminum, too.

Swent: It must have been a brand-new building at that time.

Fuerstenau: Oh yes, you could tell.

Swent: Big aluminum building.

Fuerstenau: Right, right. It still looks nice in there, after all these—

Swent: It's a beautiful building.

Fuerstenau: It was the only tall building this side of the bay, wasn't it?

Swent: Curved, and aluminum-clad. It was quite sensational.

Fuerstenau: As a matter of fact, Kaiser was so big in those days that they had offices in other buildings around the city of Oakland—the mineral engineering lab was part of the Mineral Resources Department, and that was headquartered in another building a few blocks away from Kaiser Center. That was just indicative of the size of Kaiser.

Swent: Who were you talking with?

Fuerstenau: Probably, Ed Hassan, who was head of the Mineral Resources Department. I think Al Copp was called manager of exploration. Those probably were the main two. Then, when I visited the laboratory area facilities, probably some of the people that were working there. Al Copp, as I remember, drove me down there.

Swent: How did you get from Berkeley to Los Altos Hills?

Fuerstenau: Al Copp probably drove me down to—he lived in Orinda. I remember having dinner one of those evenings at his house, and that was the first time I saw the fog lap over the hill tops that is so familiar to Bay Area people.

At the time they asked me what I wanted, and Gaudin said you should never change jobs without 50 percent pay increase, so I said I wanted 50 percent more money. And they offered that to me. In 1958 this was great; a very, very good offer but I turned it down. I said, well, I really decided that I wanted to go to Berkeley. I had no offer from Berkeley, but I had beginnings of serious conversation that was in the process of leading to that. After a bit they came back to me and said, “Well, would you come out for the intervening year?” That I accepted, and we moved from Niagara Falls at the end of September.

They shipped our car, and we went to New York for a few days, visited Peg’s folks, and then flew out, and landed in Milwaukee. All of a sudden the plane started to circle, and circle, pretty soon I could see a wheel go up and down, and up and down, and then finally they said, “Well, we’ve got landing gear problems, and we may have some problems. Put pillows around you, and take your glasses off.”

Swent: And you had your baby with you.

Fuerstenau: The whole family, and Peg was pregnant with number two. Anyway, we landed with no problem, but the place was completely lined with fire engines. I don’t know why they had fire engines parked so close to the runway, because if the plane spun off of it, it would have plowed into the fire engines. Apparently the wheel latched. So we were late getting in Minneapolis, and I guess, as I remember, had to stay over. We then spent a few days in South Dakota on our way and finally arrived out here in California.

We rented a house for that year in Mountain View, and there still were a lot of farms down there. I would drive back and forth once in a while to Oakland, you would see airplanes spraying the fields around what is now Fremont, which is all housing today. That was wide-open farming, even as late as ‘58.

Swent: Orchards.

Fuerstenau: Not orchards around Fremont, but there were orchards in Palo Alto and Los Altos. I had maybe about a ten-mile commute with no traffic problems. None

at all, just drive along country roads and through the orchards up to Permanente.

Swent: How much did you pay to rent a house in Mountain View?

Fuerstenau: Not very much, as I remember. It was one of those tract houses, I think, they were called Mackay houses which look like the one whose name—

Swent: Eichler?

Fuerstenau: Eichler was the big one. Same [kind of] house, California tract house. We rented this from a young architect for that year, and, well, it was great. One had a certain standard of living which didn't change, so that allowed me to accumulate a down payment for a house when we moved to Berkeley. We were able to sell our Tonawanda house rather quickly, roughly for the same that we paid for it. A little bit more.

Swent: The housing here hadn't begun to get out of sight the way it did later.

Fuerstenau: You'll find out, when I get to that, it's not at all—

Swent: But in those days you could still get something reasonably.

Fuerstenau: Right, right.

The Mineral Engineering Laboratory and Search for Alternate Bauxite Sources

Fuerstenau: So anyway, the situation with Kaiser was that they had a single source of bauxite, which was in Jamaica, and apparently a year or two earlier a hurricane came through there and blew the docks down, and so Kaiser got into a big problem of having a sole source for their alumina. Bauxite, as you know, is the raw material for producing alumina, which is the raw material for making aluminum metal. In those days Kaiser and Reynolds were about the same size, and Alcoa was just somewhat larger. So here was this big aluminum company with a single source of ore, and that's why they embarked on a major mineral exploration program to rectify the situation.

I learned, after I joined them, that the person they had hired for this job previously was killed. A group from the exploration crew were somewhere in Central America and were in a small airplane that hit a telephone wire or something on take-off, and killed them all.

Swent: They were in Guyana, I think.

Fuerstenau: Was that where it was?

Swent: I think so

Fuerstenau: Okay. I had never heard about that until after I had joined them. That was the job I had as part of their exploration program. The lab may have had a total of ten, twelve people. My direct boss, Al Copp said that everybody in Kaiser who heads something is designated as a manager, so I was called manager of mineral engineering for them. There were a couple of geologists, two or three chemists that did tests and analyses, two were metallurgists, and then I added an engineer, Bob Brandt, who had been a plant engineer working in the Development Group at Union Carbide, and somehow I had gotten to know him. He had originally come from the Iron Range in Minnesota, and was very familiar with iron-ore processing. I proposed to Bob that he consider joining Kaiser, which he did. I think that I may have talked to him a little about this even shortly before I left Union Carbide. So he came to Permanente and worked for me, looking after pilot plant kinds of activities on processing bauxites. Then he stayed with Kaiser for a while after I moved to Berkeley and eventually worked for somebody else that I may comment on after a bit.

One of the metallurgists in the group, A.E. McLaughlin—and he always went by the name Mac, so I don't remember what "A" stood for, but it may have been something like Alexander—graduated in metallurgy from Cal here in 1936 and had gone to the Philippines on some kind of contract that ended in 1940, or '41, and fortunately came back to this country.

Swent: Just in time.

Fuerstenau: One thing I still remember is a comment of his: he said he graduated from Cal in '36, and he said the only courses with any meat that he took were those that he took outside of the College of Mines, which may or may not have been true. He said the chemistry, physics, math that he got outside the college, the mechanics, and fluid mechanics, I suppose, and so on, were very good, tough courses that he said gave him his real background. So those were the people who staffed the Mineral Engineering Laboratory.

Anyway, Kaiser had an active world-wide exploration program, and somebody sent us bauxite samples quite regularly from India, and I heard his name often, namely Pete Fowler.¹³

Swent: I was just thinking of Pete.

Fuerstenau: I never knew him then at all, and years later after I came to Berkeley I met Pete and actually got to know him very well and had much interaction with him during his later years. At that time he was only a name on bags full of

¹³ Hedley S. "Pete" Fowler, *Mining Engineer in the Americas, India, and Africa, 1933-1983*, 1992

rocks that had bauxite potential. As I remember from his oral history, I know he was a contract geologist with Kaiser for a year or two.

Swent: He was at Kaiser for a long time, but with different divisions of Kaiser, different companies of Kaiser.

Fuerstenau: No doubt, because there was Kaiser Cement, that again needed raw materials. Kaiser Steel needed raw materials.

A large part of Kaiser's bauxite exploration related to soils, which I found interesting. Let's see, I joined about October 1, and in December they said I ought to take a trip, a tour, to see some of the Kaiser facilities and look at some of their exploration. This first exploration trip was to South Carolina, Georgia, you know where the soil is red. That soil runs 5, 7 percent gibbsite, and gibbsite is aluminum hydroxide. Gibbsite is the component of bauxite that is leached to recover the aluminum content. Very pure aluminum hydroxide is reprecipitated from the purified leach solutions, fired to make alumina, which then is smelted in electric furnaces to produce aluminum metal.

So anyway, they had this large sampling program over large parts of South Carolina, Georgia, I think, and so we looked at some the sites. I still don't know how they would have gone in and recovered this 5, 7 percent of the component of all the soils, but at least that was some of their thinking. And at that point we drove down to Dothan, Alabama, where some of the soil around there was high in gibbsite. A lot of rain, so silica, for example, would be leached out by the weathering of clays, leaving behind a high concentration of gibbsite.

I remember spending the weekend in Pensacola, Florida, with my brother Dick, who was working as a geophysicist for Mobil near Brewton, Alabama, where Mobil had an exploration program underway at the time. I had not met his wife before, so it was nice family opportunity.

Then I went to Baton Rouge where Kaiser had its largest alumina plant, where they processed the bauxite, which obviously came up the Mississippi, to produce the alumina for subsequent electrolytic reduction. At that time Kaiser dumped all those tailings, called red mud, into the Mississippi River. The tailings were submerged into the Mississippi, and this red mud flowed down the Mississippi, and I guess it surfaced about 100 miles south of New Orleans somewhere. That was a lot of fine particles that were being dumped into the river.

Swent: Nobody even questioned it, I suppose

Fuerstenau: It wasn't questioned. Then sometime later, I don't know the year, they of course had to impound them, and being very flat around Baton Rouge, they must have had to buy up part of the countryside. This was a big operation. By

the way, every little while, various companies and researchers get interested in trying to recover minerals from the red mud, one being titania. Such attempts still go on every little while today.

They were also at that time constructing a new alumina plant in which they were going to leach bauxite in big autoclaves at a place called Gramercy, which is about half way between New Orleans and Baton Rouge. This was just being built, and I remember the Kaiser Aluminum people were a little unhappy because Kaiser Engineers had to build the plant and they could inflate the costs. Kaiser Aluminum wouldn't have the clout over Kaiser Engineers that they would have had over an outside contractor.

Whoever was the plant manager took me up on top of one of these big vertical autoclaves being constructed. We rode up one of those traveling belts used as a ladder, and I got on this thing and we went way up high. I'll tell you, I was really scared when I reached the top, but I couldn't tell him I was so scared. It took me a long time to get nerve enough to step back on the belt rung to go down. You're just hanging onto a step and then up above your head will be another step that you're holding on with your hands. Of course the people who are used to that probably hold on with one hand. I was completely petrified.

Gramercy became a very big producer of alumina for Kaiser. Just, let's say, last year I read in the paper that there was an explosion in one of these autoclaves. I think maybe two or three people got killed, and then I saw that for a month or something the plant was shut down for a total inspection. And what's this? Forty years later.

Swent: It's a pretty old plant by now.

Fuerstenau: Yes, yes. Kaiser's big reduction plant then was at Chalmette. This is where you take the pure alumina, dissolve it in molten cryolite, and then electrolyze it. You can't reduce aluminum ions with water; you have to do it with molten cryolite which is an aluminum fluoride. This is the Hall process for producing aluminum. I guess the only natural cryolite comes from Greenland, and the rest is produced artificially. When melted, it's a solvent for the aluminum oxide.

Kaiser's big reduction plant was at Chalmette, which is right adjacent to where that last battle of the War of 1812 was fought, when Andrew Jackson beat the Brits after the war was over, but he didn't know it. A big, major battle. Adjacent to the site, was this big reduction works, which was the next spot on my company tour.

In addition to being the first time I had seen the inside of an aluminum plant, I remember something we were talking about. So here we're down in the Deep South, and it's hot, really hot and humid, and out in the plant was a refrigerated stainless steel water cooler, and a big sign over it, "Whites only,"

and next to it was a porcelain water fountain, no cooling, “Colored,” with a sign over it. Isn’t that absolutely—?

Swent: Barbaric.

Fuerstenau: Barbaric. But there it was, just like that. Colored people had to drink warm water, and the two were physically beside each other, whereas in the steel mills in Chicago there was none of that. There in Chicago the change—the mine would call it the change house—there was but one shower room and lockers, for everybody—for black or white, pink, yellow.

Swent: Where you had worked the summers before.

Fuerstenau: Yes, in the Chicago steel mills where I worked in the forties in the summer. But there in New Orleans, this was now ‘58, conditions were still what I just told you about.

Swent: Ten years later.

Fuerstenau: So anyway, that was a trip directed at learning something about the aluminum industry itself, and I had an open ticket coming back, and lo and behold it was a Friday, just at the end of the fall school term for all the colleges and universities, and boy, when I called up the airline—I was there in Louisiana—about getting on an airplane, “Are you kidding?” they said. “People have had those seats since September.” So I called back to Oakland, and Kaiser being such a huge customer of the airlines, the travel people got me on the airplane. These were days of Convairs; they only had forty-four seats, or something.

The word was—I remember Al Copp saying that Henry Kaiser said, “Well, the first line of action is you send a letter, but more important is maybe to talk on the telephone, but still, better yet, get on an airplane and go see them.” Traveling was a way of life for Kaiser, and they were, I think, from this area the biggest customer of United in those days, by far, because they were the only really huge company I guess out here, with wide-flung operations.

Swent: You might talk a little bit about Kaiser compared to Union Carbide. They were both very big companies.

Fuerstenau: Oh yes, but you know, when I was there at Union Carbide their business was about \$5.5 billion per year, and Dow was about \$5.3 billion per year. Recently I think I saw that Dow was like \$35 or \$38 billion, and Union Carbide was only about 7 or 8. Union Carbide really at the top must have messed it up. Of course that Bhopal thing didn’t do them any good, which caused them to spin off a lot of their divisions. It’s a good example of good and bad corporate management.

Kaiser, Kaiser Industries was going very strong then. Kaiser Industries was headed by Gene Trefethen; you know, he was the number two guy originally to Henry Kaiser. In fact Gene Trefethen came by one afternoon and spent an hour or so going through the Mineral Engineering Lab. I remember meeting him on that one occasion. Kaiser Industries, and as I said, Kaiser Aluminum, was equal to Reynolds in those days. Now today Kaiser Aluminum has been bought out by somebody in Texas or Arizona that makes the press in not a very favorable way, Maxxam. And they are nothing compared to what Kaiser used to be.

Of course, my time there was the late 1950s and I had nothing to do with any corporate level matters in either case. At that time, both corporations were strong.

But there was an apparent difference that I would like to comment on. One day I spent quite a bit of time in Oakland with the director of research of Kaiser Aluminum. The Mineral Resources Department was completely separate from the Research Division and so I had no connection to them. From my discussions with the research director or VP of research—I don't recall his name—I came away with the feeling that he thought that research should be directed from the top down, whereas in the Metals Research Laboratory back in Niagara Falls, research ideas generally flowed from the researchers up. In my opinion, researchers should know what the technical problems are in their industry and then think in terms of what might be done to improve processes or products. Of course, a lot of that is team effort, and I suppose that each section at the lab in Niagara Falls could be looked at as constituting a team. Anyway, for years I used to comment about those differences in research flow, so I assume my recollections are correct.

In the case of corporate basic research—if there is any left in this country—I believe that that research should have relevance to the long-term interests of the industry. Sometime in the sixties, Kennecott established a fundamental research lab near Boston that they called the Ledgemont Laboratory. In the late sixties, I recall meeting a couple of their researchers somewhere and I was taken aback when they told me what they were working on and, in my opinion, it had no relevance whatsoever to Kennecott's business. I remember mentioning to someone at Kennecott in Salt Lake City that there ought to be a sign over the door of the Ledgemont Laboratory: "Am I relevant?" However, later on two of my PhDs, Ralph Lai and Gale Hubred, joined the research staff there and developed interesting processes for recovery of metals from deep-sea manganese nodules—which was of great mining interest thirty some years ago—and will be again someday.

Swent: So you did recognize differences in the companies at your level.

Fuerstenau: Yes. Moving on, you mentioned a little bit about Hawaii. Kaiser had a big exploration program in Kauai, and I think Pete Fowler was the one that

actually was carrying that out. You know, there's so much rain in Kauai. Apparently the soil there is about 55 percent gibbsite. In other words, it could be considered a fairly high-grade aluminum ore. But at the time Al Copp said there's no mining law in Hawaii, so they—because it wasn't a state yet—gave up on the idea that maybe they could recover the gibbsite from the soil there. But that must have been quite exciting to the geologists at the time.

So anyway, there was a steady stream of samples coming into the laboratory to evaluate them as potential bauxite sources: Would it make a suitable product? How much reagent would be involved? The leaching is done with caustic at fairly high temperature, and you can lose alumina by precipitation. If there's silica present, you make some sort of complex silicate that consumes reagent and you can't have silica in the alumina, and—

[Tape 16, Side A]

Swent: What was your job then in the laboratory?

Fuerstenau: I was manager of the lab, so I would say it was administration, but that involved technical discussions with the people about what we were doing, scheduling, discussing the results of test runs, I suppose, and preparing regular reports on our findings. Now I don't remember whether we sent monthly reports, or probably biweekly. But it was basically technical interaction and administration of a group of a dozen people or so. Not an onerous kind of task.

However, when I first arrived there, Kaiser was cutting back on people and here I came in new while they were cutting some people off. Not that they cut anybody out of the Mineral Engineering Lab, but in the Research Lab they were cutting back. There must have been a slight recession going on in '58. Of course when I left Carbide, I was at that moment somewhat oblivious, not oblivious, but that there was a small recession going on didn't bother me. I was quite at ease with the potential of three jobs—the one I had, the one I was going to take, and the one I was really working on getting—while there were personnel cutbacks going on in the country.

Swent: What's it like to work at a job when you've told them you'll only be there for a year? Does that make a difference?

Fuerstenau: No. I think that they were contemplating trying to convince me to stay. I recall in April having a meeting with Tom Ready, who was the president of Kaiser Aluminum, during which he made a strong pitch for me to stay with Kaiser. I think at that time he might have been executive vice president, because I don't think they would have had a chairman in an internal organization, and the president, who I did not meet—his name was Rhoades, and everybody called him "Dusty" Rhoades. He apparently came up through the gravel pit business of Kaiser, with Henry Kaiser himself. I never met him, but I know that Tom

Ready later became the president of Kaiser Aluminum. Anyway he spent time, you know, to tell me that I really ought to stay.

Swent: I was just thinking it could work one of two ways: they could sort of ignore you because they thought you were only going to be there one year, or they could try to cultivate you to stay longer.

Fuerstenau: It varies with companies. For example, with my leaving Union Carbide there was no animosity there. I do know when one or two other people left, and this may have been later, but not much later, the departure was not amicable. For example, Bill Krivsky who was at the group manager level once told me that when he announced that he was leaving to join Brush Beryllium, the new laboratory manager at that time told him, "You have fifteen minutes to clean your desk out." Not a month, or two weeks, but just, clean your desk and you're out of here. That was what took place at Niagara Falls in the case of a couple of different people, whereas for me that was not the case.

I learned later from Frank Aplan—you know it's funny, it never occurred to me—that Rush Spedden was very upset when I left Union Carbide without discussing it with him, but he was in New York at Union Carbide Ore Company, and I didn't have any interaction with him then. Years later Aplan told me that for a long time Rush was distressed that I didn't give him an opportunity to see what could be done about keeping me there. It never entered my mind. As a matter of fact, only recently it occurred to me that when an offer came from Union Carbide, Spedden had obviously first gone to Gaudin asking if it would be all right if they approach me.

No, it was no problem. As I told you earlier, when you take any job, like it or not, approach it as though that's it.

Swent: Give it your best.

Fuerstenau: Sure. And so that's why that worked well. Then in January of that year our second daughter was born, Lucy, in Palo Alto—end of January, '59, and Peg named her after her mother, Lucy.

Trip to Visit Exploration Sites in Brazil and French Guyana

Fuerstenau: Kaiser had a major exploration program in Brazil, and also one in French Guyana, so they proposed that we go down there in April, to look at the various prospects and select samples for pilot plant testing since the material would need to be upgraded. This was planned, I guess, fairly rapidly, so five, six of us went: Ed Hassan, Al Copp, the chief geologist of Kaiser, whose name eludes me, and Jim Shaffer, the chief mining engineer of the Mineral Resources Department, and who you may or may not know because he used to come regularly to local AIME meetings.

I needed a passport. I didn't have a passport. This being '58, to get a birth certificate, I made a call back to Pierre in South Dakota and they sent quickly somehow a copy of my birth certificate, and in a couple of days I had my passport.

We went to New York. I have always been a very active photographer, after buying a Kodak 35 camera the first summer I worked in Chicago. The excellent technician overseeing the mineral processing pilot plant in Niagara Falls was Gene Lauer. I thought I would like to try to hire him at Kaiser, and he came down to New York City where I spent some time talking with him and maybe had him talk with Al Copp also. He looked at the camera and said, "You know, the range finder is loose on the camera." It was controlled just by a little adjusting wheel, but Gene spotted it, he being an ardent photographer. Anyway, the next morning I took a taxi into New York to a major camera store and I said, "Hey, can you fix this?" And they said, "Well, it will take two or three hours." The taxi driver said he would wait for me. He had a fare in both directions from the airport hotel—which then was called Idlewild. So my camera got fixed and all the pictures that I took were in focus. It would have been miserable later finding all my pictures out of focus, which would have been the case if Gene Lauer hadn't spotted the problem.

Somehow the job never gelled with Gene Lauer coming to Kaiser. He later worked extensively with Aplan in the Ore Company. One time later, he did fly out to the Alameda Naval Base because he was in the navy reserve, and he spent a weekend with us here in Berkeley. Then about four years later, three years later, he was in a naval reserve training flight at night somewhere in Florida, low flying over a lake, and the plane came in too low and hit the water, and broke in two right where he was sitting as the radio operator, and he was killed. Such a nice guy, and so very able.

My first overseas trip. In those days for a long flight they even weighed the passengers to calculate the fuel load, and so on.

Swent: And your baggage had a limit.

Fuerstenau: Oh yes, right.

Swent: Forty-four pounds.

Fuerstenau: Right. I remember we flew first to San Juan, which was very tropical. That was when the Puerto Ricans were starting to come to New York in large numbers. There was a line inside the airport that wound around for about two blocks inside with people waiting either to go through customs, or to get tickets, or board, or something. We were there just to get fuel and go on to Caracas where we were herded into the terminal by soldiers with machine guns. They were having one of their revolutions. We went on to Belem, which is the major port city on one of the tributaries of the Amazon. I still can recall

that drive in with the chickens, people, pigs, what not, being scared off the road with a speeding taxi driver whose most important driving tool was his horn. [laughter]

I was there in Belem for a few days, and Belem—I have learned only on my recent trip to Brazil, that something ends in “em” in Portuguese is pronounced “eng.” After a few days, we flew to a town on the Amazon called Macapá, exactly on the equator, and on the banks of the Amazon. I remember we looked at the docks that evening. Bethlehem [Steel Company] had a huge manganese mine just near by, and they shipped the ore by rail to Macapá where it was loaded on ships. I remember the manager of the manganese operation there telling us that the ship would be loaded such that it could just get out the Amazon at high tide. This was of interest in case Kaiser’s bauxite exploration would lead to a mine.

By the way, when we were back in the hotel in Belem, there was Olaf Rove, the chief geologist of Union Carbide, who had been up the Amazon looking at some manganese prospect for Union Carbide. It’s interesting hanging out in the boonies and there you run into somebody you know.

Trip up the Amazon to Select Bauxite Samples for Testing

Fuerstenau: Our group included those of us from Oakland and a bunch of Kaiser geologists stationed there in Belem. For the trip up the river there may have been a total of about a dozen Kaiser people. We rented a fairly large river boat, to carry us, our food, water, and so on with hammocks for sleeping. I remember one local Kaiser geologist, who may not have gone on the trip, but there in Belem said, “You’ve seen the first thousand meters you’ve seen it all.” [laughter] You go up the Amazon it’s just solid wall of tall palm trees on the shore. We went several days up the river with that same scenery day after day.

Swent: All looked the same.

Fuerstenau: And the first night we pulled off into a little side tributary, or at least a place where the boat could be parked. All our food was on the boat. Live chickens, and so on, provided the meat, not the first day but from the second day on. I remember at night all of a sudden was this roar, and a real loud roar, and I was told that these were clans of howler monkeys. Apparently they live in colonies of a hundred or something and at night they just sent up this roar. That was virtually the only animal activity on that whole trip. I was thinking I might be lucky to survive snakes, jaguars, caimans, but the only thing I regularly saw were these brilliant macaws, red ones and blue ones.

We went up the river for two or three days and then up a tributary where we landed and hiked for a couple, three miles, and up through the mesa area that

was away from the river. What I found interesting was that the soil was simply sort of coarse sand. It started just back from the river a little ways, and the grass was very sparse, and the cattle were all skin and bones. In other words, all the goodies probably had been leached out of the soil from all that rain over the eons. We were there in April, so it must have been the rainy season, and all of the villages were surrounded by water, and all the houses were on stilts. Canoe-like boats were the mode of transportation for the people living there to travel around in, and I imagine the water level lowered in the dry season, and these buildings then would be on land, so malaria was a problem.

Swent: I was going to ask you, did you do any precaution for malaria?

Fuerstenau: Unfortunately, no. I had yellow fever shots, but of course one was sleeping in mosquito nets. Unfortunately, I'll tell you about that after a bit—unfortunately I learned about malaria.

Swent: These were called lateritic soils?

Fuerstenau: Well, these are lateritic materials that had been weathered and leached by all of the rainfall. These would be areas where the alumina would be enriched and that was what Kaiser geologists were looking at. I don't remember what kind of the leaching sequence is necessary to leave the alumina enriched.

We hiked in from the river. I remember it was kind of long, dry hike to where some of these prospects were. By the way, to the southwest of there, on the other side of the Amazon, a few years later a major bauxite mine was developed. Where Kaiser was, on the north side of the Amazon, prospects were basically on top of mesas that had a certain thickness of bauxitic material. We looked at these areas and took samples

We then went farther up the river, and I remember somebody said 40 percent of the fresh water of the world flows out the Amazon. I think it's a little less, but it's a huge number; say a third and you're pretty close. You know when you're on the Amazon itself because the water is reddish, red mud, and you see patches of green grass rapidly going by which must be breaking off from the shoreline up above, somewhere. This one place where we started off a tributary you could not see the other side; it's like a sea. We went up the tributary where the water was clear but black. Apparently decaying organic matter gives the black or very dark brown color.

Then we went on up a little side stream in a couple of boats with outboard motors—long, narrow boats. We stopped at one point, and I guess the boatman decided he would go swimming, and anyway then a local native guide yelled, "Hey, this is the kind of stream you don't want to swim in because here's where the piranhas are," this little quiet stream. He didn't get bit by the piranha, but I still remember it was very narrow and we were right

in under the trees. We proceeded on up to another spot to get samples from bauxite prospects there.

Then we went back down to the river, and proceeded to the main mesa that they called Queixada. Here a great deal of exploration and pitting had been done. Quite a bit of excavation in the potential ore body was evident. I marked areas from which samples should be taken for shipping back to Oakland for process studies.

By the way, when we got back to Belem, we did a fair amount of preliminary test work on the Queixada samples. This material would run about 30 percent alumina, quite high grade, but it was mixed in with a lot of clay, and if you take a chunk of rock that's got clay on it and hold it under a faucet, you can work the clay off. But try to wash that clay away on an industrial scale, that is a tough problem. You could beat it up in washers, and scrubbers, and whatnot, but to really clean it, to get the clay out of the crevices and so on, in a full-scale plant, is a really tough job. Of course, the problem with any clay being present is that it greatly increases the silica content of the bauxite.

A little bit of an aside, I was doing some work years later in Egypt. There they have iron ores, porous iron ore, that apparently had been under the sea at one time, so the pores in the ore are full of salt. You well know how easy it is to dissolve salt—right—so you think you could wash that salt out of that iron ore, but it's hard to get that salt out of those pores. In their steel plant they have big problems there in Egypt because of the chlorine that's in these pores. Something that sounds simple at the teacup scale can be difficult at an engineering scale.

Anyway, I think it may be a long time before the Queixada material probably would ever be needed or used as a bauxite source. But it is fairly high grade, and a lot of it, but it has this problem of washing the clay out. I have learned that geologists like to work just with assays, you know, if an assay is quite high that's what's important, not what the recovery is when you process it. I'll tell you more about that when we start talking about Homestake. I found initially that assays were the important number to geologists, but recoveries really control whether a material is an ore, in my opinion.

Anyway, we were there for a couple of days, let's say. At that time the chief geologist, chief mining engineer Jim Shafter, and I rode horses up to the prospect. The rest of the group a couple of days earlier had taken the boat back to Macapá and on to Belem. We were going to come down three days later. After going to the Queixada prospect, we went by small motorboat for a couple of hours down the river to the town of Almerim. This was a low and flat small boat on the fast-running Amazon, whirlpools, yes, whirlpools and eddies, right down by the water; made me a little nervous. And it was a two-hour trip.

Swent: With piranhas in the water.

Fuerstenau: Right. Anyway, we went to the little town of Almerim, which is right across from the Xingu River. It's just a village, but if you look at a globe that town is on the globe, on any small map, it will show Almerim. We were there about a total of three days, I think, because we couldn't get a boat to take us to Macapá. A big passenger boat came in but there was no space on it, a ship from Manaus going down to Belem or Macapá, one or the other, and absolutely full. Maybe we could have gotten in third-class steerage, or something. We stayed in a little hotel. I still remember, the room had no windows. The only furniture in the room might have been a stool and hooks, and you hung your hammock on the hooks. And the food—

Swent: No windows?

Fuerstenau: No windows. No glass in the windows. Just open shutters. Also no screen. But we had our own mosquito netting.

Swent: Did you carry your own hammock with you?

Fuerstenau: Oh yes, I still have that hammock. I either have that or one I bought, one or the other. Oh yes, right. The walls of the hotel rooms had hooks on which to hang your hammock. And then in the backyard were chickens and a couple, three little pigs. I remember where the kitchen was at the hotel, the sink just went directly down to the ground and the pigs sitting under there waiting for the waste food to come down to them. They had an old Servel refrigerator from the 1930s that ran on bottled gas. For us they kept three beers in the freezing compartment. We had a beer for lunch and a beer for dinner; they weren't frozen—the freezer compartment just would cool it. Water in the town came directly from the Amazon and was only filtered. So boiled water for tea and the beer were all that we could drink.

We got to talking to a young priest who came from Nebraska, who clearly wanted company. I remember him saying he was more of a sociologist than a religious leader because his main duty seemed to be looking after the orphans, because, you know, the average life-span in the Amazon then was thirty, thirty-five years due to recurring malaria. On our boat ride up the Amazon two different boat employees lay in their hammocks for two or three days because they had malaria attacks. With unchecked malaria, an attack lasts for a few days, then it's gone, and in a few months it comes back, and this goes on for many cycles until the liver's gone and they die. Apparently that led to a life expectancy of thirty, thirty-five years, even in 1958.

[Tape 16, Side B]

Fuerstenau: Finally Jim Schafer arranged to lease a boat to take the three of us back down to Macapá. They made some kind of deal with the boat owner—it wasn't a

huge boat, but it was a chug-chug boat—that if he got us back to Macapá at such and such an hour he would get paid, and if it was later than that he wouldn't get paid. We went all night, no lights, and I remember we must have hit a log at one point, because there was a pretty good bump or something, but he chugged down the river the whole night. We got there early morning. Of course the river was flowing fast. We left maybe at noon or something, and then spent all night chugging down the river. He had to land about a mile or two away from Macapá because apparently he didn't have a license to go in there. We got a taxi to take us into town and to the airport, where we then were able to fly back to Belem. This was an old DC-3 with an extra row of seats so that the plane held twenty-eight passengers and not the original twenty-one passengers. I remember seeing that the pilot, co-pilot, hung a newspaper on a wire across the windshield to keep the sun out. One of them must have been able to see a bit, because we did make it back.

Anyway, we got to Belem and there ran a bunch of leaching tests on some of these samples that we had collected. Then the next objective was to go on to French Guyana where Kaiser had another exploration program. As I said, some years ago I read that Alcoa or Billiton eventually discovered a huge bauxite deposit on the south side of the Amazon and developed a large mine in the region, but not exactly where we were.

Trip to French Guyana and Coming Down with Malaria

Fuerstenau:

So we flew on a DC-3 straight north up to Cayenne. It landed three different times before Cayenne. I was in great shape in Macapá. When it landed the next time I was not feeling so good, and when it landed the third place I was just in hot misery. By the time we got to Cayenne I was in pitiful shape and then we had to go through custom formalities. When we got to the hotel—it was a nice hotel—they called the doctor who diagnosed that I had malaria. Here I was in the fever stage of malaria, and I'm sure one gets delirious at that point. It lasted for about three days.

Not at the time, but in retrospect I have thought back as to how I got it. I had little scabs on my elbow, and I think at night I probably slept with my elbow up against the net. A mosquito just doesn't land on you and give it to you with a simple bite. A female mosquito has to put an egg into your blood stream. So it happens at night when they have time to inject their egg into you. So I'll bet they got me in the elbow while I was asleep as suggested by the scabs on my elbow.

Anyway, that was bad. The only thing they had that I could eat was soup, and the only soup that they had, this being French Guyana, was French onion soup. I have hardly enjoyed French onion soup ever since.

Eventually that cleared up. Cayenne is very interesting. The ocean is slightly red-brown. What causes that is the red mud that's come out of the Amazon, and now we're five hundred miles away. This tells you how much red silt has come down the Amazon and has spread out like that in the ocean. Offshore in the distance you can see Devil's Island, you know, which is that infamous French prison. In French Guyana almost everybody is just black as a piece of coal. Of course, they descended from slaves brought there or who maybe escaped and weren't intermarried or interbred, so they're really black.

There was a French Bureau of Mines station there. As you may know, French Guyana is actually a state of France. Right now when the French do their rocket testing, for example, where they send them off from is out in the middle of French Guyana. So anyway, there was the French Bureau of Mines station, which we visited, that had working hours from 6:00 a.m. to 1:00 p.m. It was cool enough then during the morning hours. No air-conditioning. We were there over a weekend when I was getting healthy. I remember going swimming where the beach was comprised of nice black sand.

On Monday morning, we rented a boat, and went by boat up a river to the Kaw Mountains, and then a truck took us up to a camp that Kaiser had built. We were there for two or three days. It was a pretty nice camp, fairly extensive, had nice food, good cook, and so on. We looked at some of the prospects that the Kaiser exploration crew had discovered in the Kaw Mountains. I remember it rained and was hot, humid, and with plastic raincoats, you're just as wet inside as you are outside. We took a number of samples and looked at prospects in several different places.

I remember driving in this truck on a road they had cut through the jungle. It's funny, as the truck goes along, these great big, beautiful blue butterflies would just stay in front of the truck, and maybe another one behind it following. Big butterflies. [indicates size]

Swent: Six inches in diameter?

Fuerstenau: No, their wing span. Anyway, they were huge and were very brilliant blue. So I was feeling good by then.

As I said, I had samples that we had selected to be shipped back to Oakland. Just a little bit about that material: it was gibbsite, white chunks that were sort of embedded in an iron oxide. As I remember, it ran only like 20 percent alumina, but you could crush it and then separate quite easily the gibbsite from the hematite by a heavy media process. Although it only ran about 20 percent alumina, it was in my opinion a much better raw material than that clay-filled material in Brazil that ran 30 percent. Kaiser never developed a mine in the Kaw Mountains, but they eventually sold that Kaw Mountain deposit to Alcoa, and Alcoa mined that as a source of alumina for some time. This is some years later. So it turns out that the region had good prospects.

So we went back to Cayenne, and I forget what the other two did, but I flew from there to British Guyana and then to Trinidad, where I stayed overnight, and let me tell you, after being a month in the Amazon and up in French Guyana, coming to Trinidad was just like coming to paradise. It was just a wonderful experience. I hired a taxi driver to drive me around the island.

Swent: Trinidad was Dutch or English?

Fuerstenau: It's English, I'm sure. But you know, two-thirds of the people in Trinidad are Indian.

Swent: East Indians.

Fuerstenau: East Indians. They are a big part of that population. So this taxi driver, nice guy, drove me around the capital city, Port of Spain, and around the island to where the plantations were—cocoa beans for making chocolate, coffee plantations, and a few other things like that. But it was just such a pleasure. I initially said it was just like going to paradise, coming to Trinidad after being up there in the jungles for all that time.

Anyway, the next morning I left flying on a Pan-Am DC-6. Went to Maracaibo, then to Caracas, then Barranquilla, Colombia, and then Panama, and then Managua, Nicaragua, and Guatemala. Just about every couple of hours or so they would land and be there on the ground for a while. It took a day and a half to come all the way up to San Francisco on that flight. Tampico, Mexico, and then L.A., and finally at six in the morning, San Francisco.

Swent: Worn out.

Fuerstenau: A long haul. I had been about five weeks or so on this trip, and of course had been sick.

Swent: Peggy was at home with the baby.

Fuerstenau: She got a call the day I was to arrive. She's never forgotten that. The phone rang at four in the morning with a call from a Kaiser employee named Ostrowski, a chemist I think, who was part of the exploration team. Originally, he and I were to be coming together until things were delayed due to my malaria. Well, he called at 7:00 a.m. from New York and woke her up—"Sorry, your husband won't be coming home today because he's got malaria." That was the message she got. By the time it hit her, he had hung up. Of course she called Kaiser in Oakland later that morning to find out what the score was. That's a great thing to do, isn't it? "Your husband won't make it; he's got malaria."

Swent: Did you ever—have you had recurrences of the malaria?

Fuerstenau: I'll tell you, yes, once. In September, right when we moved here to Berkeley, all of a sudden, one day, I had fevers, the next day chills, next day fever, then it hit me that this was the malaria again. So I went to Kaiser, I now mean Kaiser Permanente Hospital in Oakland—I wasn't involved with Kaiser Permanente while I worked for Kaiser Aluminum, but when I came here to Berkeley we joined the Kaiser medical program.

I said to the doctor, "I've got malaria." "Oh, you can't have malaria." Then from the blood sample they found the parasite in my red blood cells. They gave me some pills to knock it, and then a second set which was for long-range control. I have never had it since. They told me—you know, suggested that I not go back where I might get malaria again. The only times I've been where I know there's malaria have been in the winter and I've done the six-week sequence of pills that you take before and after. I've been in India in the winter, and in South Africa in the winter, and never those areas in the summer. So fortunately it never came back.

Swent: Never could be a blood donor.

Fuerstenau: I never have.

Swent: You can't.

Fuerstenau: I have not given blood.

Swent: No, they won't take your blood since you had malaria.

Fuerstenau: I know, that's why I didn't even volunteer it. So the answer is—

Swent: You've been pretty lucky then.

Fuerstenau: Oh yes, because right after World War II there were so many people, soldiers back from Guadalcanal and whatnot, who had a bad form of malaria that they kept getting and getting it again. But down there in South America, even in '58, they just lived through it, and I've seen the poor people who lived in these little villages that we saw on the river, people whose life expectancy was only to thirty or thirty-five years.

Swent: It's just part of life.

Fuerstenau: Just part of life, yes.

A Trip to the Iron Ore Range to Look at Iron Ore Concentrating Plants

[Interview 8: August 20, 2001]

[Tape 17, Side A]

- Swent: One thing that you wanted to bring in that we didn't have time for the other day was a trip you took to Minnesota with Brandt to the Iron Range.
- Fuerstenau: Right. As I said, I had brought with me from Union Carbide Development Group a good young engineer. He might have been my age or even a little older—Bob Brandt, who had come from Minnesota. When I got back from the trip to Brazil and French Guyana, it seemed obvious that some of the methods of concentrating bauxite ores and aluminum ores would be somewhat similar to iron-ore washing plants. Bob Brandt and I then put together a trip which was about a week-long visiting some of the different iron-ore operations in northern Minnesota.
- Swent: When would this have been?
- Fuerstenau: This would have been, I assume, June 1959. The first plant we visited was the Groveland Mine of M.A. Hanna in Iron Mountain, Michigan, which was a new operation. I suppose we were there for a day. It was basically gravity separation and flotation. Big plant. I remember the mine manager, general manager, was Al Geist who had graduated in metallurgical engineering from the South Dakota School of Mines. He probably was a senior when I may have been about a freshman or sophomore there. The mill metallurgist was Ed Sougstad, an undergraduate classmate of mine from South Dakota.
- I remember Geist telling that they really had a big problem in the early stages because they had done a big sampling program on the ore deposit to design the mill flowsheet, but after they started actually mining, the ore was different than the samples. For iron ore you wouldn't think it would change much, right?
- Swent: No.
- Fuerstenau: With sulfide ores, you expect oxidation at the surface, and less or no oxidation as you go deeper. But here they had taken many tons of samples near the surface of the deposit, but as they mined deeper the ore changed. I imagine what happened was that was more hematite towards the surface and more magnetite—less oxidized—as they got deeper. Apparently they ran into the less-oxidized ore fairly quickly, which tells one that the sampling procedure is extremely important in flowsheet design, even for relatively simple ores.
- Swent: This was taconite?

Fuerstenau: No, no, this was a coarse-banded mixture of hematite, magnetite and silica, but still it wasn't what was called a Minnesota wash ore because it had to be ground to make a separation. It was somewhat coarser and higher grade than a taconite. The Groveland ore averaged 35 percent iron.

Swent: Were they pelletizing it?

Fuerstenau: No. They were able to produce a fairly coarse concentrate that could be blended with other materials as sinter plant feed. About half the ore produced a high-grade concentrate at 28 mesh using Humphreys spirals. Those tailings were ground to 100 mesh and processed by flotation to recover the rest of the iron. The concentrates were purposely kept as coarse as possible. If the ore had gotten progressively finer, they would have had to pelletize it. I remember talking to Luther Hendrickson of US Steel years ago who told me of their specular hematite mine at Quebec-Cartier. This beautiful material liberated at about 10 mesh. I actually have some large chunks of it somewhere. They would separate it on Humphreys spirals and make an absolutely clean separation. But I remember Hendrickson, who was head of raw materials research of US Steel, saying it really hurt his soul that they'd take this 10 mesh pure concentrate, virtually free of silica, and then grind it to 325 mesh just to pelletize it—which couldn't be done at this 10-mesh size.

We drove on over to Duluth; I'd never been in Duluth. Wintertime must be exciting there with those steep streets coming down towards the lake. On the way, we passed through a little town along Lake Superior which was the home town of Major Bong, who was the greatest fighter pilot ace in the Pacific during World War II. He shot down more Japanese planes than anybody else, like forty. In this little town there was a big statue of him and an airplane on it. Regrettably, around '48 or '49, after the war, he was flying a jet that flamed out on take off and he was killed. He came from this little town and probably was an excellent duck hunter in his youth.

Swent: I'd forgotten that name but he was very famous.

Fuerstenau: Oh yes. As I say, in my high school days. I used to read and know all of this sort of stuff. Then right near Duluth was Erie Mining, which was the big taconite Silver Bay operation that eventually had tailings problems because they dumped their tailings right into Lake Superior. About twenty-five years ago or so somebody spotted a certain amount of asbestos in the tailings and that led to a big change. Eventually they had to build the tailings ponds some miles inland and north of the plant to impound the tailings.

Swent: Taconite was something that saved the iron-mining business about this time.

Fuerstenau: Taconite is mainly fine-grained magnetite in silicates, although in more recent years non-magnetic or hematitic taconite is also being processed. At Silver Bay, it was magnetic taconite, which had to be ground to virtually minus 325

mesh in size. Without pelletizing this was just a rock. I always cite this as a nice example of mineral processing research turning a rock into an ore.

Swent: It was considered unusable.

Fuerstenau: Right.

Swent: Completely uneconomic.

Fuerstenau: Fine grinding the ore, separating the fine magnetite with magnetic separators, and pelletizing the fine concentrates was a development led by Professor E.W. Davis at the University of Minnesota. He had quite a large engineering research station there at the University of Minnesota. It was through his efforts that the pelletizing process came into being in the early fifties.

Just to fill in a bit, the pelletizing process with taconite involves feeding the moist magnetite concentrates along with a small amount of Wyoming bentonite to act as a binder into sloping, rotating drums. By the way, all of that bentonite is shipped through Rapid City. At Erie, the drums are quite small, if I remember correctly, only about six feet in diameter and twenty-feet long. Today the drums may be ten feet in diameter and thirty-feet long. As the drums rotate, the tumbling moist particles form wet balls that are discharged at the other end of the drum where a screen removes green balls that are about three-fourths of an inch in diameter and the smaller balls are recycled to the feed end of the drum where the moist concentrate is being charged. These green balls are fired to harden them, after which they are shipped to the blast furnace operations.

As I say, pelletizing turned what was nothing more than a mass of rock into a valuable ore. Taconites only run about 24 percent recoverable iron. The magnetic concentrates assay 60 to 65 percent iron with most of the silica being eliminated in the magnetic-separation stage. What really led to the success of this complex process was that these roughly three-quarter-inch or one-inch spherical pellets increased the blast furnace productivity by about a third. In the whole integrated steel industry these pellets were most valuable not only because of increased blast furnace productivity but also because of their lower silica content. The higher value of pellets led to the taconite industry displacing wash ores. A tremendous amount of energy is involved in grinding taconite, plus there's a lot of steel wear on crusher parts and balls and rods for grinding. If I remember right, it's something like five or six pounds per ton of ore. If you take how many pounds of steel per ton of iron that's in the concentrates it's something like to make a ton—it takes twenty pounds of steel to make a ton of iron from ore in pellet form.

Swent: Used in the machinery.

Fuerstenau: Yes, so about 1 percent of steel produced is consumed by wear in crushing and grinding equipment. So, this was my first visit to a huge taconite operation.

Swent: That's the Silver Bay.

Fuerstenau: Silver Bay, yes. I think it was called Erie Mining Company. The taconite companies were financed and jointly owned by different steel companies. For example, Bethlehem Steel owned half or a third of Erie Mining Company, and somebody else another part and so on. A company called Pickands Mather used to manage some of these operations on a fee basis since many of the operations were each owned by a number of companies. Since the Erie Mining operation was the first taconite installation, the plant had a large capacity but all of the equipment was not large. The pelletizing drums were quite small, maybe six feet or so in diameter but a string of them side-by-side that virtually went out of sight.

So then we went on up to Hibbing which, as you know, is in far northern Minnesota. Upper Michigan has its iron belt, and today there are still big operations in that region. But the major iron ore belt is in northern Minnesota—not being a geologist I never bothered to learn the geology situation—but that iron belt probably goes under Lake Superior and emerges again in northern Minnesota. Let me tell you, the first time I got up to Hibbing to see those Mesabi pits, I thought that they're just like the Grand Canyon. I don't know whether some people call it an environmental disaster, but I think they've created something magnificent to see out of what would be just dull, flat, dreary forest land. The trees aren't very high because of the northern climate. Have you ever seen the great Mesabi pit?

Swent: I haven't, no. I never have.

Fuerstenau: To me, it's spectacular. They have made some ski runs in a few places since it's steep and deep enough to do that. In that time the main city was Hibbing and I think directly east of there is a town called Virginia, Minnesota, which is where Bob Brandt came from, and after that Eveleth—there's a whole series of towns along the iron range where different mines were located.

We visited two or three of what were then called iron-ore wash plants which were the standard ore preparation plants before taconite displaced them. The ore was essentially massive hematite. They would simply separate out the silica by gravity methods at as coarse a size as possible. I'll just talk a little bit about that in a minute. The washed ore as shipped to the steel mills contained maybe 9, 10 percent silica. It costs real money to remove the silica by smelting. On the other hand, taconite pellets would contain only 3 or 4 percent of silica, and that proved much more cost-effective than just digging Mesabi ore out of the ground and shipping it to the steel mills. This so-called wash ore would consist of chunks or lumps that might be six or eight inches in size and

then there would be an intermediate fraction of about an inch down to maybe a quarter-inch or something like that. Then the minus-quarter-inch or so material would have to be taken into account also. In washing plants they'd try to separate quartz from hematite in these three size fractions by methods based on differences in specific gravity. The coarse material would be done in—

Swent: Hematite and quartz?

Fuerstenau: Quartz. There may have been a few other odd silicates there, but those are the two main minerals. The coarsest size was separated by heavy media, which involves making a suspension of fine ferrosilicon in water. In essence you make a fluid that might have a specific gravity of, oh, let's say about 2.8, 2.9. The separation would be carried out either in rotating drums or in another type of vessel that was just a big vertical cone where the suspension slowly moves upwards. This is known as a sink/float process. If the density of this ferrosilicon slurry is about 2.8 or so the quartz will float because it's got a density of 2.65 and the hematite will sink because its density is, I think, around 5. So such a separation can be made very cheap. That's how the coarse material was separated.

Then the intermediate size material was sent to the jigging section. Did anyone in your oral history series talk about jigs?

Swent: Frank McQuiston talked about jigs.

Fuerstenau: The jig operates by differential acceleration of settling particles. To start, you have the rock—this is usually an intermediate size of half-inch on down to maybe a quarter-inch or so. Some of it could be finer. It flows into a tank that is being pulsated by a diaphragm, and the pulse causes the water to rise and all the solids will rise momentarily. Since the denser mineral will have a greater settling acceleration than the lighter mineral, they'll eventually stratify. It's tied with Newton's laws of motion.

Swent: There were shaking tables in the old days.

Fuerstenau: Oh yes, right. Even today coal-cleaning plants handle the finer material on shaking tables. Some of them can have decks that are maybe eight feet by twelve feet or more in size. They function as a sort of flowing film concentrator. The finer material flows down the slope and the shaking moves the heavier particles sideways. You can see this happen on ocean beaches sometimes. I think up in Oregon you can see magnetite separating a bit from the quartz due to the wave action. Shaking tables are often used for this. I don't recall seeing them in the iron ore plants. They're used in coal cleaning and they were used to recover sulfides before flotation was developed. At Homestake in Lead, a shaking table recovers about half the gold—great to see a band of gold on the table deck.

The idea of jigs went clear back to the Middle Ages where they had people—see that picture there on the right, the woman? [indicates picture on the wall]

Swent: Oh yes.

Fuerstenau: She pushed her screen containing crushed ore down into the water and that caused the pulsating action. That was the first jig.

Swent: Where is this?

Fuerstenau: In Germany in the Erzgebirge near Freiberg. Anyway, by pushing the screen down you're giving that jiggling motion of causing the particles to rise, and then as they settle they'll stratify. That's how jigging started. [laughter]

Swent: And they let the girls do it.

Fuerstenau: Right. Then towards the end of the era of iron-ore wash plants in Minnesota, the finer material was separated with heavy media suspensions in cyclones. Here they made the suspension with fine magnetite because as the slurry spins inside the cyclone, it increases the density of the suspension. Somebody who wrote a lot of very good papers on that subject—and I don't know whether you ever met him or talked to him—was Earl Herkenhoff. I think he moved out here, may have been with Marcona for a while.

Swent: He's in Reno now.

Fuerstenau: Is that right?

Swent: He was with Marcona.

Fuerstenau: He was what I considered one of the very best of industrial metallurgists in the iron-ore field in the fifties. He wrote a lot of good papers. I think he worked for Pickands Mather and I think he may have had a lot to do with the development of using cyclones for heavy-media separation. Anyway, that was a widely used technique for processing the finest material in these wash ores. Undoubtedly some used shaking tables or Humphreys spirals¹⁴.

It was this kind of plant that we thought would be needed for concentrating these bauxite ores, which was why we went to the Iron Range. Our purpose really was to get more background for planning and designing a pilot plant to conduct larger scale test programs on upgrading the potential bauxite ores. I remember looking thoroughly at the pilot plant and research facilities of Pickands Mather. When we got back to Permanente, Bob Brandt went to work

¹⁴ James V. Thompson, *Mining and Metallurgical Engineer: The Philippine Islands; Dorr, Humphries, Kaiser Engineers Companies; 1940-1990s*, 1992

on designing our facility and ordering equipment to be set up in the Mineral Engineering Lab for testing larger-scale ore samples.

By the way, I remember one plant that Bob Brandt suggested we visit because it was kind of a family custom operation. Maybe they bought ore from smaller independent mine operators. I remember the owner taking us through the plant which was a fair-sized plant with heavy media, jigs and spirals. But the owner absolutely knew no technology, was not an engineer. He just knew you fed the crushed ore in one end and product came out the other. In fact, he said something to the effect that he didn't care what went on inside the machines but only was concerned with what came out. I always figured if he'd hired a young metallurgist he probably could have made a lot more money.

Anyway, we visited a total of a half-dozen of these operations. One of the largest was a US Steel plant called the Trout Lake Concentrator. I suppose that was towards the end of those so-called wash-iron ores because the taconite industry was going to displace the simple wash-ore concentrates that had high residual silica. I recall a paper written about 1987 by someone from Bethlehem Steel who stated that it costs something like \$90 a ton to remove 1 percent silica out of iron ore in a blast furnace. For each percent it costs you \$90 because of melting everything, and so on, whereas to take it out by magnetic separation and pelletizing then had costs of \$1.90 per ton for each percent of silicon.

Swent: A huge difference!

Fuerstenau: I can look up these numbers, but it shows you why physical, low temperature processes are so much cheaper in general than high temperature where materials are melted. Anyway, we spent a whole week there. I can tell you about one other situation that happened during that summer. [tape off, then on]

Swent: So that was quite a trip to Minnesota.

Fuerstenau: There's one other incident that might be of interest related to heavy-media processing of ores. What had been the world's largest cement plant, and may still have been even in 1958, was Kaiser Cement at Permanente, which is just over the hill from Los Altos Hills. They were having a problem then with the limestone cement rock that they were mining. It was becoming too high in silica, so they had to find a way to reduce the silica. One day an engineer came from Stearns Rogers Company in Denver, an engineering company. Have you heard of them?

Swent: Oh my goodness, yes.

Fuerstenau: I would think. This engineer from Stearns Rogers was there for two or three days, operating a small heavy-media separation pilot plant that he had set up

right adjacent to the building where we had our mineral engineering lab. During that time, I chatted with him a bit. Well, quartz has a density of about 2.66 and calcite is about 2.71. So he was running this heavy-media pilot plant, trying to separate the quartz from the calcite! Needless to say, it was one big flop. The next thing I heard from somebody was that after he got back to Denver, it wasn't too long afterwards that he was unemployed. Anybody that was trying to separate something that was 2.65—

[Tape 17, Side B]

Swent: You were saying that to separate at that fine size—

Fuerstenau: There's just no way of doing it by gravity at that size. He shouldn't have wasted anybody's time. By the way, Kaiser had an operation at Moss Landing. I think they're out of that business now, but they were recovering magnesia from sea water there. When you go down towards Monterey, near Salinas, you can still see the Kaiser dolomite mine. It's all shut down, but for decades they mined very high-grade dolomite. At that mine, which I thought was quite amazing, they were using a heavy-media cone to separate dolomite which has a density of about 2.82 from quartz which is 2.66 by—but it was big chunks. In other words, these chunks would be practically a foot—ten, twelve inches in diameter, but being that big—

Swent: You're indicating a bit more than that.

Fuerstenau: —they could then slowly sink or float in the ferrosilicon suspension, whereas when you get down to finer size particles, the upward flow of the suspension makes it impossible to separate particles of such low density difference. So Kaiser was able to do some upgrading of the dolomite ore in this manner. Kaiser calcined the dolomite, which is calcium-magnesium carbonate, to produce magnesium-calcium oxide, which was then added to sea water. This they did at their Moss Landing plant, where the lime (CaO) makes the pH go up and it precipitates magnesium hydroxide, both from the magnesium chloride dissolved in sea water and the magnesia (MgO) from the calcined dolomite. That's how Kaiser produced magnesium hydroxide that was calcined to form magnesia, which they used for making refractories. They were a big producer of magnesia refractories at one time. I don't know if Kaiser is any longer in the refractories business.

Swent: I don't know either.

Fuerstenau: But they were a major producer of refractories in that time period. By the way, what happened later, which I thought was very good, was mining with total resource recovery, which is where you make no tailings. Kaiser Cement did just that. As we had discussed earlier, at their Permanente cement rock mine, they had this problem of too much silica—too much quartz in their cement rock. As we discussed, this could not be solved by heavy-media separation.

But later, they built a flotation plant down there at the south end of San Francisco Bay, probably up there where the cement plant was. They ground the cement rock, and by flotation were able to separate the calcite from quartz. The calcite then was blended back into the mined cement rock to give the required composition and the pure-quartz tailings were sold to the glass industry; so they made no tailings.

Swent: Aha! They'd use every bit.

Fuerstenau: That's a wonderful example of total resource recovery. As you well know, they couldn't dump the tailings into the Bay and there was no place to impound them.

Swent: They were in a location where the shipping was favorable also.

Fuerstenau: Oh yes, yes, because I suppose right here in Oakland to Libby-Owens-Ford Glass or somebody probably bought the glass sand. I heard that that cement plant is still going strong.

Swent: We toured it once not too long ago, the local SME section.

Fuerstenau: I remember that. I didn't go on that field trip, although I did visit the cement plant in 1979 as part of a major comminution study.

Swent: It was a very interesting tour.

Fuerstenau: Then geologists found high-grade cement rock, so that eventually there was no need for the flotation plant anymore.

My time there at Kaiser was coming to an end because I had gotten the official offer to come to Berkeley. What eventually happened with regard to Kaiser's hunting for a second source—I told you about Jamaica being their only source of bauxite. Their geologists found in Queensland, in northeastern Australia, that very rich, high-grade bauxite deposit which Kaiser operated for many years. That solved their whole problem and then of course they no longer needed this effort of looking at low-grade deposits or getting gibbsite out of soil that was going on before and after my day with Kaiser.

A few years ago, about three years ago, Peg and I went to Heron Island. We first flew to Gladstone on the Queensland coast, and on landing the airplane flew for a mile or so over a big red tailings dam which was the brilliant red mud from this Queensland bauxite mining operation. These were the tailings from the alumina plant. Alongside the docks, you could see hills of white alumina that was obviously ready to be put on ships to go to various parts of the world. I know that this is the source of the alumina used to produce aluminum in Egypt, which we may talk about later.

Swent: So it is still operating?

Fuerstenau: Oh yes, in a big way. But Kaiser, as I understand, is no longer involved with that deposit.

VIII UNIVERSITY OF CALIFORNIA, BERKELEY, 1959–to date

Appointment to the UC Faculty and Moving to Berkeley

- Fuerstenau: At the end of the summer we moved from Mountain View up here to Berkeley—a short move compared to all the others.
- Swent: You had two children by now.
- Fuerstenau: Yes, I think I mentioned that our second daughter, Lucy, was born in January. We came up a couple of different weekends to Berkeley looking for housing. The housing market was absolutely totally different than it is today—unbelievable. I think maybe two or three weekends we came up. At that time Peg wanted a big old house. It ended up that we bought a house that was a Julia Morgan house built in 1913 after the earthquake. The previous owner thought that it was a Julia Morgan house, but we never checked on that. When we decided to move eighteen years later Peg did a little checking on it, but she couldn't find out anything about it. It's now listed in the books on Julia Morgan. It had all the attributes of Julia Morgan designs.
- Swent: This was on Arlington?
- Fuerstenau: Right on Arlington. It turns out our neighbor directly across who we never really knew very well was Paul Henshaw, who in '59, I think, was the exploration VP of Homestake. Not president of it. Because I know that he spent a lot of time in Grants [New Mexico] at that time. They lived across the street, and I think Henshaw was away a lot.
- I remember some eight months later meeting Mrs. Henshaw [and she] said, "Oh, you're the people who can't afford the gardener." [laughter] I guess they shared a gardener with the people from whom we bought our house. The street was divided with a few feet difference in elevation, so that you didn't cross the divider. The astonishing thing about prices then and prices today—we paid \$26,750 for this house.
- Swent: A Julia Morgan in Berkeley!
- Fuerstenau: A big Julia Morgan house. Unbelievable compared to—
- Swent: Right on Arlington near the university. It would be a million or more now.
- Fuerstenau: Anyway, of course when I went to the bank to get a loan for buying the house, the banker—and this was B of A [Bank of America] down here in Berkeley—couldn't believe anybody was as dumb as I was to leave a job that was paying me \$18,000 to take one that paid \$9,000. My starting pay here at Berkeley as an associate professor was \$9,000 in 1959. Not summer pay or anything. That

banker, whoever he was, really thought I had something wrong with me to do such a thing.

Swent: Why did you?

Fuerstenau: I wanted to teach. I wanted to get back into an academic career which—I had told you that my time at Carbide had more or less cemented this.

Swent: Peggy obviously agreed with it also.

Fuerstenau: Yes. You know, I must say in every single career move I've ever made we naturally discussed the situation at length. We have always talked regularly about everything going on, and still do. But I don't recall asking her directly for her opinion about any of the few career changes that I have made. These career moves were made on what I thought best professionally—and those decisions may have been self-evident. Of course housing, living, and everything like that was probably more in her hands than mine, but moving out here or going to Niagara Falls or going to Kaiser, I know I didn't say, "Well, what's your opinion?" Or, "What do you think?" I made up my mind that was what we should do and we did it. Fortunately, she must have agreed. Did you enter into decisions with your moves, or was it the same?

Swent: Not at any great level, no.

Fuerstenau: No, because I believe that one's professional career determines how the other aspects of your life will be. As I told you earlier, I think that if you do your professional career well everything may well fall into place. Even living two and a half years in Buffalo was something I never wanted to do for a lifetime but it was an interesting—

Swent: It fitted into the picture.

Fuerstenau: As for making moves, the very day before leaving the next day on the trip to Brazil, or a day or so earlier, I met with Tom Ready, the executive vice president of Kaiser Aluminum who wanted to convince me that I ought to stay with Kaiser. So I called up Professor Joe Pask, who was department chairman here at Berkeley, and said, "Hey, you know I really got to know. I'm about to leave the country for six weeks or a month." Pask said he and Professor Earl Parker would go visit the chancellor, who was then Glenn Seaborg. They came back and called me I guess and said it looked like everything was now in place and so on. I learned later just how slow the system is at Berkeley on making new faculty appointments. It's extremely slow; all levels of committees and whatnot. I must say, you don't win on them all either, but they have a fairly high percentage of coming up with good people.

About fifteen, sixteen years ago—I didn't realize it was that far back—at a function at Lawrence Berkeley Lab, I was talking with Glenn Seaborg, who I

think was the chancellor here for three years when I first came, and then he went to Washington to head the AEC [Atomic Energy Commission] for about ten years. As you know, he was the discoverer of plutonium. You go over here to Gilman Hall, there's a plaque out there, a National Heritage plaque—on the room where it says, "In this room in 1939—" or '40 or '41, whenever it was—"plutonium was first discovered." This was discovered by Seaborg. Later he got the Nobel Prize for that and maybe for generating all of the transuranium elements here. Then it was called the Radiation Lab.

Seaborg, I had read, had always kept a detailed daily diary—he did this all his life. This made the national news when he was leaving AEC because they wanted the pages of anything that might relate to any discussions related to atomic-energy problems or defense problems I suppose. I think he put up a long fight about not giving them his diaries but eventually I think they reached a compromise where they took out the pages that they thought might have had some significance to the national interest. One wonders [to this day?] whether any of it would have; I doubt it.

Anyway, I received this letter dated October 18, 1985, from Seaborg:

"Dear Doug—" I'll read this and then I'll give it to you or Xerox it because I want this in there. "Following our conversation today regarding your hire when I was chancellor of the Berkeley campus I was curious to look it up in my journal from that time. I thought you might be interested in the following excerpt from February 26, 1959: 'In the evening at my study at home I read a number of papers from the chancellor's office—a memorandum from Errol Mauchlin summarized as a question of replacement for Anders Carlson in the Engineering Department in mineral technology.'" [He was a petroleum engineer; he was retired when I came.)] "He is becoming incapacitated but is still occupying an FTE [full-time equivalent employee position]. The Department of Mineral Technology has made a strong case for the appointment of Douglas Fuerstenau as associate professor III and the budget committee supports the proposal. Apparently he is a very able man and would fill a need in beneficiation. However, they would need to find an additional FTE and I made note that we follow the suggestion of Mauchlin that they use an FTE for one of their vacant positions or that we might otherwise promise to give them an FTE if the erroneous cut in our budget of last fall is restored.' With best regards, Cordially yours, Glenn T. Seaborg."

Swent: So he was responsible for—

Fuerstenau: Well, he was chancellor then. I remember Pask and Parker telling me that they were going to go try to speed things up. By then it had already gone to the Budget Committee, which by the way some years later I served on for several years.

Swent: That's the key committee.

Fuerstenau: Oh yes, that is. You probably learned about that from talking with deans.

Swent: Yes, and George Maslach¹⁵.

Fuerstenau: Maslach in particular because he was the dean when I was on the Budget Committee.

Beginning Career in the University of California at Berkeley

Fuerstenau: I came here probably some time in September. Classes didn't start until about October first in those days.

Swent: They were on quarter systems?

Fuerstenau: We had what used to be called the "old Berkeley semester." They called it the "old Berkeley semester," which started about the end of September. Then you'd have a break at Christmas, and after New Year's there would be two more weeks of classes, and after that would come the finals. You can imagine the dullness to students starting up again for two weeks of classes. I don't remember how that affected me, but you always heard that it was dull to the student and to the professor because after a long break you had to crank up again for just two more weeks. Then the second semester must have started around February, and classes went way until the end of June for commencement. They called that the "old Berkeley calendar." Because, you know, with the current one, classes start next Monday and they're even done before Christmas, and commencement takes place in May. So that first fall, my first term did not begin until the end of September.

Before classes started in 1959, I started getting sick. I had a fever one day and chills the next, fever the next, then chills. Then all of a sudden it hit me that I must have malaria again. I went down to Kaiser Permanente Medical Center in Oakland, which we had joined after coming to Berkeley, and I said to the doctor, "I bet I've got malaria." He didn't think so, but they found this parasite that's in my red blood cells and gave me pills for the short-term illness and another set of pills to take care of it long term. It killed the malaria and I've never had it since, although I've been told not to go near the malaria areas again, so I watch that.

Swent: Were you teaching a class that had formerly been taught by someone?

¹⁵ George Maslach, *Aeronautical Engineer, Professor, Dean of the College of Engineering, Provost for Professional Schools and Colleges, Vice Chancellor for Research and Academic Affairs University of California at Berkeley, 1949-1983*, 2000

Fuerstenau: Yes. It was called “Mineral Dressing” then. I renamed it, but yes, it was an undergraduate class. It had half a dozen students in it. I remember the first lab session clearly because they had been remodeling part of Hearst Mining Building, and the laboratory that I had had no lights in it. So here towards the end of the afternoon it’s starting to get dark. I recall really writing a blast memo to Joe Pask, the chairman, about what an outrageous situation that you’ve given me a lab with no lights. They were doing a large amount of ventilation renovation but hadn’t gotten to that part of the building yet. But I’ll tell you, I think the lights were installed in about two days.

Hearst Mining Building had been built with an open court in the center, and it was filled in, apparently in the early fifties. They wanted more space, and that really made poor space in the innards of the building. What it did was destroy the ventilation. I think in about ‘58 and ‘59, there was something like a \$1 million-some dollars being spent on just renovating the ventilation—that’s all the building cost in 1905. I think whoever had the bright idea to fill this space in sort of ruined it.

Swent: Now they’re spending millions more to undo it.

Fuerstenau: Undo it. Right on. In fact, we’ll talk a little bit about that later. A huge amount to bring it back to where it was, I guess, while modernizing it at the same time. Anyway, I had no grad students and I taught this mineral dressing course in the fall, worked on getting labs set up for research and so on, but still progress was slow.

Swent: What was the situation in the department at that time? How many students did you have?

Fuerstenau: I think each graduating class was running around [pause] well, in metallurgy, where I was, it was maybe ten. There was a certain number in mining and a certain number in petroleum engineering. Maybe I can, at this point, kind of review the makeup of the department. It was called Mineral Technology, which was an outgrowth of what was the College of Mining.

Swent: It was no longer still the College of Mining?

Fuerstenau: No. Donald McLaughlin, who had graduated in the class of 1914 from Cal in mining, had a role in this. He received his PhD in mining geology in 1917 from Harvard and then spent some time in the army in World War I. Later he became a mining geology professor there from about ‘21 to 1941 when he came back here to Berkeley to be dean of the College of Mining, and then in 1942 the College of Mining was joined with the College of Engineering and he became dean of the College of Engineering. This of course was during World War II, and in 1943 he was asked to go down to head Cerro de Pasco in Peru which he did, as you well know. During his years at Harvard, his

students included John Gustafson, Paul Henshaw, and Chuck Meyer amongst many others.

Then what had been the College of Mining was a component of the College of Engineering. The college was not set up with departments but with what were called divisions. You may have learned more of this in your deans' oral histories. It was called the College and Department of Engineering. I don't know why—this was before my day. But the faculty in what had been the College of Mining was so distressed that they wanted nothing to do with the name “engineering” so it was called Division of Mineral Technology, but by the time I came here the college was organized on a department basis and we were called the Department of Mineral Technology. Actually I was always against the word “mineral technology,” but it was that way until about 1969.

The Makeup of the Department of Mineral Technology at UC Berkeley

Fuerstenau: The department comprised mining, metallurgy, ceramics, and petroleum. So I was part of the metallurgy faculty which would include mineral processing. The closest colleague was Fred Ravitz, who was a professor in extractive metallurgy—you know, leaching, smelting, this sort of thing. Ravitz had come from Salt Lake City. He'd been quite a while on the faculty of the University of Utah for some years. I think he may have joined the Bureau of Mines and risen to become the head of the Salt Lake City Bureau of Mines station. I think he may have gotten to that position, and then came here to Berkeley apparently in 1951 or '52.

[Tape 18, Side A]

Fuerstenau: Many years later, after he'd retired, Ravitz died of pancreatic cancer. I think he was only seventy-three or something at the time, but he'd retired and I was the chairman of the group to write his obituary for the university memoriam book. So I was given a lot of background material on Ravitz. What I was quite astonished about was that he was apparently in a major way responsible for the establishment of the Geneva Steel Works of US Steel there at Provo, Utah. They had a two-full-page newspaper article about him and his role in bringing that steel mill there to Provo during World War II. I had no idea of the role that he apparently played during his years in Utah.

I think his background originally had been chemistry and he had a PhD from Caltech in chemistry, but he became a metallurgist. Whether his BS was metallurgy I don't know, but he'd come from Utah, went back there, and then came here. A very thorough, sound person that I used to talk with a lot. I was always upset a lot that he never had an active research group here—maybe one or two grad students were working for him at any given time. That used to irritate me a lot. He had a great sense of humor. I'd always enjoyed his subtle humor. Later I learned that some of the outside world thought that when he

came here this was really going to do a lot for putting the extractive side of metallurgy at Berkeley on the map. But shortly after he got here he came down with a disease that's called the opposite—it's the opposite of diabetes where—

Swent: Hypoglycemia?

Fuerstenau: Yes. They didn't know what disease was at the time, so he, for apparently a couple of years, was really going downhill. Then when they found out what it was and how to counteract it, his health turned completely around. However, it apparently had taken a lot of starch out of him, I guess.

Swent: Why was he opposed to doing research?

Fuerstenau: I don't suppose at all that he was opposed to research, because his years in Utah were certainly involved extensively in research. You know, it takes a lot of energy and time to prepare and write proposals, and so on.

Swent: You have to get outside funds?

Fuerstenau: Oh, you live on the outside money. Faculty spend worldwide too big—at least in the US—too big a percentage of their time always hunting for money, writing proposals for grants, contracts, and—because in this university—what you get from in-house is pretty small. As a matter of fact, today when new faculty are hired, they talk about startup packages that can be large—depending on the department. I think someplace like [the Department of] Chemistry they probably are coming up with a million or more dollars for equipment to get started. In this department they probably now may get a few hundreds of thousands of dollars for some startup packages. But the money cannot be coming from the department because there isn't that kind of money. Such funds must come from sources of the dean or the campus. My startup package was zero, absolutely nothing. I wasn't even smart enough to ask for it. I had nothing for getting research underway when I came, dead zero. I told you, I didn't even have lights in the lab. It wasn't even painted.

The old man of the department was Ralph Hultgren and, you know, he was only fifty-four. He was born old, I guess. When somebody's fifty-four today they're young. [laughter] Hultgren lived—if you come up Hawthorne Terrace, just as you're aiming at LeRoy and looking right at his house, which is a Julia Morgan house. He joined the faculty here in 1941, obviously coming with McLaughlin, because he was an assistant professor at Harvard. Ralph was a graduate of Berkeley, and he said that he talked with Gaudin about whether he should come back here. Hultgren was, I think, a graduate of the class of about 1928 and was University Medalist, the first engineering graduate that was a University Medalist. Don McLaughlin was a runner-up in 1914—I remember seeing that somewhere. But Hultgren was University Medalist, and then the

next engineer to be University Medalist was John Whinnery whose name you must know. Whinnery was the dean of engineering when I came here.

Hultgren had done his BS here and then got a master's at the University of Utah in metallurgy, and his PhD under [Linus] Pauling at Caltech, I suppose on X-ray crystallography. He was teaching physical metallurgy at Harvard when he came here. In 1959 he was heavily into metallurgical thermodynamics. The three of us had offices all in a row.

Swent: That's you and Ravitz and Hultgren.

Fuerstenau: Yes. I don't think Hultgren ever appreciated my aggressive ways in those days too much, but anyway he was down the hall. In physical metallurgy the dominant person here, and really a big man in this country, was Earl Parker. Parker died just three years ago at age eighty-five. Parker's only degree was an equivalent BS from Colorado School of Mines in metallurgy. He'd been at GE [General Electric] and came out here to work on the ship breakup problem—you know the problems with Victory ships. All too often when they hit the cold water in the North Sea—you may have seen pictures—they split, cracked and broke apart. This was due to phase transformations related to welds—you know the Kaiser ships were built so quickly by welding them together—many of them were made here in Richmond, maybe most of them. The problem was a complex attribute of steel that involved a transition from ductile to brittle steel at the temperature of the ice water in the North Sea, and a lot of them just plain broke apart when that happened. So that's what Parker came to Berkeley to work on and eventually joined the faculty. He was an early member of the National Academy of Engineering and was a winner of the National Medal of Science. Outstanding soul.

He saved the department in the fifties. When Clark Kerr became chancellor—he was the first chancellor that you may have picked up in your oral history discussions—he wanted to eliminate this department, and I guess Parker went to work and took it upon himself to build things up. One area that was a problem was that there was virtually no research going on in the department and he really took it upon himself to go out and find out where and how you get money for research. Then the College of Engineering established an organization called the Institute of Engineering Research. I think a non-professor who headed that for a while was George Maslach. Later Maslach moved into mechanical engineering as a faculty member and Parker took over as head of the Institute of Engineering Research—from the story I heard purely to learn how to get research money. So research really built up within the department in the late fifties.

The most outstanding person scientifically in the department was John Dorn, who was a mechanical metallurgist but all of his degrees were chemistry. A chemist can become an engineer because I think that engineers and chemists

think a lot alike. Dorn was to me the intellectual giant of the department. He smoked all the time and unfortunately at age sixty-three died of lung cancer.

Swent: Oh, what a shame.

Fuerstenau: Yes, it really was. It's one time you think, "Couldn't you save a brain?" [laughter] Then another younger physical metallurgist was Jack Washburn, who had done his PhD with Parker. On what he did for his thesis, they won the Masterson Gold Medal for actually showing for the first time dislocations by etch pits. Dislocations are crystal lattice defects that give rise to the mechanical behavior of metals. Jack was here. Then a young dynamo came shortly thereafter, I think spring of 1960, Gareth Thomas, who I suppose one would consider worldwide the number-one person in the use of electron microscopy for studying metals and alloys and defects in solids. He's a real, real doer.

Swent: He came about the same time you did.

Fuerstenau: Yes, just a semester later. He came as a lecturer. A bit of a story was that Parker said that he would make him a lecturer—now a lecturer, which he was in England at Cambridge, is an academic title. A lecturer here is a title that was given to nonregular faculty for teaching courses, sometimes part-time, without guiding research of graduate students or being involved with other academic duties. But Gareth became a big backbone of this department with a faculty appointment in the next year. He's a Welshman, but is British in doctoral education. Then that covered the metallurgy part of the department. We were not large.

In ceramics there were three people: Joe Pask was the department chairman. Pask was ranked very high in this country as a ceramic engineer and got heavily involved in ceramics research and government panels. The second was a young person named Alan Searcy who came here about '54, and had done a PhD here in chemistry with Leo Brewer and then spent six years at Purdue. He came here, I suppose, as an associate professor in ceramics to teach and do research on high-temperature chemistry. Ceramic materials always involve high-temperature in producing them and often in their use. The third person in ceramics was Dick Fulrath who was acting assistant professor and was finishing his degree. Fulrath became an expert in electronic ceramic materials. Unfortunately he smoked all of the time and at age about 54—I was chairman then—died of lung cancer.

Swent: What a tragic thing.

Fuerstenau: Then, on the mining side, was Lysle Shaffer who I think was really responsible for bringing me to the attention of the department. Although once I was here, there was no role that he played in my functioning. Shaffer taught

mining courses and mineral economics, this sort of thing, and all the time I was here was a problem to the chairman.

Swent: In what way?

Fuerstenau: Giving him problems over almost every action that the chairman would do, I think, and eventually Shaffer became an alcoholic and at age fifty-six shot himself over problems that he felt he couldn't overcome, I guess. That evening, I called his wife. Other people wouldn't, but I called her up to talk to her. It's funny how some people know what to do or say in that sort of situation and other people, a lot of them, even some who were a lot older than I was, didn't know quite what to do. Anyway, that brought the mining program to an end at that time.

Then there was a very good man who was an old exploration geologist named Ed Wisser. Did you ever meet Ed Wisser?

Swent: I heard that name a lot.

Fuerstenau: I guess he spent a lot of time in Mexico and taught mineral exploration courses. A lot of geology students came to take his courses, including Roy Woodall¹⁶. He may have been a major advisor to Woodall. I got involved a little bit on consulting projects with him a couple times. He probably retired within two or three years after I got here. They gave him an honorary doctor's degree. You know UC now gives no honorary degrees due to Reagan's politicizing that when he was governor. Anyway, I remember writing Wisser a note congratulating him and he replied, "It's nice to get these honors while you're still living—" [laughter]—"and not afterwards."

Another outstanding person was Parker Trask whose field was beach erosion, things like that. You may have heard his name from Bob Wiegel¹⁷, possibly.

Swent: Also from Gordon Oakeshott¹⁸.

Fuerstenau: Okay. Trask, I remember, used to be a very interesting person to sit with at lunch because he was knowledgeable on nearly everything. Apparently he was a super genius who got his BS degree at eighteen or seventeen, something like that. His field was geology and beach sand erosion, basically. He had a heart attack, died at age sixty-three. Enthusiastic guy. I liked him. Dick Goodman

¹⁶ Roy Woodall, *Australian Geologist, 1953 to 1995: Success in Exploration for Gold, Nickel, Copper, Uranium, and Petroleum*, 2006

¹⁷ Stanely Weigel, *Litigator and Federal Judge*, 2000

¹⁸ Gorgon B. Oakeshott. *The California Division of Mines and Geology, 1948-1974*, 1988

had come to Berkeley as a graduate student to study and do his PhD research under Parker Trask

Another mineral person who came half a year before me was Stan Ward, an exploration geophysicist that they brought in from Canada. Very active person. Active in the local AIME and so on, and built up an applied geophysics group here in a huge way. But he got divorced and had to leave the state and went to University of Utah. Anytime I saw him there, he always told me how much project money he had—I'm speaking of research money—how many millions he had. I remember Milt Wadsworth saying that he always considered the output of students and quality research to be inversely proportional to the amount of money a professor has. That was Milt Wadsworth over there at Utah. Stan Ward was a doer.

For a few years Herb Hawkes, a recognized mining exploration geochemist, was a professor in the department. Hawkes had coauthored a classic book on exploration geochemistry and had been a number of years in the Geology Department at MIT, but he never held a regular faculty rank there, only that as lecturer. At Berkeley he never established an ongoing research program and left after a few years—a big bust here. He walked out on his family at the same time.

Swent: Personality and character do enter into the professional life.

Fuerstenau: Oh sure. Isn't that right? Virginia Mee, who I mentioned earlier, worked for him sometime during that year she was here working on a master's degree.

Swent: I was interested in something you said earlier too. You said that a chemist could become an engineer. You implied that it wouldn't work the other way because they—

Fuerstenau: No, no.

Swent: And then you said because they think alike. What does distinguish—what is the distinctive way of thinking?

Fuerstenau: Let me tell you, my education was as an engineer and I do a lot of chemistry and have had a lot of interaction at a high level, scientifically and professionally and personally, with the giants of surface chemistry around the world. I, as an engineer, have moved into doing chemistry as a fairly significant part of my research. A real example of an engineer who became a great physical chemist is Henry Eyring, whose first degree was mining engineering, as I mentioned earlier. His engineering background led to his modeling mathematically many phenomena in physical chemistry such as reaction rates and viscosity of solutions. I have always observed that if someone graduates as a physicist, the first thing they'll say is, "I'm a physicist" even after thirty years, whereas some giant metallurgists like John

Chipman did his PhD in chemistry here at Berkeley—and John Dorn of our department got all of their degrees in chemistry. Another example is Wayne Hazen¹⁹, who graduated in chemistry from Berkeley in 1940. Interestingly, some years ago Professor Overbeek, the great giant of colloid chemistry, told me that he was now more comfortable talking and interacting with chemical engineers than with chemists. Today many engineers do good chemistry and, in fact, a lot of engineers probably do good physics, particularly in electrical engineering. But there's something about a physics education that makes the person always say, "I'm a physicist." Physicists, I believe, try to explain existing phenomena and concepts on the microscale whereas applied chemists explain chemical phenomena more on a macroscale.

Swent: What's different about engineering? What's so special about it?

Fuerstenau: One often reads something to the extent that a scientist studies "what is" and an engineer produces "what never was." Well, an organic chemist, such as Guy Harris, may spend his life making new chemical molecules that never existed before, so there is that similarity between some chemists and engineers. I'm assuming that many decide to study to be an engineer due to their interest in application and in making new products, new materials, and devising new processes, maybe to better the life of people on this earth.

Swent: Be practical.

Fuerstenau: Right. I'm interested in application and how what we do might be used. Even when I've gotten involved in very fundamental surface chemistry, I have a reason why I'm interested in a basic phenomenon—namely its application to flotation or other problems related to water/solid problems. But still, all my work in this area is done in the same way someone would do it as a chemist. In today's electrical engineering a lot of research will involve the interrelation between physics and electrical engineering. Today the percentage of faculty in the College of Engineering whose PhDs are in the applied sciences or mathematics is pretty high, particularly in computer, electrical, mechanical, and nuclear engineering. It must be a large number. What's often the case is that a very difficult applied problem takes advanced math or advanced statistics to solve it.

For example, when I was chairman—a person joined the applied geophysics group in our department, Leon Borgman. He had a PhD in statistics from Berkeley and was an associate professor in math at Davis at the time. Bob Wiegel was partly responsible, maybe largely responsible, to get him here, since Wiegel's work related to the statistics of wave action. The same type of

¹⁹ Wayne C. Hazen, *Plutonium Technology Applied to Mineral Processing; Solvent Extraction; Building Hazen Research; 1940 to 1993*, 1995

statistics was important to the application of seismology in mineral exploration and was then of interest to that group in our department.

Anyway, Leon Borgman—and I remember trying to talk him out of it—wanted to move where he could raise his two boys, who were then about fourteen and twelve, outside of a city. He went to the University of Wyoming in Laramie, and I thought, “What a damn fool to do that. They’ll be out of the nest in four years.” Anyway, about two years ago Borgman was elected to the National Academy of Engineering. I think one of the persons behind his nomination was Bob Wiegel from civil engineering. I remember not long ago chatting with Bob Wiegel and Wiegel said that Borgman worked a lot on the statistics of waves interacting with oil platforms. First he did it on one-dimensional and then two-dimensional—I don’t know, maybe he got even into three—for the complex interaction between an offshore oil structure and wave action. Here he is in Laramie, and Wiegel said if anybody worldwide is going to build an oil platform or structure in the sea, they all come to Borgman as a consultant on this. Borgman had said that being in Laramie he’s equidistant from all oceans. [laughter] He was landlocked—but the world’s foremost expert on ocean platforms.

There was also a petroleum group of four people. Paul Witherspoon, who I still see now and then. He’s active as ever and he’s I think eighty-two, eighty-one. You wouldn’t know it; he doesn’t look any different than he ever did, doesn’t act any different. Witherspoon now is known as one of the giants in groundwater engineering, hydrogeological engineering. Another was John Putnam whose field was fluid mechanics and had come over from mechanical engineering. He died before he retired. The stalwart was Bill Somerton who worked on drilling and well production, who remained on the Berkeley faculty until his retirement. Petroleum engineering as an academic discipline was started in Berkeley by Professor Lester Uren, an early Berkeley mining graduate who was still around, retired, when I arrived in 1959. He was the founder really of the whole field, educationally, of petroleum engineering. There’s now a major AIME award in petroleum engineering, like the Richards Award in mineral processing, the Lester Uren Award, named in his honor.

Swent: I’ve heard the name.

[Tape 18, Side B]

Fuerstenau: I remember seeing Uren quite often but didn’t know who he was or anything about him; but what a giant he must have been in his day. One day there was great excitement: Witherspoon went running. Carlson had come after him with a pistol. I remember Seaborg’s letter said Carlson had—[looking at letter] where does it say it here?—“Question of replacement of Anders Carlson. He has become incapacitated...is still occupying an FTE.” It was just a little pistol or something, but nothing happened out of it.

Swent: Oh my.

Fuerstenau: Finally, there was an associate professor who was really intellectually outstanding, Irving Fatt, who had come from Chevron Production Research down at La Habra [California]. All his degrees were in chemistry and surface chemistry. He had written a seminal paper on network analysis; the pores in the rock are all interconnected like a network. He wrote the first classic paper on that topic. Fatt eventually got interested in oxygen transfer in the lungs in blood flow, then got interested in oxygen transfer in the eye, and eventually became a professor of optometry here at Berkeley.

Swent: For heaven's sake!

Fuerstenau: His research in optometry was still involved with fluid flow and gas transfer. He later did a lot of research related to contact lenses, because the oxygen has to move across the eye through the fluid. By the way, here is another example of the interrelation between engineering and science.

So that made up this department at that time. I was thirty when I came here then.

Swent: This is your fourth institution.

Fuerstenau: You know, it turns out I must have only been twenty-seven when I went to Carbide as section leader of that group and I would have been twenty-nine when they hired me to be manager at Kaiser. I see somebody that age today, I think, "Oh, they're way too young for this, that, and the other thing." But I came here as a top level associate professor at age thirty in 1959.

Initial Teaching at Berkeley

Fuerstenau: Then in the spring of '60 I taught a sophomore course that we called "Introduction to Materials," which all engineering sophomores had to take. They assigned this to me for the spring, which had about 120 students. That was the first time I had ever taught a fairly large class. Of course that subject matter involved the foundations of physical metallurgy, ceramics, and polymers, so I had to spend a fair amount of time studying. I don't know how many books I read constantly and worked on, preparing those lectures, since that aspect of metallurgy I had long forgotten.

Swent: If it's a course that's required it had been taught before by other people?

Fuerstenau: Oh yes. In the fall semester it might have had an enrollment over 200 students, but it was 110 or 120 in the spring.

Swent: This was the only section of it?

- Fuerstenau: There's only one section. Then it had a lab. In those days there were two professors in each lab and today there are none in the lab.
- Swent: All TAs.
- Fuerstenau: Yes. For about a year or two, professors were assigned to each laboratory session, at least for the first hour or so.
- Swent: Was there some sort of syllabus that you were given?
- Fuerstenau: We had a text, but I made my own lecture notes. I never was one to teach a course out of a book. I did it only once when I taught one of Ravitz's courses while he was on leave and really followed the book, because his lectures had followed it. This was a text on heat and fluid flow in process metallurgy written by Reinhardt Schuhmann. You may recall that some teachers you might have had just sort of read from the textbook.
- Swent: You hated to come to class.
- Fuerstenau: Yes, yes, whereas I always prepared my own lectures, wrote them out, and always right to the end. I'd rewrite them every year, every other year, or something. That first year I worked giving these lectures off the blackboard. That to a hundred-and-some people is not the best way to teach, in my opinion.
- Swent: Where was it?
- Fuerstenau: It was in the major lecture hall there in Hearst Mining Building. I'll just go ahead. I taught it the next spring also, but I changed my whole approach to using overheads and teaching aids like that, or maybe even in those days it was slides, because I don't think that simple transparencies existed yet. I probably used glass-mounted slides with a big overhead kind of projector. You know, I personally don't like that because that makes the room dark. People can fall asleep or anything else, right? But it's a lot easier to teach to a big group. Of course with a big class you can't tolerate questions really, during the class; otherwise it just breaks it up too much. Smaller classes it's easy for somebody simply to ask any question they want at any time.
- I did that the second year, and also in the first summer since I did not have research funding. I found it interesting that the grades in the summer session—there were maybe thirty-five students—were higher than they were in the spring session. Number one, there were kids that may have come from elsewhere, wanted to just take the course as a summer course at Berkeley. Number two, some of those were transfer students, maybe coming in as a junior and hadn't been able to take the course as a sophomore where they had been.

Swent: More motivated then.

Fuerstenau: Yes, I could make a comparison because I probably used roughly the same exams or types of exam. Of course it's a lot easier to teach to thirty-five. Then the following summer I taught that as a special two-week or three-week course to junior college teachers as part of some program. We went all day. I did all the lecturing, I assume, morning and afternoon. Because there would not be laboratory facilities at the junior colleges, we did not have lab sessions. There must have been twenty-five or thirty junior college teachers who taught this course at one of the JCs around northern California. Anyway, I found the experience to be very pleasant, very interesting. I remember a teacher from a JC in Vallejo telling me that in her classes there might only be a half-a-dozen or so students that really had an intellectual objective when taking her various courses. At that time she said because only a handful were planning on going to university and getting a solid education, it made teaching, she told me, for them very difficult. That was just one of the discussions I recall.

Swent: Maybe could it be that one reason there were better grades then, in the later sessions, was because you were a better teacher?

Fuerstenau: Well, no. The way I take it is this: this course was required in the fall in the sequence, so there are some students who fall behind and then get out of sequence. I think way back when I was involved on some undergraduate committees in engineering, I found that it was more typical that somebody might graduate in nine semesters in engineering than in eight. There was some fraction that made it in eight, but those who took longer were then falling a little bit behind along the way.

The average, when you've got a course of a hundred or so is kind of interesting. The GPA [grade point average] of the average undergraduate engineering student back then was something like 2.75 where A equals 4. What I did was match the peak in the grade distribution such that I gave a GPA of 2.75—how many A's, how many B's, so the peak would be there. It made it very interesting for grading because in a class like that you can give two, three F's or something. You don't do that in a grad course. But I found that—

Swent: You don't have to follow the curve?

Fuerstenau: In actuality I did, but in the spring semester it was clearly bimodal and did not have a normal distribution. In other words, there was cluster of students with lower grades and a cluster with higher grades. All of the grades were adjusted such that average equaled the 2.75. In summer school, that bimodal distribution was missing and all the grades were actually higher—interesting.

The first spring semester, they also enlisted me to co-teach a graduate course on thermodynamics, but I didn't do that. I'll come back to one other part of

what went on in Hearst Mining Building at that time. In our building was also a Bureau of Mines Station. It might be interesting later to talk a little bit about the Bureau of Mines. This group did the thermodynamics of metallurgical—inorganic kinds of compounds. They occupied—[Phone rings. Tape off] There was the Bureau of Mines Thermodynamics Station here. It looked like a half-dozen, eight people maybe. I didn't know anybody but the director.

Swent: These were the employees of the BOM?

Fuerstenau: Oh yes, they were actually Bureau people. Bureau of Mines stations were always on university campuses around the country. All of them were due to some policy at one time I guess. Anyway, this was the Thermodynamics Station, and the director was K.K. Kelley, Kenneth K. Kelley. The thing about Kelley was when I took a thermodynamics course at MIT the name K.K. Kelley was virtually a god for some of the things that he had done. I had always heard the name of K.K. Kelley because his name was often brought up in lecture notes as to how you get certain thermodynamic quantities. Anyway, Kelley was just such a modest, unassuming person. I think all of his degrees, PhD included, were from Stanford. I remember him saying, "You know, I'm really a graduate of Leland Stanford Junior University." [laughs] It's on the diploma, right?

Anyway, this Bureau group was here for quite a few years during my early times here. Kelley was a lecturer in the department and every other year taught a graduate course in thermodynamics. I remember him one time saying, "On one exam I give four problems on the midterm exam." He said, "I have different kinds of exams, but this one I want only the answer." They'd turn in only the answer, being the numbers that they had calculated for the problem. He said in his opinion people have to learn to calculate accurately and the vital thing is being accurate. "On this one exam all I want is that calculated number." [laughs] He's the only one I know that's done that kind of exam, but anyway, that was his reasoning behind it, and of course he had a point.

Swent: He didn't want to know how they arrived at it?

Fuerstenau: You read about the millions spent going to Mars where one group had calculated certain quantities in feet and pounds instead of kilograms and centimeters, as done by another group, and because no one noticed that until later, the lander crashed on Mars—a couple years ago. So anyway, Kelley was trying to avoid that sort of thing.

Kelley was interesting to talk with. He really ran things very independently, and there was no real interaction other than between him and Ralph Hultgren because Hultgren had a major research program on thermodynamic measurements and evaluation of thermodynamic data on alloys and ceramic compounds. One time Kelley came to the conclusion, he said, "I'm pushing all these papers, doing all this work, and the difference between my pay

retired and my pay for pushing all these papers would be something like \$1,500 or \$2,000 a year. I'm retiring." He discovered that the difference between retirement pay and active pay was like \$1,500 bucks. He said, "I'm through." What eventually happened was that that group of ten to twelve people were transferred. Half of them then went to Reno and the other half up to the Albany, Oregon, Bureau station. They had occupied quite a few rooms in Hearst Mining Building. I was on sabbatical leave then, and the Bureau of Mines wanted to work out some sort of cooperative deal with the college, but the acting dean at the time, someone who never was a viable researcher, turned the proposition down. I was not involved since I was away.

The Beginning of a Viable Research Program

Fuerstenau: Eventually things for me got started. By the way, because of my role in teaching that materials course, I was one of the few people in that department who understood what was going on, and I think maybe the only person, all the way from the mining side through the processing, and to ceramics and physical metallurgy and electronic materials, and so on. It meant that later on as chairman I was able to make good judgments on what the whole range of people were doing, whereas a lot of people say, "That's not my area so I abstain." I think some extra good came out of all that early teaching.

Swent: You didn't have any ceramics experience until now, did you?

Fuerstenau: No, none. But you see ceramics is really concerned with the behavior and processing of fine particles. To produce a ceramic body, you may start with clay or with particles that have been ground very fine. The body is now formed into the desired shape, almost like making pellets, you might say, and then they are fired. In ceramic processing, one deals with ceramic pastes and slips, they call them, which are just suspensions of particles in water. Control of the rheology of the suspension is closely related to mineral processing. When I worked at Kaiser, the director of their ceramics research lab in Milpitas came to visit me a couple of times—he had a degree in mineral processing maybe from Columbia.

What I studied and know a lot about, at least so I think, is the interface of a mineral, an oxide in water. Most of ceramics are oxides: alumina, silica, magnesia—all the things I've studied a lot. In ceramic processing one must be involved with grinding, size distributions, the surface area of powders, particle packing, and so on. I think mineral engineering people knew more about the fundamentals of these systems than did the ceramics people back forty years ago. Today it's probably another story.

Swent: Ceramics wasn't nearly as important then as it has become since.

Fuerstenau: That's correct. Years ago typical ceramics would be whiteware, you know from—bathroom fixtures. Of course glass is another story, because that involves melting, but that's still ceramics. And there were refractories, the bricks for furnaces and so forth. Of all the ceramics people I know it's only a rare one that's worked on glass. Today the starting material for high-tech ceramics may be fine particles produced by chemical reaction in plasmas, et cetera.

What helped me get started in research at a significant scale was that I had research projects with Pask and Fulrath for a while. They actually included me in a research program on ceramic processing. A lot of my later students have been working in the ceramics industry because they know a lot about particles. I had had no interaction with or knowledge of ceramics prior to coming here.

[Interview 9: September 17, 2001 in Berkeley, CA]

[Tape 19, Side A]

- Swent: We've had a break here of several weeks now. I took a vacation and then we've had, on September 11, a severe interruption in all of our affairs—the terrorist attack in New York. When we last interviewed it was August 20.
- Fuerstenau: One month.
- Swent: Almost a month, and our world has changed a lot since then. When we ended you had just left Kaiser and come to Berkeley. I believe we talked about your getting a house on Arlington in Berkeley. You had two children then. Let's just begin with your recollections of starting in as a teacher at Berkeley then in—this was 1959. Something that you had looked forward to for a long time.
- Fuerstenau: Yes, yes, indeed. When I look back at people in charge of the department, which probably were people like Joe Pask, who was chairman, and Earl Parker who had been his predecessor, and maybe John Dorn—Ralph Hultgren—I am pleased that they saw the need for having an active program in mineral processing. I really think when you look back at it that their foresight was good. It's pretty easy, in the academic world, to decide, "Well, what I want to do is add somebody who's in my specific area rather than broadening it."
- When I got here there was nothing; this whole field was just dead here. I may have mentioned earlier that the laboratory that I was given didn't even have lights in it. They were renovating Hearst Mining Building for the second time, all of which was minor compared to the \$96-million renovation going on today. They destroyed the ventilation by filling in the inner court. I think they were spending like a million dollars just to put ventilation into a building that in 1904 cost one million.
- [Added by Douglas Fuerstenau during editing: Sometime during my first year at UC, a person named William Monahan in the UC president's office called me and arranged a visit with the head of the Charles W. Merrill Foundation in San Francisco—Monahan probably knew him from the Bohemian Club. An 1891 Cal graduate, Merrill was the person who made the cyanidation of gold ores a viable process. Monahan thought that the Merrill Foundation might be willing to equip a mineral processing laboratory in Merrill's name. But unfortunately nothing came of that because the orientation of the Merrill Foundation apparently was directed towards philanthropic causes, I guess.]
- Swent: What was the academic year at that time?

Fuerstenau: The academic year then was semesters. That was called the “old Berkeley semester” which started around the end of September, nearly October 1. There was a Christmas break and two weeks of lectures after Christmas. Both the faculty and students found those two weeks to be pretty dead. It’s one thing to come back and do finals, but to come back and do two last weeks of lectures was bad. As I said, they called that the “Berkeley semester.” I don’t remember being distressed about gearing up again because I suppose I was eager.

First Visiting Researcher: Vladimir Nebera from Russia

Fuerstenau: At that time there were no graduate students in my general area. Just the other day a former PhD, named Abdel Abouzeid, who’s visiting here from Cairo University for a couple of months, told me that at a congress in Turkey last year he met someone who was my first researcher here at Berkeley. Abouzeid said that he had met a seventy-year-old white-haired Russian, and I chuckled because that’s a little younger than I am. A much later postdoctoral researcher, Dr. Jan Dzrymala from Poland, also e-mailed me that he too had been there and met a seventy-year-old white-haired Russian who had worked with me in 1960. This was Vladimir Nebera. There were two Russian engineers that had international postdoctoral fellowships paid for by the IREX program—I called it “behind the iron curtain fellowships.” I don’t remember what the acronym is for.

Swent: This was the height of the Cold War!

Fuerstenau: I want to tell you about that because this was the fall of 1960, and I had no grad students. Nebera conducted a research project on polymers for flocculating mineral slurries. He’d done this in Russia at the Metals and Alloys Institute in Moscow and worked for Professor Polkin, one of the very outstanding Russian professors. I’ll tell you another story about Polkin later. In Russia they didn’t give doctor’s degrees after completing their graduate education at that time; they were called “candidate in science.” In other words, it was their equivalent to a PhD in this country. He’d just completed his candidate in science degree there. He was here for the entire year. I remember he wanted to go out to Dow [Chemical Company] in Martinez, because Dow manufactured polymers for this purpose. Because of these Russians I got to know the director of International House quite well. I can picture him, and we even see him once in a while at the San Francisco Opera.

Swent: Sherry Warrick?

Fuerstenau: Yes, Sheridan Warrick, and that was over forty years ago. Anyway, a Russian could not go anywhere beyond a twenty-five-mile radius without permission. The director of I-House told me that the US government set up various areas in the US that were prohibited for the Russians to visit. How they selected these areas, he said, was that they took a map that showed areas in Russia that

were prohibited to Americans, and superimposed it on a map of the US. Wherever an American in Russia was prohibited in going, if that fell on a spot in the US, the Russians weren't allowed to go there in this country. [laughter] As simple and ridiculous as that.

Anyway, we got permission to go out to Martinez. I think if the Russian group went to a place like Yosemite, et cetera, they had to have all this pre-approved by the State Department. Remember, this was 1960.

I guess my politics are never hidden below the surface very low because sometime during that fall, late that fall, Vladimir said to me, "Gee, I didn't quite have nerve enough to tell you, but I went over to the Cow Palace to hear Kennedy speak." [laughs] You can guess about my attitude towards Kennedy then, after having lived in Massachusetts. Nebera was here for that year, and the only person that I had any research connection with because I had no graduate students, et cetera. Our facilities were pretty primitive and minimal then. He conducted some research on the effect of multivalent ions on the flocculation of quartz with polymers such as Dow Chemical's Separan—a topic that he had brought with him from Russia. We really should have written a short paper on what he did accomplish, because it was innovative then.

Swent: Have you been able to keep in touch with him?

Fuerstenau: Yes. I've seen him at congresses in Europe through the years. Periodically he was there. I'm speaking now of twenty, thirty years ago because, of course, the Russians always made a delegation and I imagine they picked the people who would be allowed to attend. He became a professor there. He could speak English, so he would be a good person to be part of their delegation.

Swent: We should get the name of this congress also.

Fuerstenau: The International Mineral Processing Congresses. We're stepping a little bit ahead, but I went to the International Mineral Processing Congress in Leningrad in 1968 and also went to Moscow afterwards. Vladimir spent a lot of time showing me around and invited me to his apartment, which, in his case he owned the apartment. Apparently some people had the opportunity to buy a place, but also there were a lot of people that had no apartment.

Swent: This was in Moscow?

Fuerstenau: Yes. They had one bedroom, one living room, and I assume a small kitchen. They had, at that time, a fourteen-year-old daughter whose bedroom, I gather, was the living room. But he owned it. He helped make my stay in Moscow very good. I haven't seen him in more recent years. The last I heard about him was this "seventy-year-old white-haired man." That's not the Vladimir Nebera that I recall, of course.

Organizing New Courses and Teaching at Berkeley

Fuerstenau: Maybe we could talk a little bit about teaching here.

Swent: You had talked earlier about your attitude towards preparing lectures.

Fuerstenau: Oh yes, I worked very hard at that, always. Even up to the final ones. Particularly graduate courses, I updated those constantly. Undergraduate courses you can kind of stay with existing notes. In fact, the last time I taught an undergraduate course I might look at my notes five minutes before going to class whereas for a graduate lecture I might work two hours on updating, et cetera, because grad students—[chuckles]—have a better background than I had gotten decades earlier, so you have to work to stay ahead of them.

Swent: Well, they're working on the things that are more—

Fuerstenau: Everything's later. That's what I meant by better. It's more recent and more intense probably. The first course in the general mineral processing area I organized and taught was an undergraduate course, often just a handful of students, like a half-a-dozen or so.

Swent: All men?

Fuerstenau: Yes, almost all, way back then. Now there are a lot of young women in engineering classes, but then it was really rare. I doubt if we had any in the department, graduate or undergraduate, early on. There was one female grad student in ceramics that I remember.

My general object was to teach principles, not just how do you process an ore but what are the principles behind crushing, grinding, behind classifying, behind making mineral separations like flotation or by gravity. The whole course was always oriented or designed on teaching the fundamental engineering science, let's say, behind all of these operations. Eventually I changed the name of this undergraduate course to "Particulate Materials," because in mineral processing you deal with particles; you grind an ore fine enough to liberate the minerals. But there are many other fields concerned with particulates: for example, the whole field of ceramics; you make particles and form them into a body and then fire it. So the engineering behavior of particles has a wide-range importance. That was something I decided to do just to broaden the scope so that maybe other people would also take the course.

All through the years my main personal efforts were always with graduate courses. Once in a while I had seminar courses that I would offer, such as on comminution and crushing-grinding, whatever, either lectures that I gave or invited outsiders to come in and give lectures. These would be for graduate students, once I got a group of grad students.

I always have had a big interest in surfaces, interfaces of minerals in water, because that's the basis of making flotation separations; you change the surface property of one mineral versus that of another. Actually, interfacial phenomena are encountered in some way in most of the operations involved in mineral processing. I told you how at MIT I had the good fortune of studying under Professor Overbeek and Professor Gaudin, et cetera, where I became oriented towards fundamentals, really almost physical chemistry. So that determined the orientation of my first graduate course. I also made the decision that if I made a course that was of interest only to my own type of person, my own grad students, then I'd only have two, three people each year in the class. So I organized my lectures to have broad appeal to a wider range of students. Now, that's tough to teach because what I was doing was then trying to include material and applications that would fit the various fields of students enrolled. So that main course would have my own students, ceramic engineers, I remember generally a few chemical engineers, a few people from soil science, wood technology; one time there was one or two from biophysics way back, civil engineering, environmental, the water-treatment engineers. I usually had two, three of those each year.

Swent: What did you call this course?

Fuerstenau: That's a good question. I called it "Surface Properties of Materials." It was really, let's say, applied surface chemistry, but I didn't want to use the word "chemistry" because I was not in the Chemistry Department. So I called it, "Surface Properties of Materials." I had generally eighteen, twenty, sometimes maybe a few more, students in that class. Way back, but not today, that class brought in more outsiders to the department than I think any other graduate class.

Swent: You mean outside students?

Fuerstenau: Outside students, because departments got real credit for students from other departments. Every other year I taught a graduate course on flotation. Then I organized a course later on that I called "Applied Colloidal Phenomena." I had gotten quite interested in the behavior of colloidal particles. These are particles about one micron and finer; very fine clay is a good example. Colloids—a lot of pharmaceuticals are colloidal, very fine particles which totally are then controlled by surface properties—they've got such a high surface-to-mass ratio. Today, these are hyped as nanomaterials. This course again involved principles, so it also attracted students in ceramics, in the environmental aspects of water treatment, some chemical engineers.

One time I remember in one of these courses was a civil engineer who was interested in making asphalt pavement. The principles are the same as flotation: for example, on a wet day if it's raining or something, you now have the rock in the pavement that is coated with water because nearly everything's water-avid. So if you try to put the asphalt on, it won't stick. Similar to the

case of flotation where you want an air bubble to stick to the solid immersed in water, how do you make the rock hydrophobic? I asked this student, and also my neighbor friend, Sam Lucchese, who worked with Caltrans about this. They actually do have big drums in which they charge the aggregate and rotate it with an amine—an amine being the same cationic flotation collector that will coat the rock and make it hydrophobic. I imagine it's expensive, but it makes asphalt adhere to the wet rock.

So you see why teaching a course like this on the fundamental basis can have a wide application. But like I said, trying to weave a path to interest the range people who are there and keeping them interested is not an easy job.

One time, in more recent years, I organized a course I called “Advanced Mineral Processing.” You asked about women in class. I remember there were a couple of young ladies who were grad students in mechanical engineering, working on solid-waste treatment, because today taking solid waste and separating components involves mineral processing operations. You break it, crush it, grind it, separate it either by flotation or by sink-float methods or by magnetic separation to pull out the steel, and gravity to separate glass, and so on.

Then it all changed when the semester system changed.

Semesters Versus Quarters in the Academic Year

Swent: How did that affect your teaching when that changed?

Fuerstenau: Let me back up a little bit. You had asked earlier about semesters. You know, when I first came here we were on what was called the “old Berkeley semester.” At that time, in the College of Engineering, I think our department had something like 128 credit hours or units, whatever they were called—they renamed them when they kept them separate. But the degree requirement was something like 128 credit hours. I think maybe civil engineering had 134, and maybe industrial engineering even a little higher. In other words, at that time—

Swent: You mean the requisite to graduate?

Fuerstenau: To graduate, to get a BS degree, required roughly 130 units or credit hours total: one credit for each lecture hour, and one credit for each three-hour lab.

Swent: Wasn't this more or less standard with universities all around the country?

Fuerstenau: Every place—I may have mentioned that—every place but MIT that gave a credit for the hours of study, so a two-credit course at MIT was a six-unit in MIT units. I think they had forty-eight instead of sixteen.

Swent: But most universities had 120 to 130.

Fuerstenau: That's what I'm really going to get back to. Then, in the late sixties came a big push about possible year-round operation. One way to have year-round operation might be to have a summer quarter. Of course, they kind of forgot that people get burned out, I think, if there were year-round operation. It never did come into being. The whole UC system switched to quarters.

Swent: When was this?

Fuerstenau: I think late sixties. Sixty-seven, '68.

Swent: Sixty-four was when the big Free Speech Movement began, wasn't it?

Fuerstenau: Oh yes. I'll get on to that.

Swent: Anyway, this kind of tied in with that.

Fuerstenau: This was after that. I don't know whether there was any connection.

Swent: Maybe not.

Fuerstenau: [pause] Sixty-four was a great period—I don't know if great period—but a period for this university. We'll talk about that.

On teaching I found I personally liked the quarters because in this department the basic teaching load has always been no more than about one course per term because there's a very high load of grad students that most faculty generally have carried, and guiding research is teaching, of course, by definition, too. As for my own teaching, every fall I taught this surface course, and in the winter and spring quarters, I would alternate courses every other year. For example, I recall teaching the applied colloid course and alternating it with one on flotation principles every other year. So that way, teaching them every other year, one had a pretty good enrollment. I may have, in a two-year period, been able to teach four or five different courses with only one per term. So I personally liked the quarters.

Swent: Were you and the faculty consulted at all about this change?

Fuerstenau: Oh, it was voted on by the Academic Senate and the faculty. I have a feeling I probably voted against going back to semesters because I probably had come to the conclusion about greater teaching flexibilities with quarters, as we were talking about.

Swent: So it was semesters when you arrived and then it changed to quarters and then back.

Fuerstenau: It was 1984 when Berkeley switched back to semesters—I know that because I was on sabbatical in Germany. In switching back to semesters, the semester now always starts August twenty-something instead of September twenty-something. My son went to UC San Diego and he started practically the first of October. Berkeley was the only campus to switch back to semesters.

Swent: Of the universities.

Fuerstenau: Campuses in the UC system. All the rest, to this day, are still on quarters. I heard somebody on the faculty—not in the department—on the campus here saying that the president must have been caught off guard or he wouldn't have approved it. But as you probably heard in some of your other interviews, each campus is semi-autonomous in its functions, which is a good thing. That's why the approach here at Berkeley or that at UCLA—

[Tape 19, Side B]

Fuerstenau: So once we switched back to semesters I dropped, for example, this advanced mineral processing course which I really would have liked to have continued, but I didn't want to load myself down with lectures. Going way back, it's maybe about my third or fourth year here, Professor Ravitz went on sabbatical. In those days, when somebody went on sabbatical leave, his courses were taught by other faculty. His course was in process metallurgy, which I had of course taken, having a degree in metallurgical engineering, and I also audited courses from Professor Schuhmann at MIT. That became the basis of my lectures. Schuhmann had written what was volume one of a book on process metallurgy. Volume two never ever saw the light of day, but volume one had to do with heat balances and flow of gases out of stacks and energy balances, that sort of thing.

Ravitz taught the course out of the book, and that was the only time I ever taught a course that I followed a book. I would always collect items from many books. Even if I had a text, I never followed the text; I think that's a dull way of teaching. If you remember back, you may have had some lectures where the professor virtually got up and essentially read out of a book to you in class. I, of course, didn't do that, but I didn't like giving a lecture that just exactly follows what's in the book, but I did that.

But in his chemical metallurgy course, I incorporated lectures from Schuhmann and added material of my own, didn't follow what Ravitz had done. I had this beautiful set of notes and one day, some years later, Professor Milton Wadsworth, who probably is the world's most outstanding person in hydrometallurgy, said that he had never taught a course in chemical metallurgy. I said, "Well, I've had these notes." So I loaned him the notes. It kind of amazes me that he had never taught a chemical metallurgy course of that nature. Then he sent them back. Some years later, I loaned the same notes

to my brother Maurie and he lost them. I regretted that. This was really the day before Xerox machines.

Swent: Ah, that's a shame.

Fuerstenau: It really is; I should have never done that. I've never taught that course again either. I wanted to say that that one semester I taught lectures at nine, ten, and eleven, Monday, Wednesday, Friday, for one semester. I'll tell you, by noon I could hardly move my mouth. It really was an exhausting thing. Of course, you know, at a state college professors have to teach maybe four lectures, four courses. At a JC, junior, community college, instructors, without a doubt, teach four or five courses a day. They may not do it as intensely as this, and they do not do research. But as I said, here our load has basically been one course. I think in civil engineering today here, I'm saying in Berkeley, faculty teach three courses per year—one semester two, and the other, one. I think electrical engineering—which is loaded with students, of course—I think everybody there teaches two courses every term, four courses per year—probably one undergraduate and one graduate. As I said, directing graduate students and supervising research is teaching.

Swent: Are you pressured to have more attendance at your classes?

Fuerstenau: Bean counting is really here. Today are strict minimums in courses in engineering.

Teaching Evaluation at UC Berkeley

Swent: How was your teaching evaluated by somebody, or was it evaluated?

Fuerstenau: Early on there were no evaluations. When I got on the Budget Committee, and I think towards the end of that, which was after the FSM [Free Speech Movement]—which we will talk about after a bit—teaching evaluation got to be a big item. The Budget Committee requested that there be two questions asked: the first to evaluate the professor's teaching, if I remember, spread out on ranking of one to seven; the other was to evaluate the course. These same two questions are still on every evaluation form today. Then, sometime in the seventies—and I was chairman then—an edict came out that every department should put together an evaluation form.

Swent: Who did the evaluating?

Fuerstenau: Students. Until then, everybody's a great teacher. So I asked Professor Ravitz to chair a committee to look at different evaluation forms that different universities had, and so on, and he got a fair collection of them. I remember seeing some of them asking, "Does the professor talk to you in the hallway?" Well, who gives a damn? Or, "Does he smile?" Personality, in my opinion, is

not an issue, but some of these places had such questions—and the committee came up with, and I had a lot of personal input on it, either five or seven questions about the professor and five or seven questions about the course—rating on a scale of one to seven. These were very good questions and they really zeroed in on teaching effectiveness.

Swent: Do you still have one of those?

Fuerstenau: I'll get to that. I don't think any of those still exist. I remember one time that one of our questions had something to do about homework. The student had marked it down like a one, one being at the bottom. Then he crossed that out and marked "non-applicable." There's always a "non-applicable" on all questionnaires. He rated it as non-applicable because the professor didn't assign homework. I promptly rewrote that question to assess if the professor assigned adequate homework. In engineering, working on solving problems is a major task.

While I was chairman, the only faculty person to see all these questionnaires was the chairman. Each questionnaire always had space for comment. I can remember somebody wrote in lavender ink, "If you want a Mickey Mouse approach to this subject, take this course." Then that same lavender-ink person had another very nasty comment about some other course. In a graduate course, with a handful of people, a student's handwriting could easily be recognized. So I had the departmental secretarial staff prepare a summary that was then given to each faculty member with typed comments. That is still the departmental practice today.

When writing up a case for promotions and merit increases today the actual copies of forms have to go forward. Teaching here at Berkeley is generally very, very good. It is supposed to take priority in a promotion or an appointment case.

Now what happened maybe fifteen years ago or a little more, the College of Engineering made its own form which consisted of about fifty to seventy-five questions—unbelievable. They hand these forms out to every student at the end of every term for every course in engineering. Obviously the student is going to get tired going down all these questions. The kind of thing we had was so good, I think, and got to the point. I must say, as a professor—when I started to get those long forms—I never ever looked at them. I only looked at those two campus questions and I never bothered to look at the rest. I wanted to read the comments, of course, which are reproduced.

At the end of the final class, the last fifteen minutes or so, you stop and let the students fill out these forms and somebody collects them and takes them to the department office. I know electrical engineering publishes and posts quite a detailed document about the evaluation of all the courses in that department. What I did was have a summary prepared for each professor, and then a

summary of those summaries prepared without course titles so that all the faculty could see how they stood relative to their colleagues.

Swent: Not identifying which—

Fuerstenau: Not identifying the specific course since the instructor was known.

Swent: When were you chairman?

Fuerstenau: Nineteen-seventy—for eight years. I was chairman for twice as long as anybody ever has been. I'll get into that later. But this evaluation of teaching related to formal lecturing and did not include research supervision.

Swent: The fact that you brought in students from other departments and divisions: was this a feather in your cap?

Fuerstenau: I assume that it was, but early on I did not know it. The way—and it's too bad it's not that way now—they used to count students was: a lower-division student in a class counted half in a weighting factor. An upper-division student counted one, and an MS student counted two, and a PhD student counted two and a half, or maybe three. So they had that weighting. Our department was very heavy in graduate students—the department as a whole turned out as many PhDs as a lot of the other larger departments.

Swent: What was the department at that time?

Fuerstenau: It was called "Mineral Technology" then, a lovely name. [chuckles] Here I had a couple of courses that were bringing in outside students to the department being weighted at two and a half. The department engineer at that time, Jim Moynihan, who had graduated in metallurgy from here in 1942 and just stayed working in the building and department ever since, told me that I was equal to three-and-a-half professors due to this weighting system. I was unaware of all of this. He told me that Gareth Thomas also was equal to about three-and-a-half professors. Moynihan said, "They're justifying new faculty on your input." I knew none of that at the time. There are some people that came here that were justified on my work load but I didn't know it. This method of counting is now no longer used. A graduate student now is weighted the same as a lower-division student in the bean count. The earlier weighting was in relation to the amount of effort per person required by the faculty for each level of student. In other words, when one works with a PhD student, there is a lot of one-on-one effort.

Swent: So you thought it was a valid way to go about it.

Fuerstenau: Oh yes, yes. Even a few years ago, Jud King—King, a chemical engineering professor who is now senior vice president for academic affairs statewide in the university. I know King well. He was chairman, then dean of the College

of Chemistry, and followed George Maslach as provost of the professional schools. Just three, four years ago there was something written about the King model for assessing workloads and so on. The King model was just what I've been talking about. In other words, a lot of people didn't know because they had come here later, that this was the former method of justifying loads and so on. That so-called King model was not adopted.

Basically teaching is looked on at Berkeley in engineering, the part that I know about, very seriously. We've got a lot of good teachers that do a good job. Like I say, teaching has many aspects to it; it's not just lecturing. Reagan, when he was governor, was saying that the professors aren't doing enough work; they're only teaching a few hours a week. I think in his mind a preacher only works one hour a week, right? There's a lot more to being a pastor in a church than the hour spent at the church service. That's true with us here.

Swent: A lot of counseling as well. [tape off, then on]. You received the Distinguished Teaching Award very early on.

Fuerstenau: I'll tell you something interesting—since we've been talking about teaching. Early on I was made a member of the Universitywide Academic Senate which existed in a certain realm for a few years. At one point the campus gave distinguished teaching awards, and somebody just—[phone interruption]. Berkeley had a teaching award and somebody had made a comment that the winner of the teaching award was determined by who wrote the case. There always were winners in the department of English because the cases were written by George Stewart. You know, he wrote those books called *Fire*, *Storm*, et cetera. So Stewart would write up the case and whoever he wrote up, won.

The powers that be decided that the whole thing must be rethought. So they stopped it for a year and assigned the committee to look at reorganizing the basis for the Distinguished Teaching Award to the Berkeley members of the Representative Assembly of the Statewide Academic Senate. In other words, it was people from a wide range of departments. By the way, I remember on this occasion that it was the first time that I met Mike Heyman, from [Boalt Hall School of] Law, who later became chancellor of the Berkeley campus.

So anyway, procedures were rewritten and the award restarted. That's its present form. By the way, it's now gotten so ridiculous that I've seen the cases put forward for the Distinguished Teaching Award that are a foot deep in paper and substantiation. It's crazy. I've even written to the Award Committee chair and said that the amount of documentation has gone haywire.

Still I think today it's gone more towards lecturing, although once in a while there are people who have done innovative things. I've gone to some of the teaching award presentations in Wheeler Auditorium, because they invite the previous recipients, and so on. Sometimes professors are cited for innovative

programs, and so on, but a lot of it is based on oratory in large classes. There are many aspects of teaching. Several faculty in our department have gotten Distinguished Teaching Awards, at least five. With regard to one of the early ones, somebody called up a former student of this particular professor and said, "I'm thinking of nominating this person for the Distinguished Teaching Award," and this former student said, "Are you kidding? He a great teacher?" Then Professor Searcy, the person who put the case together, said when he outlined what teaching might constitute—in other words, guiding graduate students, teaching research, other aspects of inspiring students, and all that sort of thing—the supporter agreed that this man indeed was an outstanding teacher, and he got the teaching award.

I don't consider myself a great orator, and yet I got the teaching award. But I spent a lot of time in preparing classes, and a lot of time in teaching research, and a great deal directed maybe towards inspiring other people. I already had maybe twenty of my former students who are professors around the world. There is some aspect of my teaching that inspired them to enter this career.

Swent: You did a lot of nurturing.

Fuerstenau: I think that's it. So anyway, I was a 1977 recipient. Al Bowker was the chancellor. I know that he signed the diploma. They now make a much bigger thing out of it than it was then.

I would like to add something else about broadening the education of graduate students. Through the years I had dozens and dozens of eminent visitors give seminars here, sometimes for the department but more often to graduate students in processing. Interestingly, in the first couple of years that I was here, three people gave departmental seminars: Professors Overbeek, John Elliott, and Phil deBruyn. All for some reason, came through Berkeley at that time.

In the early sixties I organized a seminar course on comminution and invited several outside speakers to present some of the lectures. In the late seventies and early eighties I organized two weekly seminar series, one on applied interfacial phenomena and later another one on applied colloid chemistry. The invited speakers were some of the most renowned surface and colloid chemists in the country during that time. This was really great for my graduate students, and many others in chemical engineering, et cetera.

Swent: I am sure that it was.

Fuerstenau: In fact, not long ago one of my former graduate students wrote that all of the distinguished visitors that I brought to Berkeley and he was able to meet and talk with was one of the highlights of his time here as a student.

Recruiting and Working with Graduate Students

- Swent: How did you go about recruiting graduate students, or did you actively try to recruit them?
- Fuerstenau: Let me say this first of all: only a very small handful of my grad students were Berkeley undergrads. I still am of the opinion that it's better to get a diverse background than every bit in one institution. My first grad students were Berkeley undergrads, however.
- Swent: Did you advise your undergraduate students then to go on to graduate school?
- Fuerstenau: Very specifically. I can name two students that I probably salvaged as undergrads from here to be grad students. A ceramics engineering major, McDonald Robinson, had been manager of the Cal Band. Now that's obviously a huge job, being manager of the band. You're looking after an awful lot of travel arrangements and scheduling, probably not about the finances. He virtually managed himself down towards a C average whereas he was probably a very good student. So I got him admitted as a master's student and he got a master's degree in ceramics but he did his thesis directly with me. Then he wanted to go on to do a PhD and I said, "I wouldn't stay here." I think he was probably six years minimum getting a BS because of that band activity. Then the minimum of a year, at least, for the master's. I said, "I think you ought to go elsewhere." He went to MIT. Joe Pask, who was the professor of ceramics, was pretty mad at me for suggesting that he go elsewhere for a PhD. By the way, the three of us have a very good paper published in the *Journal of the American Ceramic Society* based on his MS degree research.

[Tape 20, Side A]

- Swent: You had said that you had a couple of students that you salvaged.
- Fuerstenau: Yes, but you can see my concept of particle processing beginning to fit into ceramics. The second student was George Onoda, who did his BS here in metallurgy. He was from Sacramento, of Japanese heritage, a very personable, bright young guy. I think as a junior he just dropped taking one of the metallurgy courses without officially dropping it. It ended up that he then got an F, and an F in those days carried on in his GPA [grade-point average] even though he took the course again and got an A the following year. But in those days, the F was counted in his average recorded on his transcript. So George had something like a 2.78 GPA, as I remember. I got him admitted as a grad student and he did a very nice MS thesis with me, one that made a superb paper on quartz flotation presented at the Seventh International Mineral Processing Congress in New York.

Then he worked for General Atomics for about two years. He came and said that he wanted to come back and do a PhD and I said, "George, I really advise

you to go somewhere else, because you've had all these years in Berkeley, and why not broaden yourself?" So Onoda applied—this was related to mineral processing—to Penn State, Columbia, and MIT. I remember him saying that from one of them he never did get an answer, which meant his application probably got lost. But he was admitted to MIT and he did his thesis with Professor deBruyn whom we had talked about earlier.

George and his wife stopped by to see us in Berkeley at our house before he departed to go to MIT. I said, "George, you know you're a little prone to not work too hard, et cetera, and I really suggest when you get to MIT you work like a dog with real dedication." Anyway, as I told you, George had about a 2.78, or something, a 2.8 GPA here as an undergrad. In two years he got his doctor's degree from MIT. He's gone on into being a major figure in ceramic processing. For a long time he was a ceramics professor at the University of Florida, and then he went to IBM in their fundamental research group.

By the way, one time he told me—I think when he was with IBM—that he went to Japan. He doesn't speak much more Japanese than I do. I guess they were all taken aback because he needed an interpreter with him the whole time.

Then another young person who had taken my first mineral processing course was Danny Sullivan, who came from Auburn, [California]. He stayed and did a nice master's with me. For some reason he had a real interest in German, the language. He went to Washington DC; he thought he'd just like to go east. He earned a living translating German. I don't know where he picked up facility in German because his name is Sullivan. [laughs]. Through his translation activities in Washington, he met his wife who came from Germany. Her family had come from Breslau. Breslau was one of the German cities that they had to evacuate when the Polish border was moved west in 1945. It's now Wroclaw, Poland. She told me that she was three or five years old when they had to move west in 1945.

What I find interesting is in either three years or four years of night school he got a law degree from George Washington University.

Swent: After getting a master's here in mineral processing.

Fuerstenau: And a BS in metallurgy. I think in only three or four years of night school he got a law degree from George Washington University and he became a patent attorney. His whole career was then at Alcoa in Pittsburgh as a patent attorney. Obviously with his technical background, he probably was very good. I've seen him in Pittsburgh a couple of times.

Actually only two persons who were undergraduates at Berkeley stayed on and completed master's and doctor's degrees with me. The first was Dave Cahn, who received his BS degree here in mining. He was a very capable

person who did his MS and his doctor of engineering theses with me—both were excellent studies of mixing of solid particles. I recall Professor Witherspoon saying that Cahn's thesis should have been a PhD thesis and not a DEng thesis.

Swent: What is the difference?

Fuerstenau: Here at Berkeley, a doctor of engineering program is supposed to involve some emphasis on business and the thesis is to be more oriented towards engineering design. I believe that a total of four of my students opted for the doctor of engineering degree over the doctor of philosophy degree. Dave Cahn worked a few years in the research laboratories of Bethlehem Steel Corporation in Lehigh, Pennsylvania, but he and his wife wanted to return to California. The last time that I saw him, he was at Calmat as environmental vice president down in Los Angeles. A couple of years ago, he was president of the California Mining Association.

Some years later, another undergraduate student remained for both his MS and PhD degrees here. This was Kwadwo Osseo-Asare, who had come here from Ghana with a special scholarship. He completed his junior and senior years here at Berkeley in our department and did his master's with me, stayed on, and then wanted to do a PhD. He was interested in the field of hydrometallurgy, leaching. For his PhD he conducted a very innovative piece of research on applying interfacial phenomena, surface phenomena, in leaching. He studied losses of dissolved nickel and cobalt in ammonia leaching that can result from adsorption of dissolved species on leach residues. To this day, Osseo is recognized worldwide as an expert in interfaces related to hydrometallurgy and now is editor-in-chief of the journal, *Hydrometallurgy*.

I'll just digress a little bit, since we're talking about him. He went to Amax in Denver, to their research lab, working on nickel-cobalt leaching and separations. He always wanted to return to Ghana to teach. One day Frank Aplan, whom you've interviewed, called me and said, "I'm looking for a professor in the field of mineral processing practice." Frank was then head of the Department of Metallurgy at Penn State sometime in the early eighties. He said he was looking for a practical guy. I said, "Hey, Frank, you don't need a practical guy. What you want is Osseo-Asare." I said, "He really is a black man, because he comes from Ghana."

Frank said, "Well, we're not under any recruiting diversity drive—" something to that effect. Then I said, "But you want this guy. He's really sharp—creative." Then I called up Osseo and said, "You know, there's a job at Penn State that I think you—" and he replied, "No, I really want to stay here at Amax."

I said, “Well, if you’re going to go back to Ghana to a university you ought to learn something about how universities operate.” I always chuckle that I had to convince both parties of the opportunities. Eventually Osseo, when they offered him a position, went to Penn State. In fact, Osseo is now head of the metallurgy section at Penn State and is probably—I would say—the foremost person in hydrometallurgy of his vintage worldwide. He’s creative, bright, and just an exceptionally nice guy. Very innovative.

His wife was an undergraduate here. Before they got married he said, “If we’re going to get married I want you to know what it’s like to live in Ghana,” and he had her go there and live with his parents maybe for a summer—he wasn’t there—so she would know what her life would be.

Swent: Was she black also?

Fuerstenau: No, no. She was white, from Oregon I believe. Fran is really a lovely person. They got married, and in fact I think she later got a PhD from Penn State. I think she’s even written cookbooks on African cooking that have been published. Their three offspring have all graduated from Harvard. In fact, they were summa cum laude. Osseo was a very, very bright person, and obviously Fran is very intelligent. They passed this intelligence on to their offspring.

Swent: He never went back then to Ghana?

Fuerstenau: Only for sabbaticals, but he did not move back there. Of course now his own family is here and not long ago he finally became a U.S. citizen. A beautiful story. He’s been active in economic development exchange programs with Africa in recent years.

[Added by Douglas Fuerstenau during editing: Osseo’s daughter received her PhD in history at Harvard and after a year of postdoctoral research is coming to Berkeley as assistant professor in history. In 2004, Osseo was elected to the National Academy of Engineering.]

Those are the only ones, I think, that basically were undergrads here. I, myself, have tried to push people to expand their horizons—because I worked for degrees at three places and they all had left their mark on me. But anyway, the main aspect of my career has been oriented towards working with graduate students and graduate student research.

Swent: And how did you get them?

Fuerstenau: Well, first of all, I published a lot of research papers and these would have shown that there was an active graduate program going on here in the University of California at Berkeley. I suppose as one’s reputation grew some of them came out of the woodwork and wrote to me. As for others, I used to say for years I maybe recruited one student per year by going to the annual

AIME meeting almost regularly, either from conversations with a professor or once in a while from young engineers that might have been working a year or two or three at a company and said—“Well, I’m thinking of getting a master’s or a PhD.” I used to comment that I would maybe get one new student a year from that. A few came from Butte. I’ve had a few from there and from South Dakota, where the department head might send one here. When my brother was department head back there for about eighteen years, there were quite a number of metallurgy students who did PhDs here at Berkeley—a few with me, but in other areas of metallurgy too.

Swent: This is from South Dakota?

Fuerstenau: Yes, my brother Maurie was the head of the Department of Metallurgical Engineering at the South Dakota School of Mines and was suggesting to the best students to come here for graduate work in physical metallurgy, process metallurgy, or mineral processing, depending on their interests.

Swent: You had a number who came from overseas.

Fuerstenau: Oh yes. After my second year here at Berkeley, a fairly large group of graduate students just came all at once—the fall of 1961. One of the persons who’s become a big giant in the field was somebody I didn’t even know had applied. His name was Ponisseril Somasundaran. Soma, who had come from south India, Kerala, came into my office and introduced himself. South Indians talk at about the rate of ninety miles an hour. He just kept saying, [sound of somebody talking rapidly] “My name’s Soma.” And I said, “What did you say?” It was a ninety-miles-an-hour Soma, and I said, “You’re my TA [teaching assistant].” [laughter] I assigned him as my TA to my undergraduate mineral processing lab, which I guess is how I got him recruited into the mineral processing because I think he was interested in at that time the chemical metallurgy. Soma did his master’s and PhD with me, taking only three calendar years for both degrees. He was part of that first group that was the beginning of research at a big scale.

Swent: You’ve stayed rather close to him ever since.

Fuerstenau: Oh yes, I talk to Soma practically to this day every other week or so on the telephone. Either he calls me or I call him.

Swent: Where is he now?

Fuerstenau: He’s a professor at Columbia University in the Henry Krumb School of Mines. He was my first student to later be elected to the National Academy of Engineering.

The Objectives of a Graduate Education

Fuerstenau: Let me back up again about teaching, because he's a prime example of that. I decided that every graduate student should leave here with a good bag of tools. The College of Engineering requires two minors and a major. MIT required only a minor; you didn't even need a major. You had to pass exams so you took courses you felt you needed for that purpose. In fact, chemistry here requires no minor, no major, for the PhD but still that doesn't mean that there's not a lot of courses. I think there's no formal requirement other than the exams and a thesis for the PhD. In other words, their formal requirements are like the old PhD around the world. In chemistry Berkeley ranks virtually first in the world and some of the best chemistry PhDs in the world come out of here. So having or not having formal requirements for graduate degrees is an interesting phenomenon.

Here, in engineering, we require two minors. As I said, I always figured that I want my students to leave here with a good bag of tools, and those who are more science-oriented have to have a very thorough foundation in thermodynamics, whether it be in chemistry or chemical engineering or soils. I had all of my students take their other minor, I mean almost all of them, in chemical engineering. This would be a mix of graduate courses and maybe an undergraduate course in chemical engineering. Or, if they had a more engineering orientation I would have them take the ChemE minor, and maybe the second minor in automatic control, because in processing plants you want to devise control systems. Those courses were given in [the Department of] Mechanical Engineering. Or some who were more interested in modeling simulation, this kind of thing, might have taken a minor in applied math or statistics. In other words, the students went out of here with a solid background in these areas. I always made them take an applied math course just as an add-on.

Typical departmental programs for my master's and majors for my doctoral grad students included not only the applied interfacial phenomena courses but also courses on particle technology, process modeling, transport phenomena, hydrometallurgy, and ceramic processing, amongst others.

Maybe starting about 1970, I decided to make every one of them take a course in English writing, whether they were American or otherwise. The first student to do that, Ken Han, went down and took a course in English. English 1A wasn't the right course. Then I found out that Rhetoric 1A was pretty good because it made them write and speak. After that, two courses on technical writing were organized in Civil Engineering or in Engineering. They still give them in large numbers. So I insisted on my graduate students taking the writing courses, as I said, whether they're American or foreign.

Swent: How did they feel about it?

Fuerstenau: In the end they all feel good. I can see, I had one Indian grad student later on, named K.S. Venkataraman. After he took that course, I could see a complete change in Venkat in that he started writing with real style. Venkat got both his MS and PhD degrees with me, and for many years he's been a research engineer with Alcoa. In Pittsburgh he has started a quarterly magazine for Indians, and he sends me copies regularly. I'll show you one for interest. He gets ads from Mercedes, jewelry stores, BMW, so he obviously has well-off readers. He told me that he spends most of his hours outside of work on this endeavor. This is a prime example of turning somebody into a writer—even through a course in technical writing.

Anyway, no one advised me as a grad student to acquire what I call a bag of tools that I could have used, but I have always tried to do this with my students, and I think that's why so many have become so successful. Maybe Professor Overbeek's lectures on surface chemistry during my last student year at MIT served this purpose.

Swent: These are the things that you yourself have found were important.

Fuerstenau: Oh sure. Oh yes. And you know yourself how important communication is. Years ago I told students that I was going to quit editing theses. But with two of the most recent PhDs, I had to work over their theses almost sentence by sentence, and I did it with red ink to make an impression on them. And you know, I looked later at their transcripts and those two students hadn't taken that writing course, and I hadn't known it. They probably thought they'd outfoxed me, but they're the ones who suffer in the long run of course.

Speaking of where the students come from, quite a few of my students had undergraduate degrees in metallurgy back thirty years ago, thirty-five years ago. Quite a few had undergraduate degrees in mining and then changed into mineral processing because they would have had a course in mineral processing in their program. Quite a few had degrees in chemical engineering. Two or three had ceramics degrees, and some had degrees in chemistry. One in mathematics. You can see that my grad students may have started with a wide range of undergraduate backgrounds, but I think that they all left here well and broadly educated when they graduated. Not only were good courses available in this department, but the professors in most of the courses that they took for their minors were international leaders in their field.

Meeting the Environmental Water Chemistry Leaders

Swent: Of course this whole environmental thing hadn't come in until 1970.

Fuerstenau: You're right, in the past two decades or so environmental concern is a major part of the mineral field. However, even going back to the early sixties, I became well acquainted with the people who really were the big men in water

chemistry and water environment. The kinds of things I did related to the surface chemistry of oxides in water—the behavior of oxide minerals in water relates very much to the research of water-pollution people.

Maybe digressing a little bit—I think it was about '64 that I went to what was called a Rudolphs Conference on Water—environmental—how you treat wastewater and clean up water and this sort of thing. One of the great colloid chemists who had retired from Columbia named Victor LaMer was there. This conference was held at Rutgers in a great big auditorium, maybe five hundred people or so in this room. LaMer several times got up and said negative things about the concept of zeta potentials, the very thing I had worked on in detail for my doctorate, found very useful, and so on. Finally, about the second or third day, I stood up and I said, “I don’t think you should let Professor LaMer lead you down the path, because there are many instances where we know exactly what zeta potentials are. You know what they mean when they are zero and what it takes to make them zero and what happens when it’s this way, and so on.”

Apparently I put old Victor LaMer into orbit. That evening there was a great big outdoor steak barbecue in a big tent and tables with white paper on them. Somebody that I’m going to talk to you shortly about is Tom Healy, who was now with me and had been LaMer’s next-to-last PhD at Columbia. So Healy and I had gone to this meeting together. After the dinner—apparently LaMer had been fuming the whole time—he cornered first Healy and was giving him an examination. By that I mean the paper tablecloth was full of diagrams and equations. I walked up and then LaMer started on me. By that I mean grilling me, asking, “What’s this? Show me this. What’s this?” Finally he said, “Fuerstenau, you know your physics but you don’t know your semantics.”
[laughter]

At this meeting I met and later got to know well several of the major researchers in the water environmental field. One person was Werner Stumm who was a distinguished professor at Harvard, really the giant of water chemistry. Stumm, who was originally Swiss, returned later to head a big water institute at ETH in Zurich. You know, you can keep both your American citizenship and Swiss citizenship and, for example, he became a member of the National Academy of Engineering even after he went back to Switzerland. I visited there one time. He invited me to give a seminar, spent the day with me. One of his Harvard graduate students, Jim Morgan, had just finished a PhD with him working on adsorption phenomena on manganese oxides, an area that we had published papers on because of my interest in manganese nodules from the sea floor. Stumm and Morgan later wrote a classic text on environmental water chemistry. Morgan’s whole career has been at Caltech in environmental engineering. He’s one of the major figures in environmental engineering, was vice president of Caltech—

[Tape 20, Side B]

- Fuerstenau: Another person who I'd met before but didn't really get to know personally until that meeting was Egon Matijevic, who was a very outstanding applied colloid chemist at Clarkson, originally from Yugoslavia. A huge giant in colloid chemistry and in water treatment at that time. Clarkson University of Technology is in Potsdam, New York. There's a Yugoslav enclave up there of outstanding people. I got to know these water people at that meeting, and know them well, and have kept contact through the years. It's been part of my life that I'd like to talk about eventually, some of all these people in other fields that I've known. My interaction with water treatment researchers resulted because I worked on principles. What I did these people could use, and did. Today of current interest is cleaning up mine tailings and so on, and that's become a big thing.
- Swent: But the same principles still apply.
- Fuerstenau: Oh yes, oh sure. In fact, that relates very closely to hydrometallurgy: how does a sulfide mineral in a tailings dump oxidize? As you know, in northern California is one of the major mine pollution problems from what used to be Mountain Copper. Did you ever know Bill McClung?
- Swent: I met him, yes.
- Fuerstenau: He was president of Mountain Copper. I did some consulting for them. It's that mine that leaches iron and copper due to the oxidation of pyrite and copper minerals.
- Swent: That's become a huge cause.
- Fuerstenau: Fiona Doyle, who's the hydrometallurgy professor here, has worked a lot on it. I don't know if she's doing anything now with it, but she was involved with a major study on that a handful of years ago. I myself never got into that area; if I were twenty years younger I might be right in the thick of it. But I did deal a lot with water and the adsorption of contaminants, the type of water chemistry involving colloids in water.
- Swent: Ken Han: how did you get him?
- Fuerstenau: Ken Han, who graduated maybe in mining or metallurgy from Seoul National University, had gone to the University of Illinois where he got a master's degree in mineral processing working with Professor Norman Street. Street had a background in applied surface chemistry and had done some excellent work on relating the hardness of solids to their double layer properties. Street decided to go back to Australia, possibly for medical reasons, and wrote to me and said, "I'm going to return to Australia and I have this good young person who has just finished a master's degree, and I recommend that he come to Berkeley and do his PhD with you." So that's how Ken Han came to study

with me. That's a good example of a person coming from another institution and highly recommended by his professor.

Another example involved VPI [Virginia Polytechnic Institute] where an associate professor, Bill Foreman, wrote me and said, "I have a good student who's doing a master's degree with us and he ought to do a PhD." This was David Yang who had a mining undergraduate degree from Taiwan. David Yang conducted a thesis on comminution here and has supported himself over the last decades through processing inventions and equipment design. He has come up with a number of innovations.

Ken Han, as you know, is a very special person who has done very well. You've had the opportunity to meet him.

Swent: He's now at South Dakota [School of Mines and Technology].

Fuerstenau: South Dakota, yes. After a bit, when I talk about research, we'll get into Ken Han's research because he was very important. From Berkeley, Ken went to Australia and joined the faculty at Monash University in Clayton, near Melbourne, for about ten years. He was in the hydrometallurgy, process metallurgy, part of the Chemical Engineering Department at Monash. I'm assuming that he felt, after ten years, that he was probably at his height there. You know the Australians follow pretty much the British system of a professor and an associate professor and then lecturers and senior lecturers. He was probably a senior lecturer when he decided to come back to this country. My brother Maurie was there at South Dakota and the next thing I knew, Maurie had hired Ken Han to come to South Dakota as a professor in the Department of Metallurgical Engineering. I never entered into the process at all. Han has been there ever since.

In Berkeley, he was Kook Nam Han. Street told me that Kook Nam means Korea man—I mean a direct translation—Korea Man Han. He changed his name to Kenneth, so he's now Kenneth N. Han. They have three offspring: two daughters and a son, and of course they were born in Australia. Backing up a bit, Ken was a real prize and was a very dedicated worker—still is. When he was a graduate student he came to me one Friday and said, "I'd like to go away this weekend."

I said, "Well, why?"

He said, "Well, I'm getting married."

I said, "Why don't you take next week off?"

And Ken, "What for?" [laughter]

Swent: He really was a worker.

Fuerstenau: Oh yeah, right on. Having students like that—and there were lots of them through the years—of course makes this profession have lots of pleasantries and pleasant memories and so on.

Swent: That's the gratification, isn't it?

Fuerstenau: Oh yes. The personal interaction that one has with students and then seeing how they've progressed in later life is the reward of all the work we were talking about.

[Added by Douglas Fuerstenau during editing: Ken Han retired as professor and graduate dean at the South Dakota School of Mines and Technology in December 2006, and has taken a two-year assignment at Seokyeong University, a private university in Seoul, to teach and lead a research group. We visited him in March 2007 when I attended the Japan/Korea Symposium on Recycling and Materials Science as an invited speaker. Ken told us about an early event in his life. The family lived in North Korea, and in 1945 his father went to South Korea to find a way to improve the family's life. Shortly after, the border was closed and he and his mother and brother could not cross to South Korea. In 1947, when Ken was eight or nine years old, there finally was enough money to pay for a group to be guided at night across the border. Ken said that sometime in that dark night, they ran into some patrolling soldiers and everyone scattered. He got separated from the group. When he realized that he was lost, he said that he started to cry and cried all night. In the morning he stumbled upon some North Korean soldiers, and after he told them what had happened, they pointed which direction was south. He continued south, crying all the time, came to a stream where some women were washing clothes, and they told him to follow the stream on down because there is a woman looking for a little boy. Ken said that when he found his mother, he simply totally collapsed. The world is indeed fortunate that Ken Han found his way into South Korea.]

More on MS, PhD, and Postdoctoral Students

[Interview 10: September 18, 2001]

[Tape 21, Side A]

Swent: We were talking yesterday about your graduate students. I was just looking at the list through many years. They came from all over the world. The first one was 1955, at MIT; 1962 was your first one here; up to 1999, a total of sixty-four master's and fifty-nine PhDs or doctors of engineering or science.

Where did they come from? How did you get them?

Fuerstenau: We talked a little bit last time about that. Many of them wrote to me, but a fair number were urged to come here by former students. For example, one of my PhDs, Srini Raghavan, is now professor and has been for several years at University of Arizona, and he said, "I have this very bright person in my class, and I really recommend to him that he come and work with you." This was Jim [J.S.] Hanson, who did both an MS and PhD with me, a decade or more ago now, metallurgy graduate from Arizona. He told me that he had one two-unit B, and everything else was A's. That happened because he took a final when he had the flu.

Another outstanding student was John Herbst, who was an undergrad in chemical engineering at Northwestern, and he was working for International Minerals on a work-study kind of program, one semester working and one semester at the university. My first PhD, Somasundaran, worked there for two or three years, in the research laboratories of International Minerals, and he had urged John Herbst to come here to Berkeley.

At the same time, someone who had done a PhD at Stanford with George Parks and was also working at International Minerals was trying to convince Herbst that he ought to go to Stanford. Fortunately, Soma must have been more persuasive. That's just an indication of where people came from.

Swent: What sorts of topics were they working on? What kind of research were they doing?

Fuerstenau: Well, in the university, in technical areas—as you may know—the research that faculty do is generally carried out through the graduate students, and so all the research, let's say, that I did during the last forty years was mostly conducted for MS theses or PhD dissertations or, in some cases by postdoctoral researchers, of which I've had a fair number here.

In this department, basically everybody did a master's degree on the way to a PhD. In the College of Engineering, there are two programs: one is with thesis

and one without thesis. The one without thesis has a few more units and maybe a technical project and an examination, whereas with thesis it's so many units of courses plus the thesis. And so all master's that I have, had a thesis, which meant it was always tied to research because the research assistantship was paid for through some grant or contract. This was because, in general, all of my students had to be financially supported—I don't think I had any that paid their way. In fact, I don't think there were any in this department that paid their way in those early days.

Swent: Did you have to help them get their support?

Fuerstenau: Oh yes. Well, I got the support because I had research grants already, but some of the support came from fellowships, for example. Then again, once in a while, someone may have come with a fellowship, like an NSF, National Science Foundation fellowship. Occasionally a student was supported by his company. That was rather rare, and in fact, one of the most recent ones was a young man from Mexico named Juan Vásquez-Favela, who did a master's degree here, very able. He works for CEMEX, the huge Mexican cement company that's actually buying up plants all over the world. In fact, I think I read that they may have bought the South Dakota "socialist" cement plant in Rapid City—the state-owned cement plant in Rapid City. Governor Janklow wanted to sell that, and I think CEMEX bought it, just in the last year or so. But anyway, his company from Mexico sent him here, and he's now risen in just a handful of years quite rapidly through their hierarchy. He did a very nice thesis on grinding for his MS thesis. Of course, one of the major things in the cement industry is grinding because you've got to grind cement rock first, and then you've got to grind the cement clinker. He did a beautiful job on his research.

Anyway, these are just some examples of how a few of my graduate students came to Berkeley for their graduate studies.

IX RESEARCH PROJECTS

Fuerstenau: The research projects that we worked on over the years were conducted with my graduate students and postdoctoral researchers. The research results were the basis of the MS and PhD theses of the students. As we will discuss, my research mainly went in two broad directions—one, the engineering aspects of mineral and particulate materials processing, and two, the interfacial behavior of minerals and materials in aqueous systems, often related to flotation. Actually, colloid or surface science and interfacial phenomena play a crucial role in many of the complex processes that one routinely encounters in industrial mineral processing, not only flotation. So my general objective was to work on projects that delineated the fundamental principles underlying the various operations involved in mineral processing. As a result, we always worked with pure minerals or artificial mixtures of pure minerals, and with pure chemical reagents. Of course, exceptions to this were our various projects related to processing manganese nodules, cleaning coal, and pelletizing fly ash. On quite a few occasions, I wrote patent disclosures, but only followed up on these a few times. Unfortunately, I held back on the results of a number of research findings, thinking that they might be patentable, and papers were never written for publication.

I remember at one local San Francisco AIME meeting, someone, and I don't recall who that was, criticized me for publishing fundamental papers and not papers on what he thought were practical problems. I told him that our work filled a need, and the real problem with his criticism was the shortcomings of the many others out there who could and should fill the practical gap but were not doing so. My objective was to delineate the basic engineering science underlying mineral processing in order to provide the means for improving existing processes and possibly leading to new approaches and processes. Orienting my research towards study of scientific and engineering fundamentals came from my association with Professor Gaudin at MIT, and even starting with my research under Professor McGlashan at Montana School of Mines.

[Added by Douglas Fuerstenau during editing: Attached in the appendix is a list of my publications, from which you can see the range and specific topics that we worked on over the years.]

Research in Comminution, Ball Mill Grinding

Swent: One of the things you are credited with is developing new techniques to examine comminution. Was that the outgrowth of work with graduate students?

Fuerstenau: Oh sure. It turns out that when I got here that there was virtually no equipment or anything, and one of the simplest things to do—which required an idea and not a fancy machine—was to study grinding, ball milling. That could be done in just a simple laboratory ball mill, and so I think two or three of the first master's theses were simply on grinding. We had the idea of looking at kinetics: that's the rate at which particles are ground in ball mills. I also had the idea to look at grinding mixtures of minerals in ball mills and rod mills. One needed no new equipment; this is just sort of standard apparatus. But it was all very good work; it all got published. Maybe it should be pointed out that because the crushing and grinding of ores controls both recovery and grade of flotation concentrates, for example, and about 70 percent of operating costs in mineral processing plants is for comminution. That's why comminution research is so important.

As I mentioned earlier, Dick Charles at MIT had sparked in me an early interest in comminution. I decided to make a mill with the same dimensions as the one that Dick Charles had built at MIT, but designed it so that the torque to run the mill could accurately be measured, and had the departmental shop build the mill. The first research done with this mill was carried out by Louis Berlioz for his MS thesis. Berlioz was an engineer in his fifties, working for Bechtel. For a period of two or three years, he came over to Berkeley and took courses for his MS degree and carried out a beautiful piece of experimental work on ball mill grinding. What astonished me after he had been here for some time was he told me that he had to make up the time at Bechtel that he spent here in Berkeley taking a class. The experimental results that he obtained were analyzed using the approach of Dick Charles, but in our case we had actual grinding energies and not simply grinding time. His data were used later, not only by our group but by others, as we moved into mathematical modeling of comminution.

As we had discussed, my philosophy towards research has always been to pursue a given research area in depth and stay with a given topic. Comminution has remained as one of my research areas right from the very beginning and is still here. It turns out in about the middle sixties there came a big change in comminution research, at least in the academics, which was to do mathematical modeling of grinding. We got right into the middle of this.

Swent: What stimulated this change?

Fuerstenau: Computers, and I suppose people having a much more thorough background in math than before, and I think in the field of chemical engineering, a lot of modeling had been undertaken for various chemical operations. Modeling, in the world of grinding, followed from that. I think the research engineer types wanted to look more phenomenologically at: what is going on? You know, it's not just a ball mill that "grinds up that there ore." It was: what is going on inside a ball mill that determines grinding rates and product size distributions?

Swent: More scientific.

Fuerstenau: Oh yes, much more. The persons that I give great credit to for initiating that in this country were Robin Gardner and Len Austin at Penn State, who published a seminal paper in 1962 that sparked much interest. There were others who had worked on this; Bass in Germany, published in German in the early fifties, but you might call it an unknown paper for a decade. Austin and his grad student Gardner probably spearheaded the interest, starting with the concepts published by Bass. In 1960, Gaudin and Tom Meloy published an independent similar approach but no one paid much attention, if any, to their work. After that, in Australia at CSIRO, Denis Kelsall and Ken Reid initiated a major program on grinding kinetics and were also part of the driving force towards bringing mathematical modeling into grinding. In the middle sixties our efforts moved toward that direction and aimed at delineating the principles or determining the parameters needed for these models. The approach used is called a population balance model [PBM] which accounts for the rate at which all coarse particles inside a ball mill, for example, are broken and the rate at which fine particles are produced when the coarser particles are broken. From this, we can simulate the product produced in a grinding mill if parameters in the model have been evaluated. Over several years, determining how such operating variables as feed load in the mill, feed size, ball load, ball size, mill speed, mill diameter, and so forth affect model parameters served as the basis of the theses of several graduate students. All of this was carried out with pure, carefully sized mineral feed samples. I always wanted to work with pure minerals and not just rocks.

My first student who started to work on modeling comminution was Tom Mika, using Berlioz's results. Mika initiated our first efforts in that direction. Others in that early era were John Herbst, Garth Gumtz, and George Grandy. The student that did, perhaps, the most work on that was John Herbst, for both his master's and doctor's theses here. I've often told people that I think John Herbst's master's thesis would have been an excellent PhD thesis. When he finished it, I said, "Hey, John, do you want to waste this on a master's?" He decided he would turn it in and get his master's degree. It took him quite a while longer to do a doctor's degree. Herbst had used our torque ball mill for his grinding kinetic studies and I had suggested that he also record the torque so that we would know the energy involved. John found that he could incorporate grinding energy instead of time into the model relations, and I consider that one of the major steps forward in comminution research. Herbst also showed how the Gaudin-Schuhmann size distribution modulus enters into the comminution model parameters. Later John Herbst, George Grandy, and Subhas Malghan all contributed to establishing a procedure for scaling-up ball mill grinding using the specific energy data obtained from laboratory-size ball mills. With our approach to relating model parameters to the feed material, it is possible to scale-up small ball mill batch grinding tests to full-scale industrial ball mills. After incorporating a model for transport within

continuous mills together with a model for cyclone classification, you can scale-up to a continuous system. Very nice.

Herbst had a family and went to work at Kennecott for a while to support them, and while he was at Kennecott he built a grinding mill like we had here so that he could do the work at Kennecott in Salt Lake; whose results were what he incorporated into his doctoral thesis later at Berkeley.

Swent: Kennecott had a research facility.

Fuerstenau: Oh yes, a large facility then. Herbst worked there a couple of years and then came back for maybe a year, finished everything here. As I said, he had two sons at that point, and he needed to live a little better than he could as a research assistant, but he made good use of that time. Herbst has had an outstanding academic and industrial career applying models to the design and the control of mineral processing operations, including comminution and flotation systems.

[Added by Douglas Fuerstenau during editing: John Herbst wrote recently: “You have known and worked with many of the greats and through you I have had a chance to ‘hang out’ with many of these folks. By the way, my energy-based modeling and the use of specific selection function that started with you at Berkeley—has turned out to be the cornerstone for PBM and DEM [discrete element method] simulation modeling, optimization, and scale-up! You and Peter King are only ones who recognized it at the time.”]

Some years later, our comminution research moved in the direction of roll milling and energy savings and we will talk about that later on.

Engineering Foundation: Establishes Research Conferences

Swent: You organized a conference, one of the very first Engineering Research Foundation conferences, on comminution in 1963.

Fuerstenau: Yes. It turns out that Professor Gaudin for several years in the early sixties was chairman of the Engineering Foundation, and during that time, the Engineering Foundation did two things: one was to establish a program of Engineering Foundation research conferences, which has been a major activity for the last forty years. I think that they’re talking about eliminating them or altering their support because some of the board is concerned about insurance and suits. The other achievement that happened during Gaudin’s chairing the Engineering Foundation was, he told me, that for two years they took all their funds to study establishing a National Academy of Engineering [NAE]. In 1964, there were twenty-five founding members of NAE, of which Gaudin was one, for obvious reasons.

In '62 they wanted to have four such conferences, and Gaudin proposed that I chair one on comminution and thickening. These were the two research areas he was working on at the time. There's no relation between comminution and thickening, but I decided to go ahead with one on comminution alone. I think things didn't get started earlier enough, and so I said, "Well, I'll postpone it till '63."

Well, that summer, 1963, there were four conferences that were held at [Phillips] Andover Academy, at Andover, Mass. Anyway, I had been out of the country and when I came back to Berkeley, I was given the bad news about how things were going. At that point I had a grad student named Andy Mular, who was a more senior grad student who never finished his program here, but who was acting as sort of an assistant, right-hand soul to me. He came from Butte; a very good man. He should have finished his degree but got the urge to get out and get to work before somebody beat him in writing a book on electrostatic separation that he never wrote. Anyway, Andy told me there are seven people who applied to attend this conference. Now, I got back here in the middle of July, and I think the conference was in August.

So I kind of broke out in a cold sweat, and I got on the telephone and started to call people and write to them. When the conference was held, we had about seventy persons who came, and for many years that was the largest attendance they had at any of these Engineering Foundation conferences until they got onto social-political technical topics. The aim was to have 100, or 125 max. But only seven people! Oh boy! I'll tell you! I broke out in a cold sweat. [laughs]

In the end, the conference was very good. We had funds to invite one senior foreign person; I invited Horace Rose, a professor from King's College University of London, who had written a couple of books and many papers on comminution, a very active person in mechanical engineering. He was our main invited speaker. Some other people came from England who were grinding coal, things like that. It turned out to be a very successful conference. In the next couple of years, we broadened the name to Conference on Particulate Systems.

Then the next two were in Milwaukee. I remember we went to Milwaukee on the Sunday during the time that they had major race riots in Milwaukee, and the place was being patrolled by the National Guard. [laughs] Boy, what an introduction! Milwaukee even had curfews during that week. These were good conferences with a wider participation. In Milwaukee there were two different conferences, and that gave Americans a chance to meet the outstanding researchers from other countries, such as Hans Rumpf, Fritz Loeffler and Klaus Schoenert from Germany, Denis Kelsall and Alban Lynch from Australia, K. Tanaka from Japan, and so on. The way we ran the conferences then was that there were maybe two or three speakers only per session, with plenty of opportunity for discussion—that was the main object.

Then we arranged what we called quickies—short five-minute presentations, very good ones, where people had a few minutes to get up and present some of their own research or work. Then the afternoons were free for relaxation, discussion. Then there was an evening session for a couple of hours after dinner. That's the way these week-long conferences were organized—patterned after Gordon Conferences in science.

An outgrowth of the Engineering Foundation activities was a program of research initiation and for two or three years, I had a research grant from the Engineering Foundation that supported my research in comminution. But before that, I started some work on flotation and flotation surface chemistry. For example, I had suggested to Somasundaran that he work on surfactant adsorption. These are organic reagents, like soaps. In his case, he was working with amines, cations, on quartz, and I told him, "Hey, this work looks too good to be used for an MS—why don't you put it aside and save it for a PhD thesis?" So he returned to some work on comminution that became the basis of his MS thesis and also led to my first trip to Europe.

Sixth International Mineral Processing Conference, Cannes, 1963

Fuerstenau: In 1963 was the first International Mineral Processing Congress that I went to in Cannes. I had proposed a paper on grinding mixtures of minerals for the Congress. I must have sent an abstract in for that. I still didn't have it done, so I told Soma, "Okay, make this mixture grinding your MS thesis." I think all of the work required for that—I even dug in and helped because it had to get done in three or four weeks. That was the time that was used for getting the data that became Soma's MS, and the basis for our paper.

Swent: Did you have a physical plant, a laboratory, here, where you were actually mixing and grinding?

Fuerstenau: Oh yes. It was not a pilot plant. An 8-inch or 9-inch mill, very small laboratory-scale ball mill and rod mill. I already had set up the facilities for doing that, at really nominal cost. We were grinding quartz, which is very hard, and limestone, which is quite soft, as mixtures. Since you can dissolve limestone with hydrochloric acid, we could screen the ground product, dissolve away the limestone, then re-screen the dried residue which gave us the size distribution of the quartz since the limestone was extremely pure calcium carbonate. One then can simply back-calculate the size distribution of the limestone particles. This way we could follow the size distribution of both minerals. It was a very nice piece of work.

That was the basis of the paper that I gave at this congress, the Sixth Mineral Processing Congress, and that's where I was just prior to learning about what was feared to be a calamity with the Engineering Foundation Conference.

Other than going down to Brazil, this was my first trip overseas. Our son was just two or three weeks from being born, so Peg didn't go with me.

So I went to Cannes, where the meeting was, which was very much like California, the hills of Berkeley, the hills of Piedmont. I met a lot of people there.

[Tape 21, Side B]

Fuerstenau: For example, a young Frenchman, who's probably a little younger than I am—Jean Cases, who just retired about a year ago as professor and director of a major mineral processing surface chemistry laboratory in Nancy in France—was just preparing his doctoral thesis. His research was related very closely to what I had done, namely the adsorption of cationic surfactants on minerals. In his case, I think he measured adsorption isotherms of amines on various chain lengths on biotite, one of the mica minerals. He talked to me a lot about that research and has always given me good credit for what influence I had on explanations of his findings in his thesis. In subsequent decades I have had quite a bit of interaction with Cases. As I say, he became a major figure in applied surface chemistry and flotation in Europe. Three senior people that I met there for the first time were Professor Marston Fleming from London, Professor Per Kihlstedt from Sweden, and Professor Maurice Rey from France.

On the first Monday—the Congress was a week long—there was a cocktail reception, and I remember it was on the roof of the auditorium on the waterfront there. The meeting was held in the hall where they hold the Cannes Festival judging movies. The head of the Russian delegation was Professor Polkin, who had been the thesis supervisor of Vladimir Nebera that I mentioned last time. Somebody introduced me to Professor Polkin, whose English was not that thorough, and he said, “Why, why, you're nothin' but a boy!” [laughter] I had already published papers for a decade, and they were expecting a far more senior person than I was in 1963. “Why, you're nothing but a boy!”

Anyway, that was a very interesting congress. I had made arrangements for traveling afterwards with Elmer Tveter from Dow Chemical Company. He was an engineer who was the inventor of some of the Dowfroths—a very outstanding chemical engineer, turned mineral processor. At the end of the congress, you know, they always had technical tours, and maybe later on I might just tell you about these congresses in general, but as for planning this technical tour I called up Elmer, who I had known, and said, “Well, which one sounds best?” And he said, “Well, there's one in northern Italy that sounds like it might be a good one to do.” So we both signed up to go on the northern Italy tour. The Saturday before leaving Cannes, he and I and a Dow colleague went to Grasse, the major perfume producing town. That other person was Guy Harris, who I had never met before. Guy was a chemist for Dow who was

actively involved with producing new flotation reagents and who had been spending the year in Ghana teaching chemistry at the University in Accra. He came up to Cannes to attend the congress. That was the first time I met Guy, who in later years, of course, I had gotten to know extremely well. So he was at the congress, and then returned back to Ghana for a second year there.

The group of thirty or forty people participating on this particular tour flew on the same plane to Rome, and we arrived in Rome the evening that Pope John died, and a bunch of us took a taxi and went to St. Peter's Square that evening. There were at least a million people there, it seemed, and we were part of the throng on that occasion. The schedule for the next morning was a tour to an atomic energy research laboratory, but I decided to skip it, and I went back to St. Peter's, where they were setting up all the TV cameras and walkways. That morning I went on into St. Peter's Basilica.

Then in the afternoon I joined the tour to the CNR Mineral Processing Institute in a Rome suburb, run at that time by Professor Usoni. They established this research institute for him after 1945 because apparently he had made his name with the Italians by going into Ethiopia in 1935 and looking after the mines, for whatever they mined in Ethiopia. His efforts were important for the Italians. I remember as a kid in the thirties reading articles about Haile Selassie, and Ethiopia being overtaken by the Italians.

I'm glad I didn't skip the afternoon tour because they were anticipating my coming to their institute that afternoon to talk about their research program. In the evening there was a lovely dinner at a restaurant in the Medici Park. I naturally decided to sit next to the nicest looking young woman there, whose name is Anna Marabini. She has now just retired as director of that institute, and through the years I had very good technical, professional interaction with Anna. At that time, she was just one of the researchers at the institute; and had one young daughter and later had another one. But years later, as I said, she became the director of the institute—did very, very well and really built it up.

Swent: That's very interesting. We don't think of Italian women as scientists.

Fuerstenau: Right, right. Then we visited two or three mines in northern Italy, and one was a big operation for taking pyrite and burning it to get the sulfur, making iron oxide as a by-product. After spending a day in Florence, we then went up in the Dolomites to a lead-zinc mine. It really was just an excellent tour. With great food. [laughs]

After that, I toured a little bit more through Europe with Elmer because he bought a Mercedes that we picked up in Stuttgart and then went to London, where he met his wife, and I eventually came on back. I had written to Professor Horace Rose, and he invited me to his house, after I had visited him in King's College at University of London. We went to his home in Surrey,

had dinner, and he took me back to catch a train or bus, whatever it would be, back to London.

Boy, I stepped off a curb, and he pulled me back, jerked me back—

Swent: Oh, you looked the wrong way!

Fuerstenau: I looked the wrong way. Without him jerking me back, we might not be here talking. “You look both ways,” I always tell everybody. So anyway, upon arriving back in Berkeley I was greeted with what I told you about—the comminution meeting.

Ponisseril Somasundaran Exemplifies the Berkeley Graduate Program

Fuerstenau: Somasundaran was not involved with my problems of organizing the comminution conference because he had returned to his research on flotation chemistry. He did some very elegant studies of surfactant adsorption and its relation to flotation of oxides, both quartz and alumina. His research really became a very classic work, and he’s made surfactant adsorption I suppose about 80 percent of his main activity ever since. He completed both his MS and PhD degrees at Berkeley in three years. However, in later years, he also did some interesting work on grinding aids in comminution.

Swent: And he’s at Columbia now.

Fuerstenau: He’s at Columbia. I recall we were talking last time that I wanted to make sure people had bags of tools, and I’ll illustrate that. His first job was with International Minerals for a few years, and then a very outstanding person named Karol Mysels, who had been a very distinguished professor at USC in chemistry, colloid chemistry, was hired by Reynolds Tobacco Company to set up a research group, obviously to study things related to tobacco smoke. I don’t recall how Soma got hired, but Soma joined his group at Reynolds Tobacco. Maybe he’d read our papers related to surfactant absorption, et cetera. So Soma went there and was working there for a couple of years. He told me he learned a lot about fluid flow through porous media and how reactions might be taking place on burning tobacco and about the motion of dry colloidal particles, and so on.

Swent: Were they doing biological research? Medical research?

Fuerstenau: They wouldn’t have been. I believe, studying combustion and the behavior of the fine particles produced in the smoke. Well, those particles would be going into your lungs.

Swent: I was wondering what their motive was for studying this.

Fuerstenau: Well, having not done it myself, I can't give too much detail, but obviously this group didn't find what the tobacco company was looking for because after a while they wiped them out, after two or three years, so they were probably coming up with answers that the tobacco company didn't want. These were not good times. This must have been, '68-ish, '69. What I'm leading up to is that even with the economy being what it was, Soma had four job opportunities to choose from. I just want to illustrate how this relates to the kind of background that he had obtained as a grad student here. One offer was as a researcher with Philadelphia Quartz Company, looking at silica in water; another one was with a paper research institute on cellulose fibers in water—in other words, again, particles in water. Another job offer was with Inco Research on flotation; and the fourth offer he had was from Columbia University. He called me and I said, "If I were you, I'd take the job at Columbia," which is what he did.

You can see the wide range of positions that he had the background to fill—plus he was already at a tobacco company. At that time, he was still young enough to be nominated for the TMS/AIME Hardy Gold Medal to recognize the most outstanding young metallurgist in the country under age thirty, and I nominated Soma. But I'll tell you, it was kind of hard to put together a nomination for an outstanding metallurgist working for Reynolds Tobacco Company. Frank Aplan, who was a committee member at the time, told me that Soma was the best candidate that year, but the chairman of the committee went out and solicited more nominations so that Soma didn't get it. Well, in retrospect, it would have been fine, but, like I say, it was pretty hard to write how this metallurgist was outstanding while working on tobacco smoke or something like that.

His background was in line with what I wanted my students to achieve: broad, solid base of fundamentals, with some areas of expertise that they could apply in any direction. I have quite a number of former students working in the ceramics area: why? Because of their understanding the behavior of particles, making particles, et cetera.

Research on Mixing and Particle Transport

Fuerstenau: Early on, I had much interaction with the ceramics people here. Joe Pask and Dick Fulrath were the two ceramics faculty that got me involved with ceramic particles. My guess is they got me involved because their research was supported by Lawrence Berkeley Lab, and in those days LBL principal investigators were not permitted to have outside funding. The object of ceramics is to make homogeneous, reliable bodies, quote unquote, and Joe had suggested studying mixing solid particles to enhance uniformity, so I put together a proposal for mixing solids. That went to the air force, and so we had three years' support from the Wright Field—air force on mixing. That led to several theses, quite a few.

Swent: Why would the air force be involved?

Fuerstenau: Ceramic components. You know, radomes—a lot of things are made of ceramic materials. After that project was finished, I was able to get continued support from NSF for several years, again on solids mixing because anything we buy as solids have been mixed somehow. There are problems of mixing trace elements into fertilizers or trace elements into foods and pharmaceuticals. And in the case of ceramics, if you want something to be uniform when you've got two different kinds of particles, how do you do this?

So this ended up as theses for several people. Like Dick Hogg. Hogg was one of my very best students. He came here as a grad student in ceramics, and Dick Fulrath who was the ceramics graduate advisor suggested that Dick come and work with me. He did both an MS and a PhD on mixing problems. His BS was from the University of Leeds in ceramics, and his whole career has been at Penn State in mineral processing, so he's a ceramic person that I converted to mineral processing. Some of the mineral processors, I converted to ceramic engineers.

Anyway, let me tell you two things. One, we were doing dry mixing and wet mixing. For wet systems, in the world of colloid science, studies of suspension stability had always been done with one kind of particle. As a model system, generally people had worked with silver iodide, for certain reasons. So they would study the interaction of silver iodide particles with silver iodide particles. Later on latex particles became the standard because they could be made spherical with controlled charge characteristics.

But we were thinking in terms of mixing dissimilar particles—you know, like alumina, Al_2O_3 and magnesium oxide, MgO which were two that we actually did study. A graduate student, Owen Devereux, had done a doctoral thesis with deBruyn at MIT, where he rigorously solved the double layer interaction equations numerically. His thesis was published as a book, which is about 90 percent simply computer printouts of the results of calculations from the equations of overlapping double layers. Such results should describe what might be happening in the wet mixing of two different powders.

So, in relation to our research on wet mixing, I thought: could this really be simplified? And just how accurate or inaccurate would the results be by taking the simplified double layer equations when two different kinds of particles are brought together in water, and are they repelled? So I had Dick Hogg, for his master's, calculate the interaction of particles of different size and different electric potentials using the simplified linear model of the double layer. Later, we submitted his thesis as a paper, without change—this now is an MS thesis—to the *Transactions of the Faraday Society* in England, and this was published. This publication is really a classic paper that almost anybody today, even, who works on interaction of dissimilar particles will start off by referring to that paper. I mean if somebody looks at interaction of an air

bubble or a mineral particle in water—those are dissimilar particles—they begin with our approach. Somebody who I'll talk about shortly is Tom Healy, who came here as an assistant research engineer and lecturer, and Healy was very heavily involved in this. To this day, this is referred to as the H-H-F Theory, Hogg-Healy-Fuerstenau, and there are even papers written on analysis of Hogg-Healy-Fuerstenau. This was Dick Hogg's MS thesis.

Swent: It sounds like that was an important contribution.

Fuerstenau: Indeed. It really is a famous paper. Coming back to our general mixing research program, Hogg's PhD thesis involved the dry mixing of solid particles in a batch rotating drum, where we used white beads and yellow glass beads loaded carefully in each end of the drum and followed the rate of diffusion of beads of one color into the other as the drum was rotated. That paper was published in *Chemical Engineering Science*. Afterwards, several more students carried on mixing studies. Dick Hogg's MS was really a classic paper because it was only theoretical but it provided a way for analyzing the interaction particles of different size and different composition. Dick Hogg went from here to being a faculty member at Penn State. Just a year ago he retired. When your grad students are retiring—[laughs] you know that you are aging. Oh, he retired a little early, age sixty-two. But a very good man. Dick has done many fine things, and very high quality. A few years ago, he was the recipient of the SME/AIME Gaudin Award for extensive contributions to the flocculation suspensions with polymers.

Following various batch-mixing investigations, we studied the continuous transport of solid particles in rotating drums. We devised a way to feed the particles continuously at the feed end, and constructed a sampler at the other discharge end so that we could follow how different particles flowed through the device. By using one kind of particle as the main material—such as quartz—and putting in a little bit of calcite, which is calcium carbonate, as a tracer, we could sample the output and learn how the calcite moved through the drum. All that we had to do was dissolve away the calcium carbonate from each sample and we would have an analysis of the discharge material coming out of the drum. From that we could get the flow characteristics of particles through the drum. In other cases, we dyed some of the particles, so that we could follow the transport behavior of the bulk solids themselves.

The first student to study transport was Abdel Abouzeid, whose main career has been a professor in the Department of Mining Engineering in Cairo University. I have many papers coauthored with him, since he came back to Berkeley on several occasions. Another student who contributed a lot to our work on particle transport was K.S. Venkataraman. All of this was carried on for a decade or more, mostly supported by NSF. We started with batch mixing; then moved to continuous mixing, which really led us to studying transport through drums, as I said. Often, you encounter segregation. I mean, you've seen segregation in your spices when you're cooking. Whenever

there's coarse particles and fine particles, they tend to separate. And that happens in big rotary kilns and apparatus like that. So what we did was to put in plastic balls, and study how the plastic balls would eliminate the segregation. The next step—I had always been working on grinding—was now to study continuous grinding by changing the plastic balls to steel balls. That became the theses of V.J. Karra and H. Swaroop. So all through the years, I sort of kept progressing along in this broad area, but no longer.

Swent: Was anyone doing this sort of thing at any other place in the country?

Fuerstenau: Dick Hogg continued some of this at Penn State. More though in England. You know, particle technology was a big thing in England and even bigger, then, in Germany and not much over here. We can talk more about that. I think I had a fair amount to do with bringing particle technology ideas for study in this country—because of what we did and the kind of approach we used. Rose, for example, was a person who did a lot of work on particle technology in mechanical engineering, as I said, at King's College. But there were other groups in England that were heavily involved with particle technology, one group at Loughborough and another at Bradford. In this country, there had been a program at Georgia Tech, led in the 1940s by DallaValle. His student, Clyde Orr, who I invited to give a talk at that first Engineering Foundation conference, carried on afterwards. I don't think there's anything at Georgia Tech now. But the really great particle technology center was that of Hans Rumpf in Karlsruhe, Germany. There were other active groups in Germany also, at Clausthal with Kurt Leschonski and at Erlangen with O. Molerus—both protégés of Professor Rumpf.

[Added by Douglas Fuerstenau during editing: In 2006, I received the Particle Technology Forum Lifetime Achievement Award from the American Institute of Chemical Engineers for my many contributions to particle technology and for educating many PhD students who contributed significantly to the field.]

Research on Processing Deep Sea Manganese Nodules

Fuerstenau: Another area that I got started on again related to water/solid interfacial systems—it involved manganese nodules. Just as I was arriving in Berkeley, a person here who just had received his doctor of engineering degree in mining, named John Mero, was completing his program under a Jackling postdoctoral fellowship from the AIME Jackling Fund, I suppose. You know, that's the same Kennecott Jackling. Mero had gotten interested in mining mineral resources in the sea. Mero's BS was in mining engineering from the University of North Dakota, and he came out here to pursue graduate studies. Somehow he analyzed in more detail the manganese nodules that had been collected from the bottom of the ocean over a hundred years ago. They always refer to this as the Challenger Expedition, which was the first real attempt to map the sea floor during which these nodules were collected. They were

stored down at Scripps, and I guess nobody had analyzed them for copper, nickel, or cobalt, but he did. Of course, these things could run a percent copper and a percent nickel and maybe .3 percent cobalt or something like this—a high metal content. John Mero was the person who got the world interested in mining deep sea manganese nodules.

Swent: It was very hot there for a while. We were going to get everything we needed from nodules.

Fuerstenau: Right on! Well, John Mero from North Dakota started that. He wrote a book called *Mineral Resources of the Sea*.

[Tape 22, Side A]

Swent: You were just saying that John Mero was running a flower shop in Sebastopol.

Fuerstenau: This came after his retirement from a career promoting mining in the oceans. After leaving Berkeley, Mero went to work for Newport News Shipbuilding. They were going to build the ship for mining the oceans, and Mero was there for several years, and later on through him, I got a research grant from them for working on nodules. My first funding came from NSF where I had written a proposal to study the colloidal nature of nodules. It seemed to me that these nodules were cemented manganese oxide and iron oxide colloidal particles and that copper and nickel and cobalt would be adsorbed on the surface of the manganese oxide particles. There are a lot of postulates about how this cementing took place and whether or not they formed by bacterial action. Some of the nodules are a couple of inches or more in diameter, often more or less spherical in shape, and they lie on the surface of the ocean floor under about 15,000 feet of water. Drill cores into the sediments show only a few nodules down into the sediments on the ocean floor. I think that the differential pressure on the top and bottom of somewhat spherical bodies and vibrations that occurred due to earthquakes over the eons would have vibrated them to the surface of the sediments. Mero's idea was to go along with a bucket and just scoop them up and hoist the loaded buckets to a ship. Another major approach from Lockheed was to operate what essentially was a vacuum cleaner that sucked them up from the ocean floor. Many companies formed consortia to mine them: Kennecott, U.S. Steel, and Lockheed were going to suck them up; it was going to be the salvation of the world. Newport News and Inco, if I recall, formed another consortium and were going to use the bucket system for mining them. For two or three years there was a Bureau of Mines station working on manganese nodule mining over in Tiburon, and I recall going over there a couple of times for discussions and to get some samples. One of the problems that caused the interest to wane was the United Nations Law of the Sea, through which all third world countries thought that their economic problems would be solved. The higher grade nodules occur in large areas of the Pacific Ocean. Anyway, if nodules were on land, they would

be an interesting resource since they have no sulfur in them—but at 15,000 feet under water, mining is a problem.

So my first work involved study of the surface chemistry of manganese oxides and the adsorption of copper, nickel, and cobalt ions on the colloidal particles. This was supported by NSF and a couple of nice papers resulted from that work.

Then, a geochemist named Roger Burns who was finishing his PhD thesis in the Geology Department here at Cal came over and worked for me with my NSF project. Earlier I told you that he had married Virginia Mee, the Union Carbide geologist. Later on, Burns had a very distinguished career as a geochemistry professor at MIT. Using a technique involving new major equipment in that department, he carried out a very detailed electron probe analysis of the composition of a series of nodules, where you could look at the composition along the surface of a polished specimen micron-by-micron. Roger worked out the methods needed to handle nodules so that we could correlate the relationship between copper, nickel, and cobalt with manganese and iron. The nodules had a banded structure, like onions. Where manganese was concentrated, you would see high concentrations of nickel and copper; and in bands where iron was concentrated, it would be high in cobalt, this sort of thing. So we had really a seminal paper published in *American Mineralogist* on this aspect of nodule structure and composition.

Subsequently, I had several students do theses related to recovery of the metals from nodules. The most comprehensive ones were Mauricio Hoover's and Ken Han's PhD theses. They looked at many different ways for recovering nickel, copper, and cobalt, mostly by leaching methods using acids and ammonia, but also some pyrometallurgical techniques, particularly chlorination. If you could put your processing plant on a ship, then the tailings might be submerged back down into the ocean where they would rejoin the sediment layer.

I don't know how many patent disclosures I had put in my file. Some of these seemed so obvious that we never applied for any patent, yet after we had done a lot of different approaches to recovering the metals, Kennecott got umpteen patents. They had a big effort on it; their patents were identical to what we had already done, but I just never even put in an application. It's good I never did because nothing would have ever come out of it anyway. But we had a lot of ideas about nodule processing. I mean, many different approaches, all related to our knowledge of their structure and our concepts of how metals were incorporated into them. I think I even spent some time doing a little consulting for Kennecott at one time.

Inco also provided some support for our research program. I had a visit from two Inco people. One was Frank LeQue who was a senior, major figure in corrosion. The other person was a somewhat younger geologist in charge of Inco's exploration. I remember their saying that the world consumption of

nickel doubles every seven years. This was now back in those days. They could see the next doubling from land sources, but for the doubling after that, they figured that they had to go to the oceans, which is why Inco had joined in one of the consortia.

They asked me to write a proposal, because they were aware of our published work on processing, and all was proceeding well until Inco's lawyers got to it. They wanted patent rights. I had never done any research that gave away patent rights—the university basically hadn't given up patent rights—but it turns out that Scripps had given patent rights to Inco for some project. Scripps was part of UC-San Diego. Finally the University and Inco got together, saying that Inco would prepay the royalty. Now, we're talking long ago, so I may have had a \$30,000 or \$40,000 project, which was significant back in the sixties, and for \$3,000 or \$4,000 they would add to it as prepaid royalty.

Well, I'll tell you, that really stifles your creativity, because if you think you've got a good idea, you're not going to pursue it. Through John Mero I had had funding from Newport News, and I told them, "Hey, I'm not going to take that if you're going to tie me down by talking patents." I had no restriction on the money I got from Newport News. Eventually the papers, the work Inco might have supported, probably ended up basically as much of Ken Han's thesis.

Swent: I don't understand. What does prepaid royalties mean?

Fuerstenau: I suppose the university and the lawyers of Inco agreed that if there were any patents, they would be licensed to Inco, and that Inco will pay now for any potential royalties. I recall that those prepaid royalties amounted to 10 percent of the price of the project. You know, I'll bet I never saw the money for the royalty, which I should have.

Swent: Probably not.

Fuerstenau: It's what happens when the lawyers got to it. Well, those were two examples of industrial support that I had. You know with such support, patents always come up, and companies seem to think that they can give you a few thousand dollars and they thereby own your brains. I've seen it time and again, and often on consulting agreements, and so on. It always amazes me that for a few thousand dollars, company people think that that money entitles them to the whole works, whereas the real thing is what's in the brains of the people behind the research. Sometimes the company supporting the research may be given exclusive rights to a patent that the university might have obtained, but they pay the royalties.

There was a big flurry more recently in Japan and in Korea about mining ocean resources. About ten years ago I was in Japan, invited by the Japanese Ministry of Mines to give a long talk. It went for two hours, about manganese

nodules and processing the nodules. The Japanese government, through something they call MITI, which is Ministry of International Trade and Industry, had supported a five-year program for getting cobalt out of the ocean. Sea mounts, I think they were digging up. They had a formal translator there, and invited all sorts of people: professors and researchers and company people. It was in Tokyo. I'd written, with Ken Han, two or three reviews of manganese nodule processing, so even though I was no longer actively working on it, I had all the necessary material to make a good presentation. I remember the minister saying he wished he'd talked to me five years earlier. They spent a lot of money, but since I have read nothing about it, they must have made the decision not to proceed. I heard later that Hyundai was building a manganese nodule mining ship in Korea, so whether the Koreans—now this was only two, three years ago—are still doing that, I have no idea. But someday those resources will be used because they're there.

[Added by Douglas Fuerstenau during editing: In March 2007, I was in Korea as an invited speaker at the Japan/Korea Symposium on Recycling and Materials Science. Afterwards we visited KIGAM (Korea Institute of Geoscience and Mineral Resources). There they have built a pilot plant where they are currently running a 200 kg capacity operation to make Mn-Si as a by-product and also produce a matte consisting of Fe, Ni, Co and Cu which is then processed to produce the individual metals using conventional methods. Apparently, the Korean government, who supports this project, is not quite sure where they would like to go from here because they believe that this is not going to make a profitable business in the foreseeable future.]

Swent: Paul Henshaw, Jr., did his PhD on manganese nodules and then he worked for Chevron.

Fuerstenau: I didn't know that, but people at the University of Washington have done a lot of good work on the geochemistry of manganese nodules. I read several of their papers.

Research on Pelletizing Iron Ores

Fuerstenau: Another area: in '61 a student came here who had worked for Jones & Laughlin Steel Company, Tom Goldstick. He had received his BS degree in metallurgy from MIT. At J&L, he had started research on fundamentals of pelletizing iron ore, the balling step, the making of the green pellets, so I thought, gee, that sounded like a good thing to do. At J&L he had had big sandpaper belts made inside out so that the grinding surface was on the inside of the drum belt. Well, when you put the mud in there, it just coated it. Goldstick had started to get more and more interested in biological problems and less interested in iron ore processing. But to support him, I had gotten a project related to agglomeration, supported by the American Iron and Steel Institute. This project just wasn't going anywhere, and I had a new student

named Prakash Kapur, who had come as a ceramics grad student but who had joined me, and had just completed his MS thesis on grinding. I talked with Kapur and said, “Hey, look, I’m getting no progress on this. You take it over.”

I guess I upset Goldstick enough that he became a bioengineer and transferred to Mechanical Engineering and worked with Professor Fatt. Fatt had been a petroleum engineering professor in our department but moved into Mechanical Engineering and eventually, as I may have said earlier, into Optometry, where he was working on oxygen transfer in the eyes, under contact lenses. It was real engineering: petroleum engineering is flow through porous rock; in our body, flow through veins and whatnot, and how oxygen transfers in the lung from gas into the blood was what they were studying and that study became his PhD thesis. Goldstick eventually became a professor at Northwestern in medicine and chemical engineering.

That’s how I got interested in agglomeration. I may eventually have had about four, five, six master’s and PhDs on pelletizing. I told you that’s an example of how mineral processing technology made a rock into an ore, namely the taconite in Minnesota. Kapur studied the kinetics of agglomeration. We first had to develop a method so we could reproduce the rate of growing these moist pellets, or green balls, and so devised this method using a small drum, like a foot in diameter and a foot in length. A lot of effort went into developing our experimental method because if you can’t reproduce what you do, it’s meaningless. With this technique, we could study the effect of such variables as particle size, water content, drum speed, drum load, and so forth. Iron ore operations use small airplane tires for pelletizing drums in their test work, but that technique is not suitable at all for studying pellet growth kinetic phenomena. These early experiments were all done with powdered limestone, that they feed chickens to make strong eggshells. We bought bags of powdered limestone. I didn’t want to make my lab dirty. The liaison person from the American Iron and Steel Institute would come by and asked, “Why aren’t you working with iron ore?” And I said, “I want to keep my lab clean, white.” Our first paper on this was published in the *AIME Transactions* and Kapur told me some years ago, “You know, that paper is very, very highly cited.” It was really the initial one worldwide on the kinetics and mechanisms of pellet growth.

Kapur’s thesis also involved a model of what was happening inside the drum. To start, we would take powdered limestone and mix it in a small bakery mixer with the desired amount of water, and then rub it through a screen. It would come through that screen just looking like snowflakes. This feed was charged into the drum, and in about five revolutions, the snowflakes popped into little tiny BBs, about one millimeter diameter pellets. All this is due to the surface tension of water. By making these tight little pellets, only the external surface of that little tiny pellet has a water-air interface. Otherwise, when we had the water coating all of the individual particles, the amount of air-water interface was the surface area of every particle, right? So when they tightly

pack like this [demonstrates], there is air-water interface only on the outside of the tiny pellet. Well, that's the driving force to make pellets grow.

Swent: And it works the same with other minerals as well?

Fuerstenau: Oh yes. But because it was white, we clearly saw what was happening. We naturally progressed to working with ground iron ore later. Being dark and black, we probably would never have seen this phenomenon if we had started our work with taconite. So then the model involved the coalescence of these micropellets in the rotating drum. What's the rate at which they coalesce, and then grow? What Kapur observed was, as these micropellets roll and roll inside the drum, all of a sudden they get tighter and tighter and the pores get less and less, and then excess water gets squeezed to the surface, and the pellets begin to glisten. I gave a paper on this at the AIME annual meeting, and somebody from Nalco Chemical talked to me at length about the squeezing out of water phenomenon.

In iron ore pelletizing, bentonite is added to the taconite concentrates to help control the effects of moisture. That little bit of clay first acts as a binder when the pellets are wet, and then as they dry, it also adds to their strength. When the pellets are fired, it also helps to act as a bonding agent. You know, most of that bentonite used in the iron ore industry comes from Belle Fourche [South Dakota].

This chemist at Nalco, fairly senior person with a PhD in physical chemistry, said they were working on finding an alternative for bentonite because bentonite adds silica, SiO_2 , to the pellets. One that they worked on a lot was humic acid. Peat moss, you know, gives humic acid. They could make pellets with this, and they actually ran mill trials up at Erie, at pellet plants there in Minnesota, on taconite. In their test program they would be running with bentonite; and then they would switch to their additive, which was a humate, humic acid. As soon as they did that, after a short while, all of a sudden the water came to the surface. What he found in the plant trials—and it was a mess—was what we had observed in our lab drum. I spent a day or so with them consulting on a couple of occasions. This was quite a few years ago.

I'll tell you a funny one: He said they looked at all sorts of alternatives to bentonite. And one that he knew was plentiful in the Midwest was tons of cow manure. They ground dry cow manure in a hammer mill, and he said, "Boy, you know, that's very tough on hammer mills because it's got silica in it," from the cows, I suppose, eating grass pulled up from the soil. He said the problem with trying to make pellets with ground cow manure was when you put water back in it, it came back to the original material. [laughter] So they gave up on that.

Swent: Interesting!

Fuerstenau: We did another study that was concerned with the effect of feed size on pelletizing by grinding calcite to different sizes. When you grind it very, very fine, these little micro-pellets agglomerate into little clumps that looked like raspberries. Maybe they would make agglomerates that might be a quarter of an inch in diameter and wouldn't grow. These micropellets were too strong, and they couldn't deform. We published this in a paper, and somebody from Hanna [Mining Company] told me about a huge problem they once had with a new plant. They had built a pellet plant for a new mine, the Empire Mine, I believe. They had installed balling drums, started up the new plant, and in a handful of minutes or so they had to shut it down because all they made were these very hard raspberries. In other words, that ore had to be ground so fine for liberation that they ran into this phenomenon in a full-scale plant. Here, we had uncovered the reasons for it in our small one-foot laboratory-scale drum. This is indicative of how some of the mining companies approach research or development: mainly, they don't.

They had to redesign the balling section to use disks instead of using big balling drums—a balling drum is maybe seven to ten feet diameter and thirty feet long with iron ore concentrates and recycle pellets being fed into the rotating drum at one end and the growing pellets coming out the other end. By changing to large revolving disks, the new feed is sprinkled onto the pellets rolling on the disk and can grow by a different mechanism. But initially, literally they had run an experiment on a full-scale, new, multi-million-dollar plant that they had to shut down within minutes, whereas if they'd had an organization that would have tried to look at the phenomena ahead of time, this would not have happened.

My support was from the American Iron and Steel Institute, AISI, and for a long time they gave enough money that supported this research with one grad student. If there is anything such as American Iron and Steel Institute today, it's all just directed at selling steel. They cut out their raw material research years ago.

Another trip to the Minnesota Iron Range: My AISI contact person was Luther Hendrickson, who was a very good engineer, in charge of raw materials research for U.S. Steel in Monroeville, Pennsylvania. That's a suburb of Pittsburgh. One time, he arranged for me to spend a week visiting all the pellet plants in Minnesota, and I gave a talk either daily or two a day at each company on our pelletizing research, and maybe one to their local AIME. So I saw several of the taconite plants, the big ones and so on, all across the northern Iron Range. These were built after my earlier trip with Bob Brandt when there still were wash iron ore plants.

Swent: I think it's amazing that Hanna hadn't gotten in touch with your research.

Fuerstenau: Not at that point. Dee [Dimitri] Vedensky would have been their vice president of technology then. Maybe the Empire plant was built some years

before our paper even came out. Concerning Hanna's research leadership, Abe Dor was head of mining and metals for Bechtel here.

[Tape 22, Side B]

Fuerstenau: Abe Dor went as vice president of research at Hanna, and he probably went there when Dee Vedensky retired. You knew Vedensky. He lived out here when he retired. Dor had a good outlook on research, but that was later.

I remember several of these different plants, and one of them was at Eveleth, Minnesota. I think Ford owned it, or a big part of it. I still can see the huge tailings, which were not red, but white, almost a mile in diameter or more, and a little bit windy and this white dust storm came off of these dry tailings from the ground taconite. That's what I remember about that operation—blowing tailings.

I also would just like to make a comment about something I hadn't thought about. As I say, Luther Hendrickson was a very knowledgeable person and had a lot to do with the largest taconite operation in the region—U.S. Steel's Mintac plant. Thirty million tons of pellets a year or something. They also had a big research facility there with a pilot plant the size of most industrial mills in the mining industry. Most of those companies had research groups there of some sort. There were a couple of South Dakota School of Mines graduates that were about a year behind me. One was in charge of the Mintac mill there—Wayne Dahlke. There were two or three South Dakota graduates that were, I think, working for U.S. Steel at those operations.

Swent: Kaiser had a pelletizing plant.

Fuerstenau: Yes, I'll come to that one in a little bit because we know who ran the mine: Harry Conger²⁰.

Swent: Right. And Marcona had a big one down in Peru, didn't they?

Fuerstenau: Right, right. They sold all of their concentrates from there to Japan.

One evening or day, Luther Hendrickson was driving me around, and he made a comment that I had never thought about, about the difference between somebody in the academic world and somebody in the industrial world. He said, "You know, you in the academic world have an opportunity to make yourself famous." That thought had never entered my mind. But here was somebody who I considered a good man in industry looked at the difference

²⁰ Harry M. Conger, *Mining Career with ASARCO, Kaiser Steel, Consolidated Coal, Homestake, 1955-1995: Junior Engineer to Chairman of the Board*, 2001

between what his career was and what, say, my career was in the academic world, and as I say, I had great respect for him. I don't recall that he ever wrote technical papers, but he was a very solid engineer for in iron ore processing.

Swent: About this white dust: Was it toxic?

Fuerstenau: Well, it was mainly quartz but because the non-magnetic iron minerals are not recovered in the magnetic separation step, they would also end up in the tailings. Because of silicosis, airborne fine quartz could be considered toxic, in some sense.

I visited the non-magnetic taconite operation of Cleveland-Cliffs, which is a process where they grind the ore extremely fine, I think if you put it in mesh terms, minus 500 mesh—it's like finer than thirty microns or so. In order to reduce steel wear in grinding, the grinding mills are charged with taconite pebbles rather than steel balls. They recover the fine iron oxide by what's called selective flocculation. You add a polymer, in this case simply starch, and it will flocculate the hematite particles while the fine silica particles remain dispersed. By carrying this out in big tanks with a slow upward flow of water, the fine silica goes off with the overflow and the coarse quartz particles and the iron flocs settle together. The final separation of hematite flocs and coarse quartz is carried out by flotation. I remember flying out of there, and the tailings area is maybe two, three, square miles and bright red. This is because of losses of some of the finest iron oxide particles. Tilden produces about ten millions tons of high-grade iron ore pellets per year. By the way, this selective flocculation process was developed jointly by the Bureau of Mines and Cleveland-Cliffs.

Consortium support of research: I began to get the idea, why not get a bunch of iron ore companies to support our research on pelletizing as a collective group? So I prepared a proposal, and this would have been middle sixties or '67 maybe, and I remember Kapur was still here. He helped put it together. I sent it off to a lot of companies, from which I asked that they contribute \$10,000 each. I remember Hendrickson saying, "That's an awful lot of money." It blows your mind when you think what they called a lot of money, because, you know, if you got ten companies giving a hundred thousand bucks a year in the 1960s, of course, that would have gone a long, long way. He said, "No one will give you money like that." So that thing went into abeyance, and then—

Swent: Kapur got his master's in 1964, and PhD in '68.

Fuerstenau: Kapur was still here. By the way, you can find few engineers who write with greater style than Kapur. We wrote this detailed proposal together, and then I let it go dormant for a couple of years. Kapur had gone to work for the Colorado School of Mines Research Foundation. But finally I decided to try

again, and I reduced the amount, as I remember, to five thousand bucks because I figured anybody in a lab could, just out of his budget, write a check for five thousand dollars. I sent that out again, and that time, I got several companies to support it.

Swent: Was there no association of iron mining companies?

Fuerstenau: That's called the American Iron and Steel Institute, of course.

Swent: You couldn't work through them.

Fuerstenau: They had supported me already. I was trying to do something bigger.

Swent: You were going directly to the companies.

Fuerstenau: I thought we could do it on a bigger scale, so I got them all to give to me these funds as gifts, because they liked my research, so that there would be no overhead. So Kaiser was one, Kaiser Steel, and Cleveland Cliffs and Hanna Mining. Vedensky would have approved that. And Allis-Chalmers. Maybe somebody else. But without overhead, that gave me a fair amount of money to work with. We had that for three years, and we did a lot of interesting work. We did a lot of studies in plants.

Swent: Your nearest one was probably that of Kaiser Steel.

Fuerstenau: We did a nice study at Eagle Mountain [California], looking at retention times in their balling drums and pellet growth rates in the drums. The way to get the retention time was very interesting. Took fired pellets, threw a bucket of them into the feed end of the drum, and somebody then with a shovel took regular samples of pellets coming out the discharge end, and then all you had to do was dump the pellets, green pellets, wet pellets into a bucket of water. The wet pellets disintegrated. You could then determine what was the distribution of the remaining fired pellets, and we could see how many times they went through the system before they went out through the screens. These damp fired pellets would be coated with fresh feed and grow inside the drum in the usual manner. I think some were recycled six, seven times before they grew to product size. We did a lot of really engineering studies of the behavior of these balling drums.

We carried out a lot of studies related to the role of bentonite in pelletizing; we showed its main role is to control feed moisture. I think they were very interested in the bentonite along the Iron Ore Range because bentonite consumption ranges something like from ten, twelve pounds per ton of ore to twenty-nine or thirty. In other words, some operations required thirty pounds of bentonite to be added to every ton of pellets. I think it really had to do with particle size, distribution. And they had to be able to control the water. If the filter cake was too wet, the pellets just grow too fast.

Swent: Was their ore dry to begin with, or was it a wet ore?

Fuerstenau: The ground ore was ground wet, concentrated by wet magnetic separation and then filtered. That filter cake will run maybe 10 percent moisture. It's going to depend on how finely ground the ore is, and so on and so forth. In several papers that we published, we showed how the amount of bentonite controlled the kinetics of ball growth, and so I think it had a major effect. Only sodium bentonites, like that from South Dakota and Wyoming, can be used in iron ore pelletizing. Magnesium bentonites that come from Texas will not work. This is because Texas bentonites do not expand because the divalent magnesium ions tightly bind the crystal layers so that they can't take up water by expanding like the Wyoming bentonites that contain monovalent sodium ions.

We decided to try to simulate plant behavior by locked-cycle laboratory experiments. In an operating pellet plant, green pellets come out of the drums and go over a screen. The undersize pellets are sent back to the feed end of the drum until they grow big enough to become product. In our little batch drum, we would run the drum for so many revolutions and screen the product, remove oversize balls, add new feed, and repeat the cycle. We found that we would get surging or cycling. There would be a large production of pellets; making a lot of product, and then you'd have very little recycle, so this would result in surging.

I remember Luther Hendrickson calling me up after I submitted a report and saying, "Look what you guys have done. You've demonstrated the surging in balling drums." I didn't even know they surged, if you want to know. But this phenomenon is a big problem in operating a plant, and he was very excited over our being able to demonstrate and control this on a laboratory scale. Of course, plant operators would use bentonite to try to level things out when the balling drums surge too much.

I had several people do theses on this. The student who followed Kapur was Kal [K. V. S.] Sastry, who later worked a few years for Hanna, and then he came back here on the faculty. We can maybe talk about that later. I had a few more people work on this topic, such as Steve Clark and Vishnu Gupta, and then that was the end of my efforts on pelletizing. But our research brought an engineering science basis to green ball growth in pelletizing and agglomeration.

Swent: Earlier I asked about other minerals being pelletized. Are there other examples in the mining industry beyond taconite?

Fuerstenau: Yes, in a way. In the gold mining industry, the fines from crushing are sometimes agglomerated before heap leaching. This usually is no more than tumbling larger pieces of ore with the wet fines in a rotating drum. These agglomerates are really larger particles coated by the finer particles.

Also, some years ago, I had a series of e-mails from a research director of Codelco in Chile who wanted me to consult for them on a project that they had started on pelletizing ground copper concentrates to be leached. In the midst of our negotiations, I was informed that my contact had had a heart attack and died. That was the end of that. I have heard nothing more about it.

The Problem of Interference on Research Direction by Research Sponsors

- Fuerstenau: Those who sponsored my pelletizing research gave us entirely free rein on what we did. Again, success came from working in a given area long enough that you develop a field and new branches grow that you may not have known were there. I had an experience of getting support from the Bureau of Mines on comminution research. Ray Wells, who I knew fairly well, was in Washington with the Bureau of Mines, and he said that he was going to fund a few projects around the country, projects from the Bureau. One area they wanted to support was comminution. These were not big projects, but, you know, I think it was fifteen thousand bucks but back then that was enough to support a grad student. So I had that money for a few years. But, you know, all of a sudden the Bureau liaison person said, "Well, we in the Bureau are going down this same branch of research, so if you want to keep your money, you've got to change your direction." You know, that happened twice. The overseeing group was in Albany, Oregon. Somebody from there came down, and they were working on grinding aids to enhance the efficiency of ball mill comminution, which we had started to work on, and they said, "We want your work to be complementary. You can't work on the grinding aids because we're working on grinding aids." And they cut my direction off again. My grad student being supported by that at the time, K.S. Venkataraman, had done some really beautiful work for his MS thesis on how polymers function as grinding aids in wet ball milling and could not carry that on for his PhD thesis because of this. To this day, I always feel a little angry about what they did—what future idea might have been cut off? I mean, what didn't come about because I was told that if I wanted support money, I had to switch direction because in-house they were already working on some problem in that same area.
- Swent: It really stifles it, doesn't it?
- Fuerstenau: It really did. It upset me at the time, and it still—it still does when I think about it. It's like cutting a branch off a tree, and you don't know where that was leading.

Research of Surfactant Adsorption and Surface Chemistry in Processing

Fuerstenau: By then, several other students had been working on surface chemistry and flotation-related problems, and I wrote proposals to the National Science Foundation that led to research being supported by NSF. I also wrote a proposal to the Petroleum Research Fund of the American Chemical Society and received a grant for studying surfactants being added for recovery of petroleum, which became a big thing a handful of years ago. That provided support for three years for one of my graduate students.

For example, with Somasundaran's PhD research, we made pioneering contributions to the physical chemistry of surfactant adsorption, working with alumina and sodium dodecyl sulfonate, substantiating hemimicelle phenomena. This research was supported by NSF. Two years later, in 1966, Dr. Takahide Wakamatsu came for two years as a postdoctoral researcher and I had him continue this work in much more detail. He investigated in great detail how pH and hydrocarbon chain length of the sulfonates affect adsorption, zeta potentials, and contact angles on alumina. In all of our studies of interfacial phenomena, not only did we work with very pure systems, but I insisted that conditions and solution concentrations be the same for all of the different techniques that were being used. As a result we could make very good correlations between all of the different measurements that were being carried out. Several very classic papers came from his work. Afterwards, Wakamatsu became a professor at Kyoto University and occupied a position of stature in mineral processing in Japan until his retirement.

Later on, sometime in the seventies, I got funding for studying detergents in groundwater, from the Department of Public Health; EPA wasn't in existence. For about three years, we did some very good research, about surfactants in water and adsorption again. Those were the days when the detergents were alkylbenzene sulfates; they always had a benzene ring on them because it was cheap to make them, and that was in all your detergents. The thing was, bugs apparently didn't eat those benzene rings, and so when the detergent got into groundwater, the water would remain polluted. We did a lot of studies of how these surfactants with benzene rings on their chain adsorbed on solids. It was really close to what Soma had done for his PhD thesis. Every little while, I diverted my objective to find funding for research support but still carried on what I wanted to do.

Swent: You haven't said much about colloids. Did that come later?

Fuerstenau: Not too much, late '65-ish, although we did do some flocculation studies on alumina with surfactants when Soma and Healy were in Berkeley. This was when Somasundaran spent a few extra months as a postdoc here. Of course, the theoretical heterocoagulation study of Dick Hogg that resulted in the HHF theory was really our first major work on colloids. I did work extensively on colloidal behavior after somebody from Battelle Memorial Institute came to

see me. You probably know how Battelle was founded—namely to do metallurgical research. All their research apparently, when Battelle himself was involved, was metallurgical, including processing and the behavior of alloys. Today I'll bet almost none of it is. I remember meeting earlier Oscar Tangel and—you might have known him from—

Swent: Plato Malozemoff talked about him.

Fuerstenau: Malozemoff, yes, from Newmont. And Bob MacDonald, who was at Newmont. Both of them had come from Butte, and I think Oscar Tangel was at Butte when Malozemoff was a graduate student, both working with Professor Gaudin. I remember when I was at Carbide, going to Battelle for some project that they were doing for us, and the two people there were Oscar Tangel and Bob MacDonald. Later they both joined Newmont.

Anyway, I was told that Battelle was making a lot of money, and the IRS told them that they had to be giving away a certain amount of that money or they would be taxed, and so to satisfy that requirement, they set up a program of giving out a bunch of research grants. These were two- or three-year grants. Anyway, the Battelle person came to me out of the blue and said, "We have this program"—I don't know how they got to me, but "we want to do various things." Somebody there made the suggestion of working on selective flocculation of colloidal particles, fine particles—so I got this very nice stringless grant to study selective flocculation that probably may have gone on for three years. I brought a postdoc from England; Yosry Attia, who had done a selective flocculation thesis at Imperial College under Dr. J.A. Kitchener, to work with me with the Battelle support. Attia, originally had gone to England from Egypt. So that got me into fine-particles processing by selective flocculation, and on through the years every little while we carried on research on fine particle behavior in some way, albeit down to actual colloids or slightly larger. It's funny. Somebody just came to see me and said that they have to give out this research money to satisfy this IRS problem. [Laughs] I don't know what they've done since.

Swent: People dream of that.

Fuerstenau: Yeah, yeah, right on. My brother Maurie had published several papers showing how a certain chelating agent, a hydroxamate, is an excellent flotation collector for copper and iron minerals. Attia thought that he could make a polymer that would have strong affinity for copper or iron colloids, and tried to synthesize a polymer with hydroxamate groups on the molecule. He was not successful in putting very many on the chain, and the idea went nowhere. Today, this polymer is produced commercially and is used with great success. It's too bad that Guy Harris was not with us at that time.

Anyway, so after Yosry Attia had gone, I continued with the study of flocculation behavior, the flotation behavior of very fine mineral particles,

with Jim Gebhardt, who had come from South Dakota. Another postdoc, Dr. Jan Drzymala from Poland, joined our group and Jan carried out a superb study of the role of iron salts in regulating the selective flocculation of quartz. Once you get an interest going in a subject, it sort of continues. Two detailed and excellent theses were carried out on surfactant adsorption on fine colloidal oxide particles, one by Hyun Jang and the other by Miroslav Colic, and several excellent papers resulted from research. Of real interest to me were Miroslav's studies of pH and sulfonate chain length effects on the reversible transfer of 100 nanometer hematite and anatase particles from water into heptane. This has potential in fine mineral particle processing. Both of these graduate students had undergraduate degrees in chemistry.

Over the years, I always had several different grad students or postdocs working on determining surfactant adsorption isotherms, their electrokinetic effects, sometimes microcalorimetric measurements, contact angles and correlation with flotation behavior for a wide range of oxide and silicate minerals. Many papers resulted from all of this. My very last graduate student, Renhe Jia conducted his PhD research on many aspects of the colloidal and interfacial behavior of quartz with cationic surfactants of various molecular configurations.

It was in the seventies that research money really sort of dried up, although all the time I had one or two concurrent NSF projects. They went on for many, many years, until finally NSF project sponsors said, "'Minerals' is a bad word; you'll have to take the word 'minerals' out." This was the result of action of the engineering director of NSF, who was Nam Suh from M.I.T. I never wrote another NSF proposal again. Because of Nam Suh, NSF support of mineral processing research all but disappeared.

Support from NIH to Study Hydroxyapatite in Relation to Teeth

Fuerstenau:

There was a period when there was a lot of interest in teeth and hydroxyapatite, and with a former student who had finished his PhD, Dr. Subhash Chander, we wrote a proposal to study the surface properties of hydroxyapatite. Our bones and our teeth are made of hydroxyapatite. Well, you know, that's just calcium phosphate mineral. And so I had a six-year grant. A whole delegation of at least six people from NIH [National Institutes of Health] came out as a panel to evaluate our capabilities for doing what we had proposed, which was making hydroxyapatite colloids, determining factors that control the solubility of hydroxyapatite, its surface properties, electrical double layer nature on the surface, and so forth. The head of that delegation was a person who had written a lot of papers on the solubility of apatite; his name was W.E. Brown from NIH, a laboratory researcher in Bethesda. I made this presentation, and we met all day long, and I was sort of being grilled. Finally I almost got angry, and I said, "Hey, I don't need this money. I know what I'm doing," et cetera. You know? "I don't need you to sit here and quiz

me about this and that.” I got the money, but at the end of the day, I really was quite worn out.

Anyway, for six years I had NIH support for studying the surface properties of apatite. Up to that point, this was the largest funding that I had for research. We never had a tooth in our laboratory, but we studied how fluoride will replace hydroxyl ions at the apatite surface; that’s why the toothpaste Crest, you know, works so well on your teeth. It transforms hydroxyapatite to fluorapatite. If I remember, at about pH 4 or so, hydroxyapatite becomes quite soluble in acid, and fluorapatite is stable clear down to around pH 2 or 3, something like that. After we started doing these studies, I quit drinking Coke. People who drink Coke without a straw are dissolving their teeth. I had several people do master’s and doctor’s theses related to these reactions and surface transformations. We have quite a few interesting papers published in this area.

But still quite a bit of those findings came back towards flotation chemistry, because both fields involve surface and solubility phenomena. During those six years, we carried out many different studies—a lot of it through measuring zeta potentials, others by directly measuring the amount of different species in solution, identifying species at the surface with infrared spectroscopy.

[Tape 23, Side A]

Fuerstenau: I recall that at least two PhDs and several master’s degrees were done on this over that six-year period based on my NIH grant. You know, it was just an example of my taking my knowledge and approach and finding a way to make use of it to ferret out research funding. However, a very good start on my NIH project was made by the postdoc who helped me write the proposal, Dr. Subhash Chander. He had earlier done his PhD related to the electrochemistry of flotation and completed a beautiful thesis on the behavior of sulfide minerals, where you actually carry out an electrochemical reaction between the sulfide mineral and species in solution.

[Added by Douglas Fuerstenau during editing: On December 11, 2006, Subhash Chander died unexpectedly from an apparent heart attack. He had just reached his 60th birthday, and would have had many fruitful years ahead. He was professor of mineral processing at Penn State and editor-in-chief of the *International Journal of Mineral Processing*. He had become one of the international authorities on the electrochemical behavior of sulfide minerals and pyrite in coal.]

Electrochemical Control of Sulfide Mineral Flotation

Fuerstenau: What Chander did for his PhD was interesting in that by changing potential we could cause the flotation collector to adsorb or desorb. Chander was the first to measure the change in contact angles with flotation reagents on copper when electrochemical potentials are varied. We began to ask ourselves, could we cause some of these electrochemical reactions to take place in a laboratory flotation cell by changing the potential of the flotation cell? So we designed a Hallimond tube flotation cell in which we incorporated an electrode system and could work with about one gram of mineral for each experiment.

At that same time, Dr. Ron Woods, from CSIRO in Australia, a major person in the world of electrochemistry of sulfide minerals, came through here and gave a seminar. Through the years I always had many visitors come to Berkeley, and it's been great for my grad students. Woods gave a seminar talk where he showed how he had made a glass electrochemical flotation cell about six inches high and an inch in diameter with a set of electrodes in it. He was floating gold, so it was beautiful in color, using xanthate as the collector for gold, xanthate being the common flotation collector for all sulfide minerals. By changing the potential, he could control the reaction at the surface. And so here we were already building our own little electrochemical flotation in our shop, when Woods came and showed slides of his gold flotation system. It shows you about how research goes around the world and different people may arrive at somewhat the same idea.

So we completed building our electrochemical flotation cell and began to test it. As you may know, one of the big problems in the world is separating copper and molybdenum—you know, copper sulfide and molybdenum sulfide flotation. I had had a research project from Kennecott—that was another one that I had got from industry that really had no strings attached—to investigate improving the separation of molybdenite and copper sulfide minerals. The thing about molybdenite is it's one of the few minerals that we call naturally floatable. It's water repellent due to its crystal structure. Talc is another one. You know how they call talc soapstone. Molybdenite is the same. Graphite is another one. There are only a few minerals in nature that really fall into that category because one plane of their crystals must be held together by weak bonds. If you crush a mineral and break strong chemical bonds, the broken bonds interact with water molecules and require a flotation collector to make them water repellent. So in processing porphyry copper ores, natural floatability is made use of: first you float both the copper and the molybdenum, and then if you find a way to prevent the copper mineral from floating, the molybdenite will float without a reagent. Kennecott makes the separation by roasting the concentrates at low temperature to burn off the collector so that the copper sulfide will no longer float but molybdenite still will. With support from the Kennecott grant, Chander did a nice MS on the natural floatability of molybdenite, including some preliminary work on electrochemical reactions on molybdenite.

For his PhD thesis, Chander carried out the detailed electrochemical study of dithiophosphate collector interaction on copper and chalcocite. But the molybdenite problem got us started in this direction. With a mixture of chalcocite, which is cuprous sulfide, and molybdenite, which is molybdenum sulfide, just by changing the potential of our little flotation cell, we could find conditions where both would float or only the molybdenite would float. You could actually make a separation of the two minerals.

We published a little paper on that, and some years later, not many years later, I was at the International Mineral Processing Congress, again in Cannes, and Dr. J. Heimala from Outokumpu in Finland—you know, the big mining company that also makes machinery—talked to me about that paper. Outokumpu had quite a good fundamental group working on many problems in mineral processing. Dr. Heimala gave a paper describing how they had actually built a pilot plant and full-scale flotation machine in which they could control the electrochemical potential. By that I mean producing electrochemically oxidizing or reducing conditions inside the flotation machine. I remember him saying to me, “When we read your paper, we immediately went to work in the lab and then in the pilot plant and trying it on a full-scale machine,” and said, “We were really surprised that no American company followed up what you guys have done.” This was a Finnish research engineer from Outokumpu.

Swent: They’re building things all over the world now.

Fuerstenau: Isn’t that right? They dominate so much. I think they were the first makers of the magnetic metal detectors used in airports, for example, too.

Swent: They picked up on your research.

Fuerstenau: Right, and they were curious why American companies didn’t pick up on it. This was now fifteen years ago.

Some years after Chander’s thesis, I decided to study the electrochemical and flotation behavior of chalcocite with a collector that would not oxidize, as do xanthates and dithiophosphates. Using octylhydroxamate as the collector, Jim Hanson carried out a beautiful piece of research in this system for his PhD thesis. With this system, one could determine the effects of mineral oxidation and reduction on flotation without the collector oxidation complicating things.

Finally, we later studied the surface chemistry and flotation behavior of oxidized lead minerals anglesite and cerussite, and oxidized copper minerals chrysocolla and malachite. Most of this involved the sulfidization of cerussite, for example, and was done by Ronaldo Urbina and Francisco Sotillo. We studied the uptake of sulfide ions and showed how it could be controlled by measurement of oxidation potentials. Some good work.

I want to just maybe bring up two items. I was going to talk about two, just two more items or so of the research, and then that's done.

[tape interruption]

Research on Rare-Earth Mineral Flotation at Mountain Pass, California

Swent: We had a short break here, and now we'll go back and continue to talk about your research projects.

Fuerstenau: Yes. We're not going to talk about them all; I'm just giving you some of the thoughts behind these. One time I had a visit from two people, metallurgists including the mill superintendent, from Mountain Pass²¹, Molycorp, which is a major rare earth producer—I think maybe the largest in the world, at Mountain Pass in California outside Las Vegas on the way to Los Angeles. Their ore was bastnaesite, which is a fluorocarbonate rare earth mineral, which I'd never heard of until they came to see me. The cations in bastnaesite are calcium and the rare earths such as lanthanum, cerium, and neodymium. They have a big deposit there, owned by Molycorp, and they were going to redo the plant a bit. Now I'm operating off a vague memory, but they told me that the flotation recovery was low, and their grades were very low. The ore grade for rare earths was quite good, but the concentrate was not good because the rare earth minerals are fluorocarbonates, and they were mixed with barite, which is barium sulfate, whatever the calcium sulfate mineral is, barium carbonate, calcium carbonate, strontium carbonate, and so forth. You know, to try to separate those is very, very difficult.

We worked on that project for maybe a year, during which we studied the flotation chemistry of these various minerals. One of my grad students really got interested in this, one of the brightest, most capable students that I have had, named Pradip. And he refuses to use any name other than Pradip. He went back to India, and every time he travels in and out of the country, there is the problem of having one name only. I guess he doesn't want the Brahmin name to be known to others. Very bright. Very dedicated. Very nice person. This year—he's only in his early forties—he was made a member of the Indian Academy of Engineering. He's already received two, three awards in India and now has worked for many years for Tata Research Development and Design Centre, after some time with the India Atomic Energy Establishment. You know, Tata Industries is the main one of India.

²¹ Alexander Wilson, *Leading a Changing Utah Construction and Mining Company: Utah International, GE-Utah, BHP-Utah, 1965 to 1987*, 2000

As bright as he is, he said he wanted to work on a practical thesis, and so he undertook this detailed study of the surface and solution chemistry of these complex mineral systems and their flotation behavior, and so on. Excellent piece of work. His PhD research was not paid for by Molycorp. His MS may have had something that related to it; I think that's true. But then I had NSF money for flotation chemistry that supported his research. I remember at that point Molycorp had been bought by Union Oil Company, and one day I had a call from the research VP of Union Oil. They wanted to see Pradip's PhD thesis in order to determine whether anything in it could be released. Well, I said, "You guys had nothing to do with that research," et cetera, et cetera. But anyway, out of the blue came that call. He had probably worked on some of the mineral samples that had come from Molycorp, but that was the extent of their role in his PhD.

Swent: They had heard of it.

Fuerstenau: Oh yes. We had submitted reports to Molycorp for that one-year project. But as I said, this was from Union Oil's research people. I think that Union Oil's research has been completely wiped out in these last few years. But it shows you about companies' attitude and outlook on the ownership of university research sometimes.

[Added by D.W. Fuerstenau during editing: The Mountain Pass Mine had been shut down for a time and has just been reopened under the ownership of Chevron Minerals. For some reason, I did not even know that Chevron Minerals exists.]

By the way, you may know, at the very northern end of Vietnam is another deposit of bastnaesite. You might recall that the Chinese went in there some years ago right after we pulled out of Vietnam. The Chinese captured the area where the rare earth mines are located. So China is also a big producer of rare earth metals.

Some few years later, I had a visitor who came to see me about a rare earth deposit that CVRD [Companhia Vale do Rio Doce] in Brazil owns. I did a little consulting work on this. They have a huge deposit with another mineral that's similar to bastnaesite, but some of the rare earths in the crystal are chemically replaced with calcium and that makes the grade of the mineral half that of bastnaesite.

Swent: What are rare earths used for?

Fuerstenau: One big use right now is making these very high-intensity magnets, in catalysts for petroleum, in glass, electronics, and for a wide number of metallurgical purposes.

Swent: It has something to do with television too. I know they're critically important.

Fuerstenau: Oh yes, yes. They are used in the phosphors for color television tubes. There's cerium, lanthanum, neodymium, yttrium—there is a whole sequence of them. Many come from bastnaesite and are recovered and separated in their hydrometallurgy plant. We were not even allowed to enter that plant. Anyway, Pradip did a beautiful job on the surface and flotation chemistry of bastnaesite and barite.

Somewhat later, we applied some of these ideas to phosphate flotation. I had an excellent student who had worked for Yosry Attia at Battelle come here to do a PhD degree with me, Doug Deason. Deason was originally from Florida and was interested in working on the complicated surface chemistry involved in the flotation separation of apatite and dolomite. With the passage of time, Florida phosphate ores have become more and more contaminated with dolomite. The problem is that dolomite and apatite each have slight solubility, and the surfaces of the two minerals begin to act the same because of dissolution of carbonate ions from dolomite and phosphate ions from apatite, with readsorption back onto the two minerals. Deason conducted an excellent study of these phenomena for his PhD thesis, supported by an NSF grant. Dealing with these mutually soluble minerals was somewhat like the problem of bastnaesite flotation. To carry on this research in more depth, I prepared a detailed proposal to the Florida Phosphate Research Institute that they approved. However, these funds come from a Florida tax on the mining industry and are administered by the Florida Department of Education, which had so many restrictions on expenditures, like accounting for number of Xerox pages, their fixed per diem for meals, that the University of California would not accept the grant. I did not disagree with that.

Research on Coal Flotation

Fuerstenau: Somewhere in the mid-seventies, I started to get interested in coal, and I figured we might apply what we do to coal surface behavior, and this started when LBL, Lawrence Berkeley Lab[oratory] gave me a certain amount of funds for a couple of years to work on coal. And with that, we did some nice work on the surface nature of coal. Eventually, I had quite a number of MS, PhD theses on coal.

Our research was really directed towards producing clean coal and mainly oriented towards coal flotation for reducing ash and sulfur. This is environmentally important since more than half of the electrical energy in this country is produced from coal and the U.S. has huge coal reserves—several hundred years of coal reserves.

Swent: I am aware of those numbers.

Fuerstenau: The first person that I took on as a grad student had a degree in wood technology, and I thought maybe he could apply some of that background to

the study of coal surfaces, and it turns out that he did a nice, nice piece of work. Very innovative person, who eventually went to HP, Hewlett-Packard, where he's got about fifteen patents there on ink-jet printing, how the ink interacts with paper and the viscosity of the ink. He told me that all the other researchers in his group are physicists and chemists, whereas he as an engineer that knows something about liquid flow, liquid interaction with the substrates, and so on, and he came up with a lot of ideas in that area.

Swent: You didn't give his name.

Fuerstenau: Young Soo You. And he first went as a postdoc at IBM, and I said, "Hey, I wouldn't do that if I were you." He did, and that's where he got his first patents on ink-jet printing. He then went to HP. He was already down there before he wrote and submitted his PhD thesis. By the way, he decided to go back to Korea to teach about three years ago, and when I was in Korea a couple of years ago, I found that he's on the government board that gives out all funding for research in Korea, South Korea. In other words, that's not minerals we're talking about. That refers to all the institutes supported by government: their Bureau of Mines and Geology is one, but all other institutes, et cetera. He may have been selected for this because apparently a classmate of his is high up in the Korean government. Young Soo is very able and innovative. But he's an American citizen, so he has to sit as an outside board member rather than an inside one, which means there's some complication because of his citizenship. But when he was a grad student here, at the end of seminars he always asked a question of the speaker no matter what the field, which meant that he was thinking. Frankly, after seminars, most grad students sit there like lumps on logs and don't ask any questions. I've often kept Young Soo You's asking questions in mind. He demonstrated way back that he had ability to put things together. His PhD thesis was an innovative study of the interaction of surfactants with coal.

Another person who did a nice PhD on coal was John Rosenbaum, who finished in '81. John Rosenbaum's father was Joe Rosenbaum, who was the chief metallurgist of the Bureau of Mines. Very outstanding process metallurgist, who was elected to the National Academy of Engineering. Anyway, his son did both his master's and PhD here at Berkeley. John's research was on the flotation of western coals, which are low in sulfur but which differ in characteristics, depending upon the mine from which they come. This research was supported by the Office of Surface Mining under the Mineral Institute program that I will talk about later. After that, they supported an interesting project on pelletizing fly ash from power plants that I had proposed. Fly ash is the large amount of fine ash particles produced when pulverized coal is burned in power plants. We collected fly ash samples from a number of power plants in Utah and Wyoming to study how the ash might be pelletized to produce lightweight aggregate. We found that ash from different power plants responds differently with regard to pellet formation and

hardening of the pellets. I thought of trying to patent some of our findings—should have, but didn't.

Fuerstenau: For one year, I had research funding for coal flotation from Bethlehem Steel Corporation and Chevron Research. After that, for quite a while I had projects related to coal flotation and coal cleaning supported by the University Coal Research Program of DOE, but after some years, they decided that their funding should be used for combustion research only, and not for studies related to coal cleaning.

So with those sources of support, I had quite a number of people working on coal, generally aimed at trying to understand the surface phenomena going on with coal, such as how surface oxidation affects coal hydrophobicity. One of my graduate students, Mark Williams, had an idea of carefully placing a few milligrams of coal particles on the surface of methanol-water solutions and determining the fraction of particles that would sink or float as the surface tension was varied to estimate the distribution of oxidized sites on the coal particles. Changing the solution composition allowed us to change the surface tension of the solution. We called this technique *film flotation* and published several papers about it and its use. Others around the world have used the technique since it is so simple.

And then came a big prospective program for desulfurization of eastern coals, which have high sulfur contents due to pyrite, and in some cases organic sulfur. So we prepared a proposal that would involve four groups working on this. We were the main contractor. This included Somasundaran at Columbia University, Weibai Hu at the University of Utah, who had come from China a few years earlier. The fourth was a small consulting company, Praxis Engineers, who dealt with coal problems. The president of that company had worked for EPRI [Electric Power Research Institute] and obviously had good connections. One of their engineers, Vas Choudhry, who had done a master's degree with Frank Aplan, was to be part of our program, because we had to collect large samples and carry out some of the work on coal weathering at the mine sites. We also included Dr. Guy Harris, retired from Dow, to work with us, making reagents to depress the pyrite.

So we prepared a big proposal. I do mean a monstrous proposal. For this DOE [Department of Energy] task, you had to have a work proposal and then there had to be a complete separate financial proposal. We're talking about each being an inch or two thick. So we prepared this and at least three of us went back to Pittsburgh for a meeting where there might have been a dozen DOE people in the room to make our presentation. And then when we came out, a couple of other people I knew—Jan Miller from Utah and B.K. Parekh from Kentucky—were waiting to go in there to make their presentation. We won this project. This was, oh, like \$2.5 million or more kind of thing. It was a big project. The object was to develop a procedure to separate pyrite from coals, for desulfurizing coals more effectively. The contract called for investigation

of specific coals from Pennsylvania, West Virginia, Kentucky, and Illinois, which are high in pyrite, high in sulfur, in contrast to western coals.

This entailed biweekly progress reports, monthly budget reports, and monthly manpower reports. Jim Hanson, who had finished his PhD about that time—doing a nice study, again, on electrochemistry of sulfide mineral flotation—stayed on as an assistant research engineer. That position was equivalent to an assistant professor, only year ‘round, not nine months, so it was quite far above postdoc. Being the bright guy he was—he figured out all these reporting requirements, and he took care of that. I’m speaking of these financial and manpower reports with projections, et cetera. Also for one year or a year and a half, three days a week Vas Choudhry came to Berkeley. He was here, and he would be writing drafts of our biweekly and monthly and quarterly reports—we also had to write annual reports. Choudhry looked after the weathering studies and washability studies that were contracted out.

Then maybe twice a year we would have visits from the project director from Pittsburgh. He brought with him one time a DOE contract person—I guess these are government contract people who actually do a lot of work for an agency but they’re not government employees—they pay them more that way, I guess. I know he was a PhD organic chemist because he questioned Guy Harris once about something that Guy had already taken care of. But he wrote me a nice letter saying—just a three-sentence letter where he said, “You know, I’ve read a lot of reports for a lot of projects, and this is the best report I ever read.” That’s almost verbatim, that quote. I have it somewhere.

Swent: The importance of those English skills.

Fuerstenau: You’re right. You’re right. [laughs] Anyway, our project involved a very tough problem because, you know, the pyrite is very intimately mixed with the coal particles, and the coal must be ground fine in order to separate the free pyrite. Unfortunately, those pyrite particles that aren’t free go along with the coal.

We did a lot of work on the effects of weathering of coal. Choudhry had to go set up a weathering facility at three different mines, where he constructed a platform for a large sample of coal and let it remain exposed where it weathered outdoors over the period of a year. Every month a sample was taken and we conducted a series of studies of surface properties and flotation response to ascertain any effects of the degree of weathering. For comparison purposes, they also stored some of it in drums under argon so that it wouldn’t be exposed to oxygen in the atmosphere. We could definitely see how the seasons affected the rate of weathering the coal and changes in surface properties. We followed what DOE wanted us to do, and perhaps we should have done a little more basic research during the course of the project.

Swent: Were you working only with eastern coals?

Fuerstenau: Right. They were just the eastern high-sulfur coals designated by DOE. Earlier I had mentioned the research of John Rosenbaum on the flotation of western coals, which had been our own idea to undertake. By the way, coal gets oxidized just from the way that it is handled around a mine. Someone from Bethlehem Steel told me that often trucks are driving over the coal at the mine and it's broken up and oxidized before getting to the preparation plant. So it may change from being fairly hydrophobic to being fairly hydrophylic, and then may not respond readily to flotation.

[Tape 23, Side B]

Swent: You were saying Weibai Hu came from China and had a lot of Chinese visitors.

Fuerstenau: Right, and he didn't pay his Chinese visiting researchers very much for his part of the program at the University of Utah. And so he would turn in these man-hour reports that we had to incorporate into our plots of man-hours, or people-hours or whatever you want to call them, person-hours, actual-versus-projected on the budget. But the actual man-hours were 50 percent higher than what was in the budget, and yet the budget was right on. I had to write a little explanation, that he had all these people that were working for him on the project, for which he was paying Chinese wages. [laughs]

So we have carried on a little bit of coal flotation studies ever since. Later Guy Harris had some ideas of making a coal flotation reagent, and we submitted a disclosure to the university for a patent, and, you know, about three days before the year went by, all of a sudden we got a call from the University of California Patent Office about the one-year deadline. Normally I never write a paper that I give at an AIME/SME [Society of Mining Engineers] meeting, but the year before the SME annual meeting was in Reno and we had made a preprint, where we wrote what this reagent was, et cetera—one year earlier. The university didn't get around to applying for the patent, nor had we gotten them to release it to us, and so here this very good reagent is unpatented, or this series that will float both oxidized coal and unoxidized coal, and it might have been potentially valuable. Guy had prepared a series of nonionic surfactants, based on tetrahydrofurfuryl esters that had a strong attraction for the oxidized sites on coal, and coal responded very well to flotation with some of these. Most collectors for coal flotation are generally just simple oils, like fuel oil or kerosene. Only a year ago, I had an e-mail from somebody in Australia, asking how to make this reagent. Well, I didn't get it answered, but when I saw him at the SME annual meeting in Denver last February, he told me they made the reagent—I was going to tell him where he could buy it—and he said, "You know, it really works." He was testing it somewhere on a plant scale or pilot plant scale in Australia. And here it's our reagent but no patent. Guy had conceived of doing this, but the university let us down.

Swent: Had they gotten the patent, does the university get the benefit of the patents?

Fuerstenau: This place has a nice approach that they discovered years ago, namely that one way to get patents is to split the royalties. Basically, if there's a UC patent, first they subtract their costs, and then the inventor gets half and the university gets half.

Swent: In this case, Guy Harris was not an employee of the university.

Fuerstenau: He would have been then. This may have dated back long enough ago. We're talking back a while now, when the work was done. But anyway, that's what UC does with relation to patents, and I think the university as a whole is probably the largest academic holder of good patents in the country.

Swent: Do you, personally, get royalties from patents?

Fuerstenau: Patents have not been high on my agenda. I'll tell you a little later if we come back to that. I've not aimed my research towards patents—I've written a lot of these patent memoranda but seldom pursued them. Almost anybody I know that has a patent has told me that they aren't very valuable. Of course, some indeed are.

Research on High-Pressure Roll Mill Comminution, and Klaus Schoenert

Fuerstenau: The last thing on research is to go back to the comminution. The last few years, I've worked on what's called the high-pressure roll mill, HPRM. My very close, good German friend, Klaus Schoenert, is the inventor of that. He was a professor in Karlsruhe where he did the initial research on particle breakage in beds, and maybe constructed his first roll mill. In 1980, he moved to the Technical University of Clausthal. It was after going there that he got the German patent for this method of comminution. You know, the university let him personally get the patent for the high-pressure roll mill. He worked with two companies in Germany to manufacture and market the mill: Polysius, which manufactures cement-making equipment, and KHD, which was a huge manufacturer of mining equipment in Germany. Those two agreed to take out patents of the high-pressure roll mill in all other countries and pay for any litigation.

My guess is that Schoenert got a few million marks in royalties while the patents were valid. One time he told me that his royalties were based only on the rolls themselves and not on the cost of the installation. He's the one academic individual that I know personally who had an objective or concept and pursued it to a major successful commercial patent in the mineral field. His research led him to ask how one might reduce comminution energy, given that this is such a huge quantity. His own doctoral research was breaking particles individually. When he became a faculty member at Karlsruhe, he had doctoral students continue work on individual particle breakage and eventually they studied breaking a bed of particles in a piston-die press. His

question was: how could you break particles in a bed continuously? And his answer was: run them through a roll mill. This differs from the old roll mill in that the pressures are very high and the particles are fed at a rate such that there is a bed of particles that are passing through the roll gap. As a result, the particles undergo a lot of inter-particle rubbing and breaking, and a lot of internal flaws are generated. Because there is direct transfer of energy from the rolls to the particles rather than the indirect energy transfer that takes place in a ball mill, the high-pressure roll mill really reduces the energy required for comminution a lot. In Germany every cement plant has, as part of their circuit, one or more high-pressure roll mills. Schoenert said that he thought one of the reasons that the high-pressure roll mill became a success was that electric power costs in Germany are the same for industry as for private citizens, in contrast to power cost policies here in this country.

My first involvement with roll mill comminution took place with a couple of Schoenert's doctoral students when I spent a sabbatical leave in Clausthal when I was awarded a Senior American Scientist Award by the Alexander von Humboldt Foundation. We will come back to that later. While I was there, Schoenert was having the shop at his institute in Clausthal build a new laboratory high-pressure roll mill for them, so I had them build the rolls, the roll housing, the gear drive, and pressure-sensing system for us when he was building the new mill for himself. Everything else, we put together here in Berkeley. My graduate student, Oliver Gutsche, was primarily responsible for getting our roll mill system completed here in the machine shop of our department. We then had our own facility which allowed several people to conduct their research with this high-pressure roll mill, starting with Oliver. These roll mills can grind with energy savings up to something like 30 percent less than that required for ball milling.

Swent: Really!

Fuerstenau: Interestingly, this whole approach to comminution was the result of fundamental research, initiated by Professor Rumpf, and carried on over a period of years by Schoenert. And, as I said, Schoenert got the initial patent, not for the machine but for the process, Polysius and KHD obtained patents for the roll mill in twenty-nine countries. One time about ten years ago, I remember seeing on the TV morning news that De Beers had recovered the largest diamond they had seen in a century. In my opinion this was because they had something like thirteen of these rolls, each about two meters in diameter. The diamond ore is run through the rolls to crush it, and what happens is the roll mill breaks up the matrix preferentially and not the diamonds. It's my personal opinion that after they installed these big rolls, they didn't break the large diamonds any more. Usually in a diamond mine, the ore is comminuted with jaw crushers and followed by SAG [semi-autogenous grinding] mills or something like that to break the ore, the same as in any other mining operation. Unfortunately that breaks some diamonds, too.

It was their installation of the high-pressure rolls that led to De Beers obtaining the largest diamond that they had seen in a century.

Swent: Because they weren't breaking them up.

Fuerstenau: They didn't break the diamonds. The diamond industry was the first installation of these roll mills in the mining industry. How to handle wear on the rolls in the mining industry was a problem that had to be solved. Most installations were in cement plants. At one time Schoenert told me that over three hundred of them had been built in Germany and installed around the world. A Danish company also produced them without paying royalties because a Danish court would not impose an injunction with regard to German patent infringement, as Schoenert had told me once.

Swent: What kind of—is it air pressure or metal pressure?

Fuerstenau: No, no, just hydraulic pressure that pushes the two rolls together. The bed of feed particles is squeezed between the rotating rolls. We had a big project on grinding coal supported by EPRI. Our approach was to grind the coal in the roll mill and then take that product and grind it in the ball mill, just to loosen the particles up again. I think in Sweden they're actually doing this now. But we did this early enough that I should have tried to patent our approach, frankly. I actually did think about doing it.

What I was about to say was, if you take coal and run it through the roll mill at fairly low pressure, it comes out as kind of small chunks, but at very high pressure it comes out looking like asphalt pavement. In other words, you can get the pressure so high that you're squeezing it so much that the coal is denser than it was in nature.

Swent: I'm thinking of this as something like the old-fashioned clothes wringer.

Fuerstenau: Sure. That's exactly what it is. Except these rolls may be up to two meters diameter and two meters in length. They are pretty monstrous things.

Swent: Where does the bed come in?

Fuerstenau: It's just fed from the top, and then it comes—right here—I may have a diagram [looks through papers]. Oh, here's Oliver's thesis. He surely must have a diagram here. Here we are. So this is our lab mill, and these are the rolls. They are only twenty centimeters diameter and fifteen centimeters wide, and are run by a 50-horsepower motor. Here are the rotating rolls, and we apply the pressure with springs that can be adjusted. We may have a load of five tons in applying the pressure. The particles enter the rotating rolls from a feeder and as they pass through the roll gap, they are compressed. If you run something like limestone through there, it comes out a cake; but if you grind quartz in the rolls, it just flows out. But I'll tell you, the rate at which you can

feed material to the rolls—and I’ve seen it happen—is limited by the rate at which air is squeezed out as the bed is compacted. This gives an upward air flow, and so if you try to feed the mill too fast, the upward air flow will just cause these big chunks to come floating up the feeder.

Most of our roll mill research was supported by grants from the Comminution Center of the Mineral Institute program that we will talk about in more detail later. We found that at high energy inputs breakage in the roll mill levels off and that a ball mill at that stage is more efficient. So we conducted a lot of research to enhance energy efficiency by first comminuting the material in the high-pressure roll mill and then regrinding that product in a ball mill. We termed this hybrid comminution. I had maybe two, three master’s theses and an excellent PhD thesis by Asoke De that were carried out on this research program. These are the very recent ones, quite recent. Then my very good former student, Prakash Kapur, was back here recently for two years as a visiting professor from India, and he got heavily involved in our roll mill research and made some very significant contributions to this field. He had major input into developing models on how particle breakage takes place in roll mills. He really was central in the development of our approach to assessing the relative energy efficiency of comminution with roll mills compared to ball mills. Kapur had a lot of input into both the experimental techniques and the analytical methods that we subsequently used in our laboratories for studying comminution efficiency. Abdel Abouzeid was another former student who spent time back here in Berkeley and carried out several different investigations on high-pressure roll milling and also on ball mill grinding efficiency.

Our thing is really to study the phenomena, the engineering science, let’s say, that goes with it. We clearly demonstrated the potential for significant energy reduction with hybrid grinding systems. I understand that Cleveland-Cliffs has installed one of these roll mills in its iron ore operation at Tilden, where the SAG mill pebbles—the hard ones—build up. This is often a problem in SAG milling, and the Cleveland-Cliffs approach is to break them with a high-pressure roll mill. I’ve heard that it works well. Since the problem of roll wear has been solved and because of the energy savings, there is increasing interest worldwide by mining companies in high-pressure roll mills today. These days the usual term seems to be high-pressure grinding rolls, or HPGR, so some of the papers that I still hope to write will use the term HPGR instead of HPRM.

But as for Schoenert, I sort of call him “Mister Comminution.” Worldwide, I think people think of him as the number-one individual in the field of comminution.

Swent: How did you come to know him?

Fuerstenau: First met him at an Engineering Foundation conference in Milwaukee, as I had told you earlier. He came to this meeting with Professor Rumpf. He’s

only a year older than I am, so we're roughly the same age and really became good friends, family friends, too. In 1969–70, he was in Berkeley as a visiting professor for half a year with his family. I'll come back to all of this interaction later.

[Added by Douglas Fuerstenau during editing: In 2006, the first high-pressure grinding rolls were installed in the base metal mining industry by Phelps Dodge in their new 108,600 mt/d concentrator at Cerro Verde near Arequipa, Peru. As described in a paper by John Marsden and others from Phelps Dodge, comminution in that plant consists of crushed ore being carried out in four parallel circuits, each comprised of a HPGR 2.4 meters in diameter and 1.6 meters roll length followed by a ball mill twenty-four feet in diameter and thirty-five feet length (7.3 m diameter by 10.7 m length). The comminution energy for the HPGR/ball mill circuit is 11.8 kWh/t, compared with 17.8 kWh/t if they had gone the usual SAG mill/ball mill comminution route. These are energy savings in line with what we had found in our laboratory-scale comminution research. In 2007, I was an invited speaker at a Peruvian mining symposium, a huge technical meeting and mining equipment show, in Arequipa, where I presented a paper on the energy efficiency of comminution. I did not visit the mine, but instead we went to Cuzco and Machu Picchu.]

Women Graduate Students

Swent: Have you had any women graduate students, ever?

Fuerstenau: A few graduate students and a few postdoctoral researchers. Two graduate students conducted their research on my hydroxyapatite project. The first of them, Cherry Chiao, came to see me, and she wanted to do a PhD, and I recall that her BS and MS were in chemistry from somewhere. She was married, had a family, and lived in Walnut Creek. Her husband was an engineer at Livermore. She was here only during the day, never on the weekend, never at night, and she did her PhD in three years. Her thesis involved surface transformations with regard to fluorapatite. After graduating, she went to work for Dow Chemical in Walnut Creek. I think that one of her daughters may already have started as an undergraduate at Cal at that time.

A second graduate student was Judy Lin, who did a nice piece of research for her MS degree on surface transformations of hydroxyapatite to fluorite when sodium fluoride is added to the system. Then the last one was Maureen McGraw; who was in the groundwater engineering program in the department. She was interested in colloid transport in groundwater, which is one way contaminants are transported through the subsoil and porous rocks. She had taken classes from me on surface and colloid phenomena, and asked me to be her thesis supervisor. For some of her research measurements, she carried them out up at Hanford, Washington, where certain equipment was available and she may have worked there earlier. After graduating, she had

some sort of postdoctoral research position at Los Alamos. She's quite a motivated woman. Recently, I had an e-mail from her stating that she was now in Missoula where her husband had a faculty position at the University of Montana. As I said, I also had several women postdoctoral researchers that might be something to talk about at another time.

Swent: I think this has been a good day's work.

Fuerstenau: Yes, I think it has been very fruitful.

X UNIVERSITY ACTIVITIES AND COMMITTEE SERVICE

[Interview 11: September 21, 2001]

[Tape 24, Side A]

Memories of the Free Speech Movement in the 1960s

Swent: I had a meeting scheduled yesterday afternoon here on the campus, and started out on the bus a little after noon, but the buses were all being rerouted by a campus demonstration, and I was sort of fainthearted and decided not to try to get over here to the campus if there was a lot of commotion going on. You said you got here all right and it brought back memories of 1964.

Fuerstenau: Yes. I guess Berkeley's infamy started with the Free Speech Movement of 1964. It turns out they had rules, dumb rules, that there should be no political tables, no solicitations, et cetera, on the campus. Somebody set up a couple of card tables, soliciting membership in some organization, which I now don't recall, but it immediately invoked this campus rule about having no political activity on the campus, which started a demonstration.

The real activities for one week took place in Sproul Plaza. I remember going down there—just absolutely packed with students, and there was a police car that had driven onto Sproul Plaza to arrest somebody, and all the students surrounded the police car. The person that the police had arrested was a student, who was in the back seat of the car, and hundreds of students surrounded the car. The car was up right near the Sproul steps. It was there all week, and as the week went by, the roof got flatter and flatter because of everybody climbing up and standing on it, using it as a podium. This particular student lived in the car for the week. You know, they handed things in and handed things out as was necessary to stay alive. The chancellor, whose name was [Edward] Strong, was the person who followed [Glenn] Seaborg as chancellor—obviously not a great leader under crisis. Never saw him at all. They had, I think, a committee of seven, a committee of senior people that sort of looked after what was going on, on the campus. I mean, an officially appointed committee. My next-door neighbor, Burton Moyer, in physics, was one of those persons. At that time, the chancellor's office was in Sproul Hall and I remember Moyer telling me that they thought that Strong must come to work through a tunnel because they never saw him outside.

One day they sent a young faculty member from our department, Alan Searcy, who was vice chancellor for research, out to speak to these thousands of students. I was down there watching. He only talked a little bit before they pulled the plug off the mike, so here he was, busy talking, but nobody could hear him. Finally on the fifth day—I guess it was Friday—the police were called in, and they arrested seven or eight hundred students who were sitting

in Sproul Hall. You know, they were packed in like sardines, about five hundred policemen from Oakland, and I suppose state police came. It should have happened the first day, but they let it go till Friday. Saturday was the homecoming football game, as I recall, so they wanted things cleaned out before the parents came. But anyway, after that, the faculty had many, many meetings. There were meetings, I know, up in Pauley Ballroom and meetings in Wheeler Auditorium. Virtually all of the faculty showed up for these meetings. One major meeting was up at the Greek Theater. At that time, I recall Mario Savio had come running or walking onto the stage and grabbing the microphone, and the police ran in and picked him up, and about six of them carrying him out in a prone position.

What is interesting if you look at pictures, everybody wore neckties and short hair. I'm speaking of the student leadership. It's not what you see around campus today. Then students had neckties on and clean shirts and all that sort of thing.

But there was meeting after meeting, and it was always interesting because a lot of presentations were made by eloquent speakers on the left and a lot of eloquent speakers on the right, often the same people speaking at the different meetings. There were good people on each side of the issues. Of course, I think what you might call speakers on the left probably outnumbered speakers on the right. I remember one of the eloquent ones on the right was George Pimentel from Chemistry.

Swent: What was his attitude?

Fuerstenau: Oh, I don't know quite how one would put it. I don't know that anybody was in favor of controlling speech, except for defining time, place, and manner. A major point was keeping things under control as far as operations go. Up in the Hearst Mining Building area, where the engineers and the sciences are, you never saw any demonstrations or disturbances. But if the helicopters were circling over, then you knew something was going on down at Sproul Plaza.

Swent: But your classes continued?

Fuerstenau: Oh, all of my classes went on as if nothing was happening, as far as I can recall. But I often learned what went on that day from the evening national news. Here I was, right on the campus, with nothing significant going on at our end of the campus, but Berkeley made the daily national news for weeks, months. I remember walking past Wheeler Auditorium one noon, and maybe about seven, eight people were standing around, and a couple of news people with big TV cameras, just right in on the center, but on CBS or NBC that evening was a close-up of this small group. In other words, the news people were trying to make news. But finally, at a very major meeting in Wheeler Auditorium, the Academic Senate passed with a big majority the resolution

for what really amounts to free speech, under the proper outlook on “time, place, and manner.”

By the way, one summer some years later the International Geophysical Union meeting took place here in Berkeley, with delegates from all over the world. I went to listen to a talk in Dwinelle Plaza being given by the keynote speaker which was Adlai Stevenson. He commented that when he was running for president, 1952, he gave a talk in Berkeley, and the audience was on the grass along Oxford Street near the campus entrance. The listeners were there, but Stevenson had to stand across the street on the sidewalk to give his talk, due to these University of California nonpolitical rules. And he said, “And here I am today, talking in the very middle of the campus.”

But then, just going on ahead—again, during the Vietnam War—I remember when the Cambodian Incursion took place, spring of ‘70, the place was really bombed with tear gas bombs a lot. I recall walking through Sproul Plaza on the way to lunch and tear gas was dripping off those trees. One of our daughters was in fourth, fifth grade down at Franklin School, down near San Pablo Avenue. Tear gas got into the school, clear down there. [Ronald] Reagan was governor, and he really was gung-ho at controlling things. That was really bad business when maybe a mile and a half away, the tear gas got into the school where these little kids were.

So once again there were many, many demonstrations. By the way, I can recall walking out from a meeting in Dwinelle Hall about four o’clock one day—and I walked over to Sproul steps to see who the speaker was. The speaker was Martin Luther King. And at that time, his speeches were anti-Vietnam War with the message that money should better be spent on people here in our country to overcome our own problems, rather than wasting it over there. Of course, it ends up he was right. He talked about an hour without notes or anything, just gave an excellent, full-of-fervor talk.

Swent: Was there a big crowd?

Fuerstenau: Oh, a big crowd, of course. It was obviously a scheduled talk, not in the way of a seminar or anything, but scheduled for Sproul steps. I don’t think that I knew it was going to be there, but just kind of wandered by at the right moment to hear him. By the way, in those days, living here in Berkeley, you thought the whole world was coming apart. I used to go over to Salt Lake [City] quite regularly or to other places, and it was a whole different world. We were deluged with kind of anti-war, anti-this, anti-everything. You thought this was how the whole U.S. felt, but it was just a microcosm here. It was not at all like that in Salt Lake City, which I visited every month or two, on a regular basis.

But eventually—probably in the early 1970s, roughly, was really the last of that. Then any demonstrations in the last decade at Berkeley have really not

been much of anything. This one yesterday—I have no idea what went on there, but a pretty small percentage of the total people here were involved. Again, everything in Berkeley always has a lot of non-student participants also.

Swent: And you felt that it was students from another section of the campus.

Fuerstenau: Oh, totally. It always seems to be. I think what efforts there have been at demonstrations in the last ten years have probably been planned and those behind it tried to work out what they want to accomplish. I don't see the spontaneity that seemed to take place thirty-five years ago.

Service on the Statewide Academic Senate, Campus Prize Committee, and the College Undergraduate Study Committee

Swent: In 1964 you were chairman of the College of Engineering Undergraduate Study Committee, and you were vice chairman of the Department of Materials Science and Mineral Engineering at that time. You didn't go on the campus Committee on Budget and Interdepartmental Relations until '66, but you've been on some very significant committees.

Fuerstenau: I can tell you a little bit about some of the committees. The very first committee I became a member of was called the Committee on Prizes.

Swent: That was a statewide university committee?

Fuerstenau: It was campus. Well, let me back up. Somehow for a while there was as statewide Academic Assembly, I think it was called, and I was made a member of that, and I found that really quite interesting. That went on for a couple of years—met once maybe in Berkeley. I remember going over to Davis, and another time we met at UC Santa Barbara, also at UC San Diego. It gave one a nice opportunity to meet different faculty. Mike [Ira Michael] Heyman, who became chancellor, was then a member of that assembly group. He was in Law and City [and Regional] Planning. And Professor Washburn from Anthropology. David Krech from Psychology. Dick Jennings from Law. It was quite a wide variety. You know, Heyman is slightly younger than I am, but all the rest were far more senior than I. But it gave a nice introduction to different faculty members.

The first committee I did get on was the Undergraduate Study Committee in the College of Engineering. Having worked in industry, especially Union Carbide, which seemed to operate under a lot of committees, I thought, well, you know, you serve on committees; and you're really doing something big and important. It took me a little while to realize that serving on a committee at Berkeley, at least in my opinion, isn't a big thing in your career.

The Undergraduate Study Committee approved courses, looked to make sure that people met graduation requirements, things like that. Maybe there were petitions from students with regard to exceptions. You know, students had to have not only a grade-point average of 2.0 to graduate—that's a C average—but they also had to have the number of units and the total number of grade points—in other words, as a minimum the total units times two. I remember once in a while somebody was short a grade point or two, and they may have to come back, take another course, and make sure they got a B out of it to get a GPA [grade point average] of no less than exactly 2.0. I think I ended up being the chairman of that committee for two or three years. At first I thought this was really a big-time thing here serving on this college committee, but, as I say, I began to realize that committees aren't necessarily a main item in career evaluation, so the committees I served on were the very important ones.

Then I got on the Prize Committee, which sort of sets policy related to various prizes awarded to students on campus. We didn't read the poems or essays and do the judgment, but for the dozens of prizes in arts and literature and poetry and philosophy and areas like that on the campus, we set policy and made sure that there were people who would do the judging, and so on. But anyway, I served on that several years and was chairman a couple of years, and that one I found very interesting because I got to know very well some of the humanities faculty. One was Andrew Imbrie, for example, who's a composer, a faculty member in music. I used to sit at lunch with him every now and then and recently I read a big item in the *Chronicle* only last week about his eightieth birthday. He had an opera about gold mining days called *Angle of Repose*—the music being quite far-out modern. Another was Joe Kerman, musicologist, who has written many books and has given some great lectures on music that we went to. I got to know him when he was on that committee.

The first year I was on the Prize Committee, the chairman was Tom Parkinson in English, and we met at his office because the committee consisted of only five people, maybe six at most. One time during that year when he was meeting in his office with two of his grad students, a deranged former student who lived in Oakland, Piedmont, came in with a shotgun and killed one or two of the grad students and got him on the side of his head, face. I often shudder. We could have been meeting there.

Swent: Oh my.

Fuerstenau: You would have read about that in the newspaper when it happened, but I only remember it because we had been holding our meetings in his office. After that, the previous chairman of that committee, Werner Goldsmith from mechanical engineering, again took over running the committee for the rest of that year. Anyway, I was a member for maybe a total of four years and also chaired it at least two years. But the beauty of that is it's a way of interacting with people that normally an engineer might not do.

Service on the Committee on Budget and Interdepartmental Relations

Fuerstenau: Of course, the committee that everybody on the Berkeley faculty always knows about and seems to think of it in terms of a cloud hanging over their head is called the Budget Committee. I was on sabbatical, and I got a letter from the Chairman of the Committee on Committees asking me if I would be willing to serve on this committee. They said this is very difficult, time consuming, so they would ask ahead of time if I would be willing to do that.

Swent: You were on it from '66 to '69.

Fuerstenau: Yes, three years. I got this letter while in London. You know, I wasn't very old then, either. I got back in the summer, and we may have had an occasional meeting in the summer, which is more or less to take care of mopping up stuff because the main activity really gets going in the fall and winter.

The chairman that year was somebody who I've gotten to know much better in the years since. He was a history professor named Delmer Brown²². I guess you people did an oral history of him about a year ago. It turns out we've really become quite good friends because about ten years ago he married a good friend of my wife, the friend being much older than us, but his first wife had died, and he married Margaret. I still to this day remember the first presentation I made to the committee, which concerned somebody getting promoted to tenure, and I had written and in the presentation to the committee stated that "this individual shows great promise and therefore deserves promotion to tenure." And Delmer Brown said, "Young man, we appoint on promise, and we promote on performance." [laughter] You can tell I haven't forgotten that. Delmer Brown is now about ninety-two, and he doesn't look any different than he did in 1966, and even a year ago had another book published on Japanese religious history. When you look at him and talk to him, you think, wow, he's seventy-ish, but he's now ninety-one, ninety-two, and still got fire, and so on.

This committee handles all appointments, promotions, and interdepartmental relations, and so on, and I gather in more recent years, interdepartmental allocation of resources. More effort is spent on that than probably was thirty-five years ago because resources are getting tighter, obviously. By "resources" I mean academic slots and that sort of thing. Now any allocation of resources is taken up by the Budget Committee. I kept track of the time I spent. It was something like thirty-five hours a week in the late fall and early winter that I spent down in the committee office. Since I did not want to give up mornings, I'd work there on cases in the afternoon and maybe Saturdays. Meetings were

²² Delmer M. Brown, *Professor of Japanese History at the University of California, Berkeley, 1946-1977, 2000*

held in the morning once a week and then twice a week in the busy time of the year.

Swent: You were carrying a teaching load in addition?

Fuerstenau: Oh yes, everything else was the same.

Swent: Hmm! That's a huge amount of time.

Fuerstenau: Yes, yes, it really was. This was the time I put on it, because one really makes judgments on the performance and careers of people. Each committee member may serve or handle several different departments. For example, I naturally handled cases from engineering, but the first year, which was really the heavy one, too heavy, I had to take care of environmental design, business administration, and I think geology and maybe chemical engineering. The largest number of cases came through me and my engineering predecessor. Then they added another person to the Budget Committee, just to make those loads a little more even.

[Tape 24, Side B]

Fuerstenau: There would always be somebody maybe from English or other areas in the humanities, one from the social sciences, one person from the science of either physics or chemistry, one from engineering, and of course someone related to biology; in other words, you covered the faculty spectrum.

Doing this, I found that I could make the judgments on anybody in any field, to ask myself, hey, are they doing a good job or are they not? You don't have to be an expert in a specific field to understand if somebody is really doing the research. I spoke up on my opinion.

I found the same to be true during all my years there with Homestake, that I could make a judgment about somebody being a mine manager if they were doing what I would call an innovative job as a mine manager or just kind of a pedestrian employee. You don't have to know how to do the job yourself, but you can, at least in my opinion, make those judgments. You don't have to abstain on an issue.

Swent: How did you make these judgments in the case of a faculty member if you didn't know the person?

Fuerstenau: Well, for example, the chairman of a department in one of the humanities fields wrote that a professor had written sixteen unpublished volumes as evidence of the research that he had done, but none of it had been published, and I said, well, it's not research till it is published. You can't give someone credit for doing something like that, which I firmly believe. If it wasn't of the

quality to be of use in his field, it really isn't the kind of effort on which a merit increase should be granted.

Swent: How did you evaluate the teaching?

Fuerstenau: Everybody's a good teacher. Every person's a great teacher. It's kind of funny. I recall in one area, professional area, not engineering, the comment that "he's not very good in the classroom, but he's excellent in seminars." I don't know how many times you'd read "X was excellent in seminars." Okay? So everybody's a great teacher. [laughs] At that time there were none of the formal evaluations that we discussed earlier. Today you pass these out to students, and I think the raw data has to go forward on promotion cases, not just summaries.

Swent: There were no student evaluations in those days?

Fuerstenau: No. I think the student evaluations probably got started as a result of some or all of the demonstrations and things about improving teaching, improving this and that, with interaction with students.

Swent: Was there any pressure to put a student on the Budget Committee?

Fuerstenau: Well, that's impossible, absolutely impossible. The work of the committee involves everything completely confidential about the professional careers of the faculty. In my opinion, members of the committee should be persons who are very competent in their own field and also be capable of making judgments about professional performance in relation to the campus as a whole and not just in terms of what a particular dean might be suggesting. So I expressed my own opinion over a wide range of matters during committee discussions. As I said, I did this for the three years, and I knew everything about everybody, all about appointments of X, Y, and Z, people I'd never met but I still to this day remember lots of details about a lot of people, and sometimes I meet somebody sometime, and I remember when they got appointed.

This committee again provided a nice way for really good interaction with a few professors from other academic fields. One spent a lot of time in the Budget Committee office doing the homework necessary in writing up a case for presentation at the committee meeting. There usually was another committee member or two doing the same thing, so naturally there would be breaks where one would relax a bit talking about things in general. One person that I found very interesting was a very famous person in sociology named Leo Lowenthal, and Lowenthal became a member of the committee just after he retired, which is kind of an unusual time. It turns out that we served there together for two years, and though he was a professor of sociology, he was a very good ally for cases in the professional schools.

You know, in the humanities and sciences, teaching, research, service is what promotions, and so on, are based on; but in the professional schools it can also include professional competence. In other words, an architect is not going to be writing papers—well, he might write some, but the professional competence would be based on the buildings he designed, or landscape architecture could be lighting or some such. And an engineer might also be assessed, of course, on professional competence in design.

I found that Professor Lowenthal was a very good person on the professional competence part of a case—an ally, let's say, on this. Lowenthal had been head of what was called the Frankfurt School of Economics in Germany, and in 1933, when Hitler got elected chancellor, he said he took his whole school and moved out, and I think he said first to Switzerland, and then to Columbia University for a while. Eventually he came to UC and was here at Berkeley for decades.

I think there's a very detailed oral history of him. It may be in German because the Germans may have done it, but he was a very great person. He died not very many years ago, '90-something. But even a couple of times later [after being on the committee], he called me up and we went over and had lunch at the Durant Hotel. I'd had that kind of interaction with him.

Swent: And he was on the committee after he had retired.

Fuerstenau: For five years. He served longer than anybody ever served on that committee, and when I learned that he was going to be appointed, I commented that it doesn't seem the right thing to do to appoint somebody who's retired because you're looking at the future. But it turns out he was a superb choice.

All during my time, Jack Raleigh, professor of English, was on the committee, and he became the chairman the third year. Raleigh, after that, was vice chancellor for academic affairs for several years. Raleigh, I got to know quite well. Anyway, Raleigh told me—oh, this was a handful of years ago—that one time he was talking with Lowenthal, and he said, "Well, you know, you served five years on that committee. What's your opinion of who were the best committee members you worked with?" And Lowenthal said, "Dave Templeton," who was a chemistry professor, became dean there, quite a bit older than I am, but we overlapped a bit, and yours truly.

Swent: A real compliment.

Fuerstenau: Yes, yes. That's not in writing anywhere, but—well, as I told you—I felt that I was able to make judgments on all sorts of different aspects that the committee was concerned with.

Swent: And that, of course, is a key committee.

Fuerstenau: Oh, it is the key. I know how people used to—and I suppose still do—fear the Budget Committee, but I came away saying, “Hey, you know, 80 percent or 90 percent of your welfare”—I’m speaking of one’s welfare on this campus—”is self-determined.” In other words, it’s the performance of the individual faculty member that determines his welfare, not the Budget Committee. As I see it, a faculty member at Berkeley is on his own personal ladder. It’s not like in Europe or maybe other institutions in this country, where there’s kind of a hierarchy. Here, the way I read it, one competes only against himself, and I am not competing against any other professor in the department, so how well I do in this system is determined by my own productivity.

As I’ve told you, I was dedicated to the teaching I did, but research counts very heavily, as I think it should. You hear about places like this, “publish or perish.” I just happen to be a firm believer in that and published a lot of papers and still do. I think all of that determines one’s welfare, and not the Budget Committee. Department chairmen have a certain influence, depending on whether they aggressively push somebody ahead.

Service on the Committee on Committees

Swent: You were on a lot of committees.

Fuerstenau: Yes. And that does not include the dozens and dozens of doctoral qualification exam committees and thesis committees or departmental committees. But I’ll just tell you a little bit about a couple of others. An important committee at Berkeley is what’s called the Committee on Committees. You’re elected to it, okay? So I agreed to let my name be put forward. I think you have to get a half a dozen seconders, and the nominators usually go around to a variety of departments and so on to get supporting signatures. After that there’s an election. The first time, I didn’t get elected, but I was the first runner-up, I guess. Let’s say there might be eight, ten names and maybe there’s four or five new people, or six new people every year who become new members on the Committee on Committees. Anyway, somebody went on sabbatical leave, so they said, “Well, now, you’re the runner-up, so you’re on the Committee on Committees,” so that’s how I first got on, whatever year that was.

Swent: You were on twice. You were on 1970 to 1972 and ‘85 to ‘87.

Fuerstenau: Okay. That first time was the result of what I’m just talking about, so I served two years in that. This is a committee that appoints members to all of the other committees. Each member is assigned three, four committees, I guess, for which they are to seek out new membership. So it turns out in the spring you propose names of potential members that the committee discusses, and if they agree on these names, you get on the phone and try to convince the professors to serve on the committee. That’s when it gets kind of busy.

Swent: You were also chairman of the department at the same time.

Fuerstenau: Is that right? I would have been, right. So anyway, that committee has that function. It also has another function, suggesting names for administrative committees, such as review committees, or committees to search for a dean. Let's say they want five people to be a panel to review something. Well, then, you give them ten names.

I remember one time there was just kind of a Mickey Mouse panel to review somebody's animal farm in psychology. It kind of amazed me the big names committee members would throw in as suggestions to be on this panel. And I remember saying, "Hey, look, all we're talking about is a monkey farm. You don't want to waste X, Y, Z for this sort of thing."

There are some schools around here that have ten faculty members, but that means because they are a school they then have a dean, and you may have to come up with a search panel for a new dean. And again, people start throwing out big names, and I remember one time saying, "Hey, look"—we're talking about a dean of a faculty of seven, eight, nine people. Here you have one of the major departments on the campus, electrical engineering, with seventy-five faculty, and the chairman is appointed by the dean. So, on a number of times I would just try to keep in perspective the relative importance of some panel memberships. The chairman of the Committee on Committees is assigned one committee to handle, and that is the Budget Committee.

Then, as you said, in '85, I guess, I stood for election again, and was elected. At that time, [Chang-Lin] Tien was on the Committee on Committees. It was kind of interesting times because he was made vice chancellor for research during that time, and there often is no research vice chancellor at Berkeley; it's usually handled by the graduate dean. Alan Searcy was the first one when I was here, and he was followed by Loy Sammet. Then there was another one. Then there was none until they appointed Tien to be vice chancellor for research.

At that time, it came out in the papers that there was a little bit of a retraction on admission of students of Asian heritage to Berkeley. And two judges got actively involved with the issue, and Tien, of course, being Chinese, was asked to look into the matter. I think that was kind of the beginning of catapulting him into the administration. At that time there were at least three of us who were engineers, which included John Prausnitz from chemical engineering, on the committee. It's good to have engineers there because that always determines some engineering representation on other committees.

Service on the Campus Research Committee

Fuerstenau: Then the other committee that I got involved with a few years later was the Research Committee. I served on that several years, and chaired it for at least two or more years. Tien then was the vice chancellor for research for at least one of the years. And one of the main activities the first year I was chairman of the committee—

Swent: In 1984 to '86 you were chairman of the Berkeley campus Committee on Research.

Fuerstenau: Yes. During my first year as chairman of that, the state really got heavily involved in enforcing the conflict-of-interest rule, and I think it was San Diego and UCLA that just kind of whitewashed the rule, and the state really got tough about it. There was a committee set up to review every proposal that might have conflict of interest by principal investigators. I spent a lot of time on that, as chairman, with this other committee, where we had to sit and make a judgment and sometimes call in the professor to explain his personal financial role. I remember somebody had Exxon stock. Well, how many shares of Exxon would you have to have to have any influence if Exxon gave a research grant? Or somebody whose oral history you did had a lot of research projects from PG&E? And of course, he owns some PG&E stock, but a few shares would have no influence on anyone's research.

I think it was as low as if you collected a hundred dollars on a seminar from that company, you had to include that, an amount in the economics of that day was way too small. We had to go through all of this. It wasn't related to grants from NSF or other government agencies, but it was related to corporate grants and contracts. There was a piece of research I wanted to do on milling gold ores by pelletizing the ground ore and heap leaching the pellets, and I still would love to do that, and Harry Conger said, "Well, send me a proposal." Well, you know, I never did it because I knew even though, in my personal opinion, this would have been completely ethical—I wanted to study the engineering principles involved with this idea—but it would be pretty hard to convince anybody that a board member of Homestake had this grant from Homestake without any conflict of interest, so I never submitted the proposal.

That was a time-consuming role. Another thing we did during this time was dispense campus research funds, both for small grants and some equipment grants. Often people thought of research equipment in terms of technical people, but we ruled that people in the humanities could buy computers, and so there might be a group of two or three—remember, this is quite a while ago—who jointly in a department might buy a computer for word processing. That all got started during my chairmanship.

[Tape 25, Side A]

Fuerstenau: Just one other aspect. The Campus Research Committee again is a nice committee where one met and worked with different people. At that time it was mostly technical, including biology. Maybe today there may also be humanities faculty, due to what we were just talking about. I recall getting a call from the chairman of a search committee down at UC Irvine, saying, "I'm chairing the committee on the search for an executive vice chancellor, and our final candidate is Tien," and he said, "You're the last person I'm calling. We'd just like to know what Tien's outlook is towards the humanities." I, of course, gave good words because I guess there had been some concern that he, as an engineer, might not have an open outlook towards the humanities—anyway, I gave him very good words, and he became, as you know, the executive vice chancellor there, which eventually led to his coming back to Berkeley as chancellor.

Some Other Committee Assignments

Fuerstenau: Then just before I retired, I was on the Graduate Council, which handles graduate courses, graduate matters, and that's a very, very large committee. It looked like thirty, forty people, and it seemed so big that I found it rather dull to participate in it, and when I retired as a professor, I even dropped from continuing on the Graduate Council.

Swent: That was '91 to '93 when you were on that.

Fuerstenau: Yes. In my opinion there just were too many members. It's just too unwieldy, and you get discussions that go in different directions that may not be right to the point, and I found it unsatisfying.

Swent: You were the chair of the graduate review for the Department of Chemical Engineering. That was '88 to '90.

Fuerstenau: The graduate dean makes the appointment. All departments on the campus are reviewed by the graduate dean, they hope about every ten years. In other words, what is the status of the graduate program, what's the quality of it, whether there are any weaknesses or problems, and so on. And twice I was involved with the review of our department, that I can remember. But for the Department of Chemical Engineering, I happened to be the chairman of the review committee. Chemical Engineering here is a very strong department, and so it was kind of a pleasure to do that job. For example, we talked individually with faculty members about their assessment of things and their problems.

I remember meeting with the graduate students, and if you can believe it or not, one of the complaints of the graduate students was that there weren't enough foreign students. I don't know what it's like now, but at that time, let's say Berkeley ranked about third in the country in chemical engineering,

graduate programs. Now they worked up to number two. Anyway, they had 95 percent American students, 5 percent foreign. And I know that University of Houston at that time ranked about tenth in the country in chemical engineering graduate programs, and they were 95 percent foreign students.

In the rough draft of the report, I had written in there—"Well, if the students feel that they haven't enough interaction with foreign students, they ought to come take a few more courses in the College of Engineering." One engineering prof, on my initial draft, wrote in the margin, "Low blow." Well, it never got in the report, but that's what I had put in the initial draft.

That was a nice review of the department, essentially without problems except for space. These reviews can serve a good purpose, such as suggesting the need for additional faculty, more space or something. Interestingly, since then Tan Hall was built and [the Department of] Chemical Engineering occupies most of that space.

I had a letter a few weeks ago, asking me if I'd come over to be part of a panel on the graduate review of the Metallurgical Engineering Department, University of Utah, which I will do later on this year or in January, I guess. So I'm still doing some of that.

Chairman of the Faculty of the College of Engineering

- Swent: You also were chairman of the Department of Materials Science and Mineral Engineering, and Mechanical Engineering, and you were also Chairman of the Faculty of the College of Engineering. Those both sound pretty important.
- Fuerstenau: The chairman of the department is a little more lengthy, and real work.
- Swent: That was 1970 to '78. That was eight years.
- Fuerstenau: I'll tell you a little bit about what's involved in chairman of the faculty of the College of Engineering.
- Swent: That was later. That was '83 to '84.
- Fuerstenau: That was later, yes. To me, that is not an onerous job. One chairs college meetings, of which maybe there are two per year. Then at that time one was also an ex-officio member of the College Alumni Board. During that time I was also involved with a committee that rewrote the bylaws for the Berkeley Engineering Alumni Association and for the Berkeley Engineering Fund. This was chaired by Lou Oppenheim who had been executive vice president of Kaiser Engineers. Another person who did a lot of work was a vice president of Kaiser, Vic Cole. Did you ever meet him?

Swent: I didn't meet him. I corresponded with him. I know the name.

Fuerstenau: After he retired from Kaiser, he actually taught construction here, maybe a course in construction engineering once a semester for several years. Very active in the College of Engineering. I still have the draft—it's on the desk at home—of these rewritten bylaws. It's that kind of thing that the chair of the faculty of the college did.

The other duty is to serve as the marshal of the commencement. You know, there are about five thousand people up there in the Greek Theater, and I sort of entered into this with a little bit of trepidation, but I'll tell you, within about five seconds, I just fell right into the ceremony. The marshal leads the whole commencement, but all remarks have been prepared and are there in a big book at the podium. You introduce the dean and then you introduce each department chair, the student speaker, that kind of thing.

I did that for two years, and then the person that succeeded me broke his leg, so I think I did this a total of three times, and I enjoyed it. And still to this day, I told somebody else in our department, Ron Gronsky, who has the job right now—I think the first year he did it, I said, "Be very careful."

Dick Goodman, in civil engineering who founded the Berkeley Opera, would do the singing, starting with "The Star Spangled Banner" or whatever. I know he sang something at the opening, and I was reading from this book, and I said, "Now come join me in"—now, this was on the loudspeaker to five thousand people—"Join me in singing 'Hail to California.'" Well, I don't know "Hail to California." I said, "Hey, get Dick get up here quick."
[laughter]

I hadn't been looking ahead. This was the very last thing that the marshal had to do in the program so I've told other people, "Be careful of"—but, you know, I made sure to go through the presentation beforehand and say out loud to myself certain names. In other words, there might have been a student speaker with a difficult name—like mine—and you make sure that you don't see it for the first time and stumble pronouncing it.

Swent: Full academic regalia?

Fuerstenau: Oh sure. The BS, MS, PhDs, and some departments, like civil engineering, give almost as many MSs as BSs. Then with the doctoral people, each advisor puts the hood over their own doctoral students. The doctoral people get to sit up on the stage, which is nice, because they're in the shade, and the poor BSs and MSs are down there in the hot sun in their black robes.

The speaker at one of those commencements was Robert Noyce, who was one of the inventors of the integrated circuit, a founder of Intel. Being chairman of the faculty, I was at a lunch for maybe eight people, the dean and the speaker

and a handful of others. So each of those occasions provided that kind of opportunity to talk with the speaker.

Noyce had a brother who was professor of chemistry here at Berkeley, and he also got an invitation to be the commencement speaker at the College of Chemistry. Noyce said that his secretary didn't keep them separate and he was unaware until a few days earlier that he had two commencements. So it turned out that he was the speaker at the commencement for [the College of] Engineering at the Greek Theater in the morning and there again in the afternoon as the commencement speaker for [the College of] Chemistry. It was obviously the same speech, morning and afternoon. Noyce went to Texas to head that organization called SEMATECH, which is a research consortium of semiconductor companies. Shortly thereafter at age sixty-two he had a heart attack and died, swimming in his swimming pool. All his billions weren't there for the long haul. Interestingly, Jack Kilby, an engineer with Texas Instruments independently did the same thing. So these two are both called inventors of the integrated circuit. But that's totally out of my field.

Swent: I see that you were also on the advisory committee for the Hearst Foundation. Are you still on that? Phoebe Apperson Hearst Foundation advisory board, 1978—

Fuerstenau: Yes. Let's come back to that. That relates to our mining activities.

Scientific and Education Advisory Committee on Lawrence Berkeley Laboratory

Swent: The LBL [Lawrence Berkeley Laboratory] advisory committee.

Fuerstenau: The president of the university had a committee called the Science and Education Advisory Committee to advise him on the status of LBL.

Swent: Let's see, Chancellor's Advisory Committee on Science Research Policy. Would that be it?

Fuerstenau: Oh, that was just probably a one-year ad hoc committee to advise on policy about non-secret research, research funding policy, that should be the campus policy.

But as for SEAC, which was Science and Education Advisory Committee on LBL, this was a committee of the president of the university. We reported to the senior vice president of the university.

Swent: That's universitywide, for the whole university system.

Fuerstenau: Yes, the UC president not the Berkeley chancellor.

Swent: This was '84 to '92.

Fuerstenau: That's a long haul. The chairman of SEAC was Dr. Thomas E. Everhart, dean of engineering at Cornell and a long-time friend. He had been professor of electrical engineering here at Cal for twenty-one years. He became chancellor of the University of Illinois at Urbana, and recently retired after ten years as president of Caltech. There were two faculty members from Berkeley and maybe a total of ten people, and they were from all around the country. One person during the latter three, four years was Eugene Meieran, who was manager of packaging, I think, for Intel; he was a metallurgist by background who initially worked on materials aspects of making computer chips. By the way, Gene Meieran has one of the largest collections of major gem minerals and precious metal crystals in the country. His collection is museum quality, and photographs of his displays appear quite often today in gem and mineral magazines. Most of the other committee members were physicists, nuclear chemists, and later biophysicists. We met twice a year for a two-day meeting, where we would be given highlight presentations of some of the major research programs that were going on at LBL, plus a long presentation by the director of LBL.

Each also included an interesting tour of some of the facilities and so on. The object was to give our opinion to the president on the programs and status of LBL. SEAC was kind of like a board of directors of a company, to which only two people are really responsible, the chairman and the president. With SEAC, one duty was to advise the university president on the performance of the director.

Swent: Do you feel you had any effect?

Fuerstenau: Without a doubt—because there was a Materials Science Division, so Gene Meieran and I would be involved with any assessment related to that division, as I remember. There also was an Earth Sciences Division. Some of the concerns of the committee were just general policy anyway, so you don't really need to be, again, a detailed expert to assess some of what was happening.

For a number of years, the major thrust at LBL was the establishment of a very major facility called the Advanced Light Source. A big issue one year was safety matters, an issue throughout all of DOE and the national laboratories. Towards the latter part of my tenure on SEAC, a major focus of LBL was a human genome project; but, I don't think it ever became the major contributor that they thought it was.

Swent: Were there any changes made as a result of your committee's recommendations?

Fuerstenau: Well, there was a director change. There were recommendations or concurrence on closing a cyclotron facility, which was not down my area of expertise, but there were people on the committee who thought that even though this group did good things, that the same task was being done elsewhere and could be carried on there, and made that recommendation. That was eventually followed.

To provide UC oversight of all three national laboratories, Los Alamos, Livermore, and [Lawrence] Berkeley [Laboratory], a single scientific advisory board was established for all three laboratories, one large board in itself, so the SEAC program was stopped. The other laboratories had a Science Advisory Committee, and Berkeley being on the campus with a lot of graduate student research, its oversight committee was called the Scientific and Education Advisory Committee. So now there's one super committee and I know nothing about it.

Somebody who was on the committee at least half of the time I was, from Berkeley, was Jud King, who is now the senior vice president of the university. I think he was Berkeley provost at the time he was on SEAC. He and I were the two Berkeley people. I don't know, but he still may be involved, now from the UC president's office, with these matters.

The establishment of this broader committee was to put more effort from UC into oversight of the labs. About ten or fifteen years ago, UC was taking a lot of flak over these labs, and they weren't getting very much money for managing them. Twelve, fifteen million dollars was the management fee then while getting all the bad press that you would read about in the newspapers. For an organization as big as UC, that's not that much money, but I gather from the newspapers that things will be changing.

Swent: Were there students doing any work from your department?

Fuerstenau: The materials part of this department had a very major part of LBL, and that included all of the ceramics and physical metallurgy research that went on at one time—and I don't know the extent today. I think that was a real detriment to the department because people gave their main allegiance to LBL, which was their source of research funds. They could spend a short amount of time on writing the proposals there for hundreds of thousands of dollars per year, whereas you might spend the same amount of time for a few hundred dollars doing things here. So people oriented themselves in large measure to LBL activities, which I have always thought was a big detriment to this department, something that completely controlled much of faculty efforts. I think it did harm.

There's now less—I think due to budgets and redirection that may have taken place up on the hill [at LBL], there's probably less support through that than there used to be. But, you know, there are faculty you only see down here

when they come to give their class, and since their labs and research facilities are up on the hill, they are up there most of the time. Frankly, I think a lot of graduate students in our department miss the academic experience of being a graduate student at Berkeley because interaction down on the campus is missing. Like I say, you look at the faculty office and often nobody's around because they're up there on the hill. Not very good.

There was no such activity when I first got here. All research was funded by various agencies and carried out in Hearst Mining Building. Funding through LBL started about my second or third year here at Berkeley. Around the country about a dozen so-called ARPA [Advanced Research Projects Agency] laboratories were established for metallurgy and ceramics research at various universities, and Berkeley's research was to be supported by the AEC materials division, or whatever it was called, through LBL. The administration of the new research organization was carried out for the first year or so in Hearst Mining Building, and people just got so carried away and engrossed totally with it. Just like a new ice cream cone that they were handed. Prior to that, everybody lived off of grants and contracts from army, air force, navy, et cetera, NSF.

But those faculty that got connected with the Materials [Sciences] Division of LBL, whatever it was called, had to be sole-sourced from there, and so all of their research funding came from LBL, and it was a nice thing, but it just oriented people's allegiance to that.

Swent: So it's on the campus, but it's not really a part of the campus.

Fuerstenau: Right. All those activities up on the hill are actually on Berkeley campus land, but it's not really part of the campus. And, of course, the heyday there was when high-energy physics was in its boom.

Swent: The cyclotron.

Fuerstenau: Right. And the Bevatron, when they were cranking out Nobel Prizes up there on particle physics research. Of course, none of that is there today. It's still a big operation, but with a different orientation. I now no longer know what the activities really are up there; this human genome thing was to be a big percentage, but I have no idea what their biology program is today. As for materials, I would think that their main activities should be oriented towards corrosion and radiation damage in metals, but I think neither are a high priority there, if anything at all.

There are significant efforts related to energy conservation in some sense, and then there's a major geotechnical program; Neville Cooke always was a good part of that, namely contamination from ground leakage of stored radioactive wastes.

[Tape 25, Side B]

- Swent: You were saying there was as fairly significant amount. You didn't know what it was in percentage, but—
- Fuerstenau: Yes. Today I don't spend any effort on what their programs, and so on, might be.
- Swent: Is George Brimhall's program up there?
- Fuerstenau: No. But Paul Witherspoon, in this department, who worked on groundwater problems, was heavily involved. Another faculty member in this department, T.N. Narasimhan, was a staff member there in the hydrogeology area before becoming a professor. I remember hearing him give a talk on the movement of a plume of radioactive waste from some of the mine tailings in Wyoming—you know, following the uranium that's in the groundwater, studying the movement of this. Narasimhan another time gave a seminar on the Yucca Mountain storage facility proposed in Nevada, which was part of the LBL program and I recall his believing that a model predicting ten million years of storage there was valid. When I asked him about that, he fully defended his model. Witherspoon, who was a professor in this department, long retired now, was director of the Earth Sciences Division at LBL for quite a while. I think the one time he gave a presentation to the SEAC committee, I happened to miss the meeting; I was somewhere else. As I already mentioned, Neville Cook was quite heavily involved with nuclear waste storage and worked on such problems as the prolonged effect of the heat generated on rock structure. His efforts started with a big program of nuclear waste storage at the Stripa Mine in Sweden, also with rock behavior at Hanford, Washington.

XI SOME POSTDOCTORAL RESEARCHERS

Swent: Well, shall we move on? I've got several things here that we haven't mentioned: the Arbiter process and—

Fuerstenau: Keep going.

Swent: And postdocs. That's another thing we wanted to talk about. Pretty dear to your heart, I think.

Fuerstenau: Yes, let's talk a little bit about that. You know, postdoctoral researchers are an important part of research programs. Some came with their funding, but most I supported, and I had many different ones through the years. I'd just like to tell you about two, three of them. As I said, the very first person was Vladimir Nebera, whom we've already talked about, from Russia for a year in 1960-61.

Tom Healy

Fuerstenau: But in 1961—no, I guess in the fall of 1960, a young student and his wife came through Berkeley on their way to Columbia University, and they were from Australia—Tom Healy. Professor Victor LeMer had gone to Australia, and had offered him a fancy fellowship to come do a PhD in chemistry at Columbia. When I was at Kaiser, Healy had written to me, asking for a reprint of a certain paper, and I wrote and said I was going to be moving to Berkeley in the fall. I was fishing a little bit, and they stopped on their way, and I called up Peg, saying, “You know, there's this nice young couple here. Let me bring them home for dinner.”

I did that, and I remember broiling chicken in the backyard with martinis, and Beverly—they had just gotten married and had come by boat and had gotten pregnant, and I guess she was sick halfway across the ocean, first to Vancouver and down to San Francisco. Peg told her, when they got to New York, to look up her sister-in-law, which they did and became good friends and helped in finding an obstetrician and that sort of thing.

So we kept in nice contact with Healy, and in '63 when he finished his PhD, I proposed that he come here as an assistant research engineer, which he did. He also held the position of lecturer in the department. He was here two years, and was totally a really outstanding right-hand person. He had a BS in chemistry from University of Melbourne, and a master's that was called metallurgy and chemistry, so he fit in very nicely here.

He had worked with LeMer on the effect of polymers on the flocculation of suspensions for his PhD thesis, and he really became a very integral part of

my activity: for example, on the behavior of colloidal particles, I told you about this seminal piece of work that everybody refers to as Hogg-Healy-Fuerstenau theory; and involved with surfactant adsorption in flotation along with Somasundaran, and with the work that I was doing on the manganese oxides and how colloidal behavior might be responsible for nodule formation.

I'll tell you a funny little story. Healy tried to make manganese nodules by putting in a rotating cylinder, a glass cylinder, some colloidal manganese oxide. He was rotating this under certain chemical conditions—and this was running for several days—and he thought by agglomeration it might be possible to make manganese nodules. Well, I had a student—by the way he never did finish here—who took this rotating cylinder and put in a couple of manganese nodules, little ones, and, boy, was Healy excited. He had made manganese nodules!

Swent: [laughs]

Fuerstenau: Oh, he was truly excited. Then he discovered that this student, Dave Murray, had put the nodules in the cylinders. I don't think Tom ever forgave him to this day. It was so funny to see his excitement, how he had by colloidal flocculation made the nodules. Dave Murray, for family reasons, wanted to move closer to his wife's home in Iowa and transferred to the Colorado School of Mines, where he did a nice thesis with my brother Maurie on using phosphates for extracting copper ions from solution.

Anyway, Healys and our families are still close friends, much interaction to this day. The Australians established what are called Queen Elizabeth Fellowships, and I think there are a handful, maybe ten of these given per year. They're designed to bring good people back to Australia, and he was one of the persons to get one the first year these came out. I was already starting to think in terms of our having him here as a regular faculty member in this department. So he returned to Melbourne, eventually became professor of physical chemistry at Melbourne, became department head, dean of the School of Science at University of Melbourne, and then became chairman of their Academic Senate, which is not like ours at Berkeley where the position is just kind of running committees, activities and policies. What they call chairman of the Academic Senate in Australia is kind of like being virtually the number-two person in the university, and he did that for two years. Now retired. Retired early, but he's still very active in research, and really has become one of the big giants of colloid surface chemistry, with a lot of interest in minerals, and in flotation and so on. Very rare. You won't find a great many chemists, let's say, in this country that would go in this kind of direction. He's really one of the byword names in colloid chemistry worldwide.

I think I had some influence on his technical orientation, too, during these two years, when we worked on electrical double-layer problems, flocculation of

colloids with surfactants, surfactant adsorption, and flotation. As I say, we have maintained much interaction, been good friends, family friends. Tom's been here a couple of different times for more extended periods. Anytime he comes through here, he gets actively involved for a day or two working with some of my students, and he'll dig right into their problem and project. Very marvelous kind of person. I've visited them in Australia and in Melbourne many times through the years. The first time was in 1969, when Tom arranged for my being invited as a plenary speaker at the IUPAC (International Union of Pure and Applied Chemistry) congress being held in Australia. There my talk was on mineral-water interfaces and I met the great Russian surface chemist, B.V. Derjaguin.

[Added by Douglas Fuerstenau during editing: Tom Healy has received a great deal of recognition for his achievements, including election to the Australian Academy of Science and also the Australian Academy of Technological Sciences and Engineering. In addition to several other awards, he received the Ian Wark Medal of the Australian Academy of Science (Sir Ian Wark was one of three early giants of flotation chemistry that included Gaudin, Taggart, and Wark). Healy has been a plenary lecturer at countless meetings worldwide. He was a member of the board and also governor of the Ian Potter Foundation, the largest philanthropic foundation in Australia. We were with the Healys in June 2005, when it was announced on the Queen's birthday that he had been awarded Officer of the Order of Australia. Only about ten persons were so honored, whereas well over a hundred persons were recognized as Member of the Order of Australia. And now he has just been named the 2008 winner of the A.M. Gaudin Award of SME and elected as a foreign associate of NAE.]

Some Other Postdoctoral Researchers and Their Role in Research

Fuerstenau:

Then, you know, there have been a fair number of other people who worked with me as postdoctoral researchers. One that did a lot of very good work was totally different, Takahide Wakamatsu, who came from Kyoto University in Japan. He was here '66 to '68, and his contribution, early on, was some superb research work that I had him do on the effect of hydrocarbon chain length and pH on the adsorption of organic surfactants on alumina from water. He really followed on from what Soma had done for his PhD thesis a couple of years earlier. Wakamatsu's research, which also included measurement of electrokinetic potentials and contact angles, really further substantiated hemimicelle phenomena in these types of systems. I consider what he did here to be classic in explaining surfactant adsorption behavior. For example, I gave a seminar in Brazil this May—here it is, 2001—and included some of Wakamatsu's results.

Wakamatsu returned to Kyoto, and his two years with me really got him in position to become professor in mineral processing there. They wrote to me

several times when they were promoting him through the years. He became the major figure, because of his position at Kyoto, in mineral processing in Japan. Now retired. At University of Tokyo, they must retire at sixty; the number-one university of Japan is Tokyo, and then number two, but some say it should be reversed, is Kyoto; these are state universities. At Kyoto and Tohoku, professors must retire at sixty-three, and then what happens in that they go to some private university until they reach age seventy.

Several times my own students just stayed on a year or two, like Ken Han was back here for a while as a visiting researcher after he had gone to Monash University in Australia. Another one who had been here for extended periods several different times is Abdel-Zaher Mohamed Abouzeid, who's here right now from Cairo University. He's been with me about six times, and most of those times, he has been involved with our research on some aspect of comminution.

Another one of my former students that came back, again about five times, is Ronaldo Urbina from Mexico. Excellent person. He's professor at the University of Sonora. His spoken and written English is absolutely beautiful.

Swent: This is Herrera-Urbina?

Fuerstenau: Yes. He's Mexican, so he's got to include his mother's name. He was Urbina when he was here, but now he's always Herrera-Urbina. My brother Maurie gave a seminar course in Mexico City around 1980, and two Mexican students applied to come as grad students to South Dakota: Alejandro [López] Valdivieso and Ronaldo Urbina. And you know, Maurie said that the two arrived in Rapid City in shirt sleeves in January.

Swent: Oh my!

Fuerstenau: [laughs] Yes. They got a rude, quick awakening. Apparently neither could essentially speak English. But they're really A, A-plus students, both of them, and they both got master's degrees with Maurie at the South Dakota School of Mines and Technology. Both came here and did PhDs with me, and both are now teaching back in Mexico.

Well, Ronaldo is one of those persons who is a natural in languages. He just must have some kind of aptitude for language because I used to give him manuscripts to edit, and we have written a lot of joint papers and chapters in books. I could take his draft and it wouldn't require much work to have it in form for publication. He has a thorough background in the fundamental surface chemistry of flotation. His research involved study of the chemistry in the flotation of chrysocolla. He also carried out some interesting research on the flotation chemistry and electrochemistry of oxidized lead minerals. Most of his later visits have involved writing papers and review chapters. Two or three times now, he's been visitor in France at the University of Amiens, and I

had an e-mail from him last—oh, this past spring, and he said they wanted to have him give a course in France, in which he would lecture in French. [laughs] We're talking about a Mexican who started with Spanish, so clearly he has real facility with language.

Another one of the big giants is Prakash C. Kapur, who's been back a few times, not as postdoctoral researcher but as a visiting professor or visiting research engineer. When he has been here, I have had him work closely with some of the graduate students working on comminution. Kapur is an expert in mathematical modeling and over the years has made a number of valuable and innovative contributions to the mathematical models of grinding and the nature of product size distributions, let alone pelletizing and flotation circuits. Also, Kapur really knows how to organize and enthusiastically present material in seminars. He has contributed numerous new ideas to various aspects of our research program on comminution here at Berkeley. But I suppose the two main people who came here shortly after finishing their PhDs were Healy and Wakamatsu. I've had many other visitors and postdoctorals through the years, some of which contributed significantly, and some others, less.

Postdoctoral researchers should be a good, major part of one's program. It's good for the students to have somebody like that around, you know, someone who can help work with them and maybe advise them a bit and so on. Not everyone is that way. Sometimes a postdoc just likes to crawl in his own cubicle. I can recall a couple right now who would rather do the work themselves than interact and advise, meet somebody else in helping carry out a piece of work, even though I would tell them, "Hey, look, you know, one of the aspects of your role is to learn how to lead other people" and so on.

Swent: You hope that they'll go on into teaching?

Fuerstenau: Not necessarily, but this is true throughout industry or any kind of organization—some of them may have been in government agencies.

Swent: You still have to instruct other people.

Fuerstenau: Sure, sure. Oh yes. Of course, as we all know. If you want to go through life as an individual, you're not going to accomplish a great deal, are you?

Guy Harris and His Contributions at Berkeley

Swent: We have wanted to talk about your relationship with Guy Harris through the years.

Fuerstenau: Yes. Guy Harris certainly falls into the visiting research category—he's not a postdoc in any sense. I think he got his PhD from Stanford in 1941, after

graduating from Cal in 1938, and is now long retired from Dow Chemical Company, but he still comes in and works with us almost a day every week on making flotation reagents, this sort of thing, or just discussing some of the work. For several years now, he's worked with one or two of my students who might be working on some flotation-related problem. Quite remarkable for somebody who's about eighty-seven, I think, in age, and still is proceeding.

Swent: He was working for Dow; how did he establish a relationship with your department? How does that work?

Fuerstenau: Before he did this, he actually spent a few months or more, maybe only a short time, with Frank Aplan at Penn State, where they investigated the behavior of a lot of Dow flotation reagents. Dow had exited the business and Guy arranged for a large number of research chemicals that Dow had on hand to be given for study at Penn State. Having known Guy through the years and seeing him at technical meetings, he had said, "If you ever have any project, I'll be delighted to work with you" or something, and then when we started writing this major proposal for coal flotation and desulfurization, it all of a sudden occurred to me that we ought to get him involved, and so I incorporated his role in the proposal as a visiting research engineer and in the budget for preparing various kinds of reagents, and so on. I think it actually helped us sell DOE on giving us the project. All of this was long after his retirement from Dow. He was sixty-eight when he retired from Dow, and that's almost twenty years ago. But his knowledge of organic chemistry and organic synthesis, et cetera, is obviously superb, and that was of great value to us. In contrast to my objectives, Guy always looked at potential patents and when he made reagents for us, he thought in terms of costs so that they might be useful in industry. I really should have brought Guy into our program years earlier.

Swent: It's good for the students to have someone like that.

Fuerstenau: Oh, I think so. I think so.

XII SOME ASPECTS OF FAMILY AND PERSONAL LIFE

Swent: Now we need to go back to talk about some of your family and personal life during these years when you came here in 1959. You had at that time just two children.

Fuerstenau: Two children, and in 1961 our third daughter, Sarah, was born in January of '61, at the Kaiser Hospital in Oakland. Then my wife started to get concerned about our oldest daughter, Linda. In all the pictures I have, she was squatting. Might be outdoors on a sidewalk, or in the sun, and I remember we went that Easter to Tahoe, and I have pictures of her on the beach, and she's squatting. [pause] Now, she cried all the time. [pause]

Swent: We'll postpone this.

[tape interruption]

Fuerstenau: We went to the opera on Tuesday night, and—just the circumstances of our tickets—went to the San Francisco Symphony on Wednesday, and it was a Mahler concert. The second part of the concert was Mahler's *First Symphony*. They said they were recording it. You could see the cranes with a number of microphones. They said, "Try to be quiet" and all that. It was just a superb concert. In today's *Chronicle*, if you read the review, the writer of the review just raves about the performance of the symphony. We knew it at the time, so someday that will be a very outstanding recording.

Anyway, the first part was Mahler's *Kindertotenlieder*, *Songs on the Death of Children*. Apparently he took some poems written by somebody—[pause]

Swent: That must have been hard.

[tape interruption]

Fuerstenau: Anyway, we were up there at Tahoe, and Linda cried, and so on Monday we came back and Peg took her to Kaiser, to the doctor and left her there, and the doctor said, "Come back in the evening," which we did. This pediatrician was an excellent woman. We sat down with her. She said, "Well, you know, your daughter has about two weeks to live." What she had was called neuroblastoma, which I had never ever heard of. It's a cancer in the abdominal area, just a childhood cancer. And she said they never see it beyond about age five, and some babies, once in a while, are born with it. They operated on her, and the pediatrician said she was there during the operation and the tumors were just growing like moss on the inside of her, and they couldn't take it out, of course.

Linda responded to chemotherapy, and Peg took her sixty-five times for x-ray treatments that summer. You could see an area where her skin turned brown, where they had x-rayed her just about every other day. You saw that picture where she looked healthy and happy. That was taken during the summer, the one in the window.

And then we were down in Monterey, around Labor Day or something, and she started to limp a little bit, and then—it was recurring and spreading.

Swent: So it did take a lot longer than the two weeks that they predicted.

Fuerstenau: Oh yes. She responded to the chemotherapy. In fact, she had her fourth birthday in the hospital. She was exactly four and a half on the day when she died. She was born April 28 and died October 28.

Swent: That must have been a terrible time.

Fuerstenau: Right about then, that book called *Silent Spring* by Rachel Carson came out. When we lived in Buffalo in that apartment complex, in the fall they sprayed for crickets in the basement heavily, and Peg to this day thinks that maybe that spray when she was first pregnant may have been what brought this about. I had never ever heard of neuroblastoma. You hear of it now.

[Added by Douglas Fuerstenau during editing: I read in the *San Francisco Chronicle* that in 2001 the journal *Epidemiology* reported on a study that pesticide use around the home can more than double the chance of a child developing neuroblastoma.]

But then the incredible thing is a grandson of Tom Healy has it, and he was diagnosed with it at about eighteen months, and that's more than two years ago, maybe two and a half years ago, so you can see that there's progress being made. As you know, thirty, forty years ago, if a little kid had leukemia, they were gone, and today there's a lot of survivors. But it's just such a pathetic thing. You just don't know where that comes from or why, especially with a little child like that.

Swent: Three years.

Fuerstenau: Like I said, at age three Linda could tell time. I still have some lecture notes, which I've kept—I told you I redo them all the time—but I've got some pages where she drew a man or a person on the back side of the page my lecture notes, and you can see that it's a person—face and eyes and hands. She was only three years old.

Swent: That's remarkable.

Fuerstenau: She wasn't being told how to do it. She was sitting, apparently, by me when—I still keep those.

Swent: Of course you do. So she had your ability to draw, too.

Fuerstenau: Hey, I guess you're right.

Anyway, just another final comment on that—

[Tape 26, Side A]

Swent: Sarah was born in January.

Fuerstenau: January '61, and, you know, I guess a mother can sense when there's something just not quite right. It just started to evolve with Linda. You know, she talked to the doctor. "There's something. She's just not quite like she should be."

But anyway, one time, and I don't recall the incident, but I must have gotten very distressed when I was first telling people, and—well, you can imagine how it was. And a professor here, Earl Parker, said, "I haven't seen a grown man cry before." Just a few months later, his oldest son, who was then twenty-eight—with two friends was sailing at night under the San Mateo Bridge, crashed into a barge, and his son was drowned and killed, this very person.

And then there's another person about whom I want to comment in relation to how people deal with this sort of thing. It would have been the following spring; another professor in the department, Irv Fatt, had a son who had just finished the ninth grade, fourteen years old—the class went over to Marin for a school outing, and the son was in the swimming pool and I guess trying to see how long he could stay underwater, and died. It may have been his heart. I think he didn't die from drowning. So when we heard this, we called up Helen Fatt, the mother. Helen Fatt said, "Well, you know, when Linda died, I didn't know what to say, so I didn't." Of course, we called and said, "We're sorry" et cetera. A lot of people just don't know how—

Swent: They're afraid to talk about it.

Fuerstenau: They don't know that the people who are survivors might appreciate—well, I think you probably know—appreciate comments from people.

Swent: It doesn't matter what they say, just say something.

Fuerstenau: That's right. But I still recall Helen Fatt saying, "I didn't know what to say, so I didn't." Then, again, the same kind of tragedy hit their family.

Then, of course, finally, in '63, our son, Steve, was born. It's nice he was a boy because we didn't look at him as a replacement. But I'll tell you, you sure worry, until they all pass age five, about what might happen. As they finally all get older, that all goes in the background.

Swent: This must have been hard for Lucy.

Fuerstenau: I think it changed her personality. She was very outgoing, and I think it did. I can see it. Not that she isn't an outgoing person now, but I think it did. We talked about it then.

Swent: She's the one who's a nurse now?

Fuerstenau: She's a nurse and very much looks after other people. She's an emergency nurse, likes it, and she's very concerned. Of the people that I've talked to over this last tragedy of the New York towers, she was more concerned and more emotional about it than anybody because she knows what this can be like. In fact, she was saying, she was on duty when they had that huge traffic wreck on I-5—you know, occurred on Thanksgiving a few years ago. Or the next day she was there. She said sometimes when a single person comes in from an accident or something, and if they're not busy, they get really detailed treatment and so on, but when you're involved in such a major tragedy, if you know somebody's going to die, you just take care of them in a minute or two, then put them aside and put the efforts to those you know are going to survive. She really, I could see, feels for the people. Plus her husband is a fireman.

Speaking of firemen and tragedies, Peg's brother called up some woman that used to work with him at Bessemer Trust in New York, and this woman lives in Brooklyn, and she said the fire station right near her home lost a truck and everybody on it, and at another fire station somewhere near them in Brooklyn—because across the Brooklyn Bridge you're right down there, at that south end of Manhattan—the entire fire company was wiped out on September 11th.

Swent: Terrible.

Fuerstenau: Yes. Those were people going to help other people.

Speaking of tragedies, I know you talked with Frank Aplan, and incredibly I called up Frank by chance the very afternoon that his little daughter had swallowed the mothball. I caught him just by pure chance about an hour later, and he was still numb and it just didn't even hit me what had really happened there. Then Peg and I, with our kids had gone that afternoon down to Monterey or Pacific Grove, just for a weekend or something, and bought a local paper, and there was a short column in which they mentioned Monroe, New York: "Child Dies From Swallowing or Choking on a Mothball." It was either that night or the next day.

- Swent: Was that about the same time that Linda—?
- Fuerstenau: Well, it was a little later, of course, but not much later. I mean, it was a year or two maybe.
- Swent: But you both went through a similar thing.
- Fuerstenau: Right, right.
- Swent: You never get over those things.
- Fuerstenau: You don't, but they go in the background, don't they?
- Swent: Well, you have to keep on.
- Fuerstenau: I think that's very important.
- Swent: Plus when you have other children, you have to carry on for them.
- Fuerstenau: Yes. By "go in the background," I don't mean immediate. But eventually tragedy goes into the past—it has to, or we couldn't face going on. When you think about it, it comes back up, especially with these things you can't figure out.
- Swent: I think this last week has stressed everybody more than anything we've gone through for a long, long time.
- Fuerstenau: We'll have to see where all of that goes.
- Swent: I guess we should probably stop now.
- Fuerstenau: Yes, all right.

Some Additional Comments on Committee Service

[Interview 12: September 24, 2001]

[Tape 27, Side A]

Swent: September 24, 2001; we're continuing the interview with Douglas Fuerstenau in Berkeley, California.

Last time, we talked about the committees, especially the Budget Committee that you were on, but you had some more things that you wanted to say about that.

Fuerstenau: During that first year or two that I was on the Budget Committee, the vice chancellor for academic affairs was Bob Connick, Robert Connick, professor of chemistry, a man with real principles and straightforwardness, and it's kind of a pleasure to watch him act. Connick is the younger brother of Phil Bradley's wife.

Swent: Kay Bradley.

Fuerstenau: Yes, Kay Bradley. The first time I met Wayne Hazen, he told me he was a classmate of Bob Connick, so they both must have graduated in chemistry from here in 1940, I guess, with their BS degrees, Hazen in the same class with Connick. During my first year on the Budget Committee, several times Connick would meet with the committee if he disagreed with a recommendation. He did this more often than his successors.

I'd just like to also throw out something about how the Budget Committee enters into ordinary merit increases. In one measure, this results from the University of California having very rigid steps through all of the ranks. An assistant professor has four steps of two years each, and associate professor normally in three steps of two years each; I think they may have inserted a fourth step; sometimes for people who are slow getting promoted, they throw in a fourth step. Likewise in the full professor range. When I was on the Budget Committee, there were three steps in the full professor, and then maybe about 5 percent of the people would be over scale. Today there are now nine steps in the full professor ranks, which takes them up pretty high relatively, and still apparently about 5 percent of the people are special salary or over scale. In regard to the special salary, that is treated as a promotion, a fairly difficult step to achieve.

Most other universities don't have such a system. I remember hearing about cases in other major universities where assistant professors might be making as much as some full professors. In other words, the dean's discretion determines the pay, and it almost means that everybody is negotiating his own

pay in those universities. I think this system that's here in UC as a whole is really very good, and I think quite unique. The Budget Committee gets involved every time there is a merit increase, going from one step to the next, so that is what adds a lot to the load. Whenever the university is granted funds for general increases, say 5 percent, the scale for each step is increased by 5 percent.

I'd also like to make a comment about times that pay was politicized around here. I can still remember that when [Ronald] Reagan was governor, one year everybody in the state got a 5 percent pay increase except teaching professors in the university. In other words, the secretaries here or the staff, anybody with a non-academic title got a pay raise, but the faculty didn't. That is really injection of politics into—

Swent: And this was done by the governor?

Fuerstenau: Reagan. It was probably those protests around 1970 over the Cambodia business. I think that was roughly when it was.

Swent: Did the governor have that kind of authority?

Fuerstenau: You wouldn't think he would, but I suppose he laid down the law. He is a member of the Regents, of course. I don't know how often he attended meetings. I know he attended his first one when he fired Clark Kerr as president of the university, which was when he was first governor. That's the whole system of having the Board of Regents being between the politics of the state and the university.

Swent: At a social affair over the weekend, someone was telling me that the University of California is unique in this, that it is owned by the Regents and not by the state, and that this is what makes it in fact more like a private institution, and this is how they are able to negotiate government contracts and have contracts with state and federal governments, because it is not technically owned by the state, but by the Regents.

Fuerstenau: I never heard the word "owned," but I think we're saying roughly the same thing. If I write out a check to anything at UC, it goes to the Regents of the university, even for football tickets.

Swent: And this keeps it at arm's length from the legislature—

Fuerstenau: Right. Supposedly.

Swent: —which is not true in other states, some other state universities.

Fuerstenau: Semi-arm's length, as I told you. [chuckles] By the way, when Reagan became governor, regents prior to that had sixteen-year terms. In other words,

the length of term was so long that it would go beyond any given governor, but—and I think Reagan was behind it—it was reduced to twelve years, which is still a long time.

By the way, during the FSM movement and all the problems around here, the chairman of the Board of Regents was our friend, Don McLaughlin, who I think spent a lot of time wringing his hands over seeing what was happening to his old alma mater. I didn't know him very well then, but I knew he was chairman of the Board of Regents.

Swent: UC is also a land-grant institution.

Fuerstenau: Right, right.

Swent: So that gives it a federal mandate, doesn't it?

Fuerstenau: There are some attributes of being a land-grant university. When Mike Heyman was chancellor here at Berkeley, one year he was president or chairman of the National Association of State Universities and Land-grant Colleges, by the way. Shortly I want to talk about a Mineral Institute program, and that was tied to a land-grant institution within the state, so I made good use of that. We'll get to that later.

Along this line, too, you probably heard all about this from Maslach and others in administration, that several generations ago, several key faculty objected to what the president of the university was doing, and they pushed through the situation where the Academic Senate of the University of California has major say in policy matters, et cetera. I think there were six major professors who led this sort of revolt, one of them being the famous geologist Andrew Lawson, and another was G. N. Lewis, the very famous physical chemist here back then.

One last little thing I'd like to mention about committees, since committees are important in the Academic Senate, in the college, even in departments—it's interesting about service on committees. Some of the very active, high-level faculty around here don't seem to serve on any committees, and I know some of it may at times hurt them a bit, and I think they're wrong about ignoring committee service. People should do a certain amount of service, as I told you I did, on certain selected committees. Then again, there are other people who make a career out of it, especially if they are not particularly involved in research.

Swent: You have to strike a balance.

Fuerstenau: Oh yes. This is right. In other words, university promotions, merit increases are supposedly based on "teaching, research, and service," and some people devote most of their total effort to research.

XIII**MILLER INSTITUTE PROFESSOR, AND VISITING PROFESSORS**

Swent: Would you like to talk about the Miller Foundation? You were on the budget committee '66 to '69, and then the Miller Foundation.

Fuerstenau: Actually I never served on the Miller Foundation Board. However, here at UC Berkeley, as I remember, Alfred or Albert Sprague Miller Foundation gave obviously a large endowment many years ago to establish what's called the Miller Institute; a certain number of professors can be appointed Miller Institute professors, plus there's maybe each year ten or twelve very high-quality postdoctoral fellows.

I remember one time I looked at the list of Miller Fellows, and I saw, for example, that Carl Sagan was a Miller Fellow about 1960, so he would have been here as a postdoc under this program.

Anyway, the establishment of Miller professorships under the Miller Foundation was an absolutely outstanding idea. A Berkeley professor can write out a research proposal as an application, and if he was awarded a Miller professorship, his salary was paid by the Miller Institute. It used to be that most of them were for a year; but now, I think, because of high costs, a lot of times they're half-year appointments. The idea was that one's pay would come from the Miller Institute, and then your academic pay could be used to bring visitors to teach your courses and work with you on your research.

That's the way it was when I had this in 1969, but from what I hear today, the dean takes the money, and often only a bit of it comes back to the department to hire a replacement teacher. I would think somebody would want to press that matter, but I know that in the College of Engineering, anyway, the dean uses that for funding other temporary people, where he thinks the greatest need is, which I would think is a violation of the original concept.

Swent: So you were named a Miller Professor.

Fuerstenau: Right. I applied some years earlier, and I was very angry about it. The part that distressed me was the department chairman let three applications go forward. I didn't get it, and I don't remember if either of the other two did or not. I was for several years not happy about that. The department chairman shouldn't forward three applications. You ought to look ahead and let people do some planning, because there's only maybe eight or ten of these in the whole campus per year. At least then. Now I think people kind of look at it as a way to spend the year doing research.

Appointment as a Miller Professor, 1969–1970

Fuerstenau: I received an appointment as a Miller Professor for the '69–'70 academic year. During the time that I had it, we had here four different high-quality people, all foreigners, on my pay, which says our pay was a little better than in other countries. They taught courses that I normally would have, freeing my time for research and discussions with them.

In the fall of 1969, I had Klaus Schoenert from the Technical University of Karlsruhe come to Berkeley, and as I mentioned before, he's the big expert on fracture and breakage and comminution. He taught our undergraduate course that we called "Particulate Materials." And then I suggested that he have a seminar, a two-unit seminar each week on fracture physics. I said, oh, there would be maybe a half-dozen people there, and Klaus told me when he entered the room for the first time, there were thirty or forty people sitting there, and he said, boy, was he taken aback. He said he never worked so hard in all his life as preparing those fracture physics lectures that he gave. Even though he considered his field to be fracture physics, he had never organized a course on that before. I attended every one of those seminar lectures.

Of course, English is not his primary language. In fact, some years ago he told a funny story. In that undergraduate class, he was lecturing and apparently in the middle of the lecture slipped into German and didn't know it. Then at the end of the hour, he said, "Well, are there any questions?" I guess somebody told him, "Well, you know, you gave the lecture in German, so we don't know." [laughter]

I had a lot of research money in those days, and so I also had each of these visiting faculty spend another—we were on quarters—another quarter in our research group, paid out of my research grants and contracts, so they were each here six months. Schoenert was helpful on some of our comminution research and also some ideas on compaction in agglomeration. And so in the winter—

Swent: Did you have sole jurisdiction over who was invited?

Fuerstenau: I did.

Swent: It was your choice.

Fuerstenau: Yes, yes. But I had to write out a detailed letter requesting the appointment as a visiting professor, including initiating visa applications. I think Schoenert and then [Tom] Healy were visiting associate professors. Tom Healy, from the University of Melbourne, who we've talked about earlier, came to spend six months beginning in the winter. He taught my graduate course, Surface Properties of Materials, and really participated a lot in our research on surface phenomena in surfactant adsorption and flotation.

Then I invited also Ron Ottewill from the University of Bristol, and Ottewill is now retired as a professor, but today is basically considered the top surface colloid chemist in England, if not the world. He was then a reader in physical chemistry at Bristol, and he taught a course here that we called “Applied Colloidal Phenomena.” After that, I then organized such a course and maybe taught it every other year. Ottewill also dug in, worked in fact very closely with the master’s thesis of Osseo-Asare, who I mentioned earlier. Osseo a while ago told me that this association with Ottewill is what oriented him from pursuing physical metallurgy to mineral processing and hydrometallurgy.

[Added by Douglas Fuerstenau during editing: Ottewill gave a series of lectures here in 2003 in a course taught by Fiona Doyle, and sadly in June 2008 died at age 81 from cancer.]

Then, also in the spring but for the quarter only, Sam Levine, who was professor—or reader, I guess—in mathematics at the University of Manchester was also here. He probably had written to me to see if we might have funding to support him for a term. He gave a two-unit seminar course on the theory on the electrical double layer, the area that had I worked on for my own doctoral thesis. I attended each of his lectures that quarter. Ottewill was teaching a regular course, but I had Levine do this special seminar course. Levine was far more senior than the other visitors.

So here we had these four internationally known people teaching classes and interacting on research. Three of them were here six months, and Levine, the three months. That’s the beauty of this Miller Professorship, that it allows you to bring people that can work with you on your research and teach your classes. As I said, apparently that is no longer the case in that the engineering dean today takes the money and uses it for temporary faculty where he feels there is the need, and that’s too bad.

Other Visiting Professors on Later Occasions

Fuerstenau:

Also, through the years, I brought to Berkeley some other visiting professors. Ernie Peters, from the University of British Columbia, who worked with me for a year and a half at Union Carbide in Niagara Falls was an important one. He, along with Milton Wadsworth at Utah were really the two top people in the world in the field of hydrometallurgy, leaching ores by acid or bases, et cetera. When Peters came here, I had him present a course on hydrometallurgy. I know Osseo-Asare told me this course by Peters really directed him into hydrometallurgy. There were maybe fifteen people in that class. He gave a term paper for the final, and I remember him telling me, “You know, all of these term papers are better than any papers I’ve ever seen before.” I said, “Well, what do you mean?” And he said, “These are just better students than what we see up in British Columbia.” That’s a direct quote from

him. I found that interesting because the University of British Columbia was well known for hydrometallurgy. Two or three times I had gone up to Vancouver and given seminars. The department head was Professor Frank Forward, who was the inventor of the famous Sherritt-Gordon process for autoclave leaching copper-nickel-cobalt ores. By the way, some time later, Forward spent three years as the science minister of Canada.

Another person that some years later I invited here to give a course on hydrometallurgy was Nat Arbiter. This was probably half-a-dozen years later, and Arbiter's course would have been much more applied than what Ernie Peters would have taught. But the students liked the material that they got from Arbiter because he presented and discussed a lot of plant practice.

Swent: Was Arbiter still at Columbia?

Fuerstenau: Oh no, he was probably retired then from Anaconda, where he had gone from Columbia. He lived in Tucson and came once a week and maybe gave his lectures in a single afternoon. It was probably a two-unit course where he'd give two hours of lecture and maybe on some occasion he may have skipped a week and then was here a couple of days. We somehow were able to arrange this.

Another person that I invited here was Professor Janusz Laskowski, who was a professor of mineral processing in the Technical University of Wroclaw in Poland [formerly Breslau in Germany until 1945]. This was 1982, and he had his visa to come to the United States, and he said they didn't know whether to give his wife and two kids visas or not, and that went back and forth, and finally just the day before they were to depart, the Poles gave them an exit visa. They were here only a few weeks when it all hit the fan in Poland, and the Russians came in. You know, this was the time of that Solidarity movement.

Swent: When was that?

Fuerstenau: In 1982, I think. So he was here either half a year or longer, maybe a year, and worked with us a lot on coal flotation because he was an expert in coal. In fact, his father was director of the big coal research institute of Poland in Katowice. While he was here, Laskowski got a job as professor of mineral processing in the University of British Columbia, where he's been ever since. He dug in and worked very extensively in our beginnings of coal research. There are several papers that we've written together on coal.

There are two or three other visiting people I'd just like to mention. Bob [Robert J.] Hunter from the University of Sydney was here on a sabbatical, and he taught some of the lectures in my Surface Properties and Materials course. During his time in Berkeley, he wrote a book on electrokinetics, entitled, *Zeta Potentials in Colloid Science*. Few people seem able to dig in

and write a book and could put their nose to getting it done. He did that while here. He later wrote a fairly comprehensive two-volume book, *Introduction to Colloid Science*. I found that he incorporated a few of the ideas and approaches that I had in my lectures into his books.

About that same time. Dr. Geoffrey D. Parfitt, a surface chemist who had been a lecturer at Nottingham and then research director for Tioxide International, was unemployed and I called him up, asking him if he would want to come here to teach my course, Surface Properties of Materials, while I was on sabbatical leave in 1980. He did that and then stayed for the spring quarter and taught my course, Applied Colloidal Phenomena. During that time, he got a position of professor of chemical engineering at Carnegie-Mellon University and built up an active program there. Unfortunately, he died of a heart attack at age fifty-six.

Finally, I'm very fortunate to have had Professor J. Th. G. Overbeek spend a couple of months here. I remember at the time, he was seventy-five, and I had to write a letter stating that he was in good health and all that, to get through the Budget Committee approval process. Not only did Overbeek give these superb lectures, but several hours a day he met with students from this department and other departments, including some of them several times. By meeting with students, I mean one on one, discussing their research. Just a truly remarkable individual. Overbeek is one of all-time giants of colloid and surface chemistry, as I said before.

And by the way, he is now ninety this year, and Nat Arbiter also is ninety this year, both born 1911. I've talked to Nat on the telephone, and he still sounds no different. I have not seen Overbeek for quite some time. Professor Hans Rumpf, the great particle technology engineer at Karlsruhe, was also born in 1911 but died from a heart operation at age sixty-five. He was all set to come to Berkeley in the fall of 1976. Remarkable people.

Swent: Yes. Overbeek was at MIT.

Fuerstenau: He was at MIT the last year I was a grad student, which was what really got me going in the direction of surface physical chemistry to the extent that I have. Actually, it turns out that in future years, about every other year, Overbeek would spend half a year with the Chemical Engineering Department at MIT, and there's an excellent series of videos on surface and colloid chemistry, that present many hours of lectures by him, made during his later visits to MIT.

He told me he collects American Social Security, so that tells you he must have been over here at least forty quarters, the equivalent of ten years.

Swent: But he resides still in Holland.

Fuerstenau: University of Utrecht. Another thing I found interesting with Overbeek: when I saw him either in Holland or here, he said, “You know, I feel much more comfortable talking to chemical engineers today than with chemists.” And he’s a chemist, a very fundamental surface colloid chemist, theorist, and experimentalist.

[Added by Douglas Fuerstenau during editing: Theo Overbeek died in February 2007 at the age of ninety-six.]

XIV SABBATICAL LEAVES IN ENGLAND AND IN GERMANY

Swent: You had along the way a lot of interesting sabbaticals. We had mentioned in passing the one in London. Do you want to expand a little more on that?

Fuerstenau: Yes. Right. You know, the University of California has a very good sabbatical leave program, with a very systematic set of guidelines. In fact, most other universities do not. In a lot of places, people take so-called sabbaticals and go somewhere where they're paid. They might get a Fulbright or something to lecture somewhere else. I don't quite call that a sabbatical. I never got paid by my host institution for doing anything under them. I was simply listed there as a guest professor. I'll explain some of that shortly. I looked at a sabbatical as an opportunity to learn something more and maybe an opportunity to get away from Berkeley and an opportunity for the family to live somewhere else.

So for the first one, we went to England in 1966, six months, January to end of June.

Swent: Your university pay continues.

Fuerstenau: Oh yes. Let me back all the way up. The first year I was here—

[Tape 27, Side B]

Fuerstenau: Jack Washburn was on sabbatical for the year in Cambridge, and when he got back, he told me, "A year is too long to be away. Your research gets out of control, and you can get problems with financing, everything else." He commented that one should take a sabbatical for a half year. So I never forgot that, and I aimed my sabbaticals always at a half year.

The UC system—and they slightly alter these—after six years you could have one semester at full pay or two semesters at half pay. So somebody could go away at half pay and maybe augment their pay in some other way. Maybe that's why sometimes when people went elsewhere, they did some teaching. Or you could also up your pay to full pay on research contracts and grants if that was approved by the funding agency. In my case, we were happier on full pay, so wherever I went, I was not under obligation.

Sabbatical at the Royal School of Mines, Imperial College, London, 1966

Fuerstenau: The first year, I arranged to go to the Royal School of Mines, Imperial College of Science and Technology, which is one of the five components of the University of London.

Swent: And you took Peggy and your three children.

Fuerstenau: Oh yes. And we had—the word the Brits use for a realtor is “estate agent”—a bit of a time finding a place to live by calling an estate agent. We had a whole house about two blocks from Notting Hill Gate, and out the back side of our house was Portobello Road where all the famous antique shops are. We lived on Kensington Park Road that was parallel to Portobello. Our landlady was Lady Chamberlain, whose husband was head of Reuters. She had bought the house for her son, who had been transferred to Paris for a couple of years. This house was four stories high, one-room wide, and two-rooms deep. It had central heating, but I called it central warming. In the spring I could just walk across Kensington Gardens to Imperial College, which was right behind Royal Albert Hall.

Swent: Wonderful!

Fuerstenau: Steve was only two and a half when we went there, and Lucy was seven, and Sarah became five. They both went to a very good school just a few blocks away, called Fox School. A state school, but it was very, very good, and people liked to get their kids there. As nonresidents, we were fortunate to be able to get our two kids in there. It was just a few blocks that Peg would walk up there with them.

Then, you know—gee, almost every day—here I was, on sabbatical, and I was probably over there at Imperial College more than the faculty.

Swent: What were you doing?

Fuerstenau: A lot of writing and research discussion, reading. I was really writing technical papers related to Berkeley work, in actuality, but also talking to different people. Also, I got several new ideas for research projects. The department—this is now Royal School of Mines—was called Mineral Technology Department, the same as the name of this department way back then.

The British system has one professor, who was Marston Fleming, a Canadian, but he was more British than the Brits. A typical department then would have one or two readers, which are equal to full professors in this country, and then maybe a few senior lecturers and a few lecturers, so there's this sort of hierarchy of about nine lecturers, two readers, one professor. Some of the senior lecturers might be full professors in United States universities. Fleming had done a nice piece of research on the chemistry of xanthate interaction with oxidized lead minerals, cerussite. He published papers on that in the *AIME Transactions* rather than *IMM [Abstracts] [Institute of Materials, Minerals, and Mining Abstracts]* London in the early 1950s. Fleming was not involved with any research that I know of when I was there in 1966.

At that time, engineers and researchers working in former British Commonwealth countries like Australia and various parts of Africa still got extended travel leaves about every three years. As a result, there was a steady flow of technical visitors virtually daily. I think one of Professor Fleming's main duties was meeting and talking with different visitors almost daily. London then seemed the focal point of the world.

The professor is *the* head, titular head, and really the head. One day my friend, Tom Meloy, came to London, and he called up Fleming, and his secretary answered. Tom Meloy told me that he said, "I'd like to speak to Dr. Fleming." The secretary said, "Sir, to you, Professor Fleming." [laughter]

A reader in the department was J. A. Kitchener, probably the foremost surface chemist of England in '66, and he was in that department. He did a lot of excellent research on surface chemistry related to wetting behavior, flotation, et cetera. Then another reader that I remember was Henry Cohen, who taught courses involving electrical separation, magnetic separation, et cetera. Cohen was the successor to Fleming when Fleming retired.

Then my first roommate from MIT Grad House, Olav Mellgren, was a senior lecturer there. He told me that he worked in Africa on copper ore flotation for three years after getting his doctor's degree from MIT before moving to Imperial College in London. Olav was there many years, on the faculty. A good man. Did a lot of nice research on flotation thermodynamics, calorimetry applied to flotation collector adsorption, this sort of thing. Now Mellgren, I may have mentioned earlier, was Norwegian, and he came from Hauge i Dalane, a village on the southwestern coast of Norway. There was a large ilmenite mine there, a huge mine that supplied much of the raw material for the European titanium industry. The mining company was called Titania, and Olav went up there consulting, a lot, I think maybe spent most of each summer working on research related to improving the flotation processing of that ilmenite ore. They came up with a flotation procedure to concentrate the ilmenite that used a lot of oil, fuel oil. By that I mean maybe ten, twenty pounds per ton of ore. That's a lot of oil. He published several good papers on that subject.

By the way, years later we visited Olav in Hauge, my one and only trip into Norway. He had left London and returned full-time as director of research for Titania. We visited the mine and went out in a small boat in the fjord right there where he was taking samples of the water at some depth. They were dumping the tailings from the mill into the fjord, and the fishermen were complaining that the oil was making the fish taste of fuel oil. Some years later I ran into him somewhere, and I asked, "Did you ever solve that?" And he said, "Yeah, it was solved by changing the oil." They switched to an oil that didn't flavor the fish.

Swent:

They continued to dump it?

Fuerstenau: For some years they continued to discharge tailings into the fjord, but today they now impound them in a huge tailings dam inland a few miles. As I said, Olav spent a lot of summers there, until they finally hired him as research director, and then he left Imperial College and moved back there and stayed until he retired. Nice, nice person. We still get Christmas cards from him. Usually he spends the winter in the Canary Islands. He said that he doesn't want to be in Norway in winter where it's cold and dark.

What I found interesting about the department there in Imperial College, and this was probably the same throughout, that the Mineral Technology Department taught all their own courses. They taught the math—I mean, of the nine or eleven people that made the department faculty, somebody taught the math. Kitchener taught the chemistry—it's kind of an odd thing that each department was a little school all of its own. I don't know whether that goes on today, but I don't think a student took any course outside of that department. That system is in total contrast to the way the curricula of American universities are organized. Throughout England, the BSc degree is a three-year program, and they claim that it's very feasible, based on their very rigid high school curriculum. They start weeding kids out early along the way; they call them levels, I think.

Swent: A level and O level.

Fuerstenau: Yes, yes. In our country students get a chance for recovery later on, but I think in England or Germany, boy, if you—in Germany and you're aimed at the *Volksschule*, that's where you go, and likewise in England.

One student there that year, Brian Ball, graduated with a first class honors, and they only give about one first per class. They have an upper second class honors and a lower second. Usually in England, they will take into a PhD program, students with a first or upper two honors, and maybe on occasion they may take a lower two. Then they also have a degree they call honors, and then the bottom of the line just gets the degree. They really stretch them out into five categories.

Brian Ball came here as a grad student, so that was one thing that came from my stay there. He did his master's and PhD with me. Very capable person. His master's degree thesis was involved with modeling flotation kinetics and his doctoral thesis was a detailed study of the thermodynamics of amine adsorption on quartz. For a few years, he was at Colorado School of Mines, and then quite a few years with Newmont. Incredibly, he died at age forty-two from melanoma that started on his little toe. God! Isn't that pathetic? You know, when you think of melanoma starting on your little toe—and at age forty-two died. Terrible.

Swent: Tragic.

Fuerstenau: Very able person.

Anyway, during our time in London, I remember going on a field trip during the spring break with the students. We visited coal operations and a lead-zinc mill in the middle of England somewhere near Sheffield where there was lots of coal. You know, you always hear on TV the BBC accent, but you get up around Sheffield, and I remember the plant engineer there talking about taking the “boos to Loondon” and a “toon of coal.” [laughs]

Swent: Hard to understand.

Fuerstenau: Oh yes. And that’s not that many miles away [from London].

Swent: Did you ever meet Ian McPherson? Malcolm’s brother?

Fuerstenau: No. I don’t think that I ever did. Later on, just before we left, we traveled a bit around England. I was afraid to rent a car until about the end. Olaf Mellgren drove us to a number of different places, and a couple of the other faculty a couple of times took us to different places near London. We went by train a lot, and living in London, always by bus or underground. We took the train here and there. I did go up to Leeds, to visit the University of Leeds, which has a very significant mining and mineral processing program—spent a couple of days there. Went over to Bristol. At one point, a program of three formal lectures was organized for me to present, which were well attended in Imperial College. I probably gave ordinary seminars in Leeds and Bristol. I recall attending the monthly meeting in London of the Institution of Mining and Metallurgy [Institute of Materials, Minerals, and Mining]. The procedure there was to formally discuss papers that had been published in the recent issue of the *Transactions*. Englishmen really know how to make sarcastic comments to someone about their work if they disagree with the interpretation, et cetera. That surprised me. Afterwards, there was a dinner. After the dinner was finished, the meeting chair would say, “God save the Queen” and then the members would light up their cigars and all became very informal.

Swent: That does sound different from SME [Society of Mining Engineers] meetings.

Fuerstenau: When we rented a car, we drove to Stonehenge, and back then you could walk around and climb around on those big stones at Stonehenge. Today it’s all enclosed in a fence, so you can only look at it from a distance. We did drive all the way up into Scotland. To me, one of the high points was walking on Hadrian’s Wall, which as you know was built around 300 or 400, to keep the Scots out. And there are still places where you can really walk along the top of the wall, and you can see the wall going up and over the hills and so on. To me, that was a real high point of our traveling around.

Of course we took the kids to see the Queen every time she rode—such as on her birthday or whenever there was a visitor, the Queen rode in an open carriage, and so we were down along St. James's Park with the kids. One time in the winter we went to Windsor Castle, and I remember the guard had a gray uniform, not the red one you see in photos in the summer. At the gate, there was a little shed, a little cubbyhole where he stood, and he'd stand in front of one, and then he'd march to the one on the other side of the gate, click his heels, stand there. So our son, Steve, who was not yet three, stood in this little cubbyhole guardhouse, and the guard, when he came back, said something to Steve, and Steve came running back, and I said, "What did he say?" Steve said, "Beat it, kid." [laughter] They're not supposed to react, right? Anyway, living in London for six months was, I think, a great occasion for all of us.

Sabbatical at the Technical University of Karlsruhe, West Germany, 1973

Fuerstenau: The next sabbatical was in Karlsruhe, 1973. I wanted to go to Karlsruhe because there was an institute—in English, Institute for Mechanical Processing, and in German, *Mechanische Verfahrenstechnik*, which is part of the Chemical Engineering Department in the Technical University of Karlsruhe, under the leadership of Professor Hans Rumpf. Rumpf was a remarkable person. In my opinion, he was the leading person worldwide in particle technology, and he had built up this institute that had maybe two hundred people working there, the institute being part of the university. You know, in German universities, when you're a professor you have an institute. I used to call them professors with institute and professors without institute. Comminution was a major part of their research program.

When Professor Rumpf accepted the call to being a professor in Karlsruhe, he concluded that the great problem in particle technology was comminution, crushing and grinding, and he established that as the primary orientation of the institute. Particle sizing, classification, particle separations, and particle characterization were also big parts of their program when I came in 1973—just a fantastic setup, and the way Germans poured money into things would make you feel quite modest. Since Rumpf was a mechanical engineer by background, I would characterize his institute and that of all of his followers as being very heavily oriented towards equipment that they designed and built in their machine shops. I often felt that they all constructed a Cadillac apparatus when a Ford would have been satisfactory for the purpose.

Rumpf had spent his career in industry until he was late forties, at BASF during the war and in the early 1950s he became research director or chief engineer of a company that manufactured precision classifiers for sizing dry powders: Alpine. I'd seen papers that he'd published in *Mining Engineering* about classifier design and operation. In fact, we had a pilot-scale Alpine classifier in our pilot plant in Niagara Falls. At age forty-seven he was hired to come to Karlsruhe as a professor to establish an institute in mechanische

vervahrenstechnik. So here, at the age when you often hear that people in research are winding down, he was starting up and built really a fantastic research facility and made an amazing amount of outstanding contributions to comminution. In 1959 at the First Agglomeration Symposium, which was held in the United States, Rumpf presented a classic paper on the strength of agglomerates in pelletizing. I didn't know about that paper until years later when I started working on pelletizing. Klaus Schoenert always tells a little German humor, namely that the word pellet does not exist in German but Rumpf coined the word "*pelletizieren*" for his paper. And so that word is now in the German language for the process of agglomeration or pelletizing.

I had met Hans Rumpf for the first time at one of those Engineering Foundation conferences in Milwaukee, but I already knew of his work, and did visit that institute for a day in September 1966. This is why we wanted to go there. I was on full pay from Berkeley, so I had no commitments, but they supplied me with an office, and so on. I guess I had interacted a little bit with Rumpf on a couple of other times in between. In 1971 we were in Europe and at that time there was a huge symposium in honor of Professor Rumpf's sixtieth birthday, so I attended. There were technical speakers and humanities speakers. Rumpf was quite active in cultural affairs in the city of Karlsruhe.

In the late 1960s, he had also risen to be the rector of the University of Karlsruhe. In German universities, a person serves one year as prorector, then two years as rector, and again one year as prorector. So the position rotates amongst the faculty. Also in Germany there is an organization called the Association of Rectors; in other words, an association of the rectors from all universities and colleges throughout Germany. For three years he was the president of the Rectors Association, which meant he was in Bonn much of the time working with the government in relation to educational policy, budgets, that sort of thing. One time I visited Karlsruhe, gave a seminar, and Rumpf was there. We went out to a marvelous dinner after that. He had a big black Mercedes and a driver, because he'd come from Bonn just to spend some time with me.

Swent: You were lecturing there?

Fuerstenau: Just a seminar then. This was before my sabbatical. When we were there for half a year, he was through being president of the Rectors Association. He was the first engineer to hold that job. That person is very powerful in German education in that that's where negotiations go on with the German legislature, Bundestag I guess it's called. But when we got there in '73, that was history. That institute ran with three top people—they called it a triumvirate, Rumpf at the head, and then Schoenert was now a professor, and the third was Fritz Loeffler, a fairly young guy, a professor. Schoenert was an expert in fracture and comminution, and Loeffler in classification and particle flow in gas streams.

What I find interesting is, you know, if somebody's got a chair in this country, it's got a big title; but if you're in Germany, the highest professor is called *ordentliche* professor, ordinary professor. That's the highest. Extraordinary professor is less. Schoenert and Loeffler were *ausserordentliche* professors, and sub-professors. And only the ordinary professor could head an institute. The three of them managed this whole operation of maybe 150 or more people.

Swent: And this was research and teaching, both?

Fuerstenau: Research and teaching. There were people that would have been the equivalent of lecturers in a British system or here. They could be lecturers, too, and there may have been three or four of them who taught some of the courses. Many of the researchers would be doing full-time work with some of their research leading to a doctor of engineering degree, which might take five, ten years to complete. They were paid at the same rate as full-time engineers were paid in industry. That is why one did not often see a German graduate student in engineering in the United States. They also had a full suite of technicians.

They even had a person full-time running the Xerox machine. I remember one German grad student said, "He's just a damn Nazi." I remember chatting with the Xerox operator quite a bit at one time. He told me he had flown something like two hundred, three hundred air missions. Of course, when you're inside Germany, that might have been only a half-hour or so flight shooting at Russian tanks or something. But his full-time job in the institute was just running the Xerox machine. Of course, this was '73, when Xerox was a little more complicated.

Just to illustrate one type of project that they had, which was not mining, but which was really large scale. This huge project I think was supported by the Swiss, maybe Germans, grinding chocolate. You know, the Swiss chocolate is so good because they grind it very, very fine, and much finer than our chocolate is ground. It's ground in various types of mills, and I am assuming that the grinding mills must be cooled.

Swent: Grinding the cacao.

Fuerstenau: The bean, yes. They made candy, and they had a program of people tasting it. It wasn't done on a small scale; they apparently had virtually a pilot plant scale. If you ever want to know why some people's chocolate is better than others, it's due to the fineness of the comminution of the cocoa bean.

Swent: That would be a nice project to work on.

Fuerstenau: True, true enough. And so they had a large variety of projects like that.

When we first got there, Rumpf invited us to dinner, and there was a friend of theirs, a math professor named Johannes Weissinger, and his wife taught languages, and she had a regular teaching role of teaching German to English diplomats. We arranged that Peg and I spend an hour or two with her once a week on German. I found that their dog understood more German than we did at times. [laughs]

Our kids were then nine, eleven, and thirteen. We put them all in German schools. Lucy had studied German at eighth grade here, planning ahead for my sabbatical. Steve came home in March and said, "I don't know why I go to school. I don't understand anything." By May, he said, "You know, Mom, I understand everything, and I almost have nerve enough to answer the teacher's questions." Which tells me, you know, this thing about bilingualism: little kids can learn so fast; it's only adults that learn new languages slowly. I know that from my own efforts in learning to speak German. When we got back from that dinner with Rumpfs and the Weissingers, Steve said, "Oh, you know, that man is a king," and I said, "Well, what makes you think he's a king?" "Well, Professor Rumpf was always calling him 'your highness.'" His name was Johannes. Steve heard it as "your highness." [laughter]

Swent: Where did you live?

Fuerstenau: We rented an apartment on Rheinhafenstrasse, on the main street going down to the harbor on the Rhine River. In fact, it was the only one we could get. It had three bedrooms and a living room and maybe with the dining room being part of it, and a kitchen. It was about five miles from the institute. I walked it once in a while; drove most of the time. It was the only place that Annalene Schoenert could find for us to rent. Most of the people did not want to rent to Americans. They were very unhappy, having bad experience, I guess, renting to military people, because there was a big American army base in Karlsruhe. That's southwestern Germany, right on the Rhine. Karlsruhe is directly on the northwest corner of the Schwarzwald, the Black Forest. Heidelberg is thirty miles away, and that was, I think, the headquarters of the U.S. Army in Germany.

This woman, possibly a math teacher somewhere, rented the place to us, which was very good, as an apartment. The school was fairly nearby for our kids. I guess they went different directions to the three schools.

Swent: Isn't there a big U.S. Army hospital in Karlsruhe?

Fuerstenau: Yes. We debated sending our kids to American school, but then we decided no. Peg had the math workbooks from Berkeley, and for the rest of their education the kids were right in German schools.

[Tape 28, Side A]

- Fuerstenau: The Germans wanted to practice English with me. You know, it was very difficult for me to practice German in '73, frankly. And later that spring I spent a week in a day-long course, a commercial course, not at a Goethe Institute but something else, which got me a little more fluent in German and I should have done that earlier.
- Swent: You knew some German already.
- Fuerstenau: Oh yes. I'd had German in high school, and then I'd had to learn to read it. I had a year of German at South Dakota School of Mines, and I had German at MIT, but that's for reading and not speaking.
- Swent: I was going to say, in those days you still needed German for a scientific PhD, didn't you?
- Fuerstenau: Sure. When I first came here, one of the languages was usually German—two languages were required here at Berkeley. I think nationwide that's all sort of dropped out.
- Swent: Now they don't even require English.
- Fuerstenau: [laughs] English would be better. Right. Anyway, on this sabbatical, we drove all the way to England.
- Swent: You bought a car?
- Fuerstenau: Yes, we ordered a Mercedes from here that we later picked up in Stuttgart and afterwards shipped it back to us in California. There was an International Mineral Processing Congress in London in April, and so we drove there—boy, we made it from Karlsruhe, southern Germany, all the way to Dover, England. You know, you go through Germany, Belgium, France, the ferry at Calais, and got to Dover all in one day. In England, with two people, though, one can drive on the other side fairly well. You need the second person because you can't pull out to pass on a two-lane road because you can't see. But not once did I have any problem of getting on the wrong side. We just kept telling each other, "Make sure you stay on the left side of the road." But, you know, when you pull out to pass, your steering wheel is on the wrong side. That's when we went to Stonehenge again, come to think of it. We had been there in 1966.
- Swent: Of course, your car was American style.
- Fuerstenau: Oh yes. We had the slowest Mercedes on the road because it had all the California smog stuff on it. One moved along at about 140 or so kilometers, which is about 85 cruising speed always, but if you slowed down a little bit behind trucks and pulled out to pass, it took a long time to get back up to speed because of the reduced power on our car.

Having the car allowed us to do a lot of sightseeing. Every weekend we drove around southern Germany to such places as Baden-Baden, Heidelberg, Speyer, Rothenburg, Strassburg, et cetera, visiting castles, cathedrals and museums. One weekend we drove up to Michaelstadt where there is a Schloss Fuerstenau. Our daughter Sarah said that we should buy it. In fact, we drove all the way to Florence at Easter—Venice, Florence and Italy, driving down there through Switzerland.

It was, again, a nice experience overall. I wrote a few papers and had a lot of discussion on research and seeing how they conducted and organized research programs. I gave one formal seminar, started it with a few sentences in German and made the presentation in English. Again, that sabbatical was for just six months, and then we came home at the beginning of July. By the way, one later benefit of those sabbaticals was that four or five German students came to Berkeley to conduct their *Diplom-Theses* with me.

Our son Steve after that took German all through high school. Even though he majored in chemical engineering, he took a minor in German literature at UC-San Diego. Very unusual.

We drove on another occasion towards the end of our sabbatical in June to Amsterdam, and I remember visiting the Overbeeks on that trip, too. This was toward the end of our stay. My wife looks a little bit Dutch, and our kids are semi-blond, and we were looking at a map on a bridge, and some woman came up to me, a Dutch woman. She asked a question about where something was, and I said, “*Entschuldigen. Ich spreche nur Englisch.*” “I’m sorry, I speak only English.”

Swent: In German. [laughter]

Fuerstenau: So I had gotten a little bit proficient in German by then.

One last comment about the institute in Karlsruhe. Because Germany is geographically quite small compared to the U.S., it is fairly easy for groups to assemble from around the country. Each year the institute had an *Ausflug*, which is a day-long excursion of the staff of the institute, and former doctoral students come from quite a distance. We traveled by bus somewhere nearby in the Schwarzwald—Black Forest.

Somewhere we had big German lunch, and then hiked several kilometers through the forest to where we were again met by the bus to drive back to Karlsruhe. There seemed to be excellent collegiality amongst former colleagues.

In 1976 Professor Rumpf retired at age 65, and I had invited him to come here to Berkeley for a term. He was going to do this. That fall he had a heart operation in Heidelberg to correct some long-term defect. All had gone well,

but on the third day he stood up and dropped dead from a blood clot that had let go. That was a tremendous loss. I consider it very fortunate that I had the opportunity to know him and to interact with him for a number of years. Hans Rumpf was one of the really great men of science and engineering in particle processing.

I'd like to tell you about one more sabbatical. In between, and I think it was about 1980, I did what was called a sabbatical in residence because our son, Steve, was in the eleventh grade, and that's when they do SATs and all that, so we didn't want to go away from Berkeley. A sabbatical in residence allows you to go on leave and maybe handle a seminar or something and help bring your pay up to full pay somehow like that, so I did that. I don't really call that sort of thing a true sabbatical, unless one were spending the time writing a book or something like that. However, quite often Berkeley faculty do take sabbaticals in residence. Then our next sabbatical was in Clausthal in Germany.

I want to come back to the German system of appointing professors. Rumpf, before he retired, announced that his successor was going to be Klaus Schoenert, and someone later told me that Rumpf forgot that he didn't have his power base any more upon retirement. He'd been rector, president of rectors. Other professors in the chemical engineering faculty said that they were going to adhere to the German system of selecting professors. The German system is you can only become an ordinary professor at an institution where you did not get your doctor's degree, unless you have been several years teaching elsewhere. Schoenert had received his doctorate and all his university education at the University of Karlsruhe, and so they said no. They announced that Schoenert could not be the professor to succeed Rumpf.

They eventually got somebody who came from industry. The first I heard was that the faculty turned him down, and then a year or so later the university committee offered him the position. He's now there. I've met him several times but I don't recall his name. He has worked a lot on filtration. He's not in the league of Schoenert or Rumpf, but the Germans cut their own foot off just to obey their own rules.

Klaus Schoenert's Move to TU Clausthal; Sabbatical at Clausthal

Fuerstenau:

So anyway, Klaus Schoenert applied for a professorship in mineral processing—*Aufbereitungstechnik* is what it's called in German—at the Technical University of Clausthal. I happened to be there as part of a visit in 1979, and I asked Professor Albert Bahr there, "Would a letter from me be of value?" We were part of a panel of Americans looking at labs related to energy efficiency in comminution—maybe I'll get to that later. I spent that whole morning writing a letter, which then had to be typed into English by a secretary, about why they should make Schoenert the professor.

Swent: Typed into English? You said had to be typed into English?

Fuerstenau: It's not easy, you know, for a German secretary. I'll tell you, in Karlsruhe I tried to type on a German typewriter, and you just can't do it because letters like, a "z" is in a more important place on their keyboard because Germans use a fair number of "z's," so letters are placed differently than ours. I can touch type, so it just was a sheer misery to try to type. So anyway, I wrote this letter, which had a big impact and actually helped, I learned from other people. Schoenert then became the professor with institute, *ordentliche professor* in the Technical University of Clausthal in Clausthal-Zellerfeld. Clausthal is up in the Harz Mountains, which is the old medieval mining area of central Germany with mines still operating in 1984.

There's a great organization called the Alexander von Humboldt Foundation, which was started by the Germans about thirty years ago, a little less, partly in thanks to the Marshall Plan. Humboldt, you know, was the greatest natural scientist of Germany. Around 1800 he went to South America, and the Humboldt Current is named after him. There was a major award called the Senior American Scientist Award, which Schoenert proposed for me, and I was awarded one. It was something like seven or eight thousand German marks per month, and then it was tax free, for a year. Of course, I was paid full-time by UC. Today you're taxed on it, if you get any sort of award.

We had this wonderful support, and we went to Germany, again ordered a car, another Mercedes, which we picked up upon arrival. Our son, Steve, was doing his third year abroad in Göttingen, which was only thirty miles away, at the famous old university there, named after King George the Third from England who had come from that area. That's where all of the big physics was done before 1933. Göttingen is maybe sixty miles south of Hanover. It's not up in the Harz, but it's just below the Harz. Steve was there for a year, and we didn't see him that often, but we did see him now and then.

Schoenert had established an ongoing well-equipped mineral processing institute, *Institut fuer Aufbereitungstechnik* in German. He had an active program in comminution, particle liberation, high-pressure roll milling by then, but also in magnetic, electrostatic separation, and the jigging of very fine particles. His colleague, Professor Albert Bahr, directed research programs related to the chemistry of flotation, flotation machines, and hydrometallurgy. Bahr had invented a flotation column machine that was being used in a number of places. As had been the case in Karlsruhe, there was an excellent machine shop where specialized complicated research apparatus was built. During my stay there, with one of Schoenert's doctoral students, I carried out some high-pressure roll mill grinding experiments on some ideas that I had and they resulted in a couple of publications. Also in Clausthal was Professor Kurt Leschonski, also one of Rumpf's first doctoral students, who earlier had established an *Institut fuer Mechanische Verfahrenstechnik* in the Chemical

Engineering Department there. It was the 1979 visit to his institute during which I wrote the recommendation letter for Schoenert to come to Clausthal.

That first couple of weeks in '84, we went to a Goethe Institute in Chiemsee, which is near Munich. We were there two weeks—morning was in the German class—I was in an intermediate class; Peg was in a beginning class—and afternoon was skiing. I don't like downhill skiing, so we went cross-country skiing. Somebody from Sweden sat beside me, and said he worked for ITT, an American company. He told me that he felt pretty bad that he would come to Germany to sell equipment, and the sales discussion would be in English. He decided he had to learn more German, so he had with him his wife's Swedish-German dictionary, which several times I just sat and looked through, and you look through a Swedish-German dictionary and you think you're reading English.

Swent: Really?

Fuerstenau: It's amazing the number of words—not if you heard a Swede talk! But when you read it, a lot of their words are just spelled the same as ours are, too. Like all Germanic languages, Swedish follows Grimm's Law letter transliteration, so that when you see a Swedish word, it's spelled more like we would spell it and not the way a German would do it.

But anyway, that got us off to a good start on speaking German, and then the Schoenerts never spoke English with us, on the telephone, or Annalene, Mrs. Schoenert, only talked to Peg in German. When we got to Clausthal, Peg signed up for the intensive German for new students at the university, and this was from eight in the morning till three o'clock in the afternoon every day. The students were from Iran, China, Japan, all different countries so that the common language was German. Some of them knew some English and would like to practice their English with her a bit, but she really learned a lot of German.

Then the Humboldt had their spring meeting at a resort called Tegernsee, south of Munich, and we were cross-country skiing with a German professor, and the sun went behind the mountain. In one spot it got icy, and I fell down. Peg was behind, and she fell down at the same spot, and she said her leg hurt. She skied out about a kilometer away but it hurt all night. The next day we got to Munich and were visiting a friend who had been a postdoc with me, Guntbert Mempel. He lived only a couple of blocks from the Technical University of Munich, and so we went over there to the medical school, and an x-ray showed that Peg had broken her leg, or cracked it.

Swent: Oh my.

Fuerstenau: The sad thing was that her German class at the University in Clausthal was in a building on the fourth floor with no elevator, and she was seven weeks with

the leg in a cast. So in March, that was the end of her German lessons—which really was a sad loss because she really was becoming extremely fluent in German.

I came over to the U.S. three times, I think, during those seven months. I would call up and make airplane reservations in German on the telephone, as an indication of my own fluency in the language. Schoenert told his students to only speak German to me, because normally they want to do the reverse, practice their English.

Swent: Why were you coming back to the U.S.?

Fuerstenau: Those were two Homestake meetings, which we'll get to later on—Homestake board meetings. I think it was the one in '87 that I crossed the Atlantic in three months eight times. Twice for Homestake and once for a DOE meeting in Pittsburgh on coal research.

The Rammelsberg Mine in Goslar and Mining in the Harz

Fuerstenau: I want to tell you a little bit about a couple of things: a mine. This was a visit to the Rammelsberg Mine in Goslar. This was a thousand-year mine. Records showed that they started mining in the year 980, and they told me that it would shut down in 1989, because it had apparently been a huge self-contained ore body. They knew where the ore was; they weren't going to find another shoot or anything after a great deal of drilling. Right up to the end, they mined something like 5,500 tons a day in this mine. It was in a wonderful medieval city called Goslar just ten miles down from Clausthal on the edge of the Harz. There are a lot of houses that have the date above their door: 1480, 1450, those magnificent giant old what they call *fachwerk* houses. You know, ones with the half timber. Beautiful city. I told Schoenert, "Boy, if I were on the faculty at Clausthal, I'd live in Goslar." But they lived up in Clausthal. In winter if you drove up to Clausthal from Goslar, right as you hit the city limits, you hit snow, and that snow was about six feet deep. A street might be plowed in Clausthal, virtually with one open lane, with snow piled up high. Just an amazing amount of snow. Up on the Harz in winter there was great cross-country skiing. Because these were the only mountains that far north, one saw many cars on weekends with license plates from Denmark and the Netherlands whenever one drove out.

By the way, the road sign depicting the city limits of Clausthal-Zellerfeld had underneath the name of the city, the word *Bergstadt*. In medieval times, miners living and working in those cities or towns then were exempt from serving in the king's armies. You see old paintings or drawings of miners dressed in uniforms equivalent to soldiers.

You may know that the Harz is the region where the Brothers Grimm compiled all of the Grimm's tales. They still have a lot of activities there related to *die Hexe*, the witches, on dates like May first. This is possibly due to the fog hanging over valleys with long shadows and the rolling thunder that sometimes is there in summer. The seven dwarfs in Disney's movie "Snow White" were medieval miners up in the Harz.

The main building of the Rammelsberg Mine looked a little bit like Hearst Mining Building [on the Berkeley campus]. You know, the windows with the round top. That design came from Germany. Almost all the mining buildings, mine office buildings in Germany look like the front of Hearst Mining Building.

The Rammelsberg ore contained 23 percent combined lead, copper, and zinc, and they never even put in a flotation plant, I was told, till 1938. They simply mined the ore and smelted it directly. Can you imagine? There's probably a little gold and silver in it too. But that was a thousand-year mine, and that rich. Now I think they're turning [it] into a museum, and they have done a fair amount of filling with concrete to stabilize the mountain. I didn't get underground, but I did have a thorough tour of the mill.

Swent: What about pollution? You hear these terrible tales from East Germany. Was that a problem there?

Fuerstenau: No, because there are big tailings areas—and of course there were smelters there with huge slag heaps—and you know what? The last time I was there, they were reprocessing those mill tailings, and pumping them underground after re-treating them. Since they were never going to be back in the mine again, they were pumping them underground.

Swent: Were there forests around?

Fuerstenau: Yes, oh yes. The Harz were originally deciduous. The typical level is six to seven hundred meters high, a little over three thousand [feet]. You're so far north that they're mountains. We don't think of it as being very high—a lot like the Black Hills in some sense. For mine timber, they planted the conifers, and so the current forests are now fir and so on, and you can see that a lot of the trees are in rows, which means that they're essentially farmed. When they cut the trees, they always just leave a band near the highway so when you're driving along, you're not seeing a clear-cut area. Then they replant it. The layer of soil on top of the Harz is very thin, only a few inches deep. After one wind storm, a lot of trees blew over and you could see just how shallow the roots were.

Swent: But the smelting didn't affect that?

Fuerstenau: That's interesting. There was an old smelter up at Clausthal, and you can see that it looks pretty bare where that smelter had been. But down below in Goslar, there was a very high stack that dispersed the smelter gases enough, because there's farmland all around there, good farmland. But, the Rammelsberg Mine was really sort of fantastic.

I visited another mine at Bad Grund, which is right there near Clausthal. I suppose there are hot springs in the area, which gave the town its name. That, again, was a lead-zinc mine. Quite rich, but an underground mine and I think they've now shut that down. You know, the Harz really paid for Europe in the Middle Ages; Harz gold and silver financed Europe. The Spaniards got their gold, of course, out of Mexico, et cetera, but the rest of Europe got their money really out of the Harz. It's interesting to see all of this medieval equipment in the museum in Zellerfeld [there are two towns that are now hyphenated, mining towns, apart, at one time]. This old ancient museum is full of equipment that they built, stamps, pumps, hoists, and things like that, made in the 1400s. They had to use water power to pump, and there are hundreds of little dams called *Teichs* all through the Harz, which were used to provide the water for powering the pumps for dewatering the mines.

I remember talking to a mining professor, who said that in the 1700s they were still using fire to break the rock. They would start a fire underground, then throw water on it. I was just amazed that it was that late using that ancient mining system.

Swent: Dynamite came in the nineteenth century.

Fuerstenau: Exactly. Dynamite came from Nobel, around 1870 or '60.

Swent: Up until then, all they had was just mechanical force.

Fuerstenau: Yes. And when they built the railroads over the Sierras they first were using just gunpowder. They would do eight inches a shift on four faces; I think I remember reading that from a recent Stephen Ambrose book on the building of the railroads.

Visit to the Bergakademie Freiberg and East Germany

Fuerstenau: In the summer, we drove to East Germany. Freiberg was the other old mining university of Germany, and near there the mining area is called Erzgebirge, a major source of base metals and tin. You know, the first mining school ever was in Freiberg.

Swent: That's where Hoover went.

Fuerstenau: That's where geology started. Everything in mining technology started there.

[Added by Douglas Fuerstenau during editing: I have been reading a book by C.C. Spence, entitled, *Mining Engineers & the American West*, Yale University Press, and the following information taken directly from that book about the role of the Bergakademie Freiberg in educating early mining engineers is interesting. In the latter part of the nineteenth century, many of the mining engineers were referred to as “Freiberger men.” In the late 1860s, probably half of the more than a hundred students at the Bergakademie Freiberg came from the U.S. One was Rossiter Raymond who was one of the founders of AIME in 1871 and after whom the AIME young author’s award is named. Rossiter Raymond pointed out in 1869 that foreign-trained engineers tended to evaluate western ore veins richer than they were, forgetting that foreign labor costs were lower.

Of interest to us in Berkeley is George Hearst’s attitude towards Freiberger men. When John Hays Hammond returned from Freiberg in 1879, he sought employment from George Hearst, a family friend, who first turned him down and said: “The fact of the matter is, Jack, you’ve been to Freiberg and have learned a lot of damn geological theories and big names for little rocks. That don’t go in this country.” “Anything else?” asked Hammond, “No,” said Hearst, “Freiberg is enough.” “Well,” said Hammond, “I’ll make a confession to you if you don’t tell my father. I didn’t learn anything of importance.” He got the job and went on to a hugely successful career. A 1921 survey of deans of mining and technical colleges rated Herbert Hoover as the most esteemed mining engineer, with Hammond being rated second in the list.”

By the way, I read a few months ago in the obituary of Oscar Tangel, retired vice president of Newmont Mining, that after he received his MS degree at Montana School of Mines under Professor Gaudin in 1934, he spent a year at Freiberg but decided to return to the U.S. rather than complete a doctoral degree program there.

In 1961, I received the Rossiter Raymond Award from AIME for a paper based on research done at Union Carbide, “The Role of Retention Time in Vibratory Ball Milling.”

This was awarded at a white-tie dinner at the annual meeting of the AIME in St. Louis. At the head table dais, I remember sitting next to Jim Boyd²³, who I met for the first time. Such formalities at AIME/SME banquets are a thing of the past.]

This was ‘84, and we had to cross the border. We drove our car there. I’d been warned by Schoenert that East German police really lie behind clumps of trees

²³ James Boyd, *Mineral and Critical Materials Management: Military and Government Administrator and Mining Executive, 1941-1987*, 1988

and whatnot, and if you pass wrong or if you're going over the speed limit, they do everything they can to stop you to get a fine to be paid in hard money. We had no trouble, but Schoenert had gotten a ticket for passing somebody in the wrong way, which was legal in West Germany but not by their rules.

Schoenert went there the week before us, and we couldn't do that because we were going to the main spring meeting of the Humboldt Foundation, which was in Bonn, where they had not only the senior American scientists but also about five hundred or more Humboldt postdocs and their families who had come from all over the world. One of the activities was a day-long cruise for everybody on one of those big Rhine River cruise ships. For one meal, all of the Americans were together, and for another the table seating was set by what institution you came from, and then another meal with people related to your field. One afternoon included another reception at the German White House [Villa Hammerschmidt], where the president of Germany lives. The president is not the chancellor; the chancellor runs the country. The president is the head of state, whose name was Richard von Weizsaecker. He talked with us, and I was just a little way away.

We were at one more of those Humboldt meetings a few years later, and that time the head of the Social Democratic Party, Strauss, talked to us. He was the outspoken president of Bavaria from Munich, and he said, von Weizsaecker is away and somebody else is away, so they delegated him to be the speaker, "So I'm going to say anything I want." Of course he was saying this in German.

Right after that, we drove from there to East Germany, and Annalene Schoenert—this is now July—called and said, "You know, there are no vegetables in East Germany, and so stop and bring a carload of fresh vegetables to Mrs. Schubert," Professor [Heinrich] Schubert—

[Tape 28, Side B]

Fuerstenau: He's really one of the foremost persons in mineral processing in Europe. He's a little bit older than I am, so he's retired a bit more, but he was a big giant. East Germany put huge amounts of money into their mineral processing research institute and education.

We loaded up the car with fresh vegetables, in a place right near the border. When we got to the border, we had to fill out forms, while watching the tails of German Shepherds run back and forth behind the fence. This same wall or fence went all the way up to the North Sea and was only ten miles from Clausthal. We were there many times near Clausthal. Any time we had a visitor, we drove over to look at it.

Anyway, this little soldier kept asking, "What hotel are you staying in?" I'd say, "We're staying in the guest house [of the] university." He came back about the third time. Finally I got angry, and I said, "Look, give me my

passport back. We're going home. All we're going to do is go give some lectures and so on, and I"—this is all in German, of course. Well, then he made one more trip, came back and signed everything, and he asked, "*Haben Sie einige Waffen?*"—"Have you any weapons?" I said, "I don't even have any weapons at home, let alone in my car." I don't own any weapons. Then we were on our way into East Germany. Schuberts looked after us very well. Our oldest daughter, Lucy, had come over for a holiday, and we drove up into this Erzgebirge area. Schubert showed us the church in Annaberg—where that picture is painted on the back side of the altar in a church.

Swent: You're pointing now to a framed painting on the wall here.

Fuerstenau: Yes, this very famous painting. It was painted in 1521 by Hans Hesse. It shows you mining, everything, in the 1400s and 1500s—the processing, separating, and so on. Anyway, we saw the original painting—

Swent: It looks like Breughel, that same kind of thing. Same period?

Fuerstenau: About the same period. But anyway, this painting is on back of the altar—maybe four, five feet from the wall, so you have to stand back there to see it. They may have moved the altar out once to have made those photographs that we have here, but that's the same way they're set. They're on sort of folds or doors that close the altar in the front. By the way, just across the street from there was the house that belonged to Melancthon, the right-hand man to Martin Luther.

We visited the university and I probably gave a couple of seminars, and so on. Then its official name may have been *Freiberg Bergbau Hochschule*. About 1790 Alexander von Humboldt studied there for two years. And, of course, this was all East Germany, and there were signs all over between buildings and across streets, big red banners, "*Sozialismus ueber alles.*" All these socialist, communist slogans on big red banners, hanging in the middle of this town, on buildings and in the university. Buildings around the city did not look kept up at all. It was just their way of life under the East German regime. Schubert also drove us to Dresden where there still were bombed-out buildings with trees growing up in the ruins.

Swent: And yet they were glad to have a vegetable brought to them.

Fuerstenau: And how! Can you imagine: early in July, not a fresh vegetable. Of course the elevation is quite high there in Saxony. Then there would be no importing. As I say, for Mrs. Schubert it was just like having Christmas.

Swent: And this was in 1984?

Fuerstenau: Yes. On leaving, we drove to Torgau, where the Russians and the Americans met in 1945. We drove across the bridge where they met in 1945, and there's

a Lutheran church there that was open. Usually the Protestant churches are locked up, but for some reason, somebody had opened it, and we saw where Luther's wife is buried in that church.

From there we drove to Wittenberg, which is where Luther pounded his ninety-five theses onto the church door. When I was a kid, I thought it was on a simple church door that Luther nailed his ninety-five theses to. Actually, it's on the door of a massive cathedral, huge cathedral. He's buried there, and Melancthon is buried there. The two tombs are side by side in the floor. From the church we walked to Luther's house, which is now a museum. There's a small wooden pulpit in his house—one sort of climbed up in it—that he used. And there was a lute that he played.

Swent: The ink pot that he threw at the devil?

Fuerstenau: We saw that but not there. On another occasion, we drove into East Germany, which is no longer East Germany, and went to the castle at Wartburg where he threw the ink bottle and [we] saw the ink splash on the wall. I don't know whether it's the old one or not or if they keep renewing it; it has probably been helped along through the years.

When we left Wittenberg, we were following a sort of a country road. By that I mean two-lane road, paved, but you went through a town about every five kilometers, so traveling was a little bit slow. We came along one place where we were going past a lot of Russian army trucks. Then we saw one that had skidded off the road and plowed into a big tree, which I assume somebody had a pretty bad headache or worse; it looked like a bad wreck of a Russian army truck.

Then we got to the border near Braunschweig, and, boy, now we're leaving East Germany. You know, they poked the seats and looked into the trunk, and they put a mirror under the car to see what's underneath. Before I made the trip I had talked to a German metallurgical engineer there at Bad Grund, and he said that he had visited his relatives in East Germany at Easter, and he told me they would stick a mirror under the car, and then he said they actually put a dip stick in the gasoline tank. He told me that he said to them, "What are you looking for, an embryo?" He had warned me what they would do at the border. Anyway, they let us on through with a minimum effort.

Swent: We came through Checkpoint Charlie years before that, in a bus, and they ran mirrors under the bus.

Fuerstenau: Is that right?

Swent: They had great big mirrors on dollies that they rolled under the bus, to be sure no one was hiding there.

Fuerstenau: Yes, that's exactly what this was.

That year we lived in the guest house of the university there in Clausthal. They had only one large apartment in the building, and we had that. We did a lot of hiking in the forest because Clausthal essentially lies right in the forest except for cleared land. Right across the road there is an area that's about a mile by two miles that is just blown-up buildings, concrete buildings, hundreds of them, huge concrete buildings, flat roofs, and it's where the Germans manufactured munitions during World War II. On top of the buildings they had planted trees so that it couldn't be seen from the air. They were blown up by the U.S. Army after the war. Right beside the guest house was the cemetery, and I wandered through there. A lot of graves in one spot were all dated the same day and that was when Clausthal got bombed pretty heavily once or twice towards the end of the war and a lot of people were killed. Some were foreign, like from Rumania as I recall, so they were obviously workers in that munitions facility. But you could see from how it was laid out in the forest, it must have been hidden very, very well, at least until towards the end of the war. Anyway, you can go in there, but there were signs in several places about not walking everywhere, because there may be munitions that might blow.

By the way, in Karlsruhe—when we were there in '73, you could walk along Main Street and almost every other building was missing in places because obviously bombers had come overhead. I think the last time I was in Karlsruhe, most of the area has been rebuilt because the space is so valuable, but as late as '73 you could see there a lot of bombed-out buildings—

Swent: Thirty years later.

[tape interruption]

**Additional Stays at the Technical University of Clausthal, 1987, 1995:
Research on Roll Mill Comminution, and Mine Tours**

Swent: We had a short break here, and now we'll go back to 1987.

Fuerstenau: The Humboldt Award was for one year of support. In 1984 we were in Clausthal for seven months, and then in '87 we went back again for three months with Humboldt Award support, and finally the Humboldt Foundation said, "Use it or lose it." So we later went back in '95 for the final couple of months, all with Humboldt helping with our expenses.

So when we were there in '87, I wanted to carry out a few more experiments with Klaus Schoenert, who had invented this so-called high-pressure roll mill. I think we talked a little bit about—

Swent: The roll mill. Yes, you have talked about that.

Fuerstenau: Yes. And he had a laboratory mill that was specially instrumented with which I wanted to grind mineral mixtures, and so on. For this study, I had brought mineral samples from Berkeley, and then one or two of his graduate student researchers ground them in the mill in certain ways that I wanted done to be able to relate back to other work on the same minerals that we had done here. All of that has led to two or three papers subsequently that were credited to the Humboldt Foundation for support.

I was doing that, plus writing. We did a fair amount of driving around, sightseeing. In '87 we bought another car to ship back, which was a Saab that we picked up at the airport in Frankfurt, so we had that all during our time. We were, it turns out, ninety-one days there, and, you know, if you have a car outside of the state for ninety days, you don't pay tax on it. Boy, that was calling it close. I really counted the number of days several times when we realized how close it was. We drove around a fair amount in northern Germany this time, visiting two different towns named Fuerstenau.

I'd like to tell you about a couple of mine visits. Professor Bahr, who was the second professor there with Schoenert, was a good man in flotation. I knew of his work. Bahr had a couple of Brazilian visitors that wanted to visit a potash mine and he asked me if I would like to go along, so I did, and this is a big mine, Kali Chemie. It's a big potash mine southeast of Kassel and right on the East German border. This mine produces, like, fifty thousand tons of potash ore per day, and it has been running for decades. We put mine clothes on, went down in the mine, and then we got in a Mercedes built like an open jeep, and I remember sitting in the back, and the engineer that was showing us drove this thing, and we drove maybe five miles or more—I mean, a long way, up and down, and the road looked a little greasy. We're seven hundred meters deep, so what's that?—more than two thousand feet down. And this guy drove that Mercedes jeep like a typical German. He was going seventy kilometers an hour, which was forty-two miles [per hour]—I'll tell you, I was white-knuckled. I mean, I was scared. I thought that we'd take off and slide—

They hoisted up fifty thousand tons of potash ore per day out of that mine. We went to about three different levels in there because one [part] of it was high in KCl; another part of the mine had a mineral that I think is called langbeinite, which is a magnesium-potassium mineral. It's kind of orange colored, pretty. The places being mined looked like just huge caverns.

The thing about a potash mine is there's no problem of breathing the dust because it's just salt. You know, nobody's wearing masks, even though you can see the white dust around. It's salt. But the big thing they worry about in these potash mines are explosions of compressed carbon dioxide, CO₂. Apparently when they first started mining, they might drill right into one of

these pockets that would be very high-pressure carbon dioxide, and that would blast out and apparently killed the drillers.

And so they designed these big drilling machines that looked kind of like an armoured cage, so if something blew out, it's [not] going to hurt them. Of course, they could drill very fast—it's not like drilling hard rock. They're just drilling soft material. But the CO₂ was what they worried about—they had all sorts of CO₂ detectors everywhere because carbon dioxide is heavier than air. You know, that happens in silos on farms. You read that farmers sometimes will go down in a silo, and then pass out. Somebody goes down to rescue him, passes out, and they all die. I've known that to happen in South Dakota. If the CO₂ detectors set off an alarm in the mine, everybody's got to get out of the mine fast, but they have one room that they can lock out all the air, and just circulate oxygen in it. One person is assigned to stay in that room if they hit a CO₂ problem.

We had a great tour of the place. You can imagine, hoisting up that tonnage of material from seven hundred meters down. You know, at Lead [Homestake Mine] they mine five thousand tons a day, in round numbers, so this is fifty thousand tons of low-density material, salt.

Anyway, it's right on the border, and the East Germans mined the same deposit, and I think our guide said that in spots they sometimes actually came through together, but apparently nobody was escaping or they would have stopped it.

We were not allowed to go through the mill because Kali Chemie had a special dry separation process to separate KCl from NaCl. Their secret involved vapor-coating the KCl with some flotation-type collector and passing the dried ground ore through electrostatic separators. But the West German process included some leaching of the material, and the waste leach liquor was pumped down underground. What I remember being told was that they had wells one thousand meters deep that had a clay layer above and a clay layer below, and they could pump that waste liquor down, and then never hear from it again.

Swent: It was contained.

Fuerstenau: It was contained. But the East Germans had an equally big operation, discharged their waste liquor right into the river—the Weser River—that flowed into West Germany. I was told that all fish life was dead all the way up to the North Sea. You know, because it flowed west and then up toward the north. The East Germans were pretty nasty about it. They just dumped it and it killed all the fish, killed everything along the river. I imagine that's all, by definition, changed now.

When we came out of the mine we went to a small building that I assume was a guest change house or maybe the management change house. There were about six bathtubs, all curtained off, all in a row, fresh hot water drawn, and they had drawn a bath for each of us, so we changed clothes, got into the bath, and it was a pleasant, nice way just to get rid of all the salt dust. I never had a mine trip like that before. And then when we got dressed, we went out to the office, just before lunch, and there was a round of *schnapps* for everybody. What a nice way to tour a mine!

There was an association of faculty wives that had interesting programs. On one, Peg went on a tour of an old iron mine right near Clausthal, at a place called Salzgitter; that whole area is mainly full of these potash mines but there also was iron ore mining in the area. As you drive up towards Hanover from the Harz, you see these huge conical hills that are white, and they are the waste and tailings that they've piled up. Anyway, Peg went on a tour with the faculty wives down an iron mine where they were storing radioactive wastes. So the Germans, at least, were already using deep mined-out mines for taking care of their nuclear power plant wastes.

Swent: They had a lot of nuclear power plants, didn't they? Some were powered with uranium from Grants, New Mexico.

Fuerstenau: Germany had then as many power plants as they could possibly have with nuclear power because they couldn't raise the water temperature of the rivers any more than what they had been doing. So in other words, some of the rivers they were using for the cooling waters had their temperature raised more than a degree or two, and that was a limiting factor. The Rhine would be another story because that is such a fast-flowing one. I know nothing about whether they had any power plants there or not.

Professor Albert Bahr and Other Faculty at Clausthal

Fuerstenau: I'd like to tell you just a little bit about Albert Bahr. Bahr joined or was drafted into the German army in 1938 at the age of eighteen. Obviously, he was intelligent and was eventually assigned to running the Enigma machine. You know, this was the code machine for the German army. And he said he was in Egypt, and one evening [Field Marshal Erwin Johannes Eugen] Rommel came there and visited their group. And Bahr said, "In the middle of the night, about two in the morning, I heard two trucks or cars leave, and that was Rommel leaving," and in the morning the New Zealanders came and captured them. He was made a war prisoner, and he spent a year in prison in Egypt, which was lousy from heat and flies, I guess, and then they shipped him to Canada, where he was at Banff, in a tent, in the winter. Peg asked him what was worse: Banff in the winter in a tent or Egypt in the summer? Egypt, I guess, was far worse.

At the end of the war he was sent to some place in London, England, for a two months' re-indoctrination by the government, our side, and then went back to Germany. He wanted to visit his relatives near Dresden, so he went to see them, and the Russians captured him and made him a prisoner because he had taken this political course. They shipped him to Russia. He said at one time he was manager of a coal mine there, and he was a prisoner of the Russians until 1955 when the Germans negotiated final release of all German war prisoners.

Swent: My goodness!

Fuerstenau: Had you ever heard of that? Ten years after the war, the Russians still had German prisoners. And he was taken prisoner after the war was over. Bahr hadn't had any advanced education yet, but when he returned to Germany, he entered the Bergakademie Clausthal. He told me that originally he wanted to study physics or medicine, because he was obviously very intelligent, but then he decided he would go the mining, mineral route, that late in life. So then he studied in Clausthal all the way through his doctor's degree and stayed, teaching. A good man, full of good ideas on flotation and so on—both on fundamentals and also on machine design. He built flotation machines that I think were being manufactured and used in Brazil.

Swent: How was his health?

Fuerstenau: Actually, it seemed very good. He died of cancer rather quickly later on. I don't know whether he suffered from stomach ailments or anything like that, because he sure must have had a hard go of it, from his ten years in Russia and time in Egypt too. Guy Harris knew Bahr well. Probably got to know him through flotation reagents, and he stayed at their house on more than one occasion. Guy has told me about this.

I recall one discussion with Professor Bahr about mining education in West Germany. There were three main technical universities where mining was being taught: Aachen, Berlin, and Clausthal. Bahr said that those three universities graduated six hundred—or maybe even eight hundred—mining engineers a year and his comment was along the lines of what could Germany do with six hundred new mining engineers each year. Of course they must have gone into all sorts of industries, in addition to mining. I think enrollments have shrunk.

[Added by Douglas Fuerstenau during editing: Recently, I talked with Klaus Schoenert in Germany and asked him about mining enrollments before he retired in 1993. He said that at the Technical University of Clausthal, there were about one thousand mining engineering students, mostly oriented towards coal mining. Schoenert said that there were several professors of mining; professor of brown coal mining, professor of bright coal mining, et cetera. He said that an introductory mineral processing course for mining students had over a hundred students in it. The final exam was a fifteen

minute oral examination, and he had to question each of the hundred students—that took a lot of time. He said that there were fifteen to twenty students who were majoring in mineral processing. And the final examination for them was also oral but thirty minutes in length. At the recent International Mineral Processing Congress in Istanbul, 2006, I also asked Emeritus Professor Heinz Hoberg from Aachen about mining enrollments at TU Aachen. He said that a few years ago, they had about six hundred mining engineering students there but with the closing of German coal mines, the enrollment there is now about fifteen.]

Anyway, when Bahr retired, they hired as his replacement a professor named Gock, who had been in Berlin, far different from Bahr in that he was doing big things on environmental problems—

[Tape 29, Side A]

Fuerstenau: I think he had environmental projects, and some hydrometallurgy, some grinding with vibratory ball mills. He seemed to like to do things on a big scale. I recall that he had a large pilot facility away from the university that I visited once, but I could not quite appreciate his large-scale approach to problems.

At Clausthal, another excellent person, originally from Karlsruhe, was Kurt Leschonski. German, but with a Polish name. Excellent man. He was in chemical engineering, doing the particle work, and probably had an institute called *Mechanische Verfahrenstechnik*. Leschonski worked long and hard to establish a huge environmental institute, and I mean huge, and a whole big building with some fifty million marks from the state. To me, it seemed almost outrageous—for establishing this institute.

And when we went back to do our last couple of months of the Humboldt—I think it was '95 when this institute was dedicated. The person who came to do the dedication in the auditorium of this institute was the governor of Niedersachsen, Lower Saxony, and the governor was Gerhard Schroeder, who is now the chancellor of Germany. He was there in quite an informal way and gave the ribbon-cutting remarks. You know, I almost think he didn't have a necktie on. I can't imagine him not having a necktie. But anyway, he's now the head of all Germany, Gerhard Schroeder. This institute is called CUTEC, but I do not recall what the initials stand for. But an institute of that size and cost—I just don't see it. They were doing things like taking car bodies apart. Now, the Bureau of Mines did that twenty-five years ago, shredding car bodies and shredding old tires and separating components. I was not impressed with what they were doing—and with that huge budget.

Summers up in the Harz include a lot of gray days, but many times the days are sunny and lovely. Up on top of the Harz, the forest land is gently rolling so one could take wonderful walks on paths through meadows and forests to

other villages, where you could get refreshments. Going down the steep valleys always led to a town towards the bottom, which usually was one of the old mining towns, with typical Harz architecture. Most of the ore must have deposited when the Harz Mountains were uplifted. Also, in 1995 we could drive into what formerly was East Germany and then most of these old half timbered houses were unpainted and in need of real repair.

Just across the former East German border on the south side of the Harz, we drove to a huge tunnel where Wernher von Braun built the V-2 rockets towards the end of the war. We went into the tunnel some short distance. Next door to the tunnel opening was the concentration camp with many buildings remaining where the slave laborers lived. Many, many died there working inside that tunnel. Awful. You know from reading that Wernher von Braun would never admit that he knew anything about the concentration camp.

Then about two years ago, maybe a little more than that, Schoenert retired as the professor in Aufbereitungstechnik—that's the institute for mineral processing, and I just can't believe it. Gock said, "Well, we don't need anybody else in mineral processing." I just can't imagine somebody saying, "Hey, we don't need anybody else in my field." And believe it or not, they voted—the faculty of the mining, mineral processing department—to have Schoenert's successor be a professor to establish an institute for backfilling of mines. Can you imagine a professor of mine backfilling? Of studying how to pour sand down mines. I just can't believe it. But that is what they've done, and not replaced Schoenert in that way, but they hired just somebody whose field was to be—

Swent: Pouring sand down holes.

Fuerstenau: Pouring sand down holes. I'm extremely sad and very, very—[laughs]—it's very, VERY unsettling, because we all know what's happening in other parts of the world, too, on mineral education.

Swent: Maybe you could look at it this way: that they figured that you'd learned all there was to learn.

Fuerstenau: [laughs] Could be. Could be.

Do you want to go a little longer?

Swent: I can if you want.

[tape interruption]

XV ROADS NOT TAKEN

Swent: I know there were times when you had other choices to make, so let's talk a little bit about roads that you did not take in your career.

Fuerstenau: Okay. I'll just tell you a little of that. We already talked about my decisions not to stay at MIT or Union Carbide or Kaiser Aluminum. In 1962 I was promoted to a full professor here, which was exactly what I wanted.

Swent: That was awfully young, wasn't it?

Fuerstenau: I was thirty-three. Yes, that's fairly young, very young at the time.

Swent: Yes, for a full professor.

Fuerstenau: Anyway, the following year I had a call from somebody at Booz Allen Hamilton—you know, the big consulting company—that wanted to come and talk to me about coming to Kennecott as their research director, and so he came out to the Bay Area. I remember we had dinner over at Ernie's, the only time I'd ever been in Ernie's restaurant in San Francisco. We spent the evening talking, and I told him, you know, I was doing exactly what I wanted to do and I really had no plans to move.

It came across as almost incredible to him—and that's about the words he used—that somebody age thirty-four had attained the heights that he wanted in his life. He told me that. And I said, "Well, the person you really want to hire is Rush Spedden," because I thought Spedden had done a very fine job as a manager of research at Union Carbide.

So then the following year, I had a call or letter—I forget which—from John Kinnear, who was the general manager of Utah Division of Kennecott. Did you ever meet Kinnear?

Swent: No. I've heard the name.

Fuerstenau: You've seen his name, probably—he must have retired long ago. Well, obviously, yes, before Joklik moved up. Anyway, I arranged to meet him in Denver. It was the AIME annual meeting, and he wanted to talk, and we talked quite a while. Again I told him, "I really don't want to leave Berkeley and go into industry." I said, "Why don't you hire Rush Spedden, who I recommended a year ago?" Then he did hire Rush Spedden, and Spedden, himself, told me some while ago, quite a while ago, about the time when Kinnear retired from Kennecott, Kinnear said one of his major disappointments in life was not being able to hire me. But anyway, that was early on.

Then I suppose it was about—I just assume it was about in '67. I was again at an AIME annual meeting, and somebody named Elbert Osborn came up to see me. He was vice president of research for Penn State, and he said Penn State would like to have me come back there as head of the mineral processing department. I told Osborn that I didn't want to leave Berkeley. But I said, "Why don't you hire my good friend, Frank Aplan, because he's got all the same degrees I have?" Which is true. "I think he'd be a great addition for you people."

As you know, that's exactly what happened. By the way, we'll be talking later about Osborn's role in the Mineral Institute program. Also later he became director of the Bureau of Mines for several years, and I got to know him quite well later on.

But then it must have been about 1970—I don't have any record of this, but I had a call from somebody—I still remember his name, Jack Adamson. He said he was the vice president for academic affairs at the University of Utah, and he said, "You know, we'd like to have you come back here." And he said, "You know, we've got the best goddamned one-man Metallurgy Department in the world here." He was talking about Milt Wadsworth. "We'd like to have you come back as dean of the College of Mines." And so I finally agreed to go over to the University of Utah and talk to them, and I spent a whole day talking with different people, different department heads in the College of Mines—which was geology, mining, meteorology, metallurgy—I've forgotten—there was even more. Then I spent an hour, from three to four, talking with the president, who was James Fletcher. Fletcher eventually became head of NASA. And, you know, when the Challenger shuttle blew up, they called him back to straighten NASA out again. Now, this is the Fletcher who was president of the University of Utah when I did my dealings. Immediately after talking with Fletcher for an hour, I gave a seminar. You know, I could hardly move my jaw after talking all day with every department head and then with the president, and then to go in there and give a seminar.

Afterwards, four of us went up to Fort Douglas, which now I guess the University of Utah has acquired, where they had kind of a faculty club there, or officers club.

Swent: This is in Salt Lake City.

Fuerstenau: Right, right. And Fort Douglas was established I guess when they sent the army out to keep the Mormons under control at the turn of the [nineteenth] century or something like that. So there were five of us in a group and maybe four of us had a cocktail, and one person might have had ginger ale or something.

So then there was a dinner in the evening at the university. Big round table, with Fletcher and other deans and people like that. Anyway, at the end of the

dinner—you know, the Mormons turn their cups upside down. The only four people that didn't have their cups upside down were the four of us that had the cocktail up at Fort Douglas.

So anyway, I had a call some days later from—

Swent: We should say this is to indicate that they didn't drink coffee.

Fuerstenau: Oh yes. Yes, nothing—no stimulant other than sexual, I guess, that the Mormons will have.

Anyway, Fletcher called me up and made a solid offer. First of all, I brought home a newspaper, and the Salt Lake newspaper had quite a bit in it in one section stating that so-and-so went outside the valley to visit somebody—you know, that was how it was printed in the local paper. And that was a common expression: they “went outside the valley” or they “returned to the valley.”

Anyway, Fletcher said, “I'll make the dean's pay nine months so that you can get three-months' additional pay from summer research money,” which would make it much bigger than what the dean's pay already was, whatever it was. Then he said, “I'll give you a car,” which I suppose there would be a certain amount of driving around for student recruiting et cetera. “And then I'll pay for one trip a year to New York City for your wife.”

Swent: My!

Fuerstenau: [laughs] So he knew I wasn't a Mormon, right? [laughs] And out of his kitty he would pay one trip a year for my wife to New York. [laughs]

He called me back a couple of times. Finally I turned him down, and you could just sort of hear the air go out of the balloon, you know. I said, “I just don't want to leave what I am doing here in Berkeley.” If I had been any place but here, it might have been a fine thing to do. I could have learned to live in Salt Lake. I could easily have done that, but I just didn't want to leave here.

I also had been asked to interview to be dean of engineering at Columbia University, and that must have been about 1980. I didn't relish even the thought of it. I made a consulting trip down to a research facility that Exxon had at the south end of the Bay, where all the Silicon Valley activities are. Driving along there during work hours was just like a parking lot, coming and going, and I thought to myself, “Boy, why would I ever want to get into this sort of commuting situation?”

Anyway, I was going to fly there to New York on Sunday, and that Sunday I came down with the flu, and I called them up—or else had Peg call them, one or the other—and just said, “Hey, I've got the flu and I'm going to cancel.” I

then canceled the whole thing. I initially simply was going to make the trip without any real interest in doing it.

Swent: The idea of living in New York just didn't appeal?

Fuerstenau: Yes, well, I didn't want to leave here. So, you know, what I generally did through the years when somebody started to approach me with something was to let them know right away that it wasn't for me. They don't do it anymore, of course, but I cut a lot of things right off so that I would not have to stew over, "Do I want to do this or don't I want to do it?" The easiest thing was don't listen to them.

Swent: And you already knew what you wanted to do.

Fuerstenau: Yes. Oh yes. I got inquiries about being a dean or other administrator from a number of places, and I just immediately suggested someone else to them.

But I'll tell you one more interesting situation that happened early on, which was something different. I had a call from some organization that wanted me to go to the Philippines for two years to set up and improve their metallurgy program in the University of the Philippines in Manila. And I thought not very many minutes about it, and I said to myself, "Hey, I've got big problems building things up here in Berkeley. Why do I want to go to the Philippines and build something up there?"

What they were offering through that program was that they would pay for, for example, the college education of any of your offspring outside of the country, et cetera. This was a two-year commitment, and this was far enough back that our kids were little, so that was not an incentive. But I said—well, I'd heard that Professor Milton Wadsworth, who you've heard me talk about several times, had gone up to British Columbia on a sabbatical, and it was raining all the time, and Miriam, Mrs. Wadsworth, wanted out of there. The rain was too depressing. I suppose if you come from the desert—

So I all of a sudden thought, "Well, now, Milt is on sabbatical"—and I think he had six daughters, some of which were probably still of college age at the time, and said, "Look, why don't you approach him? I know he's up in Vancouver at the moment and not happy there because of all the rain." And lo and behold, that led to Wadsworth spending two years in the Philippines. That was apparently something that was a great family experience for them because they have always talked about their time in Manila. I remember Milt saying he took his whole family afterwards on a whole trip around the world, coming back to Salt Lake City. He made the statement that, "I intend to go to the grave leaving nothing behind, and so I might as well spend it on my kids now."

And, you know, my brother Maurie spent two years in the Metallurgy Department at the University of Utah at that same time; he went there as associate professor, and they promoted him the next year to full professor. Then he left. I've never visited anybody before who knew the neighbors' religion as they did—I remember Joyce, his wife, on the front steps of the house they were living in. "This family's a Mormon; that one's a Baptist; that's a Mormon; that's a Catholic; that's a Mormon." I don't even know the names of neighbors a few doors away, let alone their religion. But that sort of shows you what it's like in Salt Lake City.

And so anyway, Maurie said that the Mormon kids wouldn't play with his kids because they had to do things after school, naturally, and so Maurie left and went to South Dakota School of Mines. And Wadsworth has always said—I've heard him say it several times—"I really regret not being in Salt Lake when Maurie decided to go because if I'd been there he wouldn't have gone." Even in fairly recent years, he's made that comment that he really would have liked to have seen Maurie stay.

Swent: He would have pressured him more.

Fuerstenau: Oh yes, and given him a good pitch. Wadsworth is one of most enthusiastic speakers that I ever met or listened to.

Two or more of my former graduate students have spent years of their professional careers in Salt Lake and acclimated quite well, by the way.

Swent: Well, this might be a good place to stop, and the next time we'll go on to department chairman, and your editing of the journal.

Fuerstenau: Yes, right.

**XVI LONG-TERM CHAIRMAN OF THE MATERIALS SCIENCE AND
MINERAL ENGINEERING DEPARTMENT, 1970-1978**

[Interview 13: September 27, 2001]

[Tape 30, Side A]

Ernest Kuh Succeeds George Maslach as Dean

Swent: We wanted to talk today about your years as department chairman. That was 1970 to 1978; turbulent years on the campus.

Fuerstenau: Right, right. Normal chair term at Berkeley is supposed to be six years, but usually it's about four years, and it's been four years in this department, but at the end of the six years, the dean asked me to serve a couple of more, so I actually was there eight years.

Swent: Who was the dean at that time?

Fuerstenau: It was Ernie Kuh, Ernest Kuh. In fact, [George] Maslach was still dean during my first three years, because I remember when Professor Ralph Hultgren retired, I had a lot of conversations with Maslach over a replacement for Hultgren. Even the day before, I talked with Maslach about the need for Professor Hultgren's replacement, and then there was a meeting of the college committee, called DCAC, and I don't recall what the letters stand for, consisting of department chairmen and research institute directors.

At this meeting, Maslach announced, straight out, all four vacant positions in the college would go to the Department of Electrical Engineering and Computer Science. Just out of the blue—he could have told one the day before that “The pressure in EE [electrical engineering] is so great that I have to do that.” But he didn't.

Professor Kuh was chairman of [the Department of] Electrical Engineering, and sometime during that meeting I said, “George, I want to let Ernie Kuh know that he's the only one in this room that's happy today and that his happiness comes from the cannibalism of his colleagues.”

Swent: Oh!

Fuerstenau: George sort of yelled at me, “Fuerstenau, I've given you the Tom Mika position,” and, well, anyway, in '73 I went on sabbatical, and the department ad[ministrative] assistant wrote me a letter, saying that “It has been announced that the new dean is going to be Ernest Kuh.” I did kind of a double take, but Kuh and I have always gotten along extremely well through all of these years.

Early Role of Being Department Chairman

Fuerstenau: As you know, any organization runs well if there are able assistants. Fortunately, my predecessor, Jack Washburn, did a thorough job of looking for a new departmental administrative assistant. I know he put a lot of effort into that and hired Judy Roberts, who had worked about a half dozen years in the Department of Mechanical Engineering. Judy was well-organized, had good judgment. She already knew a lot about university procedures, so I could count on her for taking care of a lot of the ordinary administrative load. Budget policy, courses and faculty et cetera were the main problems that I had to deal with personally. As time went on, Judy became very good at drafting letters in my own style—really a great help.

At Berkeley, faculty appointments, merit increases, and promotions require a lot of effort. When I was chairman, all of these matters passed through the hands of the college personnel officer. At that time, one person handled academic and nonacademic personnel. I found that three of the four different women who held that job during my time were absolutely excellent for seeking advice on how to handle any complex situation. I could get very good advice from three of them. Later on, the amount of paperwork involved grew to such an extent that two people were required: one for academic personnel and a second for nonacademic. Towards the end of my chairmanship, affirmative action rules required filling out forms on every applicant for a faculty position, and we had to explain why each unsuccessful candidate was “deselected”—and this might be for several dozen different persons for each recruitment. I don’t know if it is still required, but there was a time that we even had to advertise in a journal or relevant newspaper for simple postdoc positions. Lots of people must have had wonderful careers going through all of those deselection forms in the university.

Before Judy came to our department, she had worked for the Thermal Systems Division in Mechanical Engineering, and mainly for Professor Ernest Starkman, an expert on automotive engine combustion. Starkman was named vice president for environmental affairs in the statewide university system and moved to University Hall. Sometime shortly after that, Rachel Stageberg, the college personnel officer, called me and asked whether she would have my blessing to recommend Judy Roberts for a position as his assistant in University Hall. I said, “She could recommend Judy for that position but she certainly wouldn’t have my blessing!” I don’t think that was the expected answer. Well, Judy had decided that her main interests were working with faculty and dealing with matters at the academic department level and is still in our department. Fortunately, the university advanced staff positions through the years and her type of position is now called management services officer, and I think is adequately paid for the large responsibility involved.

Swent: Sounds like all went well.

Fuerstenau: Yes, more or less. The year before I became chairman, Jack Washburn came around and talked to me, and I suppose to others in the department, about student hours per faculty and that the department had a problem with low numbers. He even talked about one way to handle that was to reduce the number of faculty in the department! So early on in my chairmanship, I had Jim Moynihan tabulate the workloads of the faculty based on the methodology in use at that time. The workloads of some of the faculty were pretty low, and in my first faculty meeting I asked the faculty to go to work on trying to find ways to help department workloads, such as getting more outsiders to take their minors in the department. What really did help was the start of a program of double majors in engineering. Whether Earl Parker conceived of the idea, or whether it came from Dean Maslach, I don't know, but Parker headed a committee to develop this for the college. The double major is not two degrees, but an engineering student can graduate with what became called a double major in, say, mechanical engineering and materials science and engineering, or in nuclear engineering and mechanical engineering, et cetera. This is accomplished by the student using all of the technical electives in the major field to take the core courses in the other field. This certainly expanded a student's background, and helped some departments, since credit for student enrollments for double majors was designated as half in one department and half in the other. That program became very successful and really worked well for our department. Unfortunately, I think that the double major program is contracting because departments don't want to give away student credits these days because of the emphasis on what I call bean counting.

An important aspect of my chairmanship was that I believed in inviting faculty and staff to our home. In the fall, we always had a cocktail reception for departmental faculty and often included the dean, and in the spring I included all departmental staff along with the faculty. In those days, there was no reimbursement for costs for that type of entertainment and no funds for hiring a caterer. Peg to this day remembers and comments that she did all of that work and prepared hors d'oeuvres, et cetera, herself. We invited many, many foreign visitors to our home for dinner through the years, and I think that this also added a lot to the growing-up experience of our children. Again, in those days, no costs were reimbursed. Another thing, about every other chairman in this department never has a faculty reception at his home, even with reimbursement policy. All through my time here only about a half dozen different faculty regularly invited groups to their house, by the way. I know how often you have entertained groups in your home, and I have enjoyed and appreciated that myself.

Swent: We both consider that important and enjoy doing it.

Fuerstenau: Another duty of department chairs was to attend college commencements and announce departmental graduates as they came to the podium. So I did that all during my term as chairman. Another aspect of commencement is that the

thesis supervisor puts the hood on doctoral students who participate in the ceremony.

Swent: With all of your doctoral students, you must have done this almost regularly.

Fuerstenau: Oh yes. Some commencements I had two or three. Anyway, the first commencement that I attended as department chairman had simple receptions behind the stage at the Greek Theater. There was a table for each department, and at the table were cookies and lemonade or other soft drinks. Awful! So the next year, I asked Judy Roberts to organize a reception for departmental graduates and their parents and guests to be held in the lobby of Hearst Mining Building. We had champagne and excellent catered hors d'oeuvres. Our departmental reception has continued ever since, using discretionary funds to pay for it. And the year after we started, each [College of] Engineering department began holding their own receptions. No more cookies and lemonade.

A Budget Cut and a Seismic Retrofit

Fuerstenau: That first year I thought being chairman is really quite straightforward, no real problems, and then all of a sudden we got hit with a huge budget cut, let's say, in January something, in the middle of that first year. Prior to that, everything had been smooth. A budget cut is just a number at the state level, a number at the university level, a number at the campus level, and a number comes down to the dean's level—but when that number comes to the department, actual decisions must be made. So what do you do? As I remember—now, this was 1970, our cut was \$50,000 or more in the support budget. Funds for equipment and supplies are not really very high here. What do you do?

I concluded that the only way to handle that was to cut people; I'm speaking of staff people, so I took a mechanic and a helper out of the shop, and an assistant engineer for the department building was moved to half-time. The other half of his pay was handled by research contracts of a couple of the faculty. The cuts were those, and a little bit taken out of our limited supplies budget. Then I'm the one who had to go around and talk to these people and tell them, "Hey, unfortunately you're going to be cut off." I remember the person in charge of the shop came to me. "Oh, you just shouldn't cut anyone out of the shop. Why don't you cut some of the secretarial staff?"

I said, "Lloyd,"—his name was Lloyd Crawford—"Lloyd, every person in the shop is going to be gone before I cut one single secretary, because I consider them the vital part of this operation." Well, that was in the days before people did their own word processing, right? Anyway, budget cuts are tough, and like I say, only when it gets down to the department level does it become more than a number.

[Added by Douglas Fuerstenau during editing: About two years before I became chairman, I clearly recall that there were twenty-two staff members and only fourteen faculty in the department. The year prior to my assuming the chairmanship, the university had also been hit with quite severe budget cuts, and some of those staff were cut by Jack Washburn, the department chairman at that time. In addition to the cuts that I made, Dean George Maslach—without consulting me or informing me—terminated two long-time lecturers in the department, one who looked after the x-ray facilities and taught the laboratory x-ray course, and the other who looked after teaching laboratories in ceramics. Professor Jack Washburn was extremely upset at this and circulated a petition castigating me for my cutting these two persons. I finally called Dean Maslach, who came over to a departmental faculty meeting and told the faculty that it was indeed he who had terminated them. Actually, I never did discuss with Maslach his reasons for making those two cuts, but it certainly gave him an additional way to effect the college budget reductions. Our low student credit hours at the time probably entered into his decision. This is an example of what sometimes a department chair has to face in the academic world.]

Then another thing that occurred when I was on sabbatical in '73 in Germany was a study of the seismic safety of Hearst Mining Building. The building report was that Hearst Mining Building was seismically unsafe or at least not fully known because plans did not give details of some of the structural aspects of the building. Also, a lot of the internal structure involves exposed bricks. In 1973, the estimate was that it would cost something like seven—I think that was the number—\$7 million to do some retrofitting for earthquake. Too much money, et cetera, at the time. And, as you know now, it's being redone at a total cost of, at least to date, I think \$96 million.

Swent: It's interesting that this was before the big Loma Prieta earthquake of 1989. They were concerned ahead of the crowd.

Fuerstenau: Yes, yes. In fact, that probably was one of the earliest seismic concerns on the campus. You know, later they looked at every building, far later than that, especially after Loma Prieta. But as a result, the large classrooms in the building could not be used; we used them for seminars for a while. The idea being that a few people scattered around labs and offices and so on is one thing, but to have a group of one hundred people or more in one room had the probability of potential disaster. Eventually classes had to be held outside the building for everything other than small research group discussions.

Swent: When you're department chair and take a sabbatical, what happens? Someone covers for you?

Fuerstenau: Yes, Alan Searcy, as I remember, would have served for that spring, two quarters, as the acting chair. Yes.

Hiring New Mining Faculty: Hunting for Stars

Fuerstenau: Then a big new situation that was happening during those years was that mineral mining in the U.S. was really starting to build up again. At that time we had within the department a group called engineering geoscience that was the remnants of the former mineral program. The group consisted of three applied geophysics, related to mineral exploration and geothermal energy problems. The department had ongoing programs in extractive metallurgy, mineral processing, physical metallurgy, ceramic engineering. What was of course missing was mining. That had been discontinued about '63 or so. Mining engineering education was building up all over the U.S.; places like Penn State and so on really mushroomed their enrollments and their faculty, as well as other traditional mining colleges such as the Missouri School of Mines, to name one.

Swent: What caused this to happen then?

Fuerstenau: I'll bet coal mining probably entered into quite a bit of it, because it was after the first oil embargo problem. That's an interesting question. Also, the oil companies had decided to invest in mining companies and that put in new capital. Arco bought Anaconda; Chevron bought something like 24 percent of Amax; Union Oil bought Molycorp. I think that Sohio bought Kennecott. Exxon had its own large mineral and coal program. Most of the mining companies had rather extensive research laboratories. Mineral activity really grew all around, and people were getting good jobs. I'm speaking of graduate engineers in mining, so the enrollment grew very sharply all around the country. In fact, probably enrollment should have been slightly controlled, right? But enrollments got big, and then after a bit came the usual boom and bust of the raw materials industry, together with excess graduates. That has always seems to have characterized the field of petroleum or mining.

Swent: Usually this happens in wartime, but that was a peaceful time.

Fuerstenau: Really it was; right. But enthusiasm for mining education really grew nationwide.

As for what we did here at Berkeley, Dean Kuh and I met with a number of people. I remember going down to Kaiser Engineers, and we had lunch with Jack Havard²⁴; he was vice president of Kaiser Engineers. I remember him showing us a large pile—and I do mean a big stack—of reports and plans on the Kaiparowits coal project in Utah that the movie star, Robert Redford, stopped. This was obviously a good project, and Kaiser had done all this design work in terms of environmental aspects, and they had a stack of

²⁴ John F. Havard, *Mining Engineer and Executive, 1935-1981*, 1992

reports, many feet high, on mine design and so on, but all was dead in the water, due to Redford. Of course, that is another story.

Anyway, we had positive discussions about mining education. Then we had another meeting later, Kuh and I, with Robert Heers, vice president of mining for Kaiser Steel Company, who was in Oakland and a mining grad from UC Berkeley, and got his input. On another occasion, Don McLaughlin and Plato Malozemoff were here, and again entered into discussions about re-establishing a mining program—they were more interested in pushing mining geology; I was interested in mining engineering.

Paul Witherspoon, who worked on groundwater problems after leaving the petroleum group when the mineral programs in the department broke up, transferred back from Civil Engineering. We were then granted an FTE [full-time equivalent position] by the dean, and we carried out a thorough search, which led to Neville Cook joining the faculty. It was Paul Witherspoon who first suggested that we look at Cook because he had observed Cook at international meetings. Neville had gained international fame for his work in South Africa on rock bursts in the very deep mines and being able to predict micro-earthquakes that would happen from rock bursts and how to control them. He was, I think, director of research at the Chamber of Mines; many papers, publications, lots of awards that he received for his work. He decided to accept our offer and they moved here. As I remember, he told me that the family went on vacation, quote unquote, to England, and then from his vacation he came here because in South Africa if they wanted to keep somebody, the law was such that he could be drafted into the army and reassigned to his previous job. So to avoid that potential, he did the route I'm talking about. I know they couldn't get money out from selling their house. South Africa at that time would not let people take their own money out of the country. When he finally did, I think the rand was worth about 20 percent of what it was when he left there.

Then after my tenure as chairman, Malcolm McPherson²⁵ and Mike Hood were added to the mining faculty. I don't even recall any discussions over their appointments. Maybe I was out of the country on sabbatical at that particular time.

Swent: These were all British. None of them were Americans.

Fuerstenau: All British, except for Cook, who was South African. Once we got Cook, he must have had the major role in getting the other two—Hood had worked for Cook in South Africa—and they must have known McPherson for his work in mine ventilation. I had written a lot of letters and talked with several different

²⁵ Malcolm J. McPherson and James, *Brothers in Mining*, 1992

people in the U.S., and two or three of them came here on interviews. But outwardly, Cook was the kind of person that fit into being here at Berkeley, being very active as a researcher and a very good lecturer. In my opinion, research is a major part of the role of a Berkeley professor.

I was responsible for quite a few new faculty coming here. In the mineral processing area, I'd been able to get an assistant professor slot, and a very outstanding student became the assistant professor, Tom Mika, but Mika never got tenure. It's interesting. Very, very bright person who was internationally famous as a grad student but he never lived up to that fame. Afterwards, his whole career was at Xerox, dealing with fine particles on toner, and so on. I assume he had more of an administrative role.

His replacement was Kal Sastry, who is still here. Kuh had given a full professor slot for Neville Cook, and he would not agree to anything higher than an assistant professor for this mineral processing slot, so we could only go the route that would bring somebody here as an assistant professor. I really wanted to get someone at tenure level, but Kuh was adamant and wouldn't give it. That was a huge mistake, in my opinion.

Then extractive metallurgy: Fred Ravitz retired, and Jim Evans came, and he does—

Swent: He's another Brit, isn't he?

Fuerstenau: He was and still sure sounds like one. When I say that to him, he says, "I'm an American." I said, "Yeah, but you don't sound like one." He was one person that on the fifth year to the day of his entry to the country became a U.S. citizen. His PhD was actually SUNY-Buffalo under an extremely able chemical engineer-process metallurgist, Julian Szekely. And by the way, persons who became U.S. citizens on their very first eligible day were the Cooks. Right on day one of their fifth year in this country. You know, there are faculty who have been in this country at various universities, some here, for thirty years or more and have never become citizens. I know of several examples, and I think that's bad. Some of these are foreign associates of the National Academy of Engineering, and I personally am against this unless they are members of the academy from their home country.

Ravitz, whom we talked a little bit about earlier, was the extractive metallurgy professor here. University departments can be just full of bitter politics; I told you that before I came, they pushed out somebody who had tenure here. That departure was the environment that opened up a position for my eventual coming here. I had this heart problem a few months ago and I was down in the emergency center at Kaiser, lying on a gurney, and a woman was lying on a gurney next to me, and after the nurse had called me by name, she said, "Are you Doug Fuerstenau? I'm Fred Ravitz's daughter." I had met her forty years

ago, let's say, and here she was a gray-haired woman, like all the rest of us now.

We were lying there in the crowded emergency room, out in the hallway, about six o'clock on a Friday, when everybody seemed to have come in, and she told me that—which I had never known—that her father was so upset here during the time that they were pushing out the tenured mineral processing professor that he had applied to go to Battelle Memorial Institute in Ohio, and they offered him a position, but he wasn't given clearance. It would have been a position that needed Atomic Energy Commission clearance, I gather. She told me that for some reason, they didn't grant him clearance, and that was why Ravitz stayed here. I had never known that, even though his office was next to mine, but I can understand the kind of unhappy politics that can happen in departments. It seems to be pervasive. Reading the biography of the famous ant biologist at Harvard, E. O. Wilson in his autobiography entitled, *The Naturalist*, writes about how bitter departmental politics in universities can be sometimes, especially when fields might be undergoing change, as Harvard's biology was doing fifty years ago.

Then a really outstanding person died of lung cancer, John Dorn. I had decided that we needed more theory in the department, and that if we didn't watch it, we would be too heavy in blacksmithing, and in writing all around the country inquiring about young theorists, I discovered Bill Morris, J. W. Morris, Jr. He had done his doctorate under Phil deBruyn at MIT, although his thesis had nothing to do with mineral processing. He was at Bell Aircraft working on impact behavior, problems like that, before he came here. Later, when Earl Parker retired, Didier de Fontaine came here to Berkeley from UCLA: another person more oriented towards theory.

In the world of ceramics: Dick Fulrath died of lung cancer at about age fifty-four, so I had decided that ceramics break and that we ought to get somebody who works on fracture of ceramics. We brought in Tony [Anthony G.] Evans, who now is really, I think, recognized as the world leader in ceramics and especially in fracture phenomena. He had been a long time at Rockwell and came here for quite a few years, six or eight, then went to UC Santa Barbara, then went to Harvard, then as a professor at Princeton, and now back to UC Santa Barbara. I guess grass doesn't grow under a rolling stone, right? [laughter] I don't understand somebody moving around like that but he's really an outstanding person—again, he's originally English. Tony Evans was educated in London in physical metallurgy but evolved into a ceramic engineer. Bill Morris is American with MIT degrees, and de Fontaine was originally Belgian, but with a Northwestern PhD.

Another program that I worked on getting into the department was corrosion. This had been taught in the Mechanical Engineering Department for decades, and when Israel Cornet retired, I spent a lot of effort getting that position transferred to this department, where it really rightfully belongs. This position

was filled by Tom Devine, who was an MIT graduate and came here after working several years for GE Research. He is professor in the corrosion area and is our current chairman.

I'd just like to throw out one comment that when you hunt for faculty, you really should hunt for stars in this business of research and education. You know, ten mediocre people don't add up to one good person. One individual can really make things fly. One of my colleagues, now retired, Gareth Thomas, always used the words when we were talking about hiring somebody, "Is he or she a star?" Most of the people who came here when I was chairman, I would put into the star category. That's so vital; sometimes people make bad judgments, and in a system with tenure, you're stuck. And in the University of California, tenure goes with associate professor, so once one is promoted to associate professor, that brings tenure. If you appoint somebody at that level, you'd better be right.

Swent: How do you define a star?

Fuerstenau: Somebody I think who's creative and very innovative in his research and works hard at building up his program and is very active and recognized in the field. I'm sure creativity and energy enter into this. I suppose I would put most weight on a high output of high quality research. That's really where the reputation comes from. Somebody who is a great teacher but does no research is only known as a great teacher by himself and the students in his class. The people I call stars are those who are recognized by their peers in their specialty as being one of the leaders, if not the leader, nationally and internationally. But I would also hope that they are excellent teachers—and there are many, many examples of outstanding researchers who are outstanding teachers at Berkeley.

Actually, I myself have always assessed or categorized my colleagues as to whether the reputation of UC results from their being here on the Berkeley faculty—or is it the reverse?—is their apparent stature simply that they are a professor at Berkeley?

The Mineral Engineering Program

Fuerstenau: Let me talk a little bit about this mineral engineering program that we had here. Malcolm McPherson wanted to have a separate mineral engineering program, and he pushed this through, and all of our courses, for example, were either called materials science and engineering courses or called mineral engineering courses. I guess those in applied geophysics have always been called engineering geoscience courses. That's a word Stan Ward made up years ago, while he was here before he left for the University of Utah many years ago. Anyway, I didn't like that because I figured any time enrollment might decrease, these numbers are going to show up and would be flagged.

Whereas if we had all courses carry just the department name, courses with a small enrollment would not appear so prominent. Of course, that's what happened when mineral engineering enrollments declined.

We had a good undergraduate program in mineral engineering. It was accredited by ABET, Accreditation Board for Engineering and Technology, the nationwide vehicle for accrediting engineering departments in all colleges and universities. By the way, one year, the person in the ABET team assigned to review the Berkeley mineral engineering program was Terry McNulty. Our undergraduate program had very active students, most of them, enthusiastic. The old Mining Association was reactivated, and I think that you attended a couple of those annual mining banquets. I think the real leader behind this was Malcolm McPherson. He really interacted well with undergraduate students.

So there for a few years, we were doing really quite nicely. McPherson was very strong professionally in mine ventilation, and he founded a company that was very active around the country working on mine ventilation problems. He never really published much. Mike Hood had a very active rock-cutting program, but he also published very little and never got promoted to full professor. Unfortunately, McPherson decided to go to VPI [Virginia Polytechnic Institute and State University, now generally known as Virginia Tech], and Mike Hood went to Australia, where he is actually doing very well, heading one of the Australian research institutes at the University of Queensland—I think it's called Mining Technology Centre. There are several, a couple of dozen of these supported by the government of Australia, and he's connected to one in Brisbane. I've visited him there. Not long ago, Hood was appointed professor at the University of Queensland. Neville Cook devoted himself to rock mechanics and headed a huge effort in the LBL research related to nuclear waste storage and waste isolation, things like that, and got very active in other campus roles of energy and so on. Cook was elected to the National Academy of Engineering for his outstanding work. But Cook had non-Hodgkin's lymphoma, and unfortunately died at age sixty.

Fuerstenau: In essence, our mineral engineering program really evaporated, through people leaving and Cook's departure. But, you know, there was a real problem getting students into this undergraduate program in mineral engineering because of the high GPA required to get into Cal. I know there were a couple of very bright students in the program, but the bulk of them were at the lower end of the GPA required for admissions, and by lower—they still might have been 3.6 in high school. Someone told me a lot of these students had trouble competing in other Cal courses, say math, physics, that they would, of course, have to take.

[Tape 30, Side B]

Fuerstenau: That I picked up just from discussions. If you take a student that's really at the bottom end of the admission pool for Berkeley, then you have a problem. I

knew quite well somebody in Electrical Engineering who was acting dean for a year, “Mac” Hopkin, Arthur M. Hopkin. I remember sitting at lunch with him one noon, and he said he was in charge of, that given year, undergraduate freshman admissions in EECS. That’s Electrical Engineering and Computer Science. He said that given the number of people that they had jurisdiction to admit—those who were not affirmative action admits—that anybody who got even one B in high school was denied admission to UC-Berkeley in electrical engineering that year. In other words, everybody they could admit that year had straight A’s in high school.

Swent: In other words, you could be admitted to Berkeley, but you had to then apply to a department?

Fuerstenau: Right, right. And then the college—everything was on quota. Maybe still is. So in the mineral engineering side, the problem was reaching that quota, which they did for a while. I know that McPherson made a great deal of effort in trying to get students into the mineral engineering program, whereas, in electrical engineering, where large numbers of students wanted to go, admission was extremely competitive.

Once again that undergraduate program was discontinued officially. Actually, in the year 2001, July. Since I’ve retired, I seldom attend departmental meetings, and I haven’t partaken officially in a lot of discussions, but the five remaining mineral engineering faculty thought they would have a better home in the Department of Civil Engineering and moved over there. I’m speaking of this engineering geoscience group of Frank Morrison, Alex Becker, and James Rector and the two petroleum engineering people, George Cooper and Tad Patzek, because petroleum had been brought back into the department, too. The whole academic field of petroleum engineering had started educationally in the College of Mining at Berkeley, under Lester Uren back maybe in the 1920s. That was the beginning of petroleum engineering as an academic field of study. Within a couple of years, three of those who transferred to Civil Engineering up and retired and the FTEs were lost to this department.

Just a little bit in the way of history: I don’t think we touched on the College of Mining. In ‘41 Don McLaughlin²⁶ came back, class of 1914 graduate. He’d been a mining geology prof at Harvard, came back here as dean of the College of Mining and then became dean of the College of Engineering in ‘43. The College of Mining joined the College of Engineering in ‘42, and then it became just a department within the College of Engineering. I guess it was ‘43 when McLaughlin went to Cerro de Pasco. And then the department went through a series of names.

²⁶ Donald H. McLaughlin, *Careers in Mining Geology and Management, University Governance and Teaching*, 1975

Trying to Settle on a Department Name

Fuerstenau: This involved another name change. As I had commented on earlier the department has had a hard time settling on a name. In 1942, when the College of Mining was incorporated into the College of Engineering, it was then the Department of Mining and Metallurgy. And then the dean of Engineering wanted to call the college the College and Department of Engineering, and so the name was changed to Division of Mineral Technology. By the time that I came here it was Department of Mineral Technology. Nobody particularly liked that name. I guess the departmental faculty at that time were unhappy over the move and didn't want the word "engineering" in their name. I was on the Budget Committee when the department proposed that the name be Materials Science and Engineering Geoscience. The chairman of the budget committee said, "If I translate that correctly, that means everything under the sun. I suggest we send it back for them to reconsider."

We were having a late afternoon meeting to reconsider the situation, I remember, at Joe Pask's house, and Jack Washburn was the chairman. It was in the summertime, and Stan Ward was away. And I said, "Why don't we just drop the word 'geoscience'?" With that, we became the Department of Materials Science and Engineering. I thought that we were the first ones to do it; years later I found out that a couple of years earlier, Cornell [University] had named their department that way, and now almost every university metallurgy department is called materials science and engineering, throughout the country. Then when we got the mineral program going again, I suggested we call it the Materials Science and Mineral Engineering Department, and now, as of last July, the current faculty dropped it back once more to Materials Science and Engineering. I have argued long and hard that it really ought to be called Department of Materials Engineering. And everything can be covered under the word, "materials engineering."

Now, at UCLA it is the Department of Materials Engineering; Santa Barbara, which has built up an outstanding materials program in just the last couple of decades, it is the Department of Materials. Now we've got so many physics-oriented people in the department that they want to accentuate the materials science name. Frankly, I think the name "materials science" has been stolen by physicists and chemists. Everybody is now doing material science. You can get that out of the newspaper, whereas I think the more traditional structure-properties people, which used to be called metallurgists, et cetera, are actually doing materials engineering. Even at a molecular level we could be doing materials engineering. Anyway, that's been how our departmental name has changed.

Departmental Review by the Graduate Division

Fuerstenau: One other thing that took place during the latter part of my chairmanship was a review of our graduate program by the Graduate Division. At Berkeley, about every ten years, the Graduate Division conducts reviews of graduate programs in all departments on the Berkeley campus. Committees of about six or eight faculty, not only from other departments in engineering but also chemistry and physics, et cetera, are appointed to do the review of engineering departments. As I recall, the review of our programs in the department went all right. At one point I complained to Associate Dean Ian Carmichael about some of the questions being raised, and that resulted in an interesting dinner with him to work things out. What I still remember is that the one lasting criticism of me was that I held faculty meetings without a published agenda! Of all things. I wrote out an agenda just before departmental faculty meetings and followed that. Nobody came to me and asked that the agenda be put out before time, but someone complained to the review committee. Well, on thinking back, through all the years that I had been at Berkeley, before my time as chairman, the department never had regular departmental faculty meetings. Really, that is incredible. The one and only one that I remember was a full meeting of all the faculty with Dean Maslach after Lysle Shaffer shot himself, and the department voted to eliminate mining because they were so upset at the chaos that had been caused by Shaffer.

Swent: Oh my...

Fuerstenau: I had decided that we should have regular meetings—but the agenda situation never even occurred to me since the committees that I had served on never had agendas because the committee tasks were straightforward and the same at every meeting. I am talking about the College Undergraduate Study Committee, the Budget Committee, the Committee on Committees, and so forth. Still that was the major criticism of me as chairman.

The last such review committee that I was involved with was chairing the review committee for the Department of Chemical Engineering. Actually, later I served on a committee to review the graduate program in metallurgical engineering at the University of Utah. Earlier, I had done this at a few other universities, such as the University of Minnesota, the University of Idaho, MIT, and Columbia. For some years, I served as an ABET accreditor, and two accreditation visits that I recall in detail were at Colorado School of Mines and Penn State.

By the way, for several years I was what is called an “under the line” professor of chemical engineering, and I regularly attended their departmental faculty meetings. That department had weekly lunch meetings at the Faculty Club, where departmental issues were discussed. Academic personnel actions were never discussed at these meetings. When I became department chairman, I began to get too busy to participate in chemical engineering departmental

activities and dropped that. But that was a real contrast to having no faculty meetings in our department.

XVII SOME EARLY MINING AND METALLURGY ALUMNI, AND SCHOLARSHIPS AND ENDOWMENTS

Fuerstenau: I would like to tell you something about early alumni, some of whom have contributed financially to endowment support in the department. I have read that in 1888, the College of Mining was 12 percent of the campus. No longer. [laughs] Amazing thing.

One early alumnus that I'm aware of was Charles Merrill, an 1891 graduate, who is the person that really made cyanidation of gold ores work. As you probably know from the Homestake history, he spent ten years up in Lead, putting in the cyanide operation there. Prior to that he had worked at Bodie. And then in the Class of 1900 was Edwin Letts Oliver, who invented the Oliver filter used in dewatering mineral slurries. That's how I knew the name: Oliver filter, long before I ever knew he was a Cal alumnus. The Oliver filter is designed with a series of parallel disks mounted on a rotating shaft, whereas the filter invented by Dorr was a rotating drum. The Oliver filter would provide greater capacity in a smaller space.

The most famous graduate in mining was Rube Goldberg, Class of 1904, who made his name in the world of cartooning. Without doubt, he was one of the two most famous Americans ever to have graduated with degrees in mining engineering. His name is now a noun in the dictionary. I have submitted a nomination for him to the Mining Hall of Fame and it will be interesting if the selection committee ever goes along with it. History will tell. The other more famous mining graduate was of course Herbert Hoover, who was an early Stanford graduate. As you know, Hoover's early mining career was in Australia. I remember seeing the house where he lived when he managed a mine along the Golden Mile in Kalgoorlie.

There are a number of others, and I will only mention a few whom I have met and can think of off the top of my head. Henry Day, who founded Day Mines up in Idaho, graduated in mining from Cal in 1922. Frank Aplan worked for Day Mines, which later became part of Hecla. Plato Malozemoff²⁷ was a graduate of the class of 1931, after which he went to Butte to study under Professor Gaudin. Also in the Class of '31 was Frank McQuiston²⁸, who in more later years I got to know reasonably well. Outstanding industrial process metallurgist. And I also got to know Plato Malozemoff reasonably well later

²⁷ Plato Malozemoff, *A Life in Mining: Siberia to Chairman of Newmont Mining Corporation, 1909-1985*, 1990

²⁸ Frank Woods McQuiston, Jr., *Metallurgist for Newmont Mining Corporation and U.S. Atomic Energy Commission, 1934-1982*, 1989

on. I guess the first time was when he came out for some of our mining discussions. He, of course, was always quite a reserved sort of soul, as you know. He was really the leading mining executive of the country, president and chairman of Newmont Mining Company, and I think was considered universally as the “dean” of mining executives. Another of those major metallurgists was Dee [Dimitri] Vedensky, who graduated in 1927—I will want to fill in something a little bit in relation to him shortly. I think their degrees were actually called mining and metallurgy, although all of these people really were metallurgists.

Dee Vedensky I think worked right here in Berkeley for quite a few years, for a company that made jigs and so on.

Swent: Pan-American, I think it was. He and Malozemoff both worked there.

Fuerstenau: Yes, yes, right. I suppose in the thirties they did that, which obviously tied them together. Frank McQuiston later was closely associated with Malozemoff in that he became metallurgy vice president of Newmont.

Then in later years was Bud Wilson²⁹, who graduated in 1948 in metallurgy. Over the years, I had interacted with him a number of times, particularly before he went to Australia. As you know, he headed Utah and then BHP. Of course, Don McLaughlin, who was Class of 1914, about whom we have already talked and someone who I really got to know well in later years.

Endowments and Scholarships

Fuerstenau: I actually had a lot of interaction with some of these people on what would be the next topic; I’d like to talk about endowments and scholarships in the mineral field here at Berkeley. And all of these have a strong relation to some of the names we just mentioned.

Sometime during my first year here, someone from the UC president’s office called me and suggested that we meet with the head of the Merrill Foundation, who he knew personally. I don’t recall the names of either person. But we had lunch one day at the Pacific Union Club in San Francisco. The discussion was that the Merrill Foundation might give a grant of something like \$50,000 to equip a laboratory facility in the name of Charles Merrill. It was a good thought but nothing ever came of it—probably the Merrill Foundation aimed its direction towards cultural programs, and not technical ones.

²⁹ Alexander M. Wilson, *Leading a Changing Utah Construction and Mining Company: Utah International, GE-Utah, BHP-Utah, 1954 to 1987*, 2000

Swent: That could have been.

Fuerstenau: Right from when I first came here, there was an Everett Letts Oliver Fellowship for graduate studies in extractive metallurgy. I never did know how it was financed. The first major endowment with which I was involved is the Jane Lewis Fellowship Program, which was first started in 1962. A person named Martin J. Heller, who had been a student here but who had never graduated, wanted to establish fellowships in the broad field of mining in the name of his mother, Jane Lewis. The beginning of this was the probating of his will in 1962. As the will got settled over a period of about two or three years, the bequests finally totaled \$800,000. Out of general interest, the 2000-2001 income—in other words, fellowships for this year—generated \$250,000. In general, UC—I know this from looking into that—pays out about 4 or 5 percent, puts the rest back into the corpus of the endowment, and a year ago, the corpus of the Jane Lewis endowment was listed as \$8.5 million.

Swent: My!

Fuerstenau: Way back, in the first five-year period, the Regents proposed that the College of Engineering set up a committee to see how you would administer these fellowships in the broad field of mining, and so that was done, and we defined mining as anything related to the mineral/petroleum field. There was membership on what we called the Jane Lewis Committee from two or three departments. This committee is a college committee; it's not administered by the Graduate Division, and things have thus far worked very well, of issuing these fellowships for all of these mineral-related areas. It's for graduate work, and it ranges from geological engineering, applied geophysics, petroleum engineering, extractive metallurgy, and of course when the mining program was active, it related to the mining people. When we got the Jane Lewis money going, the mining per se wasn't here then either, so that's why we had this broad definition of what constitutes mining.

Then another item that came along while I was chairman is what has become the Phoebe Apperson Hearst Foundation. Ann Apperson Flint gave a gift in the name of her aunt, Phoebe Hearst, and she wanted this to be called the Phoebe Apperson Hearst Foundation. Ann Apperson Flint had two husbands. One was a professor of medicine at Princeton. Must have died. Another was a professor of medicine at Yale. Her bequest, which was handled by a bank in Connecticut, stated that a third was to go to the medical school at Princeton, a third to the medical school at Yale, and a third to go to the College of Mining at Berkeley. When her will came forward, I wrote pretty lengthy reports—this is now almost thirty years ago—providing an analysis of how this department was an outgrowth of the College of Mining at Berkeley. Her will stated that funds could be used for visiting professors, as I recall, for publication in the mineral area, et cetera, but could not be used for scholarships. We have used it for bringing in visiting faculty and paying for seminar speakers. Way back, with Chuck Meyer, Professor Charles Meyer, we had a program for several

years that brought in from each continent a world mining geologist who gave a seminar course and wrote up his seminar series. Those should have been published as a book or a series, but I don't think it ever was done that way. One major visitor was Professor Eric Rudd from the University of Adelaide, and at the time he was the non-executive chairman of Poseidon, the big nickel enterprise in Australia. Overall, that was quite a nice, a good use of Phoebe Apperson Hearst Foundation funds, and that still exists.

Then one day the college organized what was called a Distinguished Alumni Day. It was college-wide, with different departments holding their own meetings. At that time, I was department chairman, and sometimes for a thing like that I might put together a brief kind of a talk and wing it. But I worked on the presentation and made a pretty good talk, which I gave just before lunch. I remember walking down to lunch with Don McLaughlin and Steve Bechtel, Sr. He, I think, was a Class of 1921 graduate of civil engineering at Cal. The lunch was held down at the student union. Anyway, there was a good turnout of alumni related to this department, maybe sixty or seventy people in the room. There was a VP of Arco for petroleum, and several of the local people who were fairly big names in the mineral industry who I hadn't even realized were UC-Berkeley graduates.

Right after lunch, Don McLaughlin stood up and opened his remarks; he said something like, "In response to that wonderful talk by Doug Fuerstenau—" As I told you, I actually put together a pretty good spiel. I spent a little time and got a little filler stuff to make it sound good, roughly related to where I thought the mineral field should be going. The next day, the phone rang in the morning, and it was Dee Vedensky, and he said, "I was so inspired that I've changed my last will and testament and I am going to give"—whatever, \$100,000 maybe—"of my Hanna stock for the establishment of scholarships." He had retired as VP of Hanna Mining Company. I told him how to express his wording (I wanted it to be in the extractive metallurgy area) as broadly as it could be. Actually, when he retired he started out by giving the university a chunk of Hanna stock, so we already had a Vedensky Fund; that's where that came from. But "I got so inspired—"

Swent: That *must* have been a good speech!

Fuerstenau: Yes. Then later—Vedensky, who had also retired from the Homestake Board, told me that his wife had died recently, and that he also wanted to give her Hanna stock to the Vedensky Fund. He told me that he wanted to find out how to do this from the best tax point of view, and so I started to look into it for him, and then unfortunately he had a heart attack and died. I called up his son—his son is an architect in the Bay Area—a few months later and I told him that his father was going to give his mother's stock to the Vedensky Scholarship Fund. Well, he didn't say, "Well, okay, I'll honor my father's wish." We didn't get any more. His mother's, he kept. Vedensky's gifts are

very valuable right now, because it supports a couple of grad students that I personally have, and it all came from that route.

Then another item that I found kind of interesting—and, you know, I don't remember what was behind it, but it was part of a program in the Department of Education that provided fellowships for graduate students in the mineral resources field. For at least three years, this minerals fellowship program was in place, starting when this buildup of mining took place. Maybe this fellowship program of the Department of Education may have also contributed, at least in the education of graduate students. All of a sudden, there was a need for advanced people. For a couple of years I was on a panel in Washington that made the allocations. This was quite a big program that required formal proposals to be submitted. There may have been a dozen people who served on this panel. A lot of the panel members were from the Bureau of Mines, but there were some from different universities. We at Berkeley were awarded twelve of these fellowships each year. I was behind the allocation of where they went here. I made sure that a couple of them went to mining geology, to the mining program, and extractive metallurgy. George Brimhall was a new assistant professor in mining geology and that helped him get going.

Let me tell you, going back to serving on that judging panel: this panel met in the office area in what was then the Department of Education and probably much smaller than it is today, and I'll tell you, I never saw anything like it in my life before. Big people sitting in big offices with nothing to do. I can still recall two or three of them sitting together in an office, not with us, doing nothing, while our panels were deliberating on fellowship allocations for two days. So when I hear things about the Department of Education, I think of these fairly senior people that appeared to me just to be sitting either by themselves or talking, without much to do, in big, fancy offices.

The Establishment of Two Mineral Engineering Chairs in the Department

Fuerstenau:

There were two other major endowment activities involved with establishing chairs during my time. One was: Mac Hopkin was the acting dean when Kuh was on sabbatical, and he talked to me about the idea of establishing a chair with Newmont Mining Company. He may have been approached by Frank McQuiston because McQuiston arranged for a meeting of myself and Acting Dean Hopkin with the president of Newmont Mining Company, and we went over to the Fairmont [Hotel]. My recollection is that the American Mining Congress was meeting there at that time. There we met first Frank McQuiston, who stated that Newmont would like to give some sort of grant to UC, and so Mac Hopkin was the one that suggested that we start working on the establishment of a chair. And then after a few minutes, we were joined by the president of Newmont, Jack Thompson, Sr. That was the first time I met him.

We had a very cordial, good meeting, and the result was that Newmont began a process of giving something like fifty, eighty thousand dollars—that much one year and that much another year. One year it was Exxon stock, as I recall, so it must have been that Newmont invested in other companies. About every year I would have to write and ask, “Are you going to make your contribution to this fund?” We just called it the Newmont Chair Fund. When Malozemoff retired as CEO, Newmont then added up all the rest to make it a fully endowed chair, to be called the Plato Malozemoff Chair. Well, I made a big mistake because there was already something like forty, fifty thousand dollars accumulated earnings. I proposed that this be put back into the corpus. They had to take that to the Regents, and the Regents approved that move. The mistake was I should have left it in the funds to be spent by me, because I was named the first Malozemoff Professor of Mineral Engineering. I remember printing up a lot of business cards that said “Plato Malozemoff Chair.” I met him somewhere, and he said, “I prefer P. Malozemoff. I always sign my name P. Malozemoff.” So I had to change everything; all the letterhead and business cards got changed to P. Malozemoff Professor of Mineral Engineering.

Then later, Karl Pister, who was then dean of engineering, proposed another mineral engineering chair. You heard Pister make a presentation at Don McLaughlin’s funeral memorial when he read some paragraphs from McLaughlin’s oral history. Towards the end of Don’s life, Pister proposed that there be established a Donald H. McLaughlin Chair of Mineral Engineering. He and I went to see Don at his home. He was in bed, and this was probably just a couple of weeks or so before he died, and I remember Pister telling him that the college had decided to establish this chair in his name. This was brought, naturally, to Homestake, and the decision was made to give—I think it was for Homestake Mining Company to match one dollar for every two dollars given towards the McLaughlin Chair—it wasn’t a one-to-one match, but could be raised. One of the Homestake board members—I think it was Bob Jaedicke from Stanford—said, “Give a little incentive to get other people to give.” And so I wrote—

[Tape 31, Side A]

Fuerstenau: I wrote many, many letters to lots of people, and quite a few people gave quite significant amounts. Then finally somebody at Homestake—this is now about two, three years down the path—said, “Let’s just finish this out.” Homestake then finished out the chair. You know, a chair here started then at \$250,000 and now the minimum is maybe \$500,000, because chair funds at UC are not used to pay nine months’ salary. It’s not like a chair at Stanford that will be paying a regular salary, or MIT or Harvard. These chairs just pay perks and add-ons such as summer salaries, secretaries, student support, equipment, travel—in other words, they make the professional life of the professor better, whereas the nine months’ pay comes out of regular university budget funds.

[tape interruption]

Fuerstenau: I was the first Malozemoff professor, and Neville Cook was the first McLaughlin Professor of Mineral Engineering. Then, when I retired—I'm still a little angry about the turn of events at the time. When people retired under what was called VERIP III, they were able to keep their chairs, and I retired under what they called the VERIP II and, you know, I lost my chair. Here I spent all these efforts getting these chairs. That chair went to Frank Morrison, and after he retired it went to Jim Evans. The current holder of the McLaughlin chair is Fiona Doyle, whose main efforts are in the field of hydrometallurgy, so that this fits very nicely into mineral engineering. She is currently the chair of this department

XVIII

THE ROLE OF TECHNICAL JOURNALS AND PUBLICATIONS

Fuerstenau: The next thing I want to talk about is technical publications in the mineral processing field. I personally believe there's a real role for technical journals in one's field. I think I've published something like four hundred papers already in my career in the technical journals. A lot of them in the original days were in the *AIME Transactions*, and I served on that editorial board way back.

The International Journal of Mineral Processing

Fuerstenau: About 1974 I was in Germany, and Klaus Schoenert showed me a letter that Professor [Hans] Rumpf had gotten from Elsevier about establishing a journal they were going to call *Mineral Processing*. I guess Schoenert sent me a copy of the letter. The editor was to be Pierre Gy, who was probably the world expert on sampling, the statistics of sampling, related to sampling mines and crushed ore samples—how do you sample an ore body; basically the mathematics of that. He lived in Cannes in France.

I looked at this outline, and I wrote to the people at Elsevier saying, “Hey, there's already an American trade journal called *Mineral Processing*. Why don't you call it *International Journal of Mineral Processing*?” I gave them a whole bunch of suggestions. Back came a letter, and the department ad[ministrative] assistant—I was then department chairman—Judy Roberts, said, “You get what you deserve; they now want you to be co-editor in chief.” [laughs] I think I started out as editor for the Americas, and then about a year later they made me co-editor-in-chief. So I was co-editor in chief from 1975 to 1999. This has built up to where it really is the primary international journal in the world of mineral processing, and the stature is very good.

Swent: Where is this published?

Fuerstenau: In Amsterdam. Elsevier is the major publisher of science journals, and they have a huge Earth Science Division; probably the biggest technical publisher worldwide. They have taken over Macmillan and more recently Pergamon Press in England, so that they're even bigger. They publish hundreds of journals and many books. When the *International Journal of Mineral Processing* started, Professor Marston Fleming in England was rather angry with me because the *Transactions of the Institution of Mining and Metallurgy* in London had always considered itself to be *the* international journal in the field.

When Pierre Gy retired, they made then Jean Cases, also from France, the co-editor in chief together with me. Cases was a distinguished professor at École

Nationale Supérieure [de Géologie] at Nancy. I had first met him in 1963 when he was finishing his doctoral thesis on flotation chemistry. Cases retired about a year ago or two years ago, and after twenty-five years I stepped down from that role and was named honorary editor in chief.

[Added by Douglas Fuerstenau during editing: When I stepped down from editing the *International Journal of Mineral Processing*, followed shortly by Jean Cases, I proposed that perhaps there could be a single editor in chief and that that be Professor Peter King at the University of Utah, originally from South Africa and an internationally renowned expert on comminution, mineral liberation, and computer simulation of mineral processing circuits. In 2003 Peter King was diagnosed with acute leukemia, stepped down from the editorship. King died in September 2006 at the age of sixty-eight, a great loss. He was succeeded as editor in chief by Professor Subhash Chander at Penn State. Chander had received both his MS and PhD degrees under me at Berkeley and had international stature for his excellent research on the electrochemical behavior, surface chemistry, and flotation of sulfide minerals. Tragically in December 2006, Subhash Chander totally unexpectedly died at the age of sixty, with great loss not only to his family but also to the international technical world. Elsevier has now appointed Dr. D.R. Nagaraj, a flotation chemistry researcher at Cytec and former graduate student of Somasundaran, and Professor Kari Heiskanen of Finland as co-editors in chief of the journal.]

Swent: It took a lot of your time.

Fuerstenau: When I took it on, I called up Al Lieberman who was the American editor for *Powder Technology*, which is an even bigger Elsevier journal, and I asked him how much time did being an editor take. It didn't sound too onerous, so that's when I accepted taking the job on. A lot of the effort is sending out manuscripts for review, and then the dinging letters—you know, "Give me back your review." Of course, then I had a secretary who did that. Sending papers to some people is like sending them into a black hole: you never get them back. [laughs] I'm kind of a black hole myself at times on that, too. Also Elsevier asked me to be the advisory editor of a monograph series on *Advances in Mineral Processing*, which also I took on—I haven't been too active in that, but there's maybe fifteen books that have come out in that series in various fields of mineral processing, and I still have that role. They want to try to increase the activity, but I will step down from that too.

Participation on Planning and Editorial Activities of Other Journals

Fuerstenau: I was also on a committee of the American Chemical Society to establish a journal in colloid science, colloid and surface science. So here I, a mineral processing metallurgist, was a member of a committee of about six to consider the problem and make recommendations about a new chemistry journal. One

of the major surface chemists in the U.S., Arthur Adamson at USC, who has written the main textbook in surface chemistry, was chairing this committee. Several people were on it. Our meetings were only by telephone. I learned from those meetings that there is a real pecking order difference between wet surface chemists and dry surface chemists. The wet ones can work with rather simple apparatus, whereas the dry ones need high vacuum equipment and rather fancy instrumentation. Finally, one member of the committee, Karol Mysels, suggested that we name the journal *Langmuir* after the great American surface chemist, Irving Langmuir. That journal now is published by the American Chemical Society. Langmuir got a Nobel Prize for his work in surface chemistry around 1930 or so—for work that he did at General Electric. Actually he wrote several papers on flotation, which is kind of interesting, way back in about 1919. By the way, he graduated in metallurgical engineering from Columbia about 1895 and pursued doctoral studies under Nernst in Germany.

I had suggested before that, that another journal get started on the applied aspects of colloid and surface chemistry, and I didn't want to edit it myself because of my efforts with the *International Journal of Mineral Processing*, so I suggested to my former student, Somasundaran, who was now at Columbia, that he make the proposal. I also suggested that he take it to Elsevier. He had talked to a couple of other publishing companies, as to whether they were interested; Marcel Decker being one. That journal is called *Colloids and Surfaces*. It has become a massive journal because that's such a big field. I think they come out with six volumes per year but now split it into the physical sciences and engineering and the biological sciences because of the great activity in this field. Soma is still the editor in chief, although Elsevier is suggesting he retire from it. But that idea came from me. Perhaps Soma doesn't even remember that I was the person that initially proposed the establishment of this journal. By the way, earlier I had also served for several years on the Editorial Board of the *Journal of Colloid and Interface Science*.

Swent: Journal publications certainly must have specific importance in technical fields.

Fuerstenau: Perhaps I can explain this by talking a little bit about AIME journals. I think AIME/SME has the wrong outlook on publications. The long-term role of any professional society should be their archival publications. What happens at annual technical meetings is here today and gone tomorrow. The SME, the hierarchy, doesn't seem to understand that concept. And, you know, from what I understand, when they were analyzing this in a big way a few years ago, they found that they were actually subsidizing the publication of *Mining Engineering*. Now the monthly members' throw-away magazine, *Mining Engineering*, ought to pay its own way through advertising. If it can't, it should be cut back. Yet they've established a journal which is called *Minerals and Metallurgical Processing*, for publishing technical papers, and they expect that to pay its way through subscriptions and page charges and this sort

of thing, and be self-sustaining. That journal is published quarterly, with only sixty pages per issue. I think it's a very poor quality journal in appearance. An organization like SME should subsidize archival publications.

The person who succeeded me as the editor of *the International Journal of Mineral Processing* is Peter King at the University of Utah, a good person for the job. I remember about three, four years ago, Peter King telling me, "You know, I'm almost embarrassed to send out reprints from the SME journal, *Minerals and Metallurgical Processing*." The editor in chief of *Minerals and Metallurgical Processing* for many years was Roshan Bhappu; he's now president of the AIME. He contributed zero to that journal, in my opinion. It just is one of the world's poorest-quality—I'm speaking not of the content of the papers, but the appearance of figures, tables, page organization—journals in the world; even the Indian ones, in my opinion, look better. I remember once at an SME meeting, the Elsevier person in charge of their earth science journals met with me, and I showed him a copy of the SME journal and his comment was that it was not even professional! And now Bhappu's been succeeded by another person, Komar Kawatra, at Michigan Tech. I have even given him comments about how to improve the journal. But it just never seems to improve; I think it's really a pathetic thing.

I have served on the editorial boards of a number of other journals related to the processing of minerals and particulate materials. This includes *Mineral and Extractive Metallurgy Review*, *Journal of Colloid and Interface Science*, *Colloids and Surfaces*, *Coal Preparation*, *Particle Characterization* and even *Deep Sea Mining*.

Another major thing on journals: there's a Japanese company that manufactures machinery and instruments for powder processing, called Hosokawa International. Hosokawa has acquired quite a few different companies around the world that manufacture particle processing machinery. A significant one in Europe was their acquisition of Alpine, where Professor Rumpf had worked before he went to Karlsruhe. The subsidiary in this country is called Hosokawa Micron International. They publish a journal, and it's up to about Volume or Issue 21 or 22, one per year. It was started by the president of Hosokawa, and it's called *Kona*, the Japanese word for particle. This president-founder of Hosokawa wanted to publish in English, Japanese papers on powder science and technology, and include papers from other parts of the world. There's an editorial board for Europe, an editorial board for the Americas, and since about 1991, I've been—not initially, but for about seven years the chairman of the editorial board for the Americas, of *Kona*. We solicit, collect and evaluate a number of papers from the Americas. And the company pays for the whole works and distributes the journal free. It's really a good, very good publication and an interesting service.

Swent:

These are papers that would not appear in any other place?

Fuerstenau: Oh no. The Japanese ones are probably published in Japan and then translated into English; they're all in English; a lot of it's German and Italian stuff, but translated or written in English for *Kona*. Some of them are original papers, some are review papers—and in my opinion, they should be mostly review papers. I have one paper published there, which was a review on grinding aids in comminution, quite a long review that I put together years ago, some years ago, and more recently, I have another review paper on grinding models coauthored with two former students, Asoke De and Prakash Kapur.

Swent: A review, that is, of other papers.

Fuerstenau: By review, that's what I mean—a paper that presents a summary analysis that reviews published papers on some aspect of a technical field, like grinding aids, particle classification, particle shape, agglomeration, and so forth.

So concerning the Americas board of *Kona*, we have U.S., Canadian, and also a couple from South America. In the past, the board was more mineral-ceramics related, but now there's somebody in pharmaceuticals, and I think as the board retires, they're going to probably move into more people dealing with pharmaceutical powders and things like that, very fine powders, nanomaterials, and less on mineral particles because that's the way their sales have gone, I have been told. That has been an interesting thing. As I say, I got on that board, and after a while they made me the chairman.

Publication of Papers in Conference Proceedings

Fuerstenau: Most symposia or congresses publish papers presented at the meeting as a book of preprints or as a proceedings. Generally, researchers do not particularly like to publish their work that way because publication in archival journals carries much more weight. I assume that you have observed how often one sees the term archival journal.

Swent: Certainly.

Fuerstenau: As for my own papers, I spend just as much time and effort on preparing a manuscript for a symposium proceedings as I do for publication in a refereed journal. So in my own case, there is essentially no difference in the quality of a paper in an archival journal or in a proceedings. Often, today some symposia papers to be published in a proceedings are refereed. In my opinion, the best way to handle papers from a symposium is to submit them as a collection to a journal, such as the *International Journal of Mineral Processing*, *Powder Technology*, or *Colloids and Surfaces*, and publish them as a special issue. I have chaired a flotation meeting and participated in several comminution symposia where papers were published in this manner. Sometimes papers from meetings are published as hard-bound books—some are excellent whereas others have little long-term value.

Service to Professional Societies: AIME, SME

Fuerstenau: Then there's another area that I devoted a fair amount of time to over the years—professional societies, and I'm speaking of mostly the mineral field. Through the years, I organized some technical meetings, chaired a lot of sessions at meetings, and served as a member of advisory committees when conferences were being organized. Way back, I got involved in—as do people in the hierarchy—the Mineral Processing Division, which I told you a little bit about when I was at Union Carbide. And I'd worked my way up in the chair system and in 1967 was division chairman of the Mineral Processing Division—then it may still have been called Minerals Beneficiation Division.

Swent: That's the division of the AIME.

Fuerstenau: Actually now a division of SME. I was also then, for three years—1967 to '70—on the board of directors of the Society of Mining Engineers. That, you know, has several divisions, and when you're division chairman, you're on the board also a year before and the year after. It's a three-year term. I personally chose not to go any higher in the SME hierarchy. Several times somebody said, "I'll put your name up for president," and I said no. My brother, Maurie, was president of SME, but I personally chose not to do it. I also did not stack the organization with friends and former students because I was fairly young when I was chairman, and, you know, then I had no students that had graduated and moved up in the system professionally at that early date. Several people through the years have stacked committees with their former students. It has been pointed out to me by others at times that that was going on. But I was not guilty of it because I had no former students back then. This was '67.

I've always told people when you become an officer such as chairman of MPD or president of SME, you ought to have an agenda, something you want to accomplish. You know, I would say that the bulk of the people who become division chairman or president of SME, or president of AIME really accomplish little or nothing new. They just do the job and add it to their resume. I always give myself some credit in this way. The MPD luncheon in those days was always Thursday, and many people had left the meeting by Thursday. There was the Extractive Metallurgy luncheon on Tuesday and the Mining luncheon on Wednesday. People said, "Well, we conflict with these lunches." And I said, "Our purpose should be to have our lunch and program for our people. We don't care about these others." I moved it from Thursday to Wednesday. And, boy, was everybody pleased! Attendance went way up because, you know, by the fourth day, fifth day, people are on their way home. So I always consider I made a significant contribution as chairman.

I served on a lot of the different AIME/SME committees. The AIME, you know, was separated into separate societies: SME, Society of Mining Engineers; TMS [The Metallurgical Society]; SPE [Society of Petroleum

Engineers]; then ISS [The Iron and Steel Society]. They're legally separated. It really is too bad that SME and TMS no longer meet together because when you're in the extractive metallurgy area, you overlap the two. The SME used to be Society of Mining Engineers. Then they changed its official name to Society for Mining, Metallurgy, and Exploration. Still keeping SME. So the person who was president of TMS was actually going to sue them for using the word "metallurgy." So TMS was The Metallurgical Society and is now the Minerals, Metals, and Materials Society. So the hierarchy then had gone back to doing name infighting.

I knew quite well Al Weiss, who was executive director of AIME. Did you ever meet Weiss?

Swent: No.

Fuerstenau: You met his successor, Nellie Guernsey. I remember being in Australia at some meeting when Weiss was there, because it was a joint AIME-Australia meeting, and so he was telling me there about a lot of the problems that he had. He was the chief honcho. And all the societies wanted to bust up the AIME and take their share of the endowment. He said they would spend a lot of time with the lawyers because of the way the original endowments and so on are written, they couldn't do that. He had to spend a lot of his time fighting these societies. Each separate society wanted their share to help pay their bills. Nellie Guernsey obviously was not able to keep the societies at bay, because they have now distributed the AIME endowment to the constituent societies and AIME is now merely a shell of itself.

Swent: Times do bring changes.

Fuerstenau: Another great change was attendance at San Francisco SME/AIME section meetings. In the 1960s and seventies, meeting attendance was several hundred. For some meetings, which then were held at the Engineers Club in San Francisco, people who could not get in for the dinner came for the talk. After the Engineers Club building was sold, meetings were held in Oakland in the Kaiser Building. Attendance over the years dwindled to about zero and now the San Francisco Section is defunct. During that period, there were several major mining construction companies going full blast and several mining companies headquartered here. But that is all in the past.

Swent: An unfortunate change with time.

Fuerstenau: Speaking of the times, during the years of the Free Speech Movement at Cal and the protests against the Vietnam War, at San Francisco section AIME meetings, anytime Cal was mentioned there would essentially some jeering and anytime Stanford was mentioned there would essentially be cheering. Probably most of those there at the meetings had no connection to either institution, but they really were pro-Stanford.

Swent: I guess that was an expression of bias brought about by the times.

Fuerstenau: The same downsizing has happened in the Engineering Foundation. You know, that started with Andrew Carnegie writing a check for a million dollars in about 1901, at which time there were five founder societies, one being AIME and the others civil engineering, mechanical, electrical, and chemical engineering. And these founder societies also worked on trying to get that endowment away from the Engineering Foundation and back into their own societies. Their main asset was a building that I think Donald Trump wants to buy to build a major high rise condominium. I think that the Engineering Foundation is no longer what it was because the building was sold and the funds have gone to those constituent societies. I have learned from Somasundaran that they even want to stop the Engineering Foundation research conferences because they're afraid they'll get sued if somebody breaks a leg at a meeting, and stuff like that. So now the Engineering Foundation only wants to give out a program of research grants. I think they've had a very good conference program back through the decades.

By the way, Somasundaran has been on the Engineering Foundation board for a number of years and I believe was chairman for a term. As you know from Frank Aplan, he served for about ten years on the Engineering Foundation board and was chairman for quite some time. In recognition of Frank's long service to the Engineering Foundation, they established the Frank F. Aplan Award during Somasundaran's chairmanship. Soma and my former student Subhash Chander spearheaded that. Chander designed the award plaque. With the change in the Engineering Foundation, the Aplan Award has been transferred to the AIME.

Perhaps the greatest contribution of an Engineering Foundation board chairman was that of Professor Gaudin. During his tenure, for a couple of years all their funds and effort were directed towards the establishment of the National Academy of Engineering [NAE]. Gaudin was one of the twenty-five founding members of NAE in 1964.

Another one: Rush Spedden was the director of research for Kennecott in Salt Lake City and very active in SME. I don't know whether he became AIME president or not, but I know he was SME president. And he, I think, was the real leader behind moving the headquarters of SME from New York to Salt Lake City. You know, they were there for quite a few years. The SME staff wanted to move out after ten, fifteen years of being in Salt Lake. I always thought it wasn't the right place for the headquarters of a major society. Eventually the SME board voted to move the headquarters to Denver, actually to Littleton, a suburb of Denver. They were able to get the land, and I suppose there's an industrial park there that offered them the land. But a very active SME person who was president of EIMCO, Wayne Dowdey, told me at the meeting when they voted to move from Salt Lake back to Denver, that Spedden took it all personally. Dowdey said he told him, "Rush, you know,

you're my friend. This has nothing to do with friends at all. This has purely to do with what is better for the society." And, you know, I never saw Spedden again at another AIME meeting or SME meeting. It was a personal thing.

[Added by Douglas Fuerstenau during editing: I have seen Rush Spedden, who was born in 1917, on a couple of different occasions in the last few years. I am always amazed at his lucidity and youthfulness. Unfortunately without success, I had tried on two occasions to interest him in doing his oral history. My friend, Frank Aplan, sent me a copy of an article published in a 2006 Pennsylvania newspaper, the *Centre Daily Times*, datelined Alta, Utah. That article states that every day in the winter ski season, there are seniors who meet for lunch at an Alta eatery reachable only by skiing. As written: "Rush Spedden, eighty-nine, with two practically brand-new artificial knees, was the most senior skier on a recent day. Last season, he skied thirty-eight days. But Spedden scoffs at any special mention. The club includes active members in their early nineties...."]

Perhaps I should add that I did serve on committees for a number of other societies. For example, for many years I was chairman of the Crushing and Grinding Committee of AIChE [American Institute of Chemical Engineers], and their Particle Processing Committee. I was a councilor for a number of years for the Fine Particle Society, and served on the board of the Marine Mining Society.

International Mineral Processing Congresses; Steering and Scientific Committees

Fuerstenau:

An area that I spent quite a bit of effort on: when the International Mineral Processing Congresses [IMPC] started, Rush Spedden went to the second one, which was held in Goslar, Germany, in 1955. He told me there were only four Americans there, and they named him the American member of the International Scientific Committee, because he was the only one of the four related to mineral processing research. He served in that role for a number of years. The mineral processing congresses had been really started in 1952, with three self-appointed leaders: Professor Marston Fleming of Imperial College in London; Professor Per Kihlstedt, of the Swedish Royal Institute of Technology; and Jacques Astier, from IRSID [Institut de Recherche de la Sidérurgie]. That's that major iron and steel research institute in France in Metz. Astier was active in this until only a handful of years ago, but these three people really sort of looked at the IMPC as their baby and were always behind it. They more or less wanted to have the congresses be European. Maybe later I will tell you a little bit about some of the events at these congresses. When Rush Spedden stepped down from being the American—

[Tape 31, Side B]

Fuerstenau: Rush Spedden stepped down, and he appointed Nat Arbiter to be his replacement. Arbiter did that, and at the congress in São Paulo in Brazil, which I think was 1977, Arbiter had to leave a day early, and he asked me to sit in on his place on the Steering Committee meeting, and the scientific meeting too. After that, he retired from it. Incredibly, the MPD committee appointed his replacement to be Roshan Bhappu, who had never once attended a meeting then and never has since. Somebody went to work and immediately got that changed to name me the American member of the IMPC Steering Committee and IMPC Scientific Committee, a job which I held until '97, I guess, when I decided to step down from it.

Early on, there always was a problem of balancing things with Eastern Europeans, the Russians, and West Europeans; that was being done by the Europeans; and between Russian input and American input. You could see global politics entered into the way they looked at it.

Swent: Was there Asian participation?

Fuerstenau: Well, let me tell you. Yes. Any country that has a technical organization can appoint a member to the International Scientific Committee. So in other words, we're talking about a technical society like the AIME in any country worldwide. The people who are on the International Steering Committee for the mineral processing congresses can only be from a country that has held a congress. So that's a smaller group, particularly since—you know, three congresses may have been in England and two or three in Germany and a couple in Sweden, and now at least two here. In '93 one was in Australia. So each one of those countries then can have a member on the Steering Committee, the member also being the representative on the Scientific Committee. The Steering Committee acts to decide where the next congress should be, et cetera.

For a while, they were having congresses at various intervals, only one year between one time and sometimes two years, one time almost four years. I proposed that the aim should be to have a congress held every three years. It makes sense to get it onto a firm time schedule, and the three-year interval is about right since there is not that much new in one or two years. Several countries apply for each because they can make profit off this. They can lose money but they can also make money. The decision of the venue, and so on, is made by the Steering Committee. The Steering Committee meets once in between congresses to visit the site of the pending congress and once the day before a congress begins. General policy, and so on, is made by the Scientific Committee, which is a much bigger group, in one meeting during a congress.

One meeting of the Steering Committee, I recall very well. In 1991 the IMPC was to be held in Dresden, and the Steering Committee was to go to Dresden to review progress and look at the venue. This was just weeks before the East German wall came down. I had read in the paper and seen on TV that the East

German soldiers in some of the cities had bullets in their guns—so I faxed Eric Forssberg that I was canceling out of the trip. Nothing happened, but I did not want to be there under those conditions. Anyway, after serving in that capacity for twenty years, I stepped down and my successor in that capacity is Somasundaran.

And now the whole governing structure has been reorganized with an IMPC Council instead of the Steering Committee. Professor Cyril O'Connor of Cape Town in South Africa nicely led the reorganization and rewriting operational rules.

Swent: Should we take a little break?

Fuerstenau: Yes, let's take a break.

[tape interruption]

XIX THE MINERAL INSTITUTE PROGRAM; THE BUREAU OF MINES

Swent: We had a little break here. You wanted to say something about the Mineral Institute.

Fuerstenau: Yes. I think it's worth recording a little bit of the history of what went on. Dr. Elbert Osborn, who was the vice president of research at Penn State, headed a study that ended up in several small soft-covered books or booklets—and I was not involved with that study—of research needs in the mineral industry. There was a book on mining, a book on maybe exploration needs, maybe a book on extractive metallurgy, et cetera. And this was kind of a bible for a few years.

Swent: When was this?

Fuerstenau: Must have been roughly 1970. I have all those books, but after having packaged stuff to move out of Hearst Mining Building, I can't quite lay my hands on them. These were kind of the bible for looking at what technology needs are throughout the mining industry. There may have been one related to coal, for that matter.

Out of that, Osborn had the idea of establishing mineral institutes in each state. To start, there was a professor of mineralogy at Penn State named Tom Bates, who I got to know quite well when we met to plan how to implement a program of establishing mineral institutes in the country. There was just a handful of us, and for some reason, I was asked to be one of the group. When we would meet at times, it seemed that Tom Bates acted as the leader, not Osborn, who must have delegated that to him, heading meetings and discussions. We also were in Washington, and the Penn State lobbyist would go with us to different offices of senators and congressmen. This was a person who knew his way around the halls of Washington.

I remember one time, as this was getting started, we met with Frank Moss, who was a senator from Utah. He came in and talked with us a bit, and most of our conversation of how we could do things was with a woman who was his administrative assistant. You could sure see that the power in a senator's office was with her, the head of his administrative staff. I could see she really had her feet right on the ground as to how to do things in Congress.

Then we had lunch with the person who then was the director of the Office of Management and Budget. You know, that was *the* budget head. I no longer remember his name, but his name was a byword back in the late sixties, early seventies. In fact, when he finally retired from the government, there was an article in *Newsweek* or one of the news magazines about him. So, you know, he was a man of stature. A little guy, but he was so powerful. Because we

were talking about this Mineral Institute program of a few hundred thousand [dollars] per institution, he said, “You guys are just talking about a Mickey Mouse program.” This was at the lunch. You know, not very many people were there—our group with him and maybe his assistant.

After lunch we met with Congresswoman Patsy Mink from Hawaii, and how we had the inside track with meeting with Patsy Mink was that her husband had done a PhD under Tom Bates in mineralogy. [laughs] She was a congresswoman, as I said, from Hawaii. We told her that the head of OMB had said, well, we’re just proposing a Mickey Mouse program. She said, “At least he should have said Minnie Mouse.” [laughs]

Anyway, sometime later, our ad hoc committee went back to work or continued working. The idea was that a land-grant college or university in each state would be eligible for one of these mineral research institutes. I was again involved because I was a member of the Mineral Resources Committee, I guess it was called, of the National Association of Land Grant Universities and Colleges. I know that’s how I stayed on that.

We met a couple of times. One time we met in San Francisco when the head of that committee was the current president of the University of Nevada, Reno. At a later time, though, when we really did a lot of testimony in Washington, the committee head was the current president of the University of Alaska. And two of us spent a couple of days writing out the testimony we would give. The second person was Don Warner, who was then Dean of the Missouri School of Mines in Rolla, and he had done a PhD here at Berkeley in about 1965 in ceramics, working on properties of clay. I didn’t know him then at all, but he did his PhD with Joe Pask here at UC. Anyway, we prepared the presentation, which was short testimony given before Senator Frank McClure from Idaho. You probably remember his name.

Swent: I certainly do.

Fuerstenau: He was the one senator that was a friend of the mining industry. We also met with Congressman John P. Murtha. I think he’s still in Congress; we testified in front of him. He was chairman of the Interior Appropriations [Sub]committee. And he was a local congressman from near where Penn State is, in that vicinity of State College. And, by the way, sitting on the committee that afternoon was the chairman of the House Appropriations Committee, Congressman Jamie Whitten, who for fifty years had been a congressman from Mississippi, and for decades, apparently, controlled the House appropriations. And anyway, he came to sit in because—not for our few minutes—but because somebody from Mississippi wanted to build a park. I mean, it was a park in some town. A small delegation came and made the presentation to this committee right before us, so that the senior man was there—it was all kind of an interesting experience.

As I say, McClure was the one that really helped things along, and so the bill to establish the Mineral Institute program was passed by Congress, and the Senate. Osborn was now director of the Bureau of Mines, and Osborn said—you know, it was really pathetic. Here, he was the father of this idea of establishing mineral institutes, but as a member of the Nixon executive department, he had to go in front of Congress and testify against these institutes. He said, “Here, it’s my baby and here I have to now go and give testimony on why we shouldn’t have these institutes.” I don’t know if he used those words—but I knew Ozzie fairly well.

Anyway, the bill passed. And I remember seeing on TV just during the election, Nixon saying, “I am going to save the taxpayers money. I have vetoed nine appropriation bills.” And one of them was this little Mickey Mouse Mineral Institute program. And it was vetoed. Then when Jimmy Carter was president, it got put back in and attached as a rider to the surface mining bill. They knew that that wouldn’t be vetoed, and so that was the beginning of this Mineral Institute program.

Swent: It did pass then, under Carter.

Fuerstenau: Yes, but then he couldn’t veto it because they wanted the surface mining bill for environmental control and all of that sort of thing. This was 1978, and my brother and I were going to South Africa to give a course in flotation for the South African Institute of Mining and Metallurgy, and I said to Neville Cook, “This proposal has got to be in,” and when we came back, Cook hadn’t done a thing—not one thing on it. He said, “Well, I decided that this could wait till next year.” That was absolutely appalling. Well, I really laid in, and in one week, the one week that we had, I got this quite huge proposal written and typed. And, you know, we got it submitted on time. You needed an endorsement of the governor, so Jerry Brown, who was governor, endorsed it; the president of the university endorsed it. You needed all that because of the land grant institutional basis.

Anyway, so there was a one- or two-day hearing in Washington where representatives from every institution that submitted a proposal came to be present and to hear their proposal being discussed, and to be there to answer questions if necessary. In other words, there was this official board making decisions as to which states would get mineral institutes. I know Osborn was chairing it, so he must have then no longer been director of the Bureau of Mines. There were three or four other people on that board. Every state had submitted at least one proposal. I’ll tell you, I was there the whole day and heard things as far ranging as to what mining goes on in the state of Connecticut, for example. In other words, they had decided maybe forty of these, or thirty-six might be funded. I know Connecticut was thrown out. But Indiana got a Mineral Institute and its director was my Butte roommate, Don Levandowski, now a geology professor at Purdue.

Stanford put in a big proposal. There were a couple of Stanfordites there at that meeting, but with our proposal, they had no chance at all because Cal is a land-grant institution. But if I hadn't sat down and put in that tremendous push, obviously this whole thing would have gone to Stanford. I felt pretty bad about Cook not doing the effort. MIT, for example, is a land-grant college, and so they were awarded one. Interestingly, Colorado School of Mines is not a land-grant college, but they were awarded an institute. I think in the end there were thirty or thirty-one totally. This program was administered under the Office of Surface Mining, with Dr. Ronald Munson as head of the program.

As for administration, each state got an allotment grant for the Mineral Institute that you could use for parts of the program, and then there were separate funds for research grants. Separate proposals could be submitted by faculty through the Mineral Institute to the Office of Surface Mining. I remember I had one research project on comminution; another time, on utilizing fly ash from power plants as a raw material by pelletizing it to make lightweight aggregate.

Then, I believe the director of the Mineral Institute in Arizona suggested that we establish an Association of Mineral Institute Directors, which was quite interesting. We met once a year and talked about problems and strategy. And somebody got behind—and if I find out, I'll add it to this, because I never ferreted it out—proposed that this program be transferred to the Bureau of Mines. That was done, and everybody thought, oh, isn't that wonderful. Then, with the demise of the Bureau of Mines, all died off. I still wonder that if the program had stayed in the Office of Surface Mining, whether those mineral institutes would still be here.

Swent: As it is?

Fuerstenau: They're now dead. They died with the Bureau of Mines. All along there was a problem with Congressman Sidney Yates—I think I saw he just died. Yates was a congressman from Illinois who was against all entitlement programs. He was in charge of the House Appropriations Subcommittee on the Interior, and he vetoed this institute program every time, and Idaho Senator McClure would put it back in. And so that's what went for several years.

Another problem centered around the head of the Bureau's Information Division—Dr. John Morgan. You may have seen his name. My observation was that he was pretty proud and fully sure of himself. The Information Division of the Bureau of Mines put out all these commodity tabulations of production, sales, tonnage of imports, and all that. Morgan was in charge of that and the Mineral Institute Program was put under his direction. Somehow in relation to the comments by Yates, Morgan got the idea that they ought to consolidate the research of the Mineral Institutes into centers, and there were to be four research centers. In other words, the research money had been that

you write proposals and to get grants of maybe \$30,000 or something per year, which was not bad in those days. He proposed that they consolidate the research by making a few large centers. Proposals were then solicited for centers concerned with comminution, one on the processing of fine particles; one on ground control, rock mechanics; one on process met[allurgy]; and one on environmental problems. Eventually there was a political one put in on coal dust, which was directly allocated to West Virginia and Penn State. The others were all on a competitive basis. We put together a big proposal on processing fine mineral particles, and I got several faculty at other universities to join us.

I learned via the grapevine that there was going to be one in VPI, one in Utah, one in Nevada and—I sort of picked up that we weren't going to be successful and I looked at a map and said there would probably be one in the middle of the country. Well, at VPI was a former director of the Bureau of Mines, Dr. Walter Hibbard, at Utah, was the chief scientist of the Bureau of Mines, Dr. Tom Henrie, who I knew at Carbide. They picked one on environmental to be at the University of Nevada. A former director of the Nevada Bureau of Mines in Reno had been director of the Bureau of Mines. I met Robert Horton when he was director of the U.S. Bureau. So I said, "Hey, the next one's only going to be in the middle of the country," and I said, "I'll bet it'll go to Missouri School of Mines." I found out that Tom Henrie and somebody else from the Bureau of Mines went to Chicago, and at O'Hare Airport met with the people from the Missouri School of Mines, and they rewrote their proposal to make it read better, and then that led to the pyrometallurgy center being awarded to Rolla. The environmental center was formed jointly with the University of Idaho and the University of Nevada.

I was really unhappy about our being excluded because we had a good proposal and a good research record. Twice this person, Tom Henrie, who, as I said, was chief scientist of the Bureau of Mines had asked me to write a support letter: 1) for him to be Undersecretary of Interior, and I wrote a nice letter on his behalf. There was another position at roughly that kind of level that he had also come to me to write a letter in support of his candidacy, which I did. So I told Nat Arbiter, "You know, why don't you indirectly tell Tom Henrie I'm very unhappy about what went on in the Bureau not selecting us for this center." Tom Henrie called me up one day and said, "I hear you're very unhappy." I said, "You're damn right, I'm unhappy that we didn't get that." He said, "Well, you know, during the discussion"—this is a Republican regime now—and he said, "Some of the panel members said, 'What do we owe Jerry Brown? Nothin'.'" So there's no reason to give a center to California."

Boy, that was the first time I have ever really seen blatant politics enter into that kind of decision. So we didn't get anything, and the pyrometallurgy center at Rolla became the fourth one, as I had told you about.

For a long time we were a sub-grantee, or participant in the Comminution Center at the University of Utah. All through the life of the Mineral Institute program, I got my own research support in comminution through that center. The director of the Comminution Center was John Herbst, my former graduate student. When he left the University of Utah to start his own company, Control International, the director of the Comminution Center was Dr. Peter King, an outstanding researcher and professor they then brought to the University of Utah from the University of Witwatersrand in South Africa. Then when the Bureau of Mines went belly up, the whole program just died.

Swent: These centers were in addition to the institutes?

Fuerstenau: Right. The Mineral Institute allotment grants of about \$120,000 per year continued but the centers were funded by taking all of the institute research money and lumping it together. Morgan had conceived of that idea—somehow he had gathered that Yates didn't like giving out all these smaller projects, and, listening to Yates's words, he proposed that they lump all the research funds to make the four centers. The fifth one, the Coal Dust Research Center, was established by direct approach to congressmen in West Virginia and Pennsylvania, and funded by separate appropriations for those two universities.

Swent: And then the money would go out to the institutes.

Fuerstenau: Yes. Well, as for the institutes—we still got the allotment, which I gave out as research fellowships and money for small research assistant grants to different areas. I think that's how I paid for a secretary, and I don't think I paid myself anything. I might have one summer month now and then. I probably didn't need it because of other grants. I know when George Brimhall was new here; I called him up out of the blue and gave him a Mineral Institute fellowship or two. He's never forgotten that.

I spread the funds around. There was a new assistant professor, Boris Rubinsky, who has become very famous for doing very local freezing of tumors in organs to kill the tumor. He's in Mechanical Engineering or Bioengineering. His first project that he got from me was to study freezing coal. One of the big problems—you know, with coal coming out of North Dakota or Montana, the cars are frozen in the winter, and when they try to empty them, they can't. So he was studying the freeze-thawing of coal. For that year, a student got a master's in ME [mechanical engineering] supported by that funding. I tried to spread the funds around in a good and useful way.

The Demise of the Bureau of Mines

Fuerstenau: With the Bureau of Mines, itself, obviously their life was always a problem. If somebody, either in Congress or in the Senate, said no, the other side would

protect it. And McClure I think was in the Senate possibly eighteen years. He didn't seem that old, but he was there for three terms, and decided, I guess, to go practice law to help pay the bills, and he retired from the Senate. His replacement was Senator Larry Craig, who appeared to be somewhat favorable to the mining industry, but I don't think he was—

Swent: Not the same dedication.

Fuerstenau: Not totally. He's probably got his own agenda. So what happened with the Bureau was all of a sudden, one year—I guess it was either the House or the Senate would say no, and the other said yes. And one year they both said no, and that was the end of the Bureau of Mines. I will add a little bit later, and I don't have that information now, but I talked with Tom Falkie at the last SME meeting, and I said, "I'd like to get your opinion of"—

[Tape 32, Side A]

Fuerstenau: —why the Bureau of Mines failed. Next week I may see him. He said he'd be glad to give his thoughts. I think I can add more on that later after I have talked with him.

The other director of the Bureau of Mines, who's a professor, retired of course, at VPI was Walter Hibbard, and that's why I thought maybe a center went to VPI [laughs]. Anyway, Hibbard gave the commencement address at Montana School of Mines in 1968, when Frank Aplan and I both were given one of these honorary professional degrees. I remember talking with Hibbard because he had just announced that he was going to step down from being the director of the Bureau of Mines. Now, he was somebody who had a physical metallurgy background and had headed the metallurgy research of General Electric Company. Why they made him director of the Bureau, I never understood but maybe somewhere he had, I guess, shown very good executive ability in materials.

Anyway, there in Butte he told me that he decided to step down as director of the Bureau of Mines because of lack of support by the mining industry. He said he went to mining company presidents, and told them, "I'd really like to get your input into Congress to increase the budget of the Bureau of Mines." And he said he always got back, "Oh, our taxes are too high already. We're not going to do anything that would raise our taxes." He said that was a typical kind of response from the mining executives, and he finally decided, well, if the mining industry isn't going to support him, he was going to quit. That was 1968, and that was why he stepped down.

Swent: That was a long time before it actually folded.

Fuerstenau: Oh yes, yes. Interestingly, I got to know reasonably well several directors of the Bureau of Mines. Years later, I had a lot of interaction with Jim Boyd,

who had been appointed by President Truman against the tirades of John L. Lewis because Boyd was a hard-rock miner and not from the coal industry. Also, Walter Hibbard, Elbert Osborn, Robert Horton, Lindsay Norman who later was president of Montana Tech for a number of years, Tom Falkie, and T.S. Ary, who I knew from Union Carbide. Anyway, I'll talk later with Falkie about the end of the Bureau of Mines. Do you know Tom Falkie?

[Added by Douglas Fuerstenau during editing: Several of these directors of the Bureau of Mines had the level of technical stature that led to their being elected to the National Academy of Engineering: Dr. Walter Hibbard, 1966; Dr. James Boyd, 1967; Dr. Elbert Osborn, 1968; Dr. Thomas Falkie, 1989. NAE started in 1964, so these three were elected very early.]

Swent: Yes. I don't really know him; I know who he is.

Fuerstenau: Well, you've seen him at the Hall of Fame of Mining. He's usually the MC, I think. I think I even talked to him about being a mining professor here, when he was still a professor of mining at Penn State way back. Anyway, that Mineral Institute program was very good for the field, but it's gone. Like I said, I began to wonder if we'd still have it if it had remained in the Office of Surface Mining.

[Added by Douglas Fuerstenau during editing: In 2003, when I presented his oral history to Frank Aplan at a local SME meeting held at Penn State, Dr. Ronald Munson, the director of the Mineral Institute program when it was in the Office of Surface Mining, was present. I talked with Munson and also with Dr. Thomas Falkie about what they recalled with regard to that transfer of the Mineral Institute program to the Bureau of Mines and neither could recall anything about why. Falkie said that he did not even know that the program had once been with the Office of Surface Mining.]

[tape interruption]

XX ACTIVITIES AS A CONSULTANT

Swent: We'll talk now about some of your consulting activities, your work as a consultant.

Fuerstenau: First of all, my own impression of consulting by University of California faculty is: when you live out here on the West Coast, a lot of the kinds of industries one might deal with are East Coast, and so consulting with a number of companies in large measure would entail a lost day for travel. I think engineers in, like, say, aerospace, Silicon Valley, or in civil engineering would have local or essentially local clients, whereas consulting for a company headquartered in Chicago or in the East involves a lost day in travel. Travel for someone living in Boston or New York may involve a short plane trip or a ride on the subway, which is a lot simpler than travel from San Francisco. I often found that that day of travel entered into a problem, and it cost more money for a West Coast consultant than if the consultant were from the East Coast. However, I did a certain amount of consulting with travel. I think it's important to see what kind of problems are out there, and it keeps you in touch with the practical world and what might be important problems.

Consulting on Minerals and Materials Processing

Fuerstenau: I'll just tell you about some of the kinds of problems that I worked on because many or most of those companies no longer exist and some of the activities are in the public record. When I first got here, somebody named Nicol called me up. He owned a shipping company. By that I mean ships on the ocean. He had an avocation of reading geological reports of different countries, and he ran onto a report about the mineral content of the Sechura Desert in Peru. It's hard to understand why land directly on the ocean is the driest place in the world, but this desert is the driest place in the world. Nicol found in a Peruvian Geological Survey report that a Union Oil geologist had drilled there, and had reported the mineral content of the drill cores. They, of course, were only looking for oil and other things didn't mean anything to them. But Nicol spotted that this area had a fairly high percentage of phosphate in the sand, and so he got control of the prospects and wanted to look into the possibility of mining this for phosphate.

And, you know, I vaguely recall somebody brought that to Homestake a long time ago, forty years ago, even. Possibly that was Earl Herkenhoff. So anyway, Nicol called me up, and I talked to him about the matter. I guess he had already put together a small lab in a building down there, in Palo Alto or Menlo Park. So I suggested that he hire—and he did—Bob Brandt, who was

the engineer from Carbide that I had brought with me to Kaiser Aluminum. Bob was still at Kaiser, but maybe he could see that that bauxite program might be coming to an end, so he joined and actually headed this lab for a year or more that Nicol had for working on recovering the phosphate minerals in the sand. The desert sand contained something like 6 or 7 percent phosphorous pentoxide.

As I remember, you have to get a product containing 32 percent phosphorous pentoxide to be able to sell it. And, you know, by flotation on some of it we could nudge 32, 30 percent, 31, 29, but it had to be 32 minimum, and so we could just almost make it. The idea then was to sell the phosphate to Japan. I think if they calcined it, they could dry out enough water from the gypsum—because the gypsum was in there in a fairly big way—that they could get it up over the 32 percent. Anyway, they never went into production. But that was kind of an interesting project, how Nicol found the deposit, just out of an avocation, by reading geological reports. I do remember having lunch at his home one noon, Bob Brandt and I. Very fancy, elegant service, with his own chef. I guess when you have that much money, you can live that way.

Maybe a year after I had come to Berkeley, another ship owner who lived in Berkeley called me up about a grinding problem. I do not remember his name but he lived only three or four blocks from where we did. He had seen the paper that I wrote on vibratory ball milling and had bought one of the Allis-Chalmers 15-inch vibratory ball mills where he had installed it in a lab in a barn at his farm somewhere in the Sonoma Valley area. His idea was to produce cement by grinding lime and silica sand together. Indeed you can react the two and make cement simply by grinding. Actually a solid-state chemical reaction is taking place. His idea was to put the grinding system on the back of a truck and be able to produce cement in remote areas. For a short time, I worked with him. I learned later that he carried this on for quite a long time with P.K. Mehta in Civil Engineering, where they tried to find ways to make the grinding energetically more efficient. It turns out that it takes almost as much energy to produce cement by grinding as it does by cooking the cement raw materials in the usual way in a cement plant kiln.

Swent: That must have been interesting.

Fuerstenau: It was. I learned a little about what is now called mechanochemistry. Another short-term but interesting consulting problem was brought to me in the early sixties by Kaiser Engineers, who were considering taking on the development of a potential process for concentrating ores with heavy liquids made with bromine salts from the Dead Sea. In the laboratory, mineralogists and mineral processors often separate small ore samples using TBE, tetrabromethane, which has a specific gravity of 2.95. If a small sample of ground ore is placed in a beaker containing TBE, lighter minerals like quartz or calcite float on the liquid, and heavier minerals like rutile or hematite, et cetera, will sink. Earlier we talked about heavy-media processing where suspensions of ferrosilicon

particles in water act as a dense fluid for making sink-float separations. In the early sixties, Israel Mining Industries proposed using TBE for making large-scale sink-float separations and had come to Kaiser Engineers to help them develop a commercial process. I remember that Dr. A. Mitzmager of IMI saying that they could produce TBE for fifteen cents per pound using bromine from the Dead Sea. Later, after running full-scale tests on this, they found that there was a loss of something like two pounds of TBE after thoroughly washing the products, and this made the process uneconomic. So that was the end of that.

Early on, I did quite a bit of work for Mountain Copper [Company] after meeting Bill McClung, the company president and a Cal grad. You may have met McClung, who quite regularly came to local AIME meetings.

Swent: I think so.

Fuerstenau: Anyway, they had this pyrite sinter from which they had produced sulfur for sulfuric acid, and I worked for them for a bit on trying to make an iron oxide pigment from the sinter, which might be used for paint, for ships, the bottom of ships. Their research director then was Pete Fowler³⁰, and that was the first time I met Pete, although I'd had all that correspondence and samples that came from him during my year with Kaiser Aluminum. Pete Fowler, at that time, was with Mountain Copper. We made some interesting products, but I guess that's probably a highly competitive, very specialized industry for iron oxide. Pigments required very special properties. We also assessed using the iron oxide particles as a drilling mud additive, but the particles were too abrasive for the drill bits.

One consulting situation I had for quite a long time was with Kennecott—often involved with continual efforts to improve recovery and grade at their operations, mostly on flotation, a bit on grinding, some of it on control, although automatic control is not my bag. Also, I met with their geochemistry-mineralogy group on some occasions. I was over at Salt Lake several times a year for quite a long time. They had then a very good research lab and engineering department there at Salt Lake, but that's all gone now, or in essence gone. It was excellent. Rush Spedden was the director of the research laboratory. I worked mainly with Art Last, who headed the mineral processing research section. Art had received his PhD degree under Melvin Cook at the University of Utah, and had done an interesting piece of research on Cook's neutral molecule theory of flotation. As an aside, Art told me that he had piloted a B-17 that bombed Dresden in 1945. One of their problems at Kennecott was improving molybdenite recovery. Kennecott gave me a research grant to investigate better ways to depress molybdenite flotation, and

³⁰ Hedley S. "Pete" Fowler, *Mining Engineer in the Americas, India, and Africa, 1933-1983*, 1992

that project became the MS thesis of J.M. Wie. That led to our conducting further research on the surface chemistry and electrochemistry of molybdenite flotation that became the basis of the MS thesis of Subhash Chander.

[Added by Douglas Fuerstenau during editing: After finishing his PhD, Jong-Minh Wie went to work for Kerr-McGee in Oklahoma City and later joined Parsons-Jurden. When I last saw him, he told me that he been nineteen years in Saudi Arabia, where he has been involved with the design and operation of desalination plants. When I saw him here, they had come for the graduation of their older daughter from Stanford. He said that when they were helping her move into the freshman dormitory, a person across the hall introduced himself and said, "Hello. I'm Bill Clinton." Wie's daughter and Chelsea Clinton became good friends and his daughter spent Christmases at the White House. Also, today a very young golfer named Michelle Wie makes the press almost daily, and that is his niece.]

Another consulting activity that went on for several years was for Marathon Oil Company, which had a great interest in so-called tertiary oil recovery. If you start pumping from a new oil well, you get about 25 percent of the oil out. Then with water flooding, they generally get another 25 percent out. And then half of it's still in the ground. They said they had wells in Wyoming that are about 15,000 feet deep, where the recovery is even less than that. There's more oil left there than what they get out. Now if you add a surfactant like a sulfonate to the water, you can lower the surface tension of the oil-water interface to nearly zero so that the oil-water mixtures can squeeze through the pores in the reservoir rock, and you can then pump more oil out of the reservoir. Since sulfonates interacting with minerals is the heart of flotation, I could make use of my knowledge of these phenomena. I spent a number of years working with them at their research laboratory in Littleton, Colorado, on this process which they called Maraflood. The process involved pumping surfactant solutions down injection wells and pumping oil out of nearby production wells. In Illinois and Ohio, their wells are only a couple of hundred feet deep so they felt that it would be economic in 1972, '73 when the price of oil was only three dollars a barrel. Most of the oil companies later had major efforts in tertiary oil recovery. Most reservoirs are in water-wet rock, but occasionally the matrix rock is oil-wet and getting oil out of such reservoirs would indeed be difficult. Later, in a big way, my former student Somasundaran went into tertiary oil flooding research by getting a consortium of companies to support his program. However, I decided not to take my own research into this area but to stay more in hard mineral research, although I did do that fairly extensive consulting on the oil reservoir flooding problem.

A different kind of interesting job, which was fairly lengthy, was with Stauffer Chemical. Stauffer had bought Mountain Copper, and so they had the Mountain Copper problem for a while but that wasn't what I was involved with. They were one of the four major miners of soda ash up in Wyoming, at

Green River near Rawlins, which is the high, windy, cold part of Wyoming. Perhaps you have been there.

Swent: Yes.

Fuerstenau: They mined a mineral that's sodium bicarbonate, called nahcolite. You know, sodium is Na, and bicarbonate is HCO₃, so when the mineral is named nahcolite, you can see some mineralogist there was being clever, right? There were four mines there in Green River, big mines. I was never underground there, but I saw pictures of the thick beds from which they mine the sodium bicarbonate. Stauffer wanted to incorporate a flotation step in their processing, and were having the flotation test work done at Hazen³¹, and so I was their consultant here in San Francisco. I met with them in San Francisco and in Richmond and maybe a couple of different times we went over to Hazen labs in Golden.

That may have been my first visit to Hazen, itself. This was a very interesting project because the soda ash has a certain solubility. After purifying the sodium bicarbonate, they calcine it to produce soda ash, and its main use is in glassmaking to give the correct sodium content of glass. Control of particle size is very important in being able to sell it to the glass industry. It's apparently a very competitive industry. Another source of soda ash, by the way, is Searles Lake in California, but the big producers are in Wyoming.

Swent: Green River, and there's also a place called Trona up there.

Fuerstenau: It's very near Rawlins. Again, it's that very high western, dry, cold part of Wyoming.

Swent: Right. Very cold.

Fuerstenau: Another significant mineral problem that I worked on for quite a long time was with Foote Mineral Company, which mined spodumene at Kings Mountain, North Carolina. Spodumene is a lithium aluminum silicate. You know that a lot of pegmatites are in the Black Hills, and right at Keystone was the famous Etta Mine that at one time contained the largest spodumene crystals ever found³². Foote was having problems with iron in the reused tailings water affecting the grade of the spodumene and the recovery. I worked with them quite a while on trying to overcome recovery problems. Foote was owned 83 percent by Newmont. There was a technical meeting for

³¹ Wayne Hazen, *Plutonium Technology Applied to Mineral Processing; Solvent Extraction; Building Hazen Research; 1940-1993*, 1995

³² Arthur I. Johnson, *Mining and Metallurgical Engineer in the Black Hills: Pegmatites and Rare Minerals, 1922 to the 1990s*, 1990

a day or two every four or six months, and I'd be there to participate in the discussions and plans. These meetings were run by the Foote vice president of production, George Knease, who came down from New York. There were always one or two people later on from Newmont itself. The person that I dealt with on a regular basis was Dr. Ronald Atwood, their research metallurgist, who was from the University of Utah. We worked on increasing the grade and the efficiency of the flotation circuit. Their main customer for lithium was Corning. You know, for making CorningWare that won't break when you put it on a stove. The ceramic material must have a high lithium content to control grain size. Chemicals were also produced from the lithium extracted from spodumene.

I'd just like to comment that the Kings Mountain operation is an example of what I think is an interesting ore body for total resource utilization. After grinding the ore, they floated the spodumene, which is lithium silicate; in the next flotation step they produced mica, which is, again, a potassium aluminum silicate; then they floated the feldspar, which is another potassium aluminum silicate; and the final tailings were quartz, which is silica, SiO_2 . By flotation you can separate all of these silicates from each other. This is done by utilizing a combination of crystal chemistry and surface chemistry. They had a waste iron silicate that was just a small amount, but it was black, so they had to get rid of it. The coarse waste iron silicate was eliminated by selective mining, and most of the finer material was removed by a flotation step directly after floating the spodumene. But to get all of the iron minerals out of their lithium product, they passed the dried spodumene concentrate through an electrostatic separator, as I recall. So what they had was an operation that virtually produced no tailings. Isn't that a nice thing—that you can make a product that just has a very small amount of tailings, and those tailings being only this small amount of waste black iron magnesium silicate? I use that as an example of what ought to be, where possible, ideal mining without producing tailings.

Swent: Ideal. To use everything mined.

Fuerstenau: That was a very interesting operation. In order not to make slimes or fine particles, they ground the ore down to something like 65 mesh with rod mills only. They were trying to keep the particles as coarse as possible. So we also did studies on the grinding circuit, how to improve recoveries and so on, and then some years ago the operation shut down, and I don't know who owns it, because Newmont sold off everything but gold. At the time, Knease said that Foote was developing lithium brine wells in Chile that had real potential. From this particular consulting, I became interested in the surface chemistry and crystal chemistry involved in the flotation of spodumene that led to the excellent PhD thesis of Kwang Soon Moon.

Swent: That certainly sounds like a good example of the interrelation between consulting and academic research.

Fuerstenau: What we have discussed were consulting arrangements that generally went on over a period of a few years, but I had a fair number of other short-term consulting jobs on a variety of processing problems that I also found interesting and useful in both directions.

Some Consulting That Involved Employment of Graduate Students

Fuerstenau: On a few occasions I took on projects that required extensive laboratory investigation or the preparation of a detailed literature search. This was the days before Internet searches. In the case of doing laboratory tests, one set up what was called a service-to-industry contract through the Office of Research Services in the College of Engineering.

Swent: What was that?

Fuerstenau: At that time, one could set up a service-to-industry contract that paid the university for the use of equipment. I have no idea what procedures might be today.

Anyway, two of these related to pelletizing iron ores. One was with Hanna Mining Company who wanted us to test the concept of generating seed pellets externally and charging the seed pellets along with iron ore concentrates. The object was to ascertain if surging in the balling operation could be reduced and the process speeded up. As I recall, I hired a couple of graduate students to run many experiments using our laboratory balling drums. A major report was written but the results never published since this was a consulting job. You know, Hanna is now totally out of iron ore mining. They've gone fully into the polymer business from what I've read.

The other large consulting effort that I did with relation to pelletizing iron ores was with KHD in Germany. KHD used to be one of the world's major manufacturers of mineral processing equipment. I think that they may be only a fraction of what they once were. Anyway, in 1975 the director of technology of KHD asked me to work with them on a new patented approach to producing pellets. They had made a belt filter with a surface that looked like a waffle. When the ground iron ore concentrates were filtered, they would come off the filter in the form of small cubic cakes. This process would eliminate the balling operation in drums or disks. We carried out a lot of tests with a small filter in Berkeley, and reported on our findings with regard to a range of variables. I recall that KHD eventually tested this on an industrial scale in Brazil, but I guess they had problems discharging the cakes from the filter, problems of breakage of the agglomerated pellets, lack of uniformity, et cetera. It might have been interesting to have added a little cement to the slurry just before filtering to have a pellet that would harden by itself. The whole concept of filter pellets was abandoned but it certainly was innovative.

Another fairly extensive consulting project was a literature search and analysis of problems associated with fine mineral particle processing. Several students worked with me on this project, which resulted in a long detailed report for the client but also some additional education for me and the couple of grad students working in the library.

Consulting on Tax Issues Related to Mining versus Manufacturing

Fuerstenau: In the way of consulting, I made a point of staying away from legal things. Sometimes I received calls about being an expert on cases but I elected not to be involved. Two or three faculty whom I know have testified on cases quite regularly, particularly those involved with metal failure by fracture or by corrosion. However, twice I became deeply involved as an expert in legal technical issues.

The first time on legal issues, Haydn Murray, a geology professor at Indiana University, called me up about meeting him and someone from Pennsylvania Glass Sand Company at the coming AIME annual meeting. He was a consultant to that company. The case had to do with royalties and the tax court. So I wrote a short report for them. There was to be a trial in front of the Internal Revenue court in Washington. I got a call from the lawyer on the government's side asking me if I would be willing to be a member of their team, and I said, "Well, you know, I've written a report for Pennsylvania Glass Sand, and they've asked me to be on their team, so I'm going to have to do my work for Pennsylvania Glass Sand." It's kind of interesting that both sides of the case came to me.

The problem was to define what's a fine particle. The matter had to do with the basis for taxing mining and manufacturing operations, called the Gore Amendment. This was taxation law written by Albert Gore, Sr., when he was senator from Tennessee. The gist of the situation involved at what stage does an industrial mineral change from being a raw material into being a product. When it is ground to the stage where it is considered to be a fine particle, then it is subject to manufacturing taxation. When it is still raw material, it is subject to mining taxation, with the depletion allowance. So when does it switch from mining to manufacturing? For agricultural limestone, fine particles are anything finer than 6 mesh, which is about three millimeters, four millimeters, something like that. That's called a fine particle. With talc, it's minus 325 mesh, for talcum powder, talc. You know what that is. For quite a few different minerals, the size at which the material becomes a fine particle is listed in the Gore Amendment. But apparently the glass sand people, when they were asked to come and testify when the bill was being written in Congress, didn't come, and there was no definition of fine size for glass sand. So the IRS made its own definition of fine particles, something like 10 mesh or so if I recall, quite a large size. Because of that, they were not able to use the mining depletion tax rates but had to apply manufacturing tax rates. If I

remember, the person from Pennsylvania Glass Sand said that there's an intermediate size—I'll just say maybe between 10 and 28 mesh or something like that, that they couldn't use under those conditions and were accumulating a great deal of this material. What they wanted to do was to be able to grind that down finer, into a finer product to have a market. The economics included the tax schedule.

This trial was in front of a judge in Washington, right near the White House, directly adjacent, almost, to the White House. So we were there about a week.

Swent: Who was suing whom?

Fuerstenau: I guess that Pennsylvania Glass Sand was suing the Internal Revenue Service over what size should be defined as a fine particle. The trial was over defining what should be the particle size in relation to taxation. Their lawyer firm was a very famous one in Washington called Covington & Burling. You see that name very regularly. And, you know, I found it very interesting that they put together a panel of maybe six of us, or more, to serve as experts, and then the government had three or four on their side. We knew each other, you know.

I know the trial lasted over a week, because I was there over a weekend, and those Covington and Burling lawyers lived in Virginia, just across the Potomac—but they stayed in the hotel, and they worked every evening with who was going to be on the stand the next day. Might be there eight till eleven or something. They were going over how they were going to run your testimony. And they did that with, let's say, the two people who were to appear each day—whereas the government lawyers went home and never did this practice run.

When it got down to my turn, I spent an evening going through what they're going to ask me as part of my testimony the next day. So I did all of this. First the other side can challenge whether you are an expert. There was no problem with me. When I was done, the judge—now, this was 1981—and the judge said, "Well, I'd like to congratulate you. I see from our discussion that you're from Berkeley, and I want to congratulate you that you're not wearing a beard like those other guys from Berkeley." [laughs]

Swent: Good heavens. A really impartial judge.

Fuerstenau: And by the way, that judge retired shortly after this, and, you know, he retired without writing up the decision. I asked somebody connected with the case a couple of years later, and was told that with a new judge, they arrived at a settlement. I suppose it was based on what had been presented earlier, but the first judge, whoever he was, retired before he settled the case.

Swent: That all just went for nothing?

Fuerstenau: Well, in essence, no. I mean, in the long run probably no, because the transcripts of the trial must have been used by that new judge in arriving at the basis of the settlement. I know nothing of why he retired because he looked like he was in his late fifties.

I now remember that I got involved with another mining tax situation, but it was settled quite early. However, I'll tell you about it because it is another interesting kind of "mining" situation. When I was working with Foote in Kings Mountain, they said they were developing lithium brine wells in Chile. They also had lithium brine wells at Silver Peak, Nevada. One day a lawyer from New York called me to ask if I would give them advice on where in this operation, the lithium recovery changed from mining to processing, again as to what stage taxation rates would change. I flew over to Reno and the young lawyer met me and we drove to Silver Peak. There the brines go through about ten very large evaporation ponds to concentrate the lithium salts. Somewhere in the middle, I believe that carbonate was added to precipitate bivalent salts, like magnesium. Here is where I would have thought that processing began. After the final evaporation pond, the solutions were pumped into a small plant where the solution is heated. Most salts become more soluble as the temperature is raised, but interestingly lithium carbonate becomes insoluble at higher temperatures. So they simply filtered out the lithium carbonate, dried it and bagged it. All quite simple. I had collected a lot of information about Dow's recovery of magnesium from brines in Michigan and had begun to prepare a report in relation of that to lithium recovery at Silver Peak, when I was told that the IRS had settled the problem.

A Royalty Lawsuit over the Definition of Milling in Processing Ores

Fuerstenau: I can tell you about another very interesting case. I had a call from a person who owned a company called Argentum Consolidated Mines. He had acquired the company, which owned half of a huge silver mine that was bankrupt, called Candelaria, over in Nevada. After he had picked up this property, he apparently studied details of the leases and ownership basis of the company. They had been mining silver, and heap leaching it. Now the previous owner was—

Swent: Armand Hammer.

Fuerstenau: Yes, yes. Armand Hammer acquired his interest in Candelaria during the time that the Hunt brothers ran up the price of silver by trying to corner the market. The suit not only involved the present time but also when Candelaria was operated by Armand Hammer and Occidental Petroleum. Later Nerco Minerals had bought all of the mineral activities from Occidental. Nerco is a power company that also had a mineral arm. So the suit was actually against Nerco, but it went back to Armand Hammer.

A young lawyer came to see me, who worked for the law firm that had been hired to handle the case, called McCutcheon—McCutcheon, Verleger, and Black—that's a major firm in California. There's a McCutcheon office in S.F. This was McCutcheon from L.A., Los Angeles. This young lawyer, who had graduated from Cal here in philosophy and gotten a law degree from UCLA, said that he had spent about two years visiting the mining industry around the state. In other words, he really had set out to become an expert in mining. He came to Berkeley for a time or two to discuss the case, and then I agreed to work with them, and so we went to visit the mine—

[Tape 32, Side B]

Swent: Okay, so you went to visit the mine.

Fuerstenau: Candelaria. I flew down to Burbank and we got on a small four-passenger or five-passenger plane. We flew to where the head of Argentum Consolidated Mines had a compound out in the desert near Fish Camp Lake, where borax was first found and produced, and from there the next day we drove a few miles to Candelaria. We had one day to go through the mine and the plant. The Nerco lawyer was along with us, and I remember chatting with him quite a bit. He was from Denver, a mining lawyer; he had an undergraduate degree in chemistry and a law degree. We were able to see everything, everything that was done all the way from the mine on through the plant, and so on. I remember lunch time—the Nerco people ate their lunch in an air-conditioned room. We had to sit in the shadow, which was only about a foot wide, of a building outdoors, where we had catered sandwiches and Coke. We were just outdoors in the heat, being treated like we were intruders, and they were putting us in our place.

One part of the suit that I wasn't connected with involved a 50:50 split of the mine output. Since the open pit was mining in areas in which there were old drifts or tunnels from earlier underground mining, one question was what fraction of the old tunnels entered into calculating ore to be credited or subtracted from this side to that side. That involved a lot of statistical analysis but did not involve me.

The main part of the suit involved royalty payments. This was a heap-leach operation, where the solutions were collected in a pond and pumped to a facility where the gold and silver were recovered by a Merrill-Crowe cementation process. They called their precipitation plant a mill. The crux of the suit was over milling, and just what is milling? That's where my role came in. According to the original lease at Candelaria, if the operator built a mill, they had to pay 5 percent royalty. If they did not build a mill, they had to pay 10 percent royalty. Well, let's just say in gold leaching, if you have a heap-leach operation, you might get 50, 60 percent recovery; but up there in Lead, at Homestake, you know, the recovery of gold in that milling operation is about 93 percent. Your investment is very low if you don't build a mill, but

the recovery is low, so somebody getting royalties would be paid adequately only at the higher royalty rate. So the issue was: What's a mill? To pay only 5 percent royalty, Nerco and Occidental called their precipitation plant a mill.

This went on for two, three years, and I hired my right-hand man then, Ronaldo Urbina, who had done a doctorate with me and was staying on as a postdoc to help me on this task. I hired him to work with me on developing a big report defining what is milling. And, boy, you looked up the writings of many experts and one was in for surprises—like Gaudin in his textbook only uses the word “milling” once in this sense, and he applies it to a coal-cleaning plant. Nobody in the coal industry calls that a mill; they call it the coal preparation plant.

I looked up milling in Taggart's *Handbook of Mineral Dressing*, and he talks about milling wash iron ore. You know, that's massive ore. In the iron ore industry, those are called wash plants because you're not grinding. Broadly speaking, milling means fine grinding, making fine particles, flour milling, et cetera. So here these two great experts misused the word “milling.” Taggart also described milling mercury ore, taking chunks of mercury ore and retorting them. He called it “milling” mercury in his handbook. Like I said, Gaudin's only use of the term was for coal cleaning and Taggart used the term for wash iron ore plants and mercury retorting. I started out with the definition in the Oxford Dictionary of what is milling, that followed it clear back to the Middle Ages, the German [*Muehle*].

Swent: It has to be grinding.

Fuerstenau: Yes, it's got to be fine, or relatively fine. At Candelaria some of the rocks being leached were a foot or more in size. We also brought processing time into our analysis. Leached material was never removed from the heaps at Candelaria. Up at Round Mountain I think they leached for forty days before they take up the ore that had been crushed to half inch or smaller and move it in order to use the pads again. But in a plant, material is moving through, with maybe twenty-four hours of retention time. So in our definition, there's flow of material in a milling operation and there must be fine particles. Furthermore, our tabulations of published recoveries showed more like 50 percent in heap leach and 90 percent in mill operations.

Also, there's a good book on gold-silver ore processing. I think there are two volumes, by Frank McQuiston and Bob Shoemaker. It's a good summary, but, you know, twice in that book—I don't think Shoemaker was doing that related to this of course, but one time, on this very plant, they used the word “mill” in that book for the precipitation plant. A couple of times. Otherwise, they had it straight, by definition.

So anyway, I prepared this document and did a deposition in Reno on this. I was there maybe two days, doing a deposition, and that's a tough way to earn

a buck, in my opinion. I was always sort of fencing with the lawyer. I was trying to think, “What in the world is he trying to get at?” Whereas an expert should just not fence with the lawyer. One time I remember I was asked something about three times; I said, “I just don’t even know what you’re getting at.” And then our lawyer, who’s sitting right next to me, said, “Well, he’s asking another kind of question that has nothing to do with this but has to do with a legal point between lawyers.” Sitting right beside Nerco’s lawyer and glowering at me the whole time was our friend, Bob Shoemaker. He didn’t enter into any oral comments—he would lean over and whisper to the lawyer every little while—this, now, was the same lawyer that was out there at the operation the day we were at Candelaria. That went for two days, and then we said something that there was going to be an addendum added to this big report I had prepared, so the other side said, “Well, we will want some time to study this report.” Then there was a recess of about a month in there.

I knew that Arbiter had edited a book from the 1964 Mineral Processing Congress called *Milling Methods in the Americas*. Their lawyer said—I suppose this is all coming from Shoemaker—“Do you consider Arbiter to be”—oh, they have jargon—you know, “an expert?” But it’s a little more than that, than an expert. Which I of course do, and did. I knew what they were getting at, but we had this recess because they wanted to study my addition to our long report. We also brought processing time into our analysis report at greater length, so I had time to add it to my addendum. It’s this: God can do the milling. As part of the Seventh International Mineral Processing Congress, Nat Arbiter had edited this book in which there was a chapter entitled, “Milling Florida Beach Sands.” I had said milling means grinding, and so in the case of the beach sand, I concluded that, “God has already done the milling. He’s done the grinding.” I figured they were going to come after me: “You just said Arbiter is an expert, and he’s calling this milling, and there’s no grinding.” Also, I did have an example of some other ore in Australia that wasn’t gold but was something else where the particles readily broke down; they didn’t have to grind. So anyway, the final deposition was in Los Angeles, and I included in my presentation that nature could lead to liberation of fine minerals, such as with beach sands. Maybe for a total of three days I was involved with giving depositions on defining milling.

Then came the trial, in a bankruptcy court, and our lawyers really wished they had done it in front of a jury because of whom the suit was against. We went into the courtroom, and all down the one side were just file cabinet after file cabinet—you know, so high [demonstrates] and a row down the whole wall of the big room—our side had a half dozen on the other side of the courtroom. I think our lawyer said that Occidental had spent something like three million bucks on preparing for this case. As I recall, our lawyer said that their firm operates not on contingency basis but on per diem costs, lower in a bankruptcy case, but their regular rate if they won.

When my turn came to testify during the trial, I was on the stand one whole morning. I had used the Bureau of Mines dictionary that had about twenty-six definitions of milling. You know, “mill” is even a raise in an iron mine in northern Michigan. There they called milling putting a raise that goes up at an angle. That’s obviously local jargon. But that’s in there, and so the lawyer said, “Well, you’ve been selective in your definition of milling. Answer yes or no.”

I said, “Well, only the first”—you know, they’ve lettered them. “Only the ones up to M or N apply to what we’re talking about.”

“Well, I want an answer, yes or no. You’ve been selective in your use of ‘milling.’”

I said, “Those definitions don’t mean anything.”

The lawyer said to the judge, “Your Honor, I want this witness declared hostile.” [laughs]

Swent: Oh dear.

Fuerstenau: Anyway, we had a break, and I told our lawyer, “God,” I said, “listen, I’m sorry about it. I’m really not doing very well.” And he said, “Gee, you’re doing just great. You even kept the judge awake.”

Swent: [Laughs]

Fuerstenau: Because the judge had apparently been sleeping through some of the testimony that had gone on earlier.

Swent: Was it your contention that a heap leach is a mill?

Fuerstenau: No, it is not. My testimony was that recovering gold or silver by heap leaching involves piling up mined and crushed rocks in huge heaps, sprinkling cyanide solution on the heaps for long periods of time, collecting the solution from the bottom of the heap in a pond, pumping the solution to a facility where the gold and silver is precipitated from the solution. Heap leaching is not milling. The plant building was for treatment of the solution. There’s no solids, no particles in the solution at that point.

Swent: So it’s processing but not milling.

Fuerstenau: Not milling. To pay the 5 percent royalty, they had to call that precipitation plant a mill. By the way, that plant was built by Bechtel. Shoemaker probably would have been in on the building of the plant. He had retired from Bechtel and maybe that was why he was involved in this case.

Swent: Was he on the other side?

Fuerstenau: Oh yes. He was an expert on the other side. I did not hear his testimony because I was only in the courtroom that one day that I was on the stand. I didn't bother to be there the day before or after.

Our side won the milling definition. The several other parts of the case, I guess, they lost. But the royalty issue was very significant because that royalty went clear back. At Candelaria, they said they were heap leaching size X, but I saw big chunks resting on the edge of their heap-leach pile that looked a foot or two in size. The records show that they never got a recovery above 35 percent of that heap-leached silver. Somebody I knew from my MIT grad student days, who worked many years for Union Carbide, named Bob Hard, came to see me one time here at Berkeley, just as an old friend. He was then working for Occidental and said, "I'm on the way to Candelaria because"—I didn't know anything about it—"because the recovery is so low and we are trying to get the recovery up." That's long before this issue, by the way.

The Argentum Consolidated Mines people really won that aspect of the case. I have a draft of a paper with Dr. Urbina on "What is Milling?" that we should publish. I've heard of another case later where there were legal complications over defining milling.

By the way, we had one example in there discussing how Anaconda re-treated old tailings from a mine at Darwin, California, near Death Valley, where they took the tailings and pelletized the tailings and then heap leached the pellets. Now, that would be milling with heap leaching, right? You're dealing with fine particles, and—

Swent: Pelletizing?

Fuerstenau: —and pelletizing, and then heap leaching. Pelletizing is agglomerating fine particles into balls or pellets and hardening them. I once suggested to Homestake that they give me a research project to study this, since I have done a lot of work on the energetics of grinding and on pelletizing and on leaching. The object was to study the engineering science in the overall process—the energy expended in grinding, how particle size affects the pelletizing, the porosity of the pellets, the kinetics of leaching, how you control and evaluate the variables. I have always thought that this would be an outstanding thesis if this were approached from the engineering science point of view.

Harry Conger years ago said to write him a proposal. I may have talked a little bit about the conflict of interest thing in the state and UC. The university has this conflict of interest restriction on researchers. I never sent in a proposal because it would be awful hard to say there's no conflict of interest on a

Homestake board member. There wouldn't have been. I would have kept things completely nonproprietary.

Swent: But now there would be no conflict of interest.

Fuerstenau: Yes, with Homestake now being taken over by Barrick. That is a research topic that I really would liked to have done, but it's no longer possible at this stage in life.

I did quite a number of shorter term consulting tasks, for example, some work with Amax on their molybdenite flotation. Amax was having Dow Chemical make a new reagent for them, which was designed by Guy Harris, and so I was consulting for Amax while they were working with Dow on this reagent. That was the first time I really saw Guy Harris at work. I'd met him earlier at a congress in Cannes. Apparently it was a good reagent, but Amax didn't proceed with it because, as Guy told me, Dow set the price too high or some such thing, but it would increase the recovery of the molybdenite a bit. There were several similar shorter consulting jobs of this nature that I did over the years.

There is a comment that I would like to add about consulting visits. Some were set up with a strict schedule where one might spend an hour or two talking with an individual or couple of researchers. They might have been working on their project for six months or a year. They'd describe their problems and expect you to come up with a solution on the spot. Now that leads to a tough day.

Swent: They really considered you to be the expert.

Consulting on Advisory Boards

Fuerstenau: Let me tell you a little bit about totally different kinds of consulting, which was serving on advisory boards. Such consulting, however, usually was done with pay. Earlier, we had discussed my work with SEAC and Lawrence Berkeley Laboratory. An early one was being a member of the advisory board to the School of Earth Sciences at Stanford, which I think I did from '70 to '73. The purpose was to give advice to the dean. I think Paul Henshaw was a member of that advisory board over part of my time there. I remember telling him something I read in the paper at our first advisory board meeting—I said, "I saw in the paper you've been made chief executive of Homestake." He told me that he had been president for a couple of years by then. At that time Stanford had a mining, mineral processing, and extractive metallurgy group. Fritz Kruger was the person in charge of the mineral side, George Parks in mineral processing, Norman Parlee was a pyrometallurgist, and Bob Bartlett was an extractive metallurgist, whom you've met, who's on our Oral History Advisory Committee now, here. Bob was a very good person. They had

decided at Stanford not to continue a program in the extractive metallurgy area, so they did not give him tenure, and they were going to let the other metallurgist, Parlee, just retire out. George Parks moved into geochemistry and environmental engineering. But that hadn't happened yet while I was there. Because the group was still going strong was why they wanted me as one of this team of advisory people.

I remember sitting at a table at the dean's house for dinner, and one person sitting at my table of four was Cecil Green, the old man who was a geophysicist and was the founder of Texas Instruments. He was a major benefactor to earth sciences. At MIT there is a twelve-story Cecil and Ida Green Building for geology and meteorology, and I think there's a Green chair there; there's a Green Building at Colorado School of Mines, and so on. There are Green chairs at Stanford and UC San Diego. At that time, he must have been well into his eighties or something, but still very active and alert.

I did this for three years, which I found very interesting because they had such people like that—both academics and industrial—serving on advising. Evan Just³³ preceded Fritz Kruger. I have not seen him for many years, but he had been vice president of research and technology or something like that for International Minerals when Somasundaran worked there, and maybe he had a PhD from Stanford.

One of the things I saw that was totally different then between Stanford and Cal was this place is much more heavily loaded in support staff than Stanford was. Boy, the School of Earth Sciences was really lean to the bone. Then. Now, that's thirty years ago. There was very little extra in the way of support, administrative support, staff support. I always figured Berkeley had way too much staff, relatively speaking. You know, way back then, Stanford's geology didn't rate very highly nationally although I think they were fairly large. Whereas now, when I've seen rankings of geology, they've moved up fairly significantly.

Currently I'm again doing something like that at University of Florida where there's a big NSF and industry center on particles, called the Engineering Center for Particle Science and Technology. They were told by NSF that they should have a scientific advisory board, so I was asked to join that. Then I was asked to be chairman of it. I fit in fairly well there because I understand mechanical behavior of particles, measurement of dust characteristics, a lot about colloidal behavior of particles, a great deal about the surface chemistry of particles, and so I'm semi-knowledgeable about almost everything they do. At the first meeting, I asked a lot of questions of different people making

³³ Evan Just, *Geologist: Engineering and Mining Journal, Marshall Plan, Cyprus Mines Corporation, and Stanford University, 1922-1980*, 1989

presentations. They weren't expecting to be asked some of those questions. Anyway, that led to my becoming the chairman. Last year, after my heart problem, I asked not to be the chairman; that's why they gave me that nice diploma on the wall. I'm willing to continue to serve on their scientific advisory board as a member, but as chairman you've got to get the report out, and you've got to do all that work of putting it together and getting delinquent persons to write their section. You know, that was the kind of pressure I don't want.

Swent: Getting someone else to do things.

Fuerstenau: But only related to getting people to finish writing their sections of the reports. The main activity was commenting on or advising on the technical aspects of what they're doing, a little bit about personnel. Of course, they tie together. The director of the center is Brij Moudgil, who's a mineral processing person, did his doctorate under Somasundaran at Columbia. So he's my "grandstudent" when you get right down to it. He put together a very solid proposal for establishing this about five or six years ago as an NSF center. Som said he wished he could have written a proposal that good, but I never did see the initial proposal. This center has a lot of industrial input, so that meant Moudgil was able to sell people at Dow and DuPont, Shell Oil, Kimberly-Clark and some pharmaceutical companies on their program. And now there is involvement in the electronic materials area. They have had programs on Florida phosphates, and I think some of the iron ore companies are giving them funding. He has done a good job in putting it together and doing very well with it. I believe that the support by NSF is for eleven years. They're approaching their seventh year, and NSF expects them to be self-supporting after that.

Something that I almost forgot about, I served for four years as a member of the advisory panel to the Engineering Chemistry and Energetics Section of the Engineering Division of the National Science Foundation. Out of the panel, two of us were involved directly with the particulate materials program under Dr. Morris Ojalvo. For part of my time, the other person was Dr. James Fair from industry, maybe Monsanto, who late in his career became a professor at the University of Texas, and another was very well known for his years and years of work on the filtration of slurries and dewatering—Dr. Frank Tiller of the University of Houston. These were the days when NSF supported the kind of research that I was interested in. By that I mean various fundamental aspects of mineral processing, such as flotation surface chemistry.

National Research Council Committees; the National Board on Mineral Resources

- Fuerstenau: Through the years, I served on a number of National Research Council panels and committees, several closely related to mining and mineral industry, some less so. Also for a few years I served as a member of the Board on Mineral Resources, which considered policy matters related to the mineral industry. In a way such service is consulting since those panels and committees consist of a group of members who spend a year or two in several meetings working up recommendations on the topic of concern. Topics that I recall include international mineral supply, marine mining, and energy expenditure for comminution. I was asked to organize the comminution committee but I suggested that John Herbst chair it and I would serve as a member. After conducting a comprehensive survey, we found that 1.4 percent of U.S. electrical energy is used for comminution of ores, rock, and cement. Another extensive committee was to assess what happens to accessory elements in large-scale mining, such as in phosphate mining. The last such committee that I served on was the Shipboard Pollution Control Committee, which made recommendations on handling waste on navy ships. My input was with the section that made recommendations of mechanical methods for processing solid wastes on ships—a completely different set of people that one worked with. So anyway, that's the kind of things I did in an advisory sort of way—to industry and to government.
- Swent: A tremendously broad area.
- Fuerstenau: Oh yes. And still doing some. One doesn't think he's getting old, but he is.

XXI

MORE ON WASHINGTON, DC; THE BUREAU OF MINES

[Interview 14: October 18, 2001]

[Tape 33, Side A]

Swent: We're picking up after a little break of a couple of weeks, when you were in Washington doing some exciting things. Perhaps you'd like to begin telling us some of the things that you learned in Washington.

Fuerstenau: This was a National Academy meeting.

Swent: Academy of Engineering.

Fuerstenau: National Academy of Engineering. And, by the way, one thing that I learned was a great deal of effort is being directed at energy. In fact, this year's NAE symposium was on energy, and the first speaker was [James R.] Schlesinger, who, you know, was Secretary of Energy I think in [President Jimmy] Carter's day. He gave a magnificent talk, actually, and without notes or slides. He has probably given that talk hundreds of times. But what I came away with is what's beginning to be looked at as the only real future in energy in this country and maybe worldwide is nuclear power, and the main discussions were directed towards that and how things would be handled, which is a turnaround from the last few decades, a couple of decades anyway. We'll see what happens on that. Senator Pete Domenici of New Mexico also had a talk in that symposium.

The Bureau of Mines, Sponsor of Important Research

Fuerstenau: I also talked at length with Tom Falkie; Falkie had been director of the Bureau of Mines after being professor for a good part of his career at Penn State, and after leaving the Bureau he became chairman of Berwynd Company, which is a big coal company, I think headquartered in Philadelphia. Now retired. But through that, he had a lot of contact with mining executives, and through the American Mining Congress.

I talked to him about the Bureau of Mines, and I'd like to just make a comment that way back, those of us participating with people who were in the Bureau, had the conception that they kind of thought of themselves as quite separate and almost better as researchers than outsiders and really made little attempt at cooperation. Let me just back up a little further. The Bureau of Mines stations were always on university campuses: one in Rolla [Missouri]; one in the University of Maryland, Avondale; University of Minnesota, the building was right on the campus; Bureau of Mines there in Salt Lake City, et cetera; and one here in Berkeley, for example. They occupied a suite of rooms

in Hearst Mining Building, on thermodynamics. A very outstanding one. The Director, Dr. K. K. Kelley, did really outstanding work on thermodynamics. He was almost kind of a god. I remember when I was taking thermo—at MIT—K. K. Kelley was always mentioned. When I got here, I found that he was just a mild guy.

I had a lot of interaction with him because they had their labs along one of the hallways in Hearst Mining Building. All of a sudden one day he said he found that he was doing all this paper pushing as head of the program and handling all the problems as station director for only \$2,000 a year more than he would get in retirement, so he decided he would retire. According to him at the time, why do all of the administration for just that little difference between active and retirement pay?

When that happened—I was on sabbatical in London, I guess, in '66. I think it was that sabbatical. Anyway, I heard later that the Bureau proposed some other kinds of activities and interrelationship with the College of Engineering, and so on, and the acting dean at that time, Iverson, who was not much of a researcher, didn't go along with the proposal, and the Bureau left here and they moved their thermodynamic activity to Reno and to Albany, Oregon. That was the end of this program here, but I was away, so I didn't participate in any discussions.

Swent: Now the whole national Bureau has been closed down.

Fuerstenau: Yes. I'd just like to make some comments on that. The Bureau of Mines had done a lot of very good work, and as I said, the work on the thermodynamics on metals and metal compounds done here at Berkeley was just a major contribution. Up at Albany, Oregon, the process for producing zirconium and hafnium was worked out by Wilhelm J. Kroll, up there at the Bureau of Mines. A lot of excellent early work on comminution fundamentals was done in the thirties by Bureau people. Names that I recall are Will Coghill, J. Gross, Oliver C. Ralston, Stuart Zimmerley, Joe Rosenbaum, and J. B. Clemmer. Their work is still classic in my mind. The carbon-in-pulp operation for cyanidation was developed by the Bureau of Mines at Homestake, a direct industrial cooperation.

Another great success that was roughly late sixties was the Bureau of Mines working with Cleveland-Cliffs in developing the important selective flocculation-flotation process for nonmagnetic taconite. That became the big Tilden Mine operation where something like ten million tons of iron ore pellets are produced per year. Donald W. Frommer of the Bureau of Mines published some papers on this and received a lot of recognition, including being named the first recipient of the Antoine M. Gaudin Award of SME. A research engineer of Cleveland-Cliffs at the time expressed real displeasure over lack of credit for their input in developing the process. I don't recall

publications authored by Cleveland-Cliffs people—their main objective of course was to put the process into production.

Swent: People can often be sensitive over receiving credit.

Fuerstenau: Even Newton was upset over not getting the credit he thought he deserved.

Although I do not recall the name of the Bureau of Mines engineer behind it, they played a major role in bringing heap leaching of low-grade gold mines into being. That Bureau of Mines engineer had the concept of heap leaching low grade gold ores, not the mining companies. Also, as Tom Falkie pointed out to me, a big effort involved their many contributions to coal mining safety.

So they had a real role—

Swent: Safety was their big focus, wasn't it? I think that's why they were established originally.

Fuerstenau: Oh sure, sure. My guess is you're probably right, with regard to mine safety. Safety is vital in all mines, but coal mines [especially]. I guess early on a lot of the effort was aimed towards coal because I remember when I was a college student seeing in the paper that when Dr. James Boyd was named director of the Bureau of Mines, that John L. Lewis, you know, was totally against Boyd because he was a hard-rock miner. I still remember seeing in the newspaper of Boyd getting his first paycheck from Harry Truman after seventeen months in the job. Do you remember?

Swent: Right.

Fuerstenau: I suppose Boyd talked about that in his oral history³⁴.

Swent: He certainly did, yes.

Fuerstenau: This was decades before I would ever meet Jim Boyd but I still recall this picture of him receiving his check from President Truman after seventeen months on the job.

Towards the end I think the Bureau people began to realize that the Mineral Institute programs, which really weren't very large but were very good, were in about thirty different states around the country, and that some plus could be gained by interacting more with the university people that might have a good word to say for them. I sort of sensed this, and it probably is true.

³⁴ James Boyd, *Minerals and Critical Materials Management: Military and Government Administrator and Mining Executive, 1941-1987*, 1988

In their latter days, some portion of the Bureau research programs were directed at the study of materials—metals and alloys. The research arms of the Defense Department, NSF Materials Division, and the Department of Energy were all supporting very large programs in these areas. There was no reason at all for the Bureau to have similar internal research programs. To me, this is an indication that they had lost some of their goals.

In Washington I talked with Tom Falkie about the Bureau. In years past, if the Senate wanted to cut out the Bureau, somebody in the House protected it; then maybe another time it was cut out in the House but someone in the Senate [protected it]; then all of a sudden one year both did [wanted to cut it out], and that was the end of the Bureau of Mines. Falkie said that—here he is, as chairman of a major coal mining company—said he called up different mining executives in the U.S. to ask that they get in touch with their congressmen, et cetera, on behalf of the Bureau; and he said not one did anything. He wouldn't say who any of these persons were, but he said, "You'd be surprised who they were." I think that those of us in universities thought of the Bureau of Mines in terms of their research contributions, whereas most of those in mining industry may have thought of the Bureau of Mines in terms of being another regulation agency.

According to Falkie, some of them said they didn't want the government to have any input into their kind of operation, that they didn't need the research that the Bureau did. Some of them said that they can get new processes, new ideas, from equipment manufacturers. You know, in this country, equipment manufacturers have done a lot of research and development of mining and processing equipment through the years. But they don't exist any more. You know, Allis-Chalmers had a big research effort on comminution. They made all crushing, grinding equipment. An important person there was Fred Bond, a major contributor to comminution research and process design; his whole career was at Allis-Chalmers. The Hardinge Company did great work on comminution fifty years ago. Flotation machine makers such as Wemco and Denver Equipment had developed new machines, new applications, very good engineers, et cetera. But most all of these have been taken over by foreign companies. Mostly Scandinavian.

Swent: Outokumpu, Svedala.

Fuerstenau: Right. And now they're not here. Several of those have been combined into Metso.

So without this kind of backing, none of the congressional people supported the Bureau. Falkie thought that towards the end the Bureau management did not have really very good rapport with the congressional staff, and that may have influenced some of them in not recommending voting in favor of the Bureau of Mines.

Swent: I think that kind of thing is important to get in the picture.

Fuerstenau: Anyway, it's really too bad because, you know, the Bureau of Mines did not have a huge budget at all but I think did some good, real good. They also accumulated statistics on the mineral industry, and that job, I think, was first dropped and now has been transferred to the USGS [United States Geological Survey]. Other people have said that—well, [the Department of the] Interior itself may not have supported the Bureau because when [Bruce] Babbitt was Secretary of the Interior, he certainly was against mining. He was governor of Arizona at the time of the huge problems that Phelps Dodge had staying afloat, and was not sympathetic to mining then. I think, if I recall, he transferred a lot of the Bureau of Mines budget—before it went out—to establish, remember, a Bureau of Biology. Babbitt also took funds away from various parts of other agencies in the Department of the Interior for this. You don't hear of that any more, it was totally and fully unneeded, but Babbitt's concepts were they would count plants and animals and their relation to the environment, et cetera. Apparently it died its well-deserved death. Babbitt is probably the main person behind the demise of the Bureau of Mines. Now, this comment didn't come from Falkie, but it was all part of the picture.

By the way—this has to do a little bit with equipment companies. One of my former students, John Herbst, had started his own company, called Control International, and this was bought out by Armco, and so they were a division of Armco. For decades, Armco produced steel balls, grinding balls, and very good ones, not just cast but forged balls. They had a metallurgical research group for improving the steel used in their grinding balls. Armco has gone into bankruptcy, and I've been told by one of the former people there that nobody in the United States now manufactures grinding balls.

Swent: Good heavens!

Fuerstenau: You can imagine the kind of problem that mining people are facing. I assume grinding balls are being imported from Mexico, Brazil, or something.

Swent: That's a very basic thing, isn't it?

Fuerstenau: For the mining industry, that really is. And it's just indicative of the direction things have gone in this country.

Another consequence of all this concerns research on mineral-related activities—the Bureau of Mines was a support source for people in universities and now a lot of that support is all dried up; you have to hunt around for it.

Swent: You mean for grants.

Fuerstenau: For grants and contracts, yes. Some of it comes from EPA [Environmental Protection Administration]. For a while, a fair amount of money was coming for cleaning coal. Some people have been able to get money from DOE related to energy. There's a little bit from NSF if the proposal is for research related to particles or surfactant behavior. The Engineering Division director of NSF now says, "we don't support mineral activity," so if you get something from there, it's got to be kind of brought in backwards. So all of this has made it very difficult for funding research in mineral processing, which really means academic researchers are having to move into other directions in many instances.

[Added by Douglas Fuerstenau during editing: The April 2007 issue of *Mining Engineering* has a short article stating the National Mining Association gave senators a detailed report on the strides made in improving underground coal mine safety at a subcommittee hearing. The group asked senators to fund the newly created Office of Mine Safety and Health to ensure that future safety improvements are more readily accomplished. Now this, after helping eliminate the Bureau of Mines a few years ago! In July 2007, Robert C. Horton, Director of the U.S. Bureau of Mines for 1981–1987, wrote a letter to the editor of *Mining Engineering* pointing out the USBM had been established in 1910 for the principal purpose of improving underground coal mine safety and that perhaps thousands of lives had been saved between 1910 and 1996 when the Bureau was killed by Congress. He asks where will the skilled mining engineers that previously led safety research be found?]

[tape interruption]

XXII OUTSTANDING ALUMNI IN MINERAL PROCESSING, STUDENTS, AND “GRANDSTUDENTS”

- Swent: You also had a few things that you wanted to add about some of the outstanding or interesting alumni.
- Fuerstenau: Yes. I happened to see in *Mining Engineering* that Teck Corporation has taken over—gosh, what’s that big lead company in Trail? Cominco. I guess they owned 50 percent and now they’ve bought the other 50 percent. It mentioned the chairman of Teck being Norm Keevil. I’d just like to comment that I remember him as a graduate student here. He did a PhD in applied geophysics in our department at about the same time that Frank Morrison, for example, was a PhD student.
- Swent: Was Keevil a Canadian?
- Fuerstenau: Keevil was Canadian. I believe his father had been the previous head of Teck. But anyway, he has zero interaction with this department. Frank Morrison, who’s been professor here ever since he received his PhD, said that they never write to him or ask for any contribution. He has no soft spot in his heart for our program, I guess. Which is too bad.
- Swent: He has done very well.
- Fuerstenau: Oh, he has done very well. He’s one of the major players as a mining executive.

I’d like to add a little bit about another person who worked with me, who came from South Dakota, Jim Smolik, who I think just retired as a vice president of Placer-Dome. He obviously had a big job. He came from I think Timber Lake, South Dakota, which had a population of thirty people, and he got a bachelor’s-master’s from South Dakota School of Mines, came here for a PhD, was here about a year, a year and a half, and after about a year he said, “I don’t like being in a city where there are so many people. I came from Timber Lake, where there are only thirty people in town, and I don’t like to be living where there are so many people.” He’s a very bright, very well-organized, hard-working young man, so I talked him into staying. But about a half year later, he finally said he really wanted to go where there are not so many million people, and he left. He did a nice piece of research on the role of crystal chemistry in silicate mineral flotation that ended up as a publication. He went to work at Kennecott, and I later had some interaction with him during those years I was consulting for Kennecott and could see how well he approached problems there.

But I didn't think fast enough—UC Berkeley has a rule that if you have a master's, you can't get another master's in that same field, and he really had done almost two years of study here, and had done a nice piece of research. I should have been thinking fast enough to have given him another master's degree by another name so that he would have been an alumnus from here also. Smolik had a South Dakota master of science degree in metallurgical engineering, and it was back far enough that our department was still called Mineral Technology, and I could have submitted the forms for a master's in mineral technology. Several people have done that. So unfortunately, he's not an alumnus. I believe that he worked for a bit for Homestake in Wyoming, and then eventually joined Placer-Dome and moved up into their executive suite in San Francisco and to vice president in Vancouver, so he got back to where people were. [chuckles]

I would like to comment a little about students who received MS degrees with me. All of them did Plan 1 master's degree programs, which includes a thesis. There is also a Plan 2 master's degree program which includes more course work, a short research topic, and an examination. None of my students did the Plan 2 program because I supported essentially all of them off of research projects. Some excellent students wanted only to obtain a master's degree and had no interest in pursuing a doctorate. There were a couple of them who did not like the atmosphere of Berkeley and left after their MS degree, one excellent student without getting any degree. One or two decided to change fields after doing their MS degree in mineral processing and moved into physical metallurgy. Several started to work with other faculty and transferred to me for their doctoral research. Then there were some that I recommended not continue for the doctorate, at least at Berkeley. Altogether, sixty-five students completed MS degrees with me.

Perhaps most perplexing were two students who completed all of their course work, passed their qualifying examination, had totally completed their research, and only had to write their thesis or dissertation to receive their PhD degree. Both had decided that they needed to take a job right away and never did submit their theses. That I have never understood.

Swent: That is a shame. It is hard to understand.

Fuerstenau: One other student, Kwang Soon Moon, at that same stage went to work for Canmet in Canada and did submit his thesis, a very good thesis, about eight years later on the role of crystal chemistry and surface chemistry in the flotation of spodumene—an area that started with my consulting work with Foote. By the way, some years ago, Dr. Moon decided to return to Korea and established a private research institute, called the Korean Interfacial Science and Engineering Institute. I served on his advisory board for a number of years.

Perhaps we can talk about a few other of my former students. One you may have met is Dave Cahn, who was president of the California Mining Association about two, three years ago, and he is vice president of environmental affairs for Calmat. He did his undergraduate degree in mining here at Cal in the early sixties, and then his master's and doctorate degrees with me working on solids mixing research. Then, another person is V. J. Karra, who is vice president of technology for Nordberg. He is originally from India, and I've found him to be extremely able. I think he may have done an MBA degree on top of it while in Wisconsin, in a part-time program or some such thing. He has done large-scale development work, such as the design of crushers that can be regulated such that they can change the shape of aggregate rock for construction purposes. By changing the design and configuration of Nordberg's big cone crushers, they can change, a little bit, the distribution of various sizes and particle shape in the crushed product. Obviously, for aggregate, you want a certain size for a certain application without producing too many fines, et cetera. Karra did a nice PhD thesis here on comminution.

Jim Gebhardt, who came from South Dakota, did some excellent research on fine particle flotation for both his MS and PhD degrees here. In his senior year, he was president of the student body at the South Dakota School of Mines. He joined the Bureau of Mines where he conducted some superb studies on the electrochemical aspects of sulfide mineral flotation. I like what he did on sulfide flotation there. After the demise of the Bureau of Mines, he joined a group that has a mineral processing consulting company in Salt Lake City.

Most of my graduate students were really excellent people and one can't talk about all of them. But the people I am really very proud of are those that have gone on into teaching, and there's a whole list of them, twenty-five who have had some role in teaching in universities around the world, many for their entire career. Somewhere along the line, they were inspired to be involved with education. I was very fortunate to have had a large number of outstanding doctoral students and also postdoctorals.

Several of my former students moved into other fields and are recognized as leaders in those fields. For example, Srini Raghavan joined the faculty of the University of Arizona and at first taught mineral processing and carried out research in flotation chemistry, but he later moved into the planarization of computer chips and has become the recognized academic leader in that area. Another student, Mark Williams, joined DOE in Morgantown, West Virginia, and he heads their fuel cell program and has received several awards for his role in fuel cells. Hyun Jang went to MIT as a postdoctoral researcher in ceramics and now is a very outstanding professor and researcher in ceramics at Pohang Institute of Science and Technology in Korea. Another Korean PhD is Young Soo You, who went to work for HP, has some fifteen patents or more on inkjet printing. He recently returned to Korea where he is busy

working for the government to make research and development links around the Pacific Rim. It was always my aim to make certain that my students acquired a good bag of tools from their education so that they could apply what they learned in many directions, and you can see that they have done that. By the way, I always insisted that my students take a technical writing course offered in the college, whether they were American born or came from overseas. I often think of the great improvement in writing style of one former student, K.S. Venkataraman. Most of Venkat's career has been with Alcoa in Pittsburgh, where he has been heavily involved with alumina powder production. Several years ago, he established a quarterly magazine for Indians in the Pittsburgh area, called the [*Pittsburgh*] *Patrika*, that he writes, edits, and gets others also to write articles. From the advertisers, such as BMW, Mercedes, jewelry stores, caterers, et cetera, he surely must have a well-off readership. He told me that this magazine takes all of his spare time, as you can well imagine.

Swent: I see what you mean by giving your students a broad background.

Some Additional Comments about Graduate Student Research

Fuerstenau: Another thing I'd like to state is: I think some of the research done by my students will be long-lasting—people are going to refer to that work long in the future. For example, P. C. Kapur wanted to work on the fundamentals of palletizing; he was the first PhD in that area and his papers were really pioneering in understanding the mechanism by which green pellets grow in balling drums. Somasundaran worked on surfactant adsorption related to flotation, in some respect following on ideas that I had initiated on surfactant adsorption at MIT. Now he's really an international giant on adsorption of surfactants—you know, how soaps and detergents and chemicals like that adsorb onto solids out of water. A postdoctoral researcher, Takahide Wakamatsu from Kyoto University, spent two years here after Soma, and I consider the work that both of them did to be absolutely classic. While Tom Healy was still here, a truly seminal piece of research was done by Dick Hogg for his MS thesis on the interaction of dissimilar colloidal particles. This is often called the HHF theory and is the starting point of everyone's theoretical research where the interaction of different phases or different kinds of particles are involved.

Swent: How much did you have to do with directing them into these areas?

Fuerstenau: Sometimes almost total direction—sometimes far less. [chuckles] When I was as assistant professor at MIT, almost daily I talked to my graduate students about details of their research. After coming to Berkeley, I decided they should work as independently as possible for their own development—as I had done as a graduate student. So at Berkeley, I backed off from close direction. For about thirty-five years I had a large group working with me—

sometimes maybe twelve to fifteen or more people. This hierarchy of advanced grad students down to new students, plus a postdoc or two, a lab technician and a secretary moved things along very well. Quite often there was a visiting professor here too, and they generally interacted a lot with students in their specific fields.

Usually their research involved ideas that I had formulated in proposals to funding agencies. We had very well-equipped labs for particle engineering research and for surface chemistry research and could accommodate the interests of new grad students in many different directions. As I had mentioned earlier, my philosophy was to carry on extended research over a period of years on a given topic, and might suggest to a new student that they consider working in the area. For example, early on, I became interested in extracting metals from deep-sea manganese nodules and wrote several different proposals to support our work. Several MS and PhD theses on this topic were carried out in our laboratories, but the best was that of Ken Han, who became interested in the problem and had major, independent input into that research. In a different way, Kwadwo Osseo-Asare had a beautiful thesis on interfacial phenomena involved in the ammonia leaching of nodules. An area that we worked on over my entire career here was comminution, and many different students were involved over the years. Tom Mika got us oriented towards modeling ball mill grinding—some of the very finest work in determining parameters needed for grinding modeling came out of the work of John Herbst.

[Tape 33, Side B]

Fuerstenau: John Herbst, whose BS was chemical engineering, did a master's thesis, and I said, "Hey, this is so good that you ought to save it for a PhD," and he said no, he'd get the master's and continue for a PhD, which took him a little while longer to get his doctorate, but that's how good his MS thesis was. His research on the modeling and simulation of ball mill grinding is classic work. His papers are going to have lasting value for researchers in the years to come, the way I see it. As the years went by, student after student extended our work in comminution into ever more complex systems. By the way, John Herbst is, perhaps, the world-recognized leader in the design and control of comminution systems for the mining industry.

[tape interruption]

Several of my former students came back to Berkeley for periods of a few months up to two years. These former students were of great help to the current grad students and some of them worked extensively in the lab themselves. Abdel Abouzeid returned many times for perhaps a year each time, and contributed extensively to our work on comminution. Prakash Kapur was here several times, once for a two-year period, and he was of great help in our comminution program and modeling efforts. Ronaldo Urbina was also

here several times and each time he helped write various review chapters on adsorption and flotation. Off the top of my head I recall Subhash Chander being back for nearly two years and was the backbone of our research on hydroxyapatite—also Pradip and Ken Han on applied surface chemistry and surfactant adsorption. Others have come through and given seminars to the grad students.

As I mentioned, several of my students have gone on into teaching, and some of them have produced very good students, who in turn have produced good students. There are a number of four generations of PhDs. That's really quite unique to have this sequence of four different generations, and I vaguely recall that next year there might even be one with a fifth generation.

[Added by Douglas Fuerstenau during editing: As of 2005, sixty-five students completed master's degrees under my supervision and sixty students their doctor's (PhD, ScD or DEng) degrees. Those former students who became professors have themselves produced 126 PhDs (third generation), and some of those in turn have already produced 76 (fourth generation) PhDs.

A short while ago, it occurred to me that Professor Taggart at Columbia did not produce doctoral students. A number of his students who achieved high distinction in academic careers had never obtained a doctorate: A.M. Gaudin, Nathaniel Arbiter, Herbert H. Kellogg, and M.D. Hassialis, the latter three all had long careers as professors at Columbia University. On the other hand, my former student, Somasundaran has produced some fifty doctorates at Columbia.]

Honors Received by Former Students

Fuerstenau: Four of my PhDs have been elected to the National Academy of Engineering: John Herbst, Ponisseril Somasundaran at Columbia, Ken Han in South Dakota, and Kwadwo Osseo-Asare at Penn State. I would like to add that there are only five other graduates of our department here at Berkeley who are or were members of the National Academy of Engineering. The first was Plato Malozemoff, Class of 1931. The others are Bill Prindle (BS, 1949, MS), Oleg Sherby (BS, 1950, MS, PhD), Austen Chang (PhD 1963), and Sungho Jin (PhD 1980). One of my very best students, Prakash Kapur, who returned to India and was professor at the Indian Institute of Technology at Kanpur, has been a fellow of the Indian Academy of Sciences for a number of years, a very high honor indeed, and recently was elected a fellow of the Indian National Academy of Engineering. One of my more recent students, Dr. Pradip, who works for Tata Consultancy Services in Pune, is also a fellow of the Indian National Academy of Engineering. Two former students who returned to Korea are members of their academies: Kwang Soon Moon and Hyun M. Jang of the Korean Academy of Science and Technology. Jang is also a member of

the Korea Academy of Engineering. So amongst my former students are now quite a number of academicians.

Another thing that I would like to add is that several of my students have won the major AIME/SME awards in mineral processing. The major AIME award in this field is the Robert H. Richards Award, which has been won by Somasundaran, John Herbst, and Ken Han. In SME/AIME, the Antoine Gaudin Award, which honors persons for specific achievements, has been awarded to Somasundaran, John Herbst, Dick Hogg, Ken Han, and Subhash Chander. The Milton Wadsworth Award of SME/AIME recognizes researchers in metallurgical processing, and Ken Han and Osseo-Asare have been recipients. The Frank Aplan Award, which started in the Engineering Foundation and now is an AIME award, has been awarded to Somasundaran, Prakash Kapur, and Subhash Chander. There are others out there, but off the top of my head, these are some that I recall. I was fortunate to have had such students.

Swent: That's truly impressive.

Fuerstenau: One of my grandstudents has just been elected to the National Academy of Engineering, so that will be—

Swent: Wonderful.

Fuerstenau: Brij Moudgil, who did his doctorate under Somasundaran at Columbia. He has established the large Particle Engineering Research Center at the University of Florida that we talked about. He will be SME president in 2006.

Swent: That's the gratification of teaching.

Fuerstenau: Oh yes. That's part of, as you say, the gratification out of this kind of academic career. I am extremely proud of my former students and their accomplishments. Also, I think that I also had some role in several of my postdocs choosing an academic career.

[tape interruption]

XXIII

INTERNATIONAL MEETINGS AND TRAVEL EXPERIENCES

Swent: Let's move on into your international activities, your own globalization.

Fuerstenau: Right. I'd like to tell you just certain things about what took place, maybe start with some of the International Mineral Processing Congresses. I told you about the very first one, which was the sixth one, 1963, and the twenty-first was in Rome last year, and I think I'm probably the only one around that had gone to every one from the sixth through the twentieth, and then my heart problem came four days before we were to go to Rome, so I missed that one. [Added by Douglas Fuerstenau during editing: I presented a paper and chaired a session at the IMPC in Cape Town in 2003, at the congress in Istanbul in 2006, and have a special invitation to attend the congress in Beijing in 2008.]

Leningrad IMPC, 1968

Fuerstenau: In 1968, the eighth IMPC was held in Leningrad. The congress sessions were held in a large building with an auditorium that had been the government congress hall before the capital was moved to Moscow around 1920. I remember that speakers at the podium stood under an imposing statue of Lenin. When I registered at the IMPC desk on the first Monday morning, I was taken aback that all calculations were being done with an abacus. In fact, at every hotel desk, restaurant, and store the clerk had an abacus for their calculator.

For that IMPC, I had a paper on flotation kinetics. Normally I spent a lot of time on the papers, but for this was one I had a very bright, critical student named Tom Mika, who had drafted the paper criticizing a lot of prior work, before developing our model on microprocesses involved in flotation kinetics—and I didn't tone it down too much. When I got there, Rush Spedden, who was the American representative to the Congress Scientific Committee, said, "You know, the Russians are really laying for you, and they're going to attack you, and they're very upset at your paper." I looked at the paper again, and I couldn't see that there was any real problem, that we had credited the Russians appropriately and so on. So anyway, when I gave the paper, which you had only a few minutes to give a paper—

Swent: But they had read an advance copy.

Fuerstenau: Oh yes. That had been submitted, although we were too late for the preprints. It was printed in the final proceedings, translated into Russian. At these congresses at that time authors had maybe seven, eight minutes to present their paper and maybe a half-hour was allotted for discussion. And to lead off for the Russians was a very, very famous person, academician P. Reh binder, a

very elegant man and internationally known in colloids for a theory of what would weaken metals or solids so that they would break at lower loads due to surface tension phenomena. He was a professor of colloid science at University of Moscow, and when he died, I recall reading about that in American newspaper obituaries. Anyway, he got up—he looked like an American senator; you know, very elegant, like [Everett] Dirksen almost. I have a great picture of him. He gave an oration for about fifteen, twenty minutes, and I listened for a while, and finally I thought he's kind of saying the same thing as we had written in the first part of our paper. I practically broke out in a cold sweat, and I turned my microphone off—you know, from the translator—to decide “what in the world am I going to say?”

So I scribbled out some notes, and then afterwards, when I went up to read them as my rebuttal to the discussion—all of this seemed to go fairly smoothly—and as I walked down, I passed Professor Marston Fleming from the Royal School of Mines in London and the British member of the IMPC Scientific Committee. As I walked by, he said, “Great recovery.” [laughter] But, you know, I had been told on a Sunday by Spedden that they were laying for me, and my paper may have been Tuesday or something, not the first day. So I spent a little time stewing about it.

But anyway—

Swent: Reh binder had stolen your thunder?

Fuerstenau: No. No, it was just his critique that the Russians thought would undercut me. However, I found pretty much that we were in agreement in slightly different ways, but it had a little bit to do with not toning down some expressions in the writing. “Such a theory is unacceptable,” would be the way Tom Mika would have expressed it, and I probably hadn't caught a few of those. Maybe when those words got translated into Russian, they became harsher.

The Russians organized very interesting evening cultural and social functions. One evening was a performance at the Kirov Ballet for all participants, another was an evening going through the Hermitage Museum, a marvelous art museum in the Czar's palace. There are some spectacular rooms there. One was the green room with pillars and door frames and maybe walls made of malachite. Another room had a lot of azurite in it. Of course, the Hermitage is famous for impressionist paintings, too, that the czars had collected. Another evening excursion involved a boat ride to the summer palace of Peter the Great. The Germans had used this as their headquarters during the siege of Leningrad, and when they left, they blew much of it up. The Russians had completely rebuilt this palace, and it was just being finished in '68. You could see the new part and the old part. I found it very interesting, that they rebuilt it during the height of Russian communism. They had even rebuilt the trick watering system in the gardens where visitors could get suddenly sprayed as they walked through or sat down on a bench.

One afternoon I joined a laboratory tour to Mekhanobr, the huge institute in Leningrad that conducted research and designed all nonferrous mineral processing plants for all of the Soviet Union at that time. The director was Professor O.S. Bogdanov, who was chairman of the congress. It is interesting that a lot of academic type research was carried on at Mekhanobr while their main role was process design. I recall taking a picture of a Hallimond tube flotation device setup there exactly in the way shown in our early paper. On another afternoon, I went with Vladimir Nebera, who had been here at Berkeley, out to the cliffs where you could overlook Leningrad, and that was as far as the Germans had gotten. From these cliffs, the Germans could shell the city. That was about thirty miles out. So I'd exceeded that twenty-five-mile limit, you know, that I'd told you about earlier. During that week I visited most of the sites in Leningrad, and went back another time through the Hermitage, which is a marvelous art museum. During the congress, somebody I think from Finland was taking a picture from the steps of the Admiralty Navy Building, not of the building but away from it, and they took his camera and confiscated the film. They may even have interrogated him for two or three hours, as I vaguely remember. This was '68.

After all International Mineral Processing Congresses are several different technical excursions. I want to tell you about the excursion that I went on. Several Americans, Englishmen and West Germans and so on took the excursion that went all the way out to Uzbekistan. The end of that congress was June 21st, an evening called that the "Night of the White Lights." The flight to Tashkent departed from Leningrad about two in the morning. Just a year earlier, there'd been a huge earthquake in Tashkent, and you could see that their buildings were in very bad shape. The next day we visited a huge copper mine, called Almerik. After first seeing the huge open-pit mine, we went into the mill since this was a mineral processing group. And some members of our group included engineers from industry—for example, Abe Dor. He had been head of the Mining and Metals Division at Bechtel and then was vice president of research and development for Hanna Mining. He probably succeeded Dee Vedensky, by the way, when Vedensky retired. So anyway, he was there and maybe a few other people of that kind of background were also part of our group. I remember we went through this large mill, and you could see maybe a ball mill section that was totally shut down and was well rusted, and a flotation bank shut down there, and so on. Dor and a few other engineers were estimating what was the load on the conveyor belts, and so on. In our discussions, the conclusion was that this mill was probably running at about half capacity.

Swent: Oh my.

Fuerstenau: Which was kind of an interesting thing to see: a huge pit, sort of like Bingham Canyon, and then the mill. It was interesting to travel with these design engineers and operating engineers. Then we went to the copper smelter, probably smelting the concentrates from the mill. I remember they had a lot of

women working in the smelter, and nobody in there wore helmets. I remember seeing women working there just with a cloth tying up their hair. That certainly indicates the kind of safety practices that they had in the smelter.

Then after that—which was part of the real reason I had chosen that trip—was to go to Samarkand and Bokhara. We were sitting in the airport in Tashkent, and there was no airplane, and the temperature was Celsius 45 or 50 degrees, well over a 100 Fahrenheit. No air conditioning, just a fan. I talked with the translator, a Russian woman, and also the Intourist guide, who was another Russian woman. By the way, the guide said she had been five years in Egypt when the Russians built the big aluminum smelter there.

But anyway, it was hot and whatnot. And I was talking there quite a bit to the translator, and after a while I said, “Boy, you’d expect this in Russia.” And she snapped back, “We’re not in Russia. We’re in Asia.” And then later she said, “Oh, don’t ever tell anybody I made that statement.” It turns out that she and her husband were Jewish and had already applied to go to Israel. Her husband was V. L. Khainman [later spelled Hainman], who had written some very fine flotation fundamental papers with Bogdanov but had been transferred to some biology institute at the time of the congress, and that probably happened because he had applied to emigrate to Israel—I didn’t know any of this at the time. She didn’t say anything about that then—I’ve seen both of them, once at an AIME meeting in the U.S. ten years later. But because he had applied for an exit visa, they had taken him out of his flotation research job at Mekhanobr.

Swent: Oh my.

Fuerstenau: She was afraid—you know, about snapping that “We’re not in Russia; we’re in Asia.”

Swent: And she, herself, was also a metallurgist?

Fuerstenau: No, no. She purely was a translator, which was not her profession. Obviously, she knew English well and maybe had worked for a newspaper or something at one time, or taught English. I don’t recall now.

So anyway, we flew to Samarkand and had two, three days there, and I remember, that’s the first place I saw lots of Moslem worshipers. You can look at a mosque and see them all bowing. So the Russians didn’t put any squash on the Moslem religion at the time in Uzbekistan. In Samarkand are several old magnificent mosques, plus the remains of the palace and astronomical ruins of Tamerlane, the great conqueror around 1400. Then we spent another day or two in Bokhara, which dates way back to the Tea Road. In fact, we saw one building that was there that apparently was hidden by palm trees when Genghis Khan came through there and destroyed everything. It’s the oldest building in Bokhara, and it was not destroyed, and it was

constructed with mud held together by ostrich eggs and something else they used for the binder for the bricks a couple of thousand years ago.

Swent: My!

Fuerstenau: Then we flew back—

Swent: There are carpets in Bokhara.

Fuerstenau: Oh yes, yes. I have bought a lot of carpets since, but I didn't buy a Bokhara carpet, but I know precisely what they are.

Swent: They're beautiful.

Fuerstenau: Oh yes. Right. So we flew back to Moscow, and right over the Aral Sea—and you could look down and see how large it was then—but today there is no Aral Sea. You've been reading maybe about how the Russians put in a couple dams to get water for cotton, and they've dried the Aral Sea up. It's a terrible, huge disaster.

Then that night back in Moscow, the final night, we had a banquet, as you always do, a going-away banquet, and everybody chats and talks freely after some vodka. And the Russian Intourist guide in her remarks after the dinner told us about the flight which was in two small airplanes from Samarkand to Bokhara, only about twenty minutes, very short flight. All the Americans, English, West Germans, et cetera, were in one plane, and the East Europeans and Russians were on the other plane. Anyway, the Intourist guide said that their plane started up on the runway and all of a sudden the motors cut back, brakes went on, and the pilot opened the door and said, "Where are we going?" [laughter]

So then I spent a week, an additional some days in Moscow, because that's where my former postdoc, Vladimir Nebera, was now a professor. I had a thorough tour of some of the museums of Moscow, the Kremlin, and went down into Lenin's tomb because I apparently had arranged tours through Intourist. One day, Nebera and I went out to visit the coal research institute where the well-known Russian flotation researcher, Professor V.I. Klassen was head. Klassen had published a lot of very good fundamental work on flotation, particularly coal flotation, and I got to know him quite well at subsequent congresses. In Leningrad I also had met several of the major Russian researchers, such as Professor Glembotsky, who had devised an induction time device for measuring bubble/particle attachment in flotation. We also were invited to visit Professor Rehbinder at his institute in the University of Moscow, so I had very cordial relations with Rehbinder and with him visited the Academy of Sciences. I took a photo of Rehbinder in his lab showing Dr. J.A. Kitchener from the Royal School of Mines how the addition of a small amount of reagent could weaken a metal specimen.

Another thing I'd like to mention that took place during the congress itself: I was staying in the Astoria Hotel, which was a big old hotel there, and they even had displayed the invitation for the 1941 Christmas dinner from Adolph Hitler to be held in the Astoria. But they didn't make it, as you know. Right outside is St. Isaac's Cathedral and the statue of Peter the Great sitting on his horse that is standing on its two hind legs. I am sure that you have seen pictures of that statue many times.

Swent: Of course.

Fuerstenau: I now recall that the IMPC committee leaders, and others, were put up at a brand new high-rise hotel about a mile or so away. The elevators in this new building did not work, and Spedden told me that they had to walk up the six floors to their room each day.

We ate our meals in the Astoria. Slow. Every meal took about a half hour for every course. Apparently the waitresses had to pay for each dish, and so they did everything one at a time, so you could spend two, three hours to eat. I'm not stretching it, because there first was a half hour to get your order, half hour to bring the salad, the soup, a half hour each way.

I often sat at lunch with some Czechs from Czechoslovakia, and they were very upbeat and were really talking about how much freedom there is and how they could do this, that, and the other. You know, I told my self, "Boy, if I were these guys, I would be a little bit cautious"—this was 1968—you know, the Czech Spring was getting underway. It was obvious to me that whoever these Czech engineers were, they were really openly expressing how life was changing very fast there, and I remember telling this to myself, that, "Hey, I think they're just a little too exuberant." And it turns out, as you know, a few months later the Russians invaded their country. The next IMPC, two years later, was in Prague. The Russians went in there in the fall of '68. Jim Moynihan and his wife—he was the engineer in charge of Hearst Mining Building for years—were at the World Geology Congress in Prague in 1968 when the Russian tanks came in there. I guess their evacuation from that meeting was a bit of a problem.

Prague IMPC, 1970

Fuerstenau: So just two years later, 1970, the IMPC was held in Prague and then you could see that many, many of the buildings had been shelled by the Russian tanks that came in and put down the Czech Spring. Prague is one of the world's really beautiful cities that had escaped destruction during World War II.

In the fall of 1969, Professor Frantisek Spaldon, professor and rector of the University of Kosice in Czechoslovakia, came to Berkeley under an IREX

fellowship for four or six months. After being awarded the fellowship, he was made the rector of his university, the Technical University of Kosice. In Berkeley, he carried out some experiments on adding flocculents to a pelletizing drum, never published, and today I would like to have done more on that—too late. Before the IMPC, we first went to Kosice, near the Russian border. We visited the university, did some local touring with the Spaldons. In our conversations, Mrs. Spaldon told us that her uncle was one of the small group of anarchists that shot Archduke Ferdinand in Sarajevo that started World War I. After the couple of days in Kosice, we drove to the High Tatras, which are wonderful high mountains on the Polish border. They are only maybe twenty or thirty kilometers across—the last gasp of the Alps. By the way, when we were out in the forest with the Spaldons where they were sure of no one listening, he said that he thought it would be at least one hundred years before their country would be free of the Russians.

Swent: What a change they have seen.

Fuerstenau: I remember the congress was more or less straightforward, but under tight restrictions in hotels and so on. Rush Spedden was still the American representative, and in Leningrad and in Czechoslovakia, each congress committee representative had his own private big black limousine. I remember once I rode with Spedden in his limousine to the congress hall. They provided that for every member of the International Scientific Committee. That was typical East Europe in those days. When I became the American representative, none of that ever. [laughs]

Anyway, as I recall that, the congress went along fairly well, but the typical thing of congresses in those days was: an author had about seven or eight minutes, as I said, to make a paper presentation, and then there was about a half hour for discussion, and the Russians would come in there with a paper. They would get up in the discussion and give a paper absolutely unrelated. That happened time and time again. Obviously, they had a paper rejected or something, but they gave this paper, complete paper—longer than regular authors were allowed—they may go for fifteen, twenty minutes—whereas as authors we had to make our presentation in eight minutes. They pulled that all the time. Of course, they could go back and say they delivered the paper at the congress. And, of course, nobody could do anything about it. That went on and on.

Similar to the Leningrad congress, the IMPC in Prague had several cultural events. At the Prague IMPC, they also had daily excursions, and I went on one which was a visit to an old silver mine. We're talking about a mine dating back to about 1400. We went down into this mine, and I'll tell you, people were very small in 1400. Nobody was my size at all—they were probably all under five feet tall, as you could tell from the corridors down in the mine. I really had to stoop down as we walked through some of the old workings underground. That was quite interesting. It's now a tourist facility with

electric lighting. Miners would have used oil lamps five hundred years earlier. I also remember stopping in a church on the way there, and the whole basement of the church was completely built up of skulls and bones. Apparently people were buried for a few years in the cemetery, and then the bones were transferred—and we're speaking hundreds of years back—to where the walls of this whole huge basement under the church were just careful stacks of bones, including even the light fixtures were made of leg bones and skull bones. You know, it's a religious thing. It was not an anti-religious thing. But that visit was just part of that day's excursion.

By the way, Professor Spaldon did not attend the congress. Also Professor Heinrich Schubert from Freiberg in East Germany, not far from Prague, did not attend the congress. My guess is that was political. Schubert had a paper there, presented by someone else, and I was quite upset and said so in the discussion because much of what he wrote in that paper concerned my ideas of hemimicelles without credit to me.

After the congress, we chose to go on the technical tour that went to Yugoslavia, and Guy Harris was along on that one. The first day, Guy and I were riding—because we were both taking pictures—towards the front of the bus, but after a while we rode in the back of the bus. That driver was a little wild. We went as far down into what is Macedonia and Kosovo. We flew to Zagreb and then directly to Belgrade, and after touring Belgrade and visiting a mineral research institute went by tour bus to Skopje with a Yugoslavian engineer as our guide. Not long before, there had been a severe earthquake in Skopje with considerable damage to buildings.

By the way, on that trip I talked with our guide who also had been at the IMPC in Leningrad. In Leningrad we had to pay about two U.S. dollars for one Russian ruble. On one occasion during the congress, Ernie Peters and I walked into a couple of stores in Leningrad and thought that the exchange rate should have been no more than half that. Because Yugoslavia traded in both directions, the Yugoslavs would get the true exchange rate. Our guide told us that they paid only thirty-three American cents for one Russian ruble.

We visited a lead mill up near Skopje—mine and mill—went underground there, too. And we sat around afterwards with the manager and their engineers and discussed things, and they said that they had Russian equipment in the plant and their replacement parts came from Russia. They told us that they had long delays, so they would need to have almost two years of supplies of parts because the Russians just simply were unreliable on delivery. When we finished the tour, it might have been about eleven in the morning, and during our discussions they served everybody slivovitz, a shot or two.

What my wife noticed was that someone who was partaking of the slivovitz was also the bus driver.

Swent: Oh dear.

Fuerstenau: So we all got in the back of the bus when we left that operation, watching the driver with caution. The next day we went to Bor. Bor was the huge copper mine of Yugoslavia. At Bor I saw something for the first time, namely that they precipitated dissolved copper from the oxidized ore with sodium sulfide and floated it without adding a flotation collector—that was new to me at the time. During that visit, the manager there told us that all of their equipment was either American or Swedish. And he said copper is so important that they want to make sure that spare parts and things like that could be flown in and so on. There was never any wait for spares as in the case of the lead-zinc mine.

Swent: So they had top priority.

Fuerstenau: Top priority. This was 1970. At that same time there was a big new mine under construction, and all we could do was sort of look at it from a distance, called Majdanpek. Majdanpek is a big copper operation today, but they wouldn't let us see anything because it was new and I suppose some secrecy involved. It was in the vicinity of Bor. Then afterwards the last part of the tour was flying to Dubrovnik where we were for two nights, I think, in this ancient, wonderful city of Dubrovnik, that had never been destroyed—until a few years ago when the Croats were shelled by their own countrymen, the Serbs.

London IMPC, 1973

Fuerstenau: So that was the IMPC in 1970. We were on sabbatical in '73 when the congress was in London. We drove there with our family from Karlsruhe. The only thing I could say about that was I was then getting started with the *International Journal of Mineral Processing*, and the person from Elsevier, who was manager of that whole division of their publications—Jan Van Eysenga, came from Amsterdam to the congress for one day to promote the new journal. My brother, Maurie, was there, and I decided to spend that particular day showing him the sights of London. We had lived in London. So I gave him a one-day tour of all the highlights of everything you want to see in London. I knew exactly how to do it by underground and this, that, and the other thing, and we got back late in the afternoon. Professor Fleming was really upset because during the coffee break, Van Eysenga had put a brochure about this new mineral processing journal on everybody's chair in the auditorium, and Fleming was incensed about this.

Swent: Why?

Fuerstenau: Well, he wanted the IMM *Transactions* to continue to be the world leading mineral processing journal—Institution of Mining and Metallurgy in London. It sort of functioned as the international journal up to that time. I remember

him saying to me, “What are the people at AIME going to say to you about this?”

First of all, I said, “I don’t even know what you’re talking about.” I didn’t know that these brochures had been put on every chair. I was out with my brother, so I didn’t even know what he was talking about. I said, “What are you talking about?” Then he said, “What is AIME going to think about your being involved with starting this new journal?” I said, “That has nothing to do with it. That’s a complete separate issue.” As I said, I had nothing to do with it at that stage other than I was listed as editor for the Americas that first year.

[Tape 34, Side A]

Fuerstenau: All congresses have a final banquet, and so when we came to the banquet—usually I’ve always been put up towards the front or at the head table—our table was clear at the back, by the elevator, and the garbage receptacles. I mean, clear at the back. I was with a few unknowns and a couple of students, and that was obviously all deliberate, really very deliberate. So I was sitting beside somebody from Czechoslovakia and trying to talk with him; his English was extremely marginal. So then I thought, well, I could talk with him in German, which I could do fairly well; as soon as we switched to German, he went to ninety miles an hour speaking German, whereas he had been just slowly searching for words in English. Anyway, we were able to carry on a conversation. But this thing of sticking us right back by the door where they brought the food in: was very deliberate.

At that congress, the American representative was now Nat Arbiter, but instead of coming to the congress banquet, he spent the evening on some consulting business. Frankly, I thought that was a bit much. Another odd occasion with regard to the attendance by the American representative involved Rush Spedden in 1963 for the Cannes IMPC. A special fare was arranged on PanAm if there were twenty-five persons in a group. At the airport, I saw Spedden in the group with a large pile of papers but did not talk to him thinking that there would be plenty of time for that on the flight. Lo and behold he was not on the plane. Unbelievably huge Union Carbide did not approve of his going! I guess his stack of papers related to the proposal that an IMPC be held in New York in 1964. This turned out to be the seventh IMPC held in commemoration of the centennial of the Columbia School of Mines. There were some excellent papers, but the attendance was only three hundred or less, although published numbers state something like 1,500 because it was held in conjunction with the fall meeting of AIME/SME.

Cagliari IMPC, 1975

Fuerstenau: The next IMPC—I think it was probably '75. I always looked at what would be the tours afterwards. More recently I sort of quit that, but that congress was in Sardinia. The time between congresses got down to sometimes two years, and then there was only one year between '63 and '64, and then four years elapsed before the Russian IMPC. I think the Czechs wanted to show that, hey, we're still alive. So their congress was held in '70. Then came the London IMPC in '73, and then the Italians held the next one in '75, in Sardinia. A very well-known professor, Mario Carta, who was the long-time Italian representative to the congresses was the chairman. The meeting was uneventful except that we had about a thirty-mile bus ride each day from a fancy resort to the congress hall in Cagliari. Also some people did not arrive until the next day because of the chaos of limited flights from the mainland to the island of Sardinia. Afterwards I went on the excursion to Morocco. Morocco is Europe's major source of phosphate, and obviously it must have been submerged under the sea at one time. Morocco produces very high-grade phosphate ore, from fossilized fish bones, et cetera. We visited a couple of the phosphate operations, where they just dig it out of the ground and load it on trucks—I mean, that's virtually all they do without having to upgrade it or anything. Just beautiful, beautiful stuff.

We were in Casablanca, which is a nothing place. Got a great-sounding name. As part of our tour, the group then went to Marrakesh, and we were in Marrakesh over May 1st, and that's a holiday. All the Europeans, those from Norway, et cetera, spent the day around the swimming pool. Earlier in Casablanca I visited the loading docks with somebody—and I do not remember his name—who was the vice president of Kali Chemie in Germany, a potash producer and phosphate fertilizer producer. He wanted to look at their phosphate shipping facilities. In Marrakesh, he suggested that he and I rent a car and drive over the Atlas Mountains out into the Sahara Desert, on that holiday. That was a great excursion. The Atlas are the kind of mountains that the Bighorns are. They just tilt up and get quite high. We went out into the Sahara and visited a couple of casbahs and places like that. Up on top of the Atlas Mountains every little while was someone with a stand selling mineral samples that they had mined in caves up there. We stopped at a couple of the stands, and I bought a specimen that on close inspection back home in Berkeley turned out to have been painted with a dye—I guess I had been taken for a bit. The trip over the Atlas into the Sahara is what I really recall about the excursion, plus a visit to the bazaar in Marrakesh, where I bought a Berber rug that was colored with all natural dyes. Afterwards I flew to Rome and spent a day or so at the CNR mineral processing institute there, and was very nicely, beautifully treated by them. I'm glad I did that. That sort of cemented my relations with the Italian researchers at the mineral processing institute there.

When I went to the airport in Rome, there had been one of those Italian two-hour strikes of the airport firemen early in the morning. My noon flight finally departed at about six in the afternoon and I missed my New York to San Francisco connection. On calling home, I learned that my brother Dick had died that day at age forty-three from lung cancer. He had worked for Mobil as a geophysicist, and died just after reaching the twenty-year service. As I recall his two daughters were something like twelve and six.

Swent: Age forty-three is far too young.

Fuerstenau: As a matter of fact, let me just go ahead. The director of that Italian institute by this time was Professor Giorgio Rinelli. I think Professor Usoni had died, and Rinelli replaced him. Some few years later, Rinelli became the Italian scientific attaché to the U.S., and I remember having dinner at his home in Washington. I think he was there two or four years as their scientific attaché. After that, I think he served in the same role in Spain for the Italians. His successor as the director of this institute was Professor Anna Marabini, who I think I've already mentioned. By the way, in Italy persons who have received a doctor's degree can later on write another thesis to get the more prestigious title of professor. And that has no relation to teaching.

Some Other IMPCs

Fuerstenau: In 1977, the IMPC was held in São Paulo, Brazil, and Nat Arbiter had replaced Rush Spedden after the Prague congress as the American member of the Steering Committee and the Scientific Committee of the International Mineral Processing Congresses. And Nat was there in São Paulo but had to leave a bit early, so he asked me to sit in for him, which I did. What I could observe from that was the attempt to make sure that there was this balance between East Europe or West Europe, or U.S. and Russia. I didn't look at this as any political thing, but I could see how the Europeans did by making sure there was always a balance on the program and on committees of so many Russians or so many Americans and westerners and so on. As a result, maybe it's fifteen years ago or so by now and I was the American representative to the IMPCs, they reorganized the IMPC organization so that there would be an executive secretary—and the person selected was Professor Eric Forssberg from Lulea, Sweden. He made a very good choice because first of all, he's very able, speaks several languages, and Sweden was neutral in relation to east-west-north-south, right? Forsberg is in the middle maybe of his second ten-year term of that. It's really the one person that kind of supplies the cohesion, let's say.

Swent: Was the level of scientific accomplishment about the same between the two spheres?

Fuerstenau: Oh yes. Oh, at a lot of these congresses and other international meetings also there's always a lot of input that's, you know, not really first-rate.

Swent: But it was a political division rather than a—

Fuerstenau: The political division was in the minds of the Europeans, I think. Because, for example, both Russian representatives, that I recall, clearly were big people. One was a very outstanding man, Professor O.S. Bogdanov, earlier the director of Mekhanobr. I remember, oh, some years ago, that they sent me a wire asking me to write a letter of congratulations on his eightieth birthday. Bogdanov not only led that institute that did all of the research and design for processing Russian nonferrous ores but also had written many outstanding flotation papers.

And then for a number of years, the head of Mekhanobr was a fairly young man, Retsinov. I know he was what they called a corresponding member of the Russian Academy of Sciences, which meant he was a very good man. He became the Russian representative to the International Mineral Processing Congresses. Sitting at the head table beside him at the banquet for the 1982 congress in Toronto, he gave me a nice compliment. He said that in Russia they consider the three big men in flotation to be Bogdanov, Gaudin, and Fuerstenau. Unfortunately, not very many years later at age fifty-four he had a heart attack and died.

Swent: Oh my.

Fuerstenau: Just bango. As the years went by, he learned—his English became really quite good, so he must have worked pretty hard at it, whereas early on his English was not particularly great. Early at these congresses, Vladimir Nebera was always at these meetings, and he seemed to be acting as an interpreter to the Russians because first of all he had a technical background in mineral processing and his English was very good.

Between the Cagliari and Toronto congress, the 1979 IMPC was held in Warsaw under the chairmanship of Professor Janusz Laskowski, who later moved to UBC in Vancouver. I recall Laskowski standing with me on the steps of the huge congress center in Warsaw, saying that from that spot was the best view of Warsaw—because from there you could not see the congress center, which was identical to those ugly buildings that Stalin had designed for Moscow. In 1985 a congress was held again in Cannes, in 1989 in Stockholm, 1991 in Dresden, and 1993 in Sydney. You can see that the proposed schedule of holding a congress every three years was not adhered to very often.

San Francisco IMPC, 1995

Fuerstenau: Finally we decided to hold a congress in San Francisco in 1995, and it took some input on my part to squeeze it in ahead of one tentatively scheduled for Aachen. I made the point that in 1991 there had been the IMPC in Dresden, which was now part of Germany. Always the representative to the IMPC would be the chairman of the congress, but I just didn't want to undertake all the responsibility and all the requirements of being chairman of the meeting, so my former student, John Herbst, who was then MPD/SME chairman, was named the general chairman of the San Francisco IMPC. I served on the executive committee, chaired the editorial committee, and played a major role in the program selection. Peg [Fuerstenau] was chairman of the accompanying persons program and activities like that. The congress was very successful and profitable to SME.

Three nice things happened at that congress. John Herbst worked on establishing the International Mineral Processing Congress Lifetime Achievement Award. I was named the first recipient of this award. Also, the proceedings from the congress were dedicated to me as mentor, teacher and researcher in mineral processing.

At the banquet, in addition to the IMPC Lifetime Achievement award, Professor A. Abramov from Russia announced that I had been elected as a foreign member of the Russian Academy of Natural Sciences. Quite an occasion.

Swent: Wonderful, well deserved.

Fuerstenau: So after the San Francisco congress, and at that point, I said I wanted to step down from the Steering Committee and the Scientific Committee—and did so at the IMPC in Aachen in 1997. I had been the U.S. member for twenty years.

Swent: That's a long time.

Fuerstenau: Every country can have a member of the International Scientific Committee, but only countries that have had a congress have a member of the Steering Committee. Plans are underway to reorganize the IMPC structure to have a council, et cetera. But an important role of the current Steering Committee is to select the site for the next congress. For example, very formal presentations are made by different countries asking to be the venue for a coming IMPC. In fact, I was responsible for the 2000 IMPC going to Rome. The Brits thought they deserved it, but I kind of pushed having it in Rome. The Italians were very appreciative of the fact that I got the 2000 congress for them in Rome. The next one will be in 2003 in South Africa, Capetown, and I think the one after that will be in Turkey in 2006. I'm certainly hoping that I might still be going to the congresses then. [laughs]

Swent: I hope so!

Fuerstenau: But these are quite formal presentations. Obviously, the Chinese are working on one. I know from my former student, Dr. Pradip, that India has made a proposal.

Those mineral processing congresses, I think, play a very useful international role. But I've also been to a number of professional society meetings, actually many, that may be in one country or in a region involving several countries, such as the European Comminution Symposium, the European Particle Symposium, and many others. I'll just tell you about two or three of these.

South American Symposium on Extractive Metallurgy, Chile, 1973

Fuerstenau: One was in 1973. I think it was called the Second South American Symposium on Extractive Metallurgy, and I had been invited there to Santiago, Chile. I talked to many people about whether we should go or shouldn't—I meant myself—should I go there? Including Professor Warren Kaufman in Civil Engineering here who had just come back from there. He said that it would be safe and he gave me his Chilean money, which he said was worth pennies. I called a former Chilean student, Mauricio Hoover, who had done his PhD with me, working for Amax at the time, and discussed whether I should go. So I flew down there with Andy Mular, who had been a student of mine here, a professor now up at University of British Columbia, and one of his colleagues, George Poling—the three of us flew down together. I don't recall whether there were any other Americans. There might have been, but not of the academic type. A Canadian, Charles Cooper, working for Codelco met us at the airport, and he ran out of gas driving back to the city.

Swent: Oh my. What a beginning.

Fuerstenau: The meeting, you know, was uneventful, but one thing you could see were long, long lines, and I asked what they were for. "Oh, that line there is"—I mean, lines that went around the block—"for toothpaste," or lines around the block for cooking oil, somebody said. Another thing they told me was that when they bought a bottle of wine, they always felt under the wrapping around the cork because you couldn't take the bottle back if it was chipped, and you could only get a bottle of wine if you took a bottle back. This was when [Salvador] Allende was the Chilean president.

We had a field trip one day up to El Teniente, which used to be Braden Copper [Company], as you know. As I remember, there were seven parallel circuits in the mill, and two of them were just dead, rusted, nothing. They had been shut down for a long time. And another one was also down. So there were only four of seven lines running in that big mill. At that time, the really outstanding engineers of Chile had left when Allende came in to power.

Codelco was not staffed with great leadership at that time. Codelco today is first rank. Today's Chilean engineers are really first rank.

Swent: Was Bob Haldeman³⁵ there?

Fuerstenau: Probably no Americans. The mines had been taken over by the Chileans. At El Teniente there was a huge copper smelter that they were converting to the Outokumpu process, a massive new furnace was being constructed in the smelter. This was August, so that's winter. I remember it was kind of cold up there, up at the mine and smelter.

Swent: How high is El Teniente?

Fuerstenau: My guess—I'll just pull out of the air, 7,000 feet. It's not one of those that's at 14,000 feet; as some of them, like Escondida, are—very, very high.

A funny incident took place on that visit to El Teniente and the copper smelter. There was a very famous Australian process metallurgist, Howard K. Worner, who had developed a smelting process for CRA, which is now Rio Tinto. Worner was recognized as a very distinguished process metallurgist in Australia. CRA always named processes, machinery after the inventor. I saw a flotation cell made by a flotation engineer in Broken Hill named Davis; they called it Davcra cell. So there was the Worcra smelting process. Worner was on this visit up to El Teniente, and when we were touring the smelter, we were in the room where they were taking molten copper and casting it into anodes—you know, it's a big wheel with these forms that they make the copper anodes, about that thick [demonstrates] and so big, and there's copper splattering all over the place. I remember Worner stroking his chin a bit, and saying, "You know, I think they're getting 85 percent recovery on a casting wheel." [laughter] That's really funny.

Things were extremely cheap then because inflation was so bad that people traded their Chilean money into dollars, and you almost felt bad at their exchange rate, but what they did is then when they wanted to buy something, they would then turn those dollars back into Chilean money—I'm just speaking of ordinary people—so it wasn't surreptitious. I would never have changed money in Russia. Maybe the actual costs to us might have been 10 percent because of this horrendous inflation. I remember taking some Chilean friends to dinner. I think the dinner for four of us cost me only ten bucks for a big fancy dinner. And I bought a lot of blankets and poncho capes, whatever the cowboys wear, which we use up in the mountains to this day, for virtually nothing, because of that out-of-control exchange rate.

³⁵ Robert M. Haldeman, *Managing Copper Mines in Chile: Braden, CODELCO, Minerac, Pudahuel; Developing Controlled Bacterial Leaching of Copper from Sulfide Ores; 1941-1993*, 1995

Anyway, four days after I left, they bombed Allende, right where the hotel was because the government palace was on the central city square. The presidential palace was bombed out right across from the hotel. Several Japanese were at this symposium, and after the meeting went up to Chuquicamata, the former Anaconda mine. They were there on an excursion during the time that the bombing had taken place, and they couldn't leave for a couple of weeks because of that. Chile was under full rebellion, being led by Pinochet. So all of my apprehension was really right—and to have the good fortune of leaving just four days before that took place. I don't recall what led up to my apprehension, but there must have been a lot about Chile in the newspapers then.

About fifteen years later, I participated in an Armco Symposium on Comminution in Santiago and I talked with the taxi driver coming in from the airport about life under Pinochet. He was really quite positive about the economy of Chile under Pinochet. That huge inflation problem was gone so the bargains were also gone.

Hundredth Anniversary of Flotation Symposium in East Germany, 1977

Fuerstenau: In '77 there was another significant meeting that I participated in. You know that flotation as we know it started about 1905 in Australia, and it started in this country in Montana at the Timber Butte Mill in Butte in 1911. But flotation really started in Germany in 1877 by two brothers who had a graphite operation near Dresden, the Bessel brothers. The Bessel brothers patented a flotation process and built a plant where they ground graphite ore, made a slurry and added some oil, and then heated the slurry to near boiling, and so they made bubbles out of steam and dissolved air that attached to the graphite particles and floated to the surface where the graphite was scraped off. This works because graphite is hydrophobic [water repellent] due to its crystal structure. In my opinion this was the beginning of flotation in one sense. What happened a few years later was that high-purity graphite was discovered in Ceylon, and that put the Bessel brothers operation out of business, and flotation disappeared for a while.

Swent: But you had to boil it.

Fuerstenau: That's how they were producing their bubbles. Didn't occur to them to just blow in air. If they had, they would have led the way to modern day froth flotation.

So anyway, the East Germans organized a symposium to recognize the hundredth anniversary of flotation, and I was invited there. I think I was the only American. It was held in Freiberg, near Dresden, at the Bergakademie Freiberg and chaired by a very famous, outstanding professor, Heinrich Schubert. I had to go to Berlin, to go into East Germany. Schubert drove up

there to meet me. I had gone for the weekend to Munich and called him at his home and he said he would meet me at a certain gate in Berlin. I went there by taxi. Of course, I couldn't cross, but he was on the other side of the wall, on the telephone, and said, "I gave you the wrong directions. You have to go to Checkpoint Charlie." So I walked out, found a taxi, and got to Checkpoint Charlie. I was very apprehensive going through there because the East Germans were snarly and whatnot. Even the persons at the immigration window were surly, sneered and snarled, and whatnot. Anyway, as I say, with apprehension I went through there, but on the other side was Professor Schubert. First, he showed me around a little bit of East Berlin. And, boy, you only had to go about one or two blocks from the wall, and it obviously looked about like it did at the end of World War II. They had not rebuilt it. A lot of damaged buildings most run down, and so forth, and so on.

After eating in a restaurant where the food was nothing to write home about, quite greasy, we drove down to Freiberg, for the symposium, which was to me very interesting. I had prepared a detailed paper on the role of surface and crystal chemistry in the flotation of silicate minerals. The published paper looked very good in their proceedings. For a couple of days afterwards, Schubert and his wife drove around to show me various places, such as the Saxonian Alps. Up on top, looking down on the Elbe Rive is a castle where an alchemist was imprisoned and told to produce gold. What he invented became Meissen china—in the long run real gold. We went to Dresden one day. Dresden was right near by. The Zwinger palace, which is now a museum, was intact and open. A palace in the center was still blown apart, with trees growing out the windows. The opera house was still rubble. There was a Lutheran church that they made a monument out of that was just a pile of rubble, with a statue of Martin Luther standing in front of the ruins. By the way, when the IMPC was held in Dresden in '91, that opera house had been rebuilt. The palace was being redone, and this church—they had numbered every block and brick, and they were going to put this church back together again. But it shows you what the Germans were doing to bring things back to what had been there before. But anyway, that turned out to be a very interesting meeting, without great apprehension once I was inside East Germany. At that time along the streets of Freiberg, nothing was painted. Even the house of Alexander von Humboldt was unpainted. In Freiberg the cathedral there was all oriented towards mining, with statues of medieval miners, and so on, that held up the pulpit. Wonderful.

[Tape 34, Side B]

Fuerstenau:

Here it was, about the end of June, and, boy, I got a cold that got deep into my chest, and when I got back I was told by the doctor at Kaiser that I had pneumonia, and he said, "You got the real thing." My daughter, Lucy, was graduating high school and I couldn't go to the high school commencement.

[Added by Douglas Fuerstenau during editing: We had discussed earlier my role in observing the Fiftieth Anniversary of Froth Flotation in the United States in 1961. In 2005, the Centennial of Froth Flotation Symposium was held in Brisbane to celebrate the first century of flotation as we know it today. Since it started at Broken Hill in 1905, a few years ago I suggested that the symposium be held in Australia, and it was organized jointly by the AusIMM and SME/AIME. I served as a member of the Organizing Committee, and gave the opening plenary lecture on the first century of developments in the chemistry of flotation. An excellent monograph entitled, *Froth Flotation: A Century of Innovation*, edited by Maurice C. Fuerstenau, Roe-Hoan Yoon, and Graeme Jameson, was published in 2007 by SME/AIME. At the symposium a number of people were recognized as living legends, including Frank Aplan, Nat Arbiter, Tom Healy, and myself.]

South African Institute of Mining and Metallurgy Flotation Course, 1978

Fuerstenau: Then in '78 Maurie [Fuerstenau] and I were invited to go to South Africa to give lectures on flotation, at what they called a winter course. So we went down there. It's a one-week course on different subjects each year where interested engineers, and so on, come from all over South Africa, under the auspices of the South African Institute of Mining and Metallurgy. This was in Johannesburg at the University of Witwatersrand. A few of the lectures were given by South Africans, but I gave about eight of the lectures and maybe Maurie, about seven. There were maybe 100, 120, 150 people in attendance; it turned out to be a very good course. There were lectures, and they had some workshops in the afternoon and maybe some labs, in which we didn't participate. Manuscripts of all the lectures were prepared and they ended up as a book, *Principles of Flotation*, which was published by the South African Institute and edited by Peter King, who's been now many years, quite a few years, at the University of Utah. It's a good book. Recently, a professor from Bulgaria said that he had bought a copy of it in Germany, and he considered this to be the best book there is on flotation. But anything from South Africa in those days couldn't really be sold here—you know, '78.

But then we had one week where we visited various mining operations. Have you been to Johannesburg?

Swent: I've never been to South Africa. I'd love to go.

Fuerstenau: So at that time, the city had many virtual mountains or high mesas in it, and these mountains were big tailings piles. Quite high, probably a couple hundred feet high, and flat on the top, of course, and large. Then the roads went around them and so on. But in 1978 they were in the process of repulping those tailings, and pumping them out to a flotation mill, and then pumping the final tailings farther out for final stacking. We were in one new Anglo American mill which was very new; in fact, it had just been opened, treating fifty, sixty

thousand or more tons of tailings per day, by flotation, followed by leaching. What they wanted to do was get the uranium out of these tailings, and the pyrite for sulfur.

Swent: They had been mined originally?

Fuerstenau: Oh yes, these were tailings from past gold mines.

Swent: From gold, and they had residual uranium.

Fuerstenau: And they needed uranium because they were under embargo, and they needed the sulfur for sulfuric acid, so they were floating pyrite and then trying to recover the residual uranium. Huge flotation machines. At the time, they were the biggest I'd ever seen. In the mill, a worker was climbing over an operating flotation machine—and if he had fallen in, that would have been the end. And they had a thickener that I think EIMCO³⁶ had installed, which was about the largest in the world, four hundred feet in diameter, and on this thickener you could see whitecaps, from the wind blowing! [laughs]

We visited several other mills. Another new mill that was just being finished by Bechtel, and it was different from the construction at the McLaughlin Mine in that Bechtel had a big room totally full of blueprints, and I mean, thousands of blueprints hanging on racks. So everything had been drawn—and in those days; this would be '78—for this entire plant. By the time they did the McLaughlin Mine, they more or less made models that could be used to lay out where the pipes go, and so on. At that mill, I saw for the first time, a very large belt filter being installed.

Anyway, found that very fascinating. Then Maurie and I flew up—spent three days in Kruger Park, which is where all the animals are in the wild. Kruger Park is as big as Massachusetts, and we were there in August, so the grass was brown, and you could easily see the animals. At that time, they had water tanks run by windmills to attract the animals close to the road. Our guide said the typical Germans would come in their winter, which is South African summer. He said the grass would then be three feet high and you don't see the animals. But we saw lions, giraffes, elephants, antelope by the thousands, boks, zebras, wildebeests, hyenas, and hippopotamus. The only thing we didn't see was a leopard. Our guide had talked with his colleague, and the colleague went on one road and we went on another, and he said there was a leopard up, resting in a tree on the other road. We were there on a three-day tour, which was very good, just a few people in a VW bus.

³⁶ Joseph Rosenblatt, *EIMCO, Pioneer in Underground Mining Machinery and Process Equipment, 1926-1963*, 1992

Palabora is at the entrance to Kruger Park, so afterwards we visited the Palabora mining operations, which was the big copper mine then owned in some measure by Newmont. In the mill they had two of the world's—at that time—biggest SAG mills, and one of them had a crack in it. You know, misdesigned by Koppers Company in this country. And the shell couldn't take the load. At that time, they were busy fighting the crack in it, and I guess eventually they got a new mill and sort of split the huge cost of replacement. But apparently it had not been designed right, structurally.

We had quite a tour. They recovered some uranium-vanadium there out of that ore, too. I remember seeing the small plant where those were recovered. The Palabora ore contains about 10 percent magnetite, which is magnetic iron oxide. But it runs about 3 percent titanium. They decided to remove the magnetite, so what they did is pass the tailings through a magnetic separator. And so here was this massive, and I mean maybe a mile long and a block-wide- or a half-block-wide pile of magnetite, which some day might be valuable. The engineer who was our tour guide said that they had proposed selling this rather cheaply to the coal companies that used magnetite suspensions to make a heavy fluid for sink-float cleaning coal. With a slurry of density 1.8 or somewhere in through there, you can float the coal and sink the slate. But they wouldn't even take this magnetite he said. They had always used some other magnetite, which was high-grade, cost them money. That tells you something of the kind of thought process in the industry.

Anyway, we drove on out to see the tailings impoundment, and this tailings dam—oh, must have been two, three miles across. It was big. And what they do is they build up the dam by cycloning the tailings to recover the sand, coarse fraction, that they use to build the wall of the tailings dam, and then the fines just go on inside the dam itself. Like, at Homestake they would call those the slimes, if you remember.

Swent: Yes.

Fuerstenau: Anyway, I always take pictures, but unfortunately I did not get this particular picture, which was this big cyclone operating on the top of the tailings dam with two baboons sitting there, watching it.

Swent: [laughs]

Fuerstenau: And this engineer said, "Those baboons stay there all day every day, watching that cyclone." The underflow is kind of spiraling out of the bottom, and they're fascinated by it. He said they sit and watch it all day. Anyway, we drove down to the far end of the tailings pond and saw, living in the water, a family of hippopotamuses. And so when you talk about environment, obviously the flotation reagents were no problem. Our guide said they'd been there for years, and they live in the water part of the pond.

Some of the Palabora tailings are split because the mine is adjacent to a big phosphate mine called Foskor, which I think was a state-owned company. I know Gaudin spent a lot of time—I knew but didn't realize what it was—working on the flow sheet for Foskor, the flotation of this phosphate mineral. Apparently the two mines abut each other, and so they must have worked out that Palabora would mine the ore where they abutted and would get the copper out, and then those particular tailings were then pumped to Foskor where the phosphate minerals were recovered by flotation. That was a big operation too—maybe fifty thousand tons a day.

So we had a very nice tour. Back in Johannesburg, we asked if we could go underground in one of the mines. Those mines are something like ten to twelve thousand feet deep. They said that they really needed a week or two weeks' advance notice—because going underground is a two-, three-hour operation, just going down. We were told that the largest mine of Goldfields had thirty-five thousand men underground, so moving people in and out of such a mine is a very major operation.

Swent: Oh my!

Fuerstenau: I think at Lead [Homestake Mine], at most they may have had twelve hundred or something years ago, fifteen hundred maybe. But thirty-five thousand people underground! And getting them in and out was such an ordeal, so we didn't make it underground in the mine.

You know, a lot of the workers in the mill were black in Palabora, and I'm trying to think whether—because I know I asked about relative pay. I think at Palabora mill operators and truck drivers received the same rate of pay whether black or white. But for the miners around Johannesburg, you couldn't have thirty-five thousand people underground and pay them very much money and stay in business, right? I just don't recall how that was there.

Australian Symposia

Fuerstenau: Over the years I participated in six or eight congresses or symposia in Australia—in Sydney, Melbourne, Adelaide, Brisbane, Broken Hill, Mackay, and Kalgoorlie, and I've probably given nearly a dozen seminars in colleges and universities around Australia. But there's just a bit about a couple of Australian ones that I'd like to mention. I'd been invited in 1969, I guess it was, to go to Australia for IUPAC Congress, International Union of Pure and Applied Chemistry, and I was a plenary speaker. I gave a talk called "Mineral-Water Interfaces." It was one of the main talks of the meeting. Because a lot of what I deal with is surface phenomena of minerals, and that was one of the broad topics of that international meeting. There I met the great Russian surface chemist, Boris Derjaguin. Colloidal interactions are analyzed using the very well known DLVO theory. Overbeek is the "O" and Derjaguin is the "D"

in the acronym. During those sessions, I also met a number of the major surface chemists from Australia and England.

After this meeting, which was in Sydney, I spent a day in Canberra and then went down to Melbourne and visited the University of Melbourne and CSIRO, which has big flotation research facilities and grinding research and so on. CSIRO, the Commonwealth Scientific and Industrial Research Organization, is the big government research agency of Australia. There are many different laboratories. It's Commonwealth Industrial Scientific and something. There are many different laboratories around the country, with two different mineral laboratories in the vicinity of Melbourne at that time. My friend, Tom Healy, who was really behind my being invited to the IUPAC meeting, planned to send me up for the weekend to the Great Barrier Reef. However, the head of the Minerals Division of CSIRO, Ivan Newnan, said, "Oh, you don't want to go there. You want to go to Alice Springs." He personally changed my itinerary and redid my trip. And so I flew to Broken Hill on a DC-3 in '69 and spent a full day visiting different mills. You know, Broken Hill, which is in the southwest corner of New South Wales, is one of the great mining districts of the world. Flotation started there in 1905. I think they still have ore that runs something like 25 percent lead, plus silver, so it's a big union area. Unions can say a lot when you've got ore running 25 percent. Broken Hill was another place like Butte. Well, you know that. I remember visiting the Zinc Corporation; they had Broken Hill North, Broken Hill South. I think a lot of it became CRA and is now renamed Rio Tinto. There I saw the Davcra flotation cell in operation.

Flew down to Adelaide and then the next day up to Alice Springs, so I was at Alice Springs for a couple of days, and that's a lovely spot, right dead center, and kind of an oasis in the center of the outback of Australia.

Swent: Did you climb Ayers Rock?

Fuerstenau: Couldn't go to the Ayers Rock because I didn't have the time. Now, this was '69, and I was scheduled to give a talk at the local Australian Institute of Mining and Metallurgy in Mt. Isa, and the only way to go was there was with a bush plane that flew only two or three days per week. I had to leave Alice Springs with maybe five of us as passengers on this plane that flew from Alice Springs to Mt. Isa on Saturday afternoon. By the way, I have heard that today Alice Springs has major airline service because of the huge influx of Japanese tourists—it's one of their prime destinations.

But Saturday morning, there was a great annual event that they called Henley-on-the-Todd. A dry river that runs through Alice Springs, except on rare occasions when there might be rain, and they had a lot of activities that day on this wide flat sand bed. For one of the events, mine rails were laid down on the sand. They had rowing shells with wheels that ran on the tracks, and races were similar to those with shells on the water at Henley, but this was on the

sand. The crews were paddling on the sand, and having great fun on this dry river bed. Later back in Sydney, I saw on TV the races that I witnessed there in Alice Springs.

Anyway, I sat up in the front of this little airplane, and we flew about three hours, I guess, to Mt. Isa at about 10,000 feet or something, and you could look down and here and there you could see what they call a station, or they call it [imitating Australian accent] sty—shun, which is a ranch. You'd see one here, and then after a while you'd see one there, because the outback is so dry.

I spent the weekend and a couple of more days at Mt. Isa, where I gave a talk to their local meeting of the Australian Institute of Mining and Metallurgy, and toured the mill and so on. On Sunday with another visitor I hiked out into the outback for a ways. Mt. Isa is a large underground copper-lead-zinc mine—I think all three of them³⁷. At that time Mt. Isa was owned by AS&R. Later Mt. Isa became a separate company and I think bought AS&R. As you know, AS&R, which was the basis of the Guggenheim fortune, just isn't any more. The mine obviously is a very good mine with an excellent engineering staff, both for the mill and the mine. A lot of rock mechanics has been worked out there because ground control has always been a problem. They needed good rock mechanics people for design of their underground workings, and so on. Mt. Isa is a town all by itself right out in the outback of Australia, with major airline service to Brisbane.

On that trip I also visited BHP labs in Newcastle—about eighty miles north of Sydney. I had been invited there by Tom Calcott, who was in charge of some of their processing research. Calcott had written a series of classic papers on modeling comminution grinding when he was working for the Coal Board in England. Through his comminution work, I had gotten to know him earlier. At BHP, as I recall, he developed a process where they would grind coal in water with a small amount of oil and make small dense coal pellets from low-grade coal there. These pellets could be transported by pipeline. I know that BHP tested this on fairly large scale some years ago.

³⁷ G. Frank Joklik, *Exploration Geologist, Developer of Mt. Newman, President and CEO of Kennecott, 1949-1996; Chairman, Salt Lake 2002 Olympic Winter Games Committee, 1997*

The Ian Wark Symposium, 1983

Fuerstenau: Before we quit, one more item is: in '83 Tom Healy decided to organize a symposium in honor of Sir Ian Wark. Now, there are in flotation science of the twenties and thirties three big names, which are: Gaudin, then at the University of Utah and Montana School of Mines before going to M.I.T.; Taggart, a big man at Columbia University but not quite the same stature; and then Ian Wark at the University of Melbourne in Australia. Wark did a tremendous amount for several years on delineating the principles of sulfide mineral flotation—mainly by using the measurement of contact angles on pure mineral surfaces. If you put an air bubble on a solid surface immersed in water and the air bubble adheres, you see a definite angle through the water, and that's called the contact angle. If you get your car waxed and you see a drop of water on it, the drop sits on the surface with a sharp angle at the contact. Or on silverware, if you use soap, you see the water droplets stand up that way. For flotation, you need conditions where the mineral has a fairly large contact angle. So Wark developed a procedure for measuring contact angles, and his work was so good that his results on determining conditions under which various sulfide minerals respond to flotation are still used today. With Alwyn Cox, he published a tremendous series of papers on this work, many in the *AIME Transactions*. Healy found that Cox was still alive and invited him to the symposium. In 1940, Cox had left science and had gone into the banking business. In 1938 Wark published his outstanding book, *Principles of Flotation*, and in 1955 a second edition coauthored with a later associate, Keith Sutherland.

Anyway, during the forties, Wark became the overall director of CSIRO and was not actively involved with flotation research. Anyway, to recognize Wark's great role in the field, Tom Healy had organized a symposium on flotation in his honor that was held in Adelaide in 1983, and he invited me to be the opening plenary speaker. Major flotation researchers from around the world came there for this symposium.

I had met Wark on that first trip in '69, but just to shake hands with him and Keith Sutherland. I really got to know Wark—Sir Ian—on this occasion. He was a "sir" by then. I remember Healy saying, "Well, I know I'm going to have a hard time getting your paper, so I'm going to tape it." Well, he taped my talk, and sent me a transcript of my presentation. I used that only as kind of a beginning for actually preparing the final paper. When the published volume from the symposium came out, my paper is pretty good, but my transcribed talk really was disjointed—that's no way to write a paper.

[Added by Douglas Fuerstenau during editing: Recently I found the following letter from Ian Wark dated August 2, 1983, that I had pasted in the back of his 1955 flotation book:

“Dear Doug,

Your letter of June 13, handed to me at the dinner in Adelaide, is far too generous. To be placed on a pedestal with Taggart and Gaudin is more than I could even have hoped for or expected.

It was good to have you at the symposium and I look forward to reading your address at leisure. It seems a bit unlikely that I will pay a return visit to you but I do wish you and your department a very successful future.

Yours sincerely,

Ian Wark”

]

After the symposium, we rode on a bus up to Broken Hill, where there was a major annual meeting of the Australasian Institute of Mining and Metallurgy. It was either the centennial of the Australian Institute or the centennial of Broken Hill. A special meeting. And anyway, I remember we stopped and had lunch at a winery on the way up, and during the long ride I talked at length with Wark and his wife, and particularly with Wark. So I really got to know him at that time. He told me that he had been a postdoc under G.N. Lewis at Berkeley for a year in 1923—something that I had not known. I gathered—but this part I didn’t know—that he was a very ardent fly fisherman, tied his own flies, and apparently a lot of flies sold in Australia are called Ian Wark flies.

First Visit to the Gold Mines of Kalgoorlie

Fuerstenau:

At that time, I already had a relationship with Homestake, and so I had arranged to go to Kalgoorlie, so I flew from Broken Hill down to Adelaide, met the Homestake accountant, John Horan, and flew with him to Perth. I remember meeting Hugh Morgan, who was the manager of Western Mining [Company] out there. We spent an hour or two in his office. He’s now head of Western Mining.

Then I flew to Kalgoorlie. At that time, Homestake owned half of the operations there, but only 15 percent, I think, was owned by Western Mining, but they managed it and arranged a very nice tour for me. I went underground at Mt. Charlotte, which then was a mine that you go down in a truck. You sort of spiral down in tunnels outside the orebody. To mine the orebody, a huge block is drilled in preparation to do an annual blast. They let everybody in town know when the blast will be. And I guess the whole town shakes when that big blast goes off. After that, the broken ore is taken from the mine by truck and crushed. The crushed ore was transported to the mill where it was ground and floated to recover the pyrite as a flotation concentrate. The pyrite was then roasted to remove the sulfur and open up the mineral grains to

recover the gold. As I remember, about 90 percent of the gold was recovered in the flotation concentrate, so the amount of material needed to be sent to the leaching circuit was greatly reduced.

This was '83, and I stayed in the guest house of Western Mining, and the other person staying there those couple of days was Sir Brodie Hall, who had been president of Western Mining. He had graduated from the Kalgoorlie School of Mines and keeping the institution alive and thriving was now his main interest. I had dinner with him one night. Since he and I were the only two persons in the guest house, I had a tremendous evening learning some of the history of mining in Kalgoorlie from him.

The so-called Golden Mile was still there. All along the road in a row was just a complete mass of head frames, in Kalgoorlie. They're all gone now, most of them, because that Super Pit has taken them; the land is all gone. But to me, that was a most interesting site to see. And the mine that Homestake was involved with, Mt. Charlotte, was the only one operating at that time in Kalgoorlie. The pit wasn't there yet.

Swent: This was still an underground mine then?

Fuerstenau: Yes. It was only an underground operation, 1983.

Swent: Was the Sons of Gwalia Mine there? That was the mine that Hoover found.

Fuerstenau: You know, I couldn't tell you, but I saw that house that Hoover lived in; Dick Tastoula pointed that out. Dick Tastoula was the general manager. Later on, he eventually became a Homestake employee, but then he was the Western Mining manager—that was eighteen years ago. I remember afterwards we went to a bar—

[Tape 35, Side A]

Fuerstenau: Dick Tastoula and I and maybe a couple of other people, after work went to this very famous bar for a beer. In the building there was a great, fancy central bar down the middle, and on one side were the engineers and the managers and whatnot, and on the other side were the miners and laborers, and in between, as I remember, were rather scantily-dressed barmaids. As the Super Pit was developed, they had to purchase this historical stone building and move it back from where the limits of the pit would eventually reach.

Swent: It was so important.

Fuerstenau: Yes, to the history of the area. I don't remember the name of it at all. But it cost a fair amount of money to buy it out and move it back away. But seeing this part of mining history was an opportunity presented by going to a technical meeting.

On thinking about it, attending a technical meeting provided the opportunity to make a visit that had these other mining aspects to it. You know, I had never thought of that, but my former student, John Herbst, who had been a professor at University of Utah, said, "You know, one advantage of being a professor is travel." That had never entered my mind, although I traveled a lot. Herbst considered that as one of the advantages of this profession.

Swent: That you do have the time and the means to do that.

Fuerstenau: I'll tell you, in my travels, I always would try adding a day or two on a trip to visit the place, see the area. Like, when I went to Leningrad, I went first to Stockholm and added two days just so I could see Stockholm. I walked all over that city on my own. I always did that, to try to take advantage of it, rather than just go somewhere and do your thing and come back, which is generally the case if you're in industry.

Lecture Trip to Changsha, China, 1979

[Interview 15: October 19, 2001]

[Tape 36, Side A]

Swent: We were dealing with international affairs.

Fuerstenau: Right. I'll just change the sequence a little bit, but talk about a first trip to China, which was in 1979. I was away at the IMPC congress in Poland when the Chinese delegation of the minister of nonferrous metallurgy came to the U.S., and in particular one of his objectives was to propose a program of lectures related to mineral processing and leaching of ores.

One of the delegates was Professor Hu Weibai [I will refer to him as Weibai Hu from here on out]. Hu had gotten a master's degree from the University of Utah about 1948 or '49, and had started a PhD program at Berkeley. Later Weibai Hu told me one time that his wife wrote that she and their two children were starving, and so he returned to China in 1949. Anyway, the delegation visited Salt Lake City and set up the groundwork for this program, which involved Milton Wadsworth of the University of Utah, who I consider was the world's premier hydrometallurgist at that time, a former student of mine, John Herbst, who was then professor of metallurgy at the University of Utah—his field was automatic control of mineral processing, grinding flotation, et cetera—and myself, in the world of flotation chemistry. They wanted us to give a series of lectures for about two weeks in each of these three areas: flotation, hydrometallurgy, and automatic control. This was really before people went to China. I think roughly the first delegations were 1978, so this was the next year.

Swent: We had just had diplomatic recognition of China early in '79.

Fuerstenau: Yes, right. They financed the entire thing, which included our airfare from the United States to China at that time. We went to Hong Kong, and there we were to buy—Milt Wadsworth took care of that—three professional Kodak projectors to take into China. In other words, they wanted to make sure that these projectors worked. We bought those in Hong Kong and then went by train on into China, were met in Canton (Guangzhou now) by Weibai Hu and a couple of others, and stayed overnight and then went by train up to Changsha.

Our lectures were to be at what then was the Central South Institute of Mining and Metallurgy. This was really the number one mining and metallurgy institution in China. It's now called Central South University of Technology. We met two people who had gotten doctor's degrees at MIT during World War II, went back to China after the war. [They were Professor Shen and

Professor Huang.] They went back in 1945, and were the two people who established the institute in Changsha, which, as I said, grew up to be the major mining and metallurgy institution in China.

As for Changsha, we were there in August. Even the locals there called August, “Terrible Tiger.” Really hot, terribly hot and terribly humid, and so on. Changsha is where Mao [Zedong] came from. He went to a normal school, a school for learning to become a teacher. Since in his early career, he was a school teacher there in Changsha, there were big statues of Mao all around the city. He was born nearby. At that time, all of the Chinese wore Mao suits, both men and women, mostly gray but some were blue. There were a few cars, some buses, some trucks, but the main transportation was bicycles and walking, some horses. The roads were virtually empty of cars in 1979.

Each morning at about 7:30 or 7:45 there were three cars and three drivers, one for each of us, to take us the five or ten miles from the city center to the campus. I still recall passing each morning persons pushing wheelbarrows with a squealing pig tied down—obviously on their last ride. Anyway, we lectured from 8:00 a.m. till about eleven or so every day. I thought we were to be giving lectures for students, but it turns out these were lectures—well, I suppose there were a few grad students—but they were professors and directors and institute people from all over China. There may have been 120, 130, 140 in the lecture room that I had. Like I said, I thought they were going to be students—and later on Weibai Hu told me that they had had five thousand applications for this program.

Swent: There were translators?

Fuerstenau: That was kind of interesting. Yes, that’s interesting because first of all, the best translator—and I’ve seen her a handful of times since—was Milton Wadsworth’s translator, a woman named Nova Chiao, who had graduated from Radcliffe about 1945, ‘44 in chemistry and had married Professor Huang, one of the two MIT doctorate people, and returned to China, whereas her sisters and brother all stayed in this country. Her father was a professor here at Berkeley, retired, but one of the world’s most famous linguists. She said he had something like thirty-five honorary doctor’s degrees, Chiao, Professor Chiao. She said her father had a PhD from Cornell, had gone back to China, and that about 1913 Bertrand Russell visited China, and her father was his translator/interpreter, which I guess got him into philosophy and linguistics, and so on. Nova Chiao was Wadsworth’s translator.

My translator was a doctoral student in mineral processing named Chuan Li. He didn’t do so well. About on the first sentence, he was stumbling. And out in the back were two senior professors, who started to yell out the translation. One was Professor Hu Shi-Gung. He had also a master’s degree from University of Utah. He worked five years after that in South America in Peru for American Smelting and Refining Company. But they would not let his

family come. I'm speaking of after the revolution. So he returned to China also.

And then another professor from the Northeast Institute, Professor Qiu, who had taught himself English, he told me, took over and served as my translator. Excellent. Senior, relatively senior person.

So giving those lectures was not bad, because one would talk a couple of minutes, and I noticed that the translation usually took about four, so the translator really seemed to get into his element.

Swent: Were you and Wadsworth having sessions at the same time?

Fuerstenau: Three parallel sessions. In other words, there were different people in each audience.

Swent: So they had to choose which one they wanted.

Fuerstenau: They were all pre-chosen. After a while, I found out that all three of us were getting up around five a.m. and working on our lectures. We didn't know that to start with. We independently sort of discovered that. Fortunately, I had taken everything with me I needed—you know, slides, notes, papers, and so on. They were the right ones because, as I said, it was aimed at a slightly different audience than I had originally intended.

Swent: Were there discussion periods?

Fuerstenau: Yes, in China we lectured eight till eleven, and in China there's a siesta. They went home at eleven, and things don't start until about two or three again in the afternoon.

Swent: Did they do eye exercises?

Fuerstenau: That one I don't know anything about, but I know they take this siesta, because often we would go out walking around while the streets were pretty empty during this siesta, and the younger people that were probably studying English that lived in the hotel and were acting as our guides wherever we went, didn't like that. They liked to do their siesta. When we walked out, they had to go with us.

But in the afternoon—I don't think it was every afternoon—but it was maybe two or three at least of each week, they had discussion sessions, which were really consultations. Different researchers were all in the room, and we were sitting—I don't quite recall whether all three of us were in the same room, or maybe they had three rooms for this. But the Chinese researchers would come with what they were working on and present it, and then want the answers—you know, a real consultation session. That went on no less than two or three

times a week. That's probably about right, those so-called consultations, not one-on-one, but one person would get up and talk about what his research problem was, and then ask us for any ideas we might have as to how they might proceed or suggest something as to why it wasn't working.

Swent: And did the other people offer suggestions as well, or was it just asking you for your advice?

Fuerstenau: I would say they probably took their turn, yes. Maybe as the time wore on, other Chinese may have informally participated.

Swent: I was wondering if you had any feeling whether it was a different situation than it might be here in the States. It was a similar kind of thing.

Fuerstenau: I guess I can't quite recall how that went, but they actually discussed their research problem, which was a practical one that had no easy solution. Obviously they must have had pre-discussions over what they would ask about. They certainly weren't going to ask about something that might be a defense mineral or something like that, so they surely must have worked it out amongst them ahead of time.

I remember one of the persons telling me that they were using a chelating chemical called a hydroxamate for the cleaning step in oxidized copper mineral flotation, and asking about why they were having problems because the froth was brittle and was difficult to handle. What I find interesting is that my brother, Maurie, had discovered the excellent flotation collecting properties of hydroxamates for copper and iron ores. He patented it, but the reagent was not put into practice here then. But the Chinese had read Maurie's publication from 1965 and had started using the reagent in one of their plants.

[More recently, Cytec worked on producing hydroxamates at lower costs, and these reagents are now being used in several different industrial applications, such as the flotation removal of anatase from kaolin to whiten the clay.]

Then each day—I guess somebody sat up all night and translated and printed the previous day's lecture, so they must have taped them. I gave copies of the slides of each day's lectures to someone—eventually I got an 8 x 12 book about so thick [demonstrates], which contained all of my lectures on the principles of flotation.

I later learned from somebody in China—and I never thought about this until quite a few years later—that their typical research had been to take an ore and test it and then find out how they might develop a process to treat it—what's the recovery and the grades. This apparently was a typical research paper. And somebody from China, in China, said that my lectures completely changed their approach to academic research—I'm speaking research at universities, et cetera. My aim has always been to understand what's the principle involved—

you know, the fundamental surface chemistry that might be involved in flotation processing. And after that, they completely reoriented their academic approach to research in this field. I don't remember who told me this, but it was at least ten, fifteen years later that somebody mentioned that I had had that effect on it.

Swent: Doing more on the science.

Fuerstenau: More towards the scientific explanation, the underlying principles, rather than just going and run a flotation test on an ore.

So anyway, these lectures took place over a period of two weeks, and after we were there one week, they also arranged for our wives to come. They paid their costs once they got to Hong Kong, and they then came in by the same route we did by train through Canton [Guangzhou] and were there the second week.

Excursion to a Lead-Copper Mine and to Sites around Changsha

Fuerstenau: The first weekend, Weibai Hu took us to visit a mine, quite a big mine, where they mined copper and lead, and we traveled maybe two hours or three hours by train. And this mine had an unbelievable grade—the ore grade was 2 percent lead and 0.08 percent copper, which I think in this country you'd call a rock. We went through the mill where we saw that all the equipment was homemade. They had made all of the equipment—you know, flotation cells, grinding mills, classifiers, et cetera. The mill building was constructed of brick and was two or three stories high.

I don't know whether this was an underground mine or open pit. There was no head frame in sight. I do remember seeing the train pulling mine cars to the mill. The train pulling the mine ore cars to dump the ore into the crusher was an ordinary railroad engine, a coal-fired steam engine.

Swent: My goodness.

Fuerstenau: In other words, they were making use of what they could—and at this mine, we were told, the total number of employees, which meant everybody—schoolteachers and so on of this town, which was there only for the mine—was five thousand people that lived off of this one mine. Really incredible, isn't it? Of course, as we walked around, given my height, I was a spectacle—they had never seen a non-Chinese before, out in the boonies, and anybody as tall as I am was half again higher than the people there. So wherever we walked, people would stand around, looking at this zoo walking by.

Swent: What did they use for power?

Fuerstenau: It must have been coal generated, I assume. You know, a coal-fired power plant. I'm sure the train ran on coal, because it really poured out black smoke. In fact, the train that we rode on—being hot, you try to keep the window open a little bit so that the air would flow through, and there was a little bit of black coal dust, which meant it was coming out of the engine.

So anyway, that evening we had a great banquet there. Every time we went somewhere, they had a nice banquet. There were two English-major translators. Professor Hu, I know was in there, and always traveling with us was the Chinese equivalent of their KGB man. There always was somebody in a jeep, always in front of wherever our cars went on any field trip or excursion.

But anyway, we had this really great banquet at a big round table, maybe fifteen total, including Hu and the two younger translators and the KGB guy, who joined right in, and the management of the mine. I think we must have had about thirty courses, including frogs that I'm sure just came out of the rice paddy an hour earlier. Marvelous food and plenty of it, and lots to wash it down with. I still remember this fairly young—in his twenties—translator I don't think he had ever tasted any *mai tai* or anything like that, and we said, "You have to *gambei*." The translator got a little red in the face after a while, because I don't think he had ever tasted a bit of their firewater before. Anyway, it was a banquet to remember.

Later we went to the smelter, on another trip simply by car, because the smelter was closer to Changsha, a lead smelter and probably zinc plant that would be tied with it, and I'm sure some or all of the ore came from the mine that we had visited. I gathered that China is not endowed very well with lead, zinc, or copper, based on what we saw.

So anyway, we were there two weeks and really got to know these people really quite well. On one occasion, Weibai Hu told us what it was like as a university student during World War II. They were west of where the Japanese had gotten to, and his metallurgy studies continued under very difficult circumstances. He said that they had only one textbook, which I think was an American metallurgy text. There was an around-the-clock schedule so that each student would get an hour to study from the book. He also said the way that they got salt out there was that a chunk of it hung on a string, and they could lick it now and then.

On afternoons that we did not have research consulting discussions, we had various excursions to local sites, such as a nearby mountain and an island recreation park. One evening we went to a Chinese opera and I still remember that with pleasure. We were up close to the stage in the theater. At an angle we could see the music makers at work—can't call it an orchestra. Dress by the opera patrons was extremely informal. On another afternoon they took us

to a commune, and we went through one farmer's house, which obviously was a showplace.

Swent: Setup.

Fuerstenau: All set up, with how nice it was and so on. But I do remember seeing a fairly big fish pond, where they raised carp as a fish farm, and that probably provided the seed idea for farming fish that now goes on worldwide. There they were making Chinese rice noodles, those transparent noodles, which were hung on racks outdoors like clotheslines—a big operation. I have a picture of the man stirring this massive container where the rice noodles were being made, with ashes dropping off his cigarette into the giant pot.

The railroad station in Changsha was a great, new marvelous marble-like building that was a show piece, with giant portraits of Mao and Hua, the new head of China, overlooking the massive lobby. Also the hotel we were in was a new building about six stories high, still partly under construction, which had, I think, only two air-conditioned floors and, of course, they had us on one of them. But you could see a lot of rebuilding going on in the city, such as rebuilding streets.

Tour of Guilin and Beijing as Payoff

Fuerstenau: Then afterwards, the payoff for our lectures was as much travel as we wanted, and we had decided just to be there three weeks, and so we went to Guilin. You know, when Nixon went to China, the second time he went, the one place he wanted to go again was Guilin. That's where all those sugar-loaf mountains are.

Swent: Karst.

Fuerstenau: Karst. We were taken down a magnificent limestone cave, about the most magnificent one that I have been in. Beautiful. And everybody naturally takes the boat ride down the River Li, which we did—did you do that?

Swent: I've never been to Guilin. We were just assigned. We had no choice. We went to sort of their substitute for Guilin, which was called Seven Stars. It was similar but not quite as good. Xiao Qing was the name of the place we went.

Fuerstenau: Was it the same limestone mountains?

Swent: Right.

Fuerstenau: It was Kweilin and now it's Guilin. They've changed their spelling as you know—it was once Peiping, then Peking and now it's Beijing, although it's still Peking duck. [laughs]

- Swent: What was the sanitation situation?
- Fuerstenau: I would say very good. I know the three wives always had their little thing of rating the sanitation on a one to ten, on their own scale, and even the trains were quite all right. Did you travel on the trains?
- Swent: Some.
- Fuerstenau: Okay, so you know what I'm talking about. Well, we were always put up in good hotels, so I would say it was always good. One time in a hotel in Beijing, the water stopped flowing for a bit in the morning in the shower.
- Swent: Did you have any sense of the safety or health at the mine, or the processing plant you visited?
- Fuerstenau: We had hard hats, and the hard hats were made, woven out of straw that would be, I'd say, almost a quarter-of-an-inch-diameter straw, because I know in the lead smelter I hit my head on a low pipe, but I could feel the hard hat give, with this woven straw, and I thought, "You know, that's a pretty good thing." It didn't give a real clunk because the hat gave—we were given hard hats, and maybe a kind of gaiter to wear.
- Swent: What about the workers?
- Fuerstenau: They all wore the hard hats there, that I saw. Like I said, we saw none of the mine itself, so I have no idea about that.
- Swent: But in the plant—
- Fuerstenau: In the plants—in contrast to what we had seen in Russia.
- Swent: The machinery. They were protected from the machinery?
- Fuerstenau: No, no, I don't think so. I recall open belts and things like that on the operating machines. I would say probably no protection at all. In fact, I think in the mill it seemed like even the building was moving a little bit, the floor. Well, once in a while you run into that in the U.S., the floor being a grillwork. What made more of an impression was the machinery—that they had made it themselves was extremely obvious. I mean, it even looked homemade. And they were, of course, proud that they had done that.
- I should back up and tell you about my own experience. During our first week there, I wanted to move the bed and strained my back a bit. They decided that I should go to a hospital in the afternoon, and when I arrived, there were at least three or four doctors waiting for me, and when I lay on a table, no less than a dozen or more other people stood around to watch this tall stranger. After some consultation, the doctors had three alternative approaches, one

being pain relieving medicine. Another being acupuncture. I guess I was chicken and took the pain relief approach. I maybe should have tried the acupuncture approach, but chickened out. Anyway, all cleared up quickly.

[Added by Douglas Fuerstenau during editing: In 1983, Weibai Hu spent a year as a visiting professor in our group at Berkeley. During the time, he presented a two-unit seminar course on processing minerals in China. I personally went to most of his lectures, and learned a number of different things about their approach to industrial processing. He told the story that in one large mill three or four stories high, the method for concentration of the ground ore, which might have been a tungsten ore, they had many large-scale shaking tables. Apparently, one night all of the shaking tables got into synchronization and in resonance with the building, which must have been made of the usual brick, and it all came crashing down in a pile of rubble. Of course, he did not tell us about that when we were in China. After his year in Berkeley, Professor Hu moved permanently to the University of Utah, and eventually brought most of his family out of China.]

Another thing: In driving around China a bit, often you saw big caves, big tunnels in the hills, and I mean major tunnels, with signs over them. Our guide/interpreter said that these were for protection in case of war, and there were lots of these huge man-made tunnels. And they had been built only in case they were going to be bombed. I don't know if they were worried about the Russians or about the Americans, but we were told that the signs said that they were to save the people—something to that effect.

Swent: You didn't think they had been used previously?

Fuerstenau: No, no. They looked fairly fresh. We were told that this was their purpose.

Swent: I see.

Fuerstenau: It may have been one way to help keep people busy, too. [laughs]

Another thing that one saw regularly along the road: The farmers would put their ripened wheat on the road, and when cars and trucks drove over the dried grain, it would in essence thresh it. At the end of the day, they picked it up and moved it to large concrete platforms where they would drop it and let the wind carry away the straw and collect the grain itself—the same principles of old fashioned threshing machines.

So anyway, after Guilin, we went to—then it was Peking—Beijing, and—you know, this was almost September 1. The food in the south is very hot, and as you go north, it's very bland. You know that from your own trip. As for Beijing—there's a lot of wheat up there, in contrast to rice in the south, for example. And if you go south to north, you find that the food doesn't have

that zing to it that it has in southern China—and one should really do the reverse travel.

But we were in Beijing on an absolutely clear day. Something like up in the Sierras. You know, where the sky is blue and no haze, no smog, and wonderful. And that one day that was so perfect, we saw most all of the palaces—you know, Temple of Heaven and the Summer Palace and—

Swent: What we used to call the Forbidden City?

Fuerstenau: Oh, the Forbidden City, of course. And then we went into the parliament building, which has that great hall. Did you get into that?

Swent: Yes. Great Hall of the People.

Fuerstenau: Hall of the People, yes. This is at Tiananmen Square, as you know. Oh, and then we went down to see Mao in his tomb. Was that open when you were there, Mao's tomb?

Swent: He was displayed out there.

Fuerstenau: Did you go down in and see him?

Swent: Did we go down in? We saw him. Yes, we had to. We had no choice. We were taken down.

Fuerstenau: Okay, so they still were displaying Mao. Okay. You know, when I was in Russia we actually went down, as part of our tour, to Lenin's tomb.

Swent: Yes, you had no choice. You had to.

Fuerstenau: And, in my opinion—I watch people. I'm speaking now of Russians. And it looked to me that the Russians were doing almost the equivalent of praying as they walked by Lenin's tomb. You know, he was in essence the equivalent of their god. They didn't say any of that, but I could see they seemed to have a certain reverence as they walked by Lenin's tomb. That was in Russia.

But anyway, we visited Mao's mausoleum down underground. But to me, one of the real high points of China was going to the Great Wall.

[Tape 36, Side B]

Fuerstenau: We hiked along the wall, and they were just starting to work on the repair. Where we were was where you would have also been, that close part to Beijing. There were a lot of tourists there, but all the other tourists were Chinese soldiers because there were no other travelers in 1979, and they weren't guarding it; they were just the only visitors. And our two traveling

guides, the young English majors in the college—for them to make this trip to Beijing was really a prize thing. It was their first opportunity to see all of this they said.

What always surprised me was when you look to the northwest from the wall, you really look out into mountainous desert. You know, they put the wall right where there was a demarcation between where it was somewhat green and where it wasn't; you probably made the same observation.

Swent: It's really pretty spectacular.

Fuerstenau: They wanted us to give some more lectures in Beijing at a couple of research institutes, but we said no; this was our relaxation time. Then we flew back to Guangzhou and took the train back to Hong Kong, and it was sort of like night and day, getting back to Hong Kong. In fact, we stayed at pretty fine hotels. It was just a real revelation to finally leave.

Oh—we had dinner in Beijing one night with the minister of nonferrous metallurgy of China, and I could tell you're dealing with a very high-level person who was in charge of all the mining and production of copper, lead, and zinc of China. You knew you were talking with, dealing with somebody that was professionally at the top. They had Mao suits, but he had an elegant, dark blue Mao suit that was probably made of a little more than just simple cotton, probably silk. But everybody in '79 wore Mao suits. There really was no color at all. The minister, whose name I do not recall, had been president of the Central South Institute of Mining and Metallurgy in Changsha, which is an indication of the stature of that institution in China.

After that, the young man, Chuan Li, who initially acted as my interpreter, wanted to come here as a grad student, I admitted him, and he came, not as a Chinese scholar but as regular graduate student. There were a lot of Chinese scholars that came to this country, paid by the Chinese, but Chuan Li was admitted through normal Berkeley application procedures as a PhD student here, and I paid him as a research assistant. So after that, a lot of Chinese delegations, and so on, came through here, and several were from Changsha. I would send him to the airport—you know, to meet them with a van. A couple of times I arranged to rent a van to drive them around, and I would have him go as the driver. I only learned about a year or two later that each time the head of the delegation harangued him that he should go home, that he was stopping Chinese scholars who should be coming here instead of him. I knew none of that. Every time a delegation came, I would send him out to meet them or talk to them, and they would harangue him for being here. I was absolutely unaware of that because he never told me until two or three years later!

So when Chuan Li, now named Charles Li, eventually finished his PhD, he went to—and he still is in Australia, working for CRA, Rio Tinto. But he has

made trips back to China for Rio Tinto in more recent years. He was a very good person. He was then thirty-five years old, which brings me back to some discussion of—what was that bad period in China?

Swent: Well, there were several. The Cultural Revolution?

Fuerstenau: Cultural Revolution. Weibai Hu, who probably was considered the top mineral processing professor in China, said that during the Cultural Revolution, they shipped him somewhere, burned all his Chinese books but let him keep his Russian and American textbooks, and so he said he wrote textbooks of mineral processing, using the English books he had, like Gaudin's textbook, et cetera, that they hadn't taken and burned. So he wrote a number of books on mineral processing and that made his fame within China. But during that time, his wife was a librarian, and she was so badly treated that she committed suicide, he told me.

Swent: Oh my.

Fuerstenau: The president of the institution, the Central South Institute of Mining and Metallurgy, during the Cultural Revolution was made to clean the bathrooms, the latrines. It was all run by these—

Swent: Red Guards.

Fuerstenau: Red Guards, which were just teenaged kids about fourteen years old typically. In fact, Chuan Li's wife had been a Red Guard. She's just a normal person today. But they did this—

Swent: What did Hu—what did they do to him?

Fuerstenau: He had to go out and be on a farm somewhere, but he wrote books while he was there. And most of them were sent out to farms to work—but the real tragedy of China in that period was that there's a whole generation, a decade of very intelligent people who didn't go to college, and in '79 we were told they were now truck drivers, et cetera. I think the Cultural Revolution got over in '75 maybe. It lasted about ten years.

Swent: I used to know the exact dates, and I can't remember now.

Fuerstenau: And these were a lot of super-unhappy people that lost their chance of going to a university because let's say they were twenty-five, twenty to thirty during that time, and their whole life was ruined. They're the ones that had the worst part of it. But every university throughout the country was shut down, and so on.

I do know that when we were in Changsha—we were there two weeks—every Friday afternoon all the students and faculty and staff had to attend a lecture

on the philosophy and politics and the party—a political thing that they all had to attend every Friday afternoon. But the Cultural Revolution was just an unbelievable thing.

Swent: Terrible.

Fuerstenau: After that, we had these various delegations come through Berkeley fairly regularly, because of our interaction.

Swent: You forged a good relationship.

Fuerstenau: One time, six of them came—it was just the day before Thanksgiving, and this included the president of the university at Changsha, Lo Lao, who looked more like a boxer than an academician—he was president but he had no intellectual component. He was there as the head of the institution, just to run the place. But he looked more like a boxer or wrestler, which he probably was. There were about five or six of them who came on Wednesday before Thanksgiving, and we were already having a German visiting professor and his wife and son to dinner (Fritz Loeffler from Karlsruhe), and my former student, Prakash Kapur, who happened to be in Berkeley, and a visitor (W. Scheibe) from East Germany, a researcher at the Freiberg Mineral Processing Research Institute who was traveling. We were already going to have this big group to our house for Thanksgiving dinner, plus our three children and our daughter Sarah's roommate from UC Santa Cruz also.

So Peg says, “Well, we might as well have the Chinese too.” So we had twenty people, I think it was, to Thanksgiving dinner. Of course, the Chinese had never been involved in anything like this, or how to eat—our style of eating, but it turned out to be quite a great affair. Some could speak no English, of course, but then Weibai Hu was with them and his English was good. Other delegations came through, a couple of times to our house and other times we ate just at the Faculty Club.

A Second Visit to China, 1981

Fuerstenau: In '81 they asked us to come back, which we did, but John Herbst could not go. He had another kind of commitment. This time the group included Milt Wadsworth, myself, and my brother, Maurie, and so there were the six of us that went this time, including our wives. Somehow our plane got delayed by a day, and so we were one day late getting into China and we couldn't go to Guilin, which was the first stop on our agenda. At that time, you could not change any itinerary because there just was no extra space on airplanes. So this time our commitments were only to give seminars here and there. We were in Changsha a few days, which is interesting. At the airport, I ran into somebody I knew from American Cyanamid, Dr. Peter Avotins, who had graduated from South Dakota School of Mines.

Swent: Well!

Fuerstenau: In the airport in Changsha. He was there negotiating flotation reagent sale or manufacture. And also there at Changsha I ran into the president of Colorado School of Mines, who was probably on an advisory kind of tour. Plus my former student, Somasundaran, was there.

By 1981, you could already see color on people's dress, clothing. Just two years later, people had begun to change already. In the magnificent Changsha train station in 1979 were two huge pictures of Mao and his successor, Chairman Hua. As we left the train at Changsha, I told my wife, "I'll bet those pictures are gone." And you know, they were indeed gone. Before, those two pictures virtually covered a whole wall.

We were there a few days, but this time, as I said, each of us gave a seminar or so, and then we flew all the way to Kunming, which is where the Burma Road ended. I used to say if I were going to live in China, I'd love to be there because it's about five thousand, six thousand feet, elevation and lots of pine trees around and nice climate. I guess they call it the "Green City"—kind of eternal spring. I hear now it's just smog. But it was lovely then. On a Sunday afternoon we went by van to a tourist place some miles from Kunming, a couple hours away, called the Stone Forest. It's just large stones that stand up, looking like a beautiful forest. On the way, you saw people in a very dark blue, bright dark blue shawls and clothing, and our guides said, well, these are the indigenous people, not Chinese, but maybe Tibetan, or something. There were a fair number of them in the area, but the Chinese separated them out.

But anyway, we were at the Stone Forest, and I remember asking—our two guides that went on the bus with us out there were English majors at the university—and I asked this girl, young woman, "Have you seen Professor Wadsworth?" And she said, "Gee, which one is he?" She said, "You know, you white people look all alike." [laughter] We had an expression, as you know, that is the reverse.

We were there at the Kunming Institute of Technology, which had a number of very nice buildings. And I remember seeing that some of the marble steps on the stairways had been really smashed. Well, they were smashed during the Cultural Revolution by the Red Guards. They just sort of desecrated university buildings throughout the country. Just pathetic.

We visited the Precious Metals Institute, which is there in Kunming. At the time, I gathered—I asked a bit about it—that their gold mining consisted of just small mines, lots of small mines, no major, single gold mining operation. But they did research, I think, not only on the processing but also on uses—you know, more than gold. It's called Precious Metals Institute, so their work included platinum, et cetera. The Kunming Institute was quite a large research operation.

Swent: Did you lecture there?

Fuerstenau: Gave a seminar at the Kunming Institute of Technology, not at the Precious Metals Research Institute. At each place we went, we each gave a seminar. Yes, we were there maybe three days, and I'd see in the hotel at a nearby dining table a professor from Berkeley named Robert Scalapino, who's a major political science expert on Asia and China. He and his wife were obviously spending lots of professional time in China.

Swent: Were you lecturing to students or were these professionals?

Fuerstenau: They were probably more like grad students. Each of these were what I would call a typical seminar. I suppose we each gave one. I don't think we were all in the same room.

By the way, one of my last PhD students came from Kunming and had worked for several years at the Precious Metals Institute. This was Picheng Huang, now named David Huang. Just before coming to Berkeley, he also worked for a couple of years for Anna Marabini at the Mineral Processing Research Institute in Rome, which we had talked about earlier. It's from him that I learned that Kunming now has real air pollution problems. By the way, after completing his PhD at Berkeley, Picheng Huang worked several years for Hazen Research in Golden and directly with Wayne Hazen, before moving to Intel and now to Praxair.

So then we eventually flew to Xian. That's when they were still just in the early stages of opening the great tomb with all the terra cotta statues—you were there, right?

Swent: I've not been there.

Fuerstenau: Oh, no kidding! That's where the terracotta soldiers are.

Swent: I saw them here in San Francisco.

Fuerstenau: Yes, here there were a half dozen or so, but there, it's hundreds and hundreds of them. It's like a football field. They had uncovered about two-thirds of them at that time. You could still see they were digging up the area. Whenever I go somewhere, I always take pictures of everything. You know, it's funny: They wouldn't let you take pictures inside where the terracotta soldiers were being uncovered. Obviously, they'd had a deal of selling photos and slides.

We then also had toured around a bit, including the place where Chiang Kai-shek was hiding out in about 1936, and you could see a place where there's a bullet hole, which they still keep. Somebody shot at him as he was escaping out the back window of the villa in a kind of lovely resort area where he had been staying or hiding.

I remember giving a lecture there at a technical college in Xian, and then we flew to Beijing. But when we began to take off, the airplane started down the runway a little bit and then came to a stop. I'll tell you—maybe you know this: the distance between seats in Chinese airplanes was about three, four inches.

Swent: They're not built for you.

Fuerstenau: Yes, not for me or anybody. So we were sitting in the clear back. This was a Russian propjet plane, and the stewardess was sitting on a three-legged milk stool. Not buckled in or anything.

Swent: Oh my.

Fuerstenau: Anyway, the plane all of a sudden had a problem and stopped, and we walked back into the terminal. After a while they told us to go out and get our luggage. As we walked out to the plane, there were three Chinese technicians of some sort, or mechanics, sitting with the blueprints of the wiring diagram under the wing. I have a picture of that. When I took the picture, they looked up and smiled at me and laughed.

Swent: I'm surprised they let you take the picture.

Fuerstenau: Yes, yes. But this was simply a passenger plane and airport.

Swent: Trying to figure out the wiring.

Fuerstenau: Anyway, we were there many hours, and finally in the evening they sent a jet out from Beijing to pick up the passengers, and we flew into Beijing airport, and our flight was the last one in. And as we walked down the long hall—that was a fairly big terminal, as I remember—the lights were turned out behind us. And when we drove in to the city in some kind of van, the driver would speed up to about thirty miles an hour, then he'd put it in neutral, turn the engine off and coast, and when it got down to about fifteen or twenty, he'd start the engine and do this again and again.

Swent: To save fuel.

Fuerstenau: They were thinking it was a way to save fuel. The next day, we did the tourist thing around Beijing. We were only there, I think, two days. And went again to the Great Wall, and in the bus with us was the president of the Colorado School of Mines; he was not part of our group but on the same tour to the Wall.

Swent: What was his name?

Fuerstenau: McBride. Guy McBride. He was president when I was a member of the ABET team accrediting the Colorado School of Mines. For this trip, we had the same driver who had a big knot on his head—and he did the same drive and coast and drive and coast again all the way up to the Great Wall. When we were almost there, the bus overheated, and we had to walk the last block or two uphill. I remember the Colorado School of Mines president saying, “You know, I almost wish I had a hammer so I could have put another knot on his head.” Apparently he had this same driver on several earlier occasions. Just go a bit, coast, go a bit, coast.

Then I remember on that trip on the way back, we also visited the Ming Tombs, which were those ancient tombs. You know, the huge tomb was dug, buried with the emperor and treasures, and then all of the people that had worked on making the tombs were also buried inside, and the designer knew that he wouldn’t live, so he threw out in a field a stone or something that described where the tombs were, and that stone was not found until some hundreds of years later. It was this way they found where the Ming Tombs were located. That was how things were done in those early dynasties.

Then we went by train to the Northeast Institute, and Professor Qui, who had been my translator two years earlier, was professor there.

Swent: And where was this?

Fuerstenau: In Shengyang in Manchuria. By the way, on the train you could see partially blown over—probably every kilometer along the railroad tracks—a Japanese guard station built out of concrete, since that’s where the Japanese were in the thirties. You went by them on a regular basis, right along the railroad. They were obviously designed to protect the railroad during the Japanese occupation of Manchuria.

At the Northeast Institute we each gave some lectures, and that’s a very good university, technical university.

Swent: A lot of mining up there.

Fuerstenau: Oh yes. Right. And good people on the faculty. I don’t know if I had any students from there. I don’t think so although I had a lot of people wanting to come here from there. But anyway, I thought that the Northeast Institute was excellent. We had just a superb banquet-dinner cooked by the chef in the university, and it was probably the best food I ever had in China. I remember butterflied shrimp that were huge, and just delicious and obviously fresh because it was near the sea. But I’ll tell you, some of the food you get in China: ancient eggs and seaweed soup and sea slugs can turn you off, but they actually taste pretty good.

We flew directly back to Canton and then back out through Hong Kong again on that trip. But all along on that trip, you could see the beginning of change in color in clothing. Again—let me get back to one comment about Beijing—we were in the Forbidden City and at that time you could go directly into a lot of the buildings, right into smaller rooms that had furniture and statues. Today they're glassed off; you can just look through glass in the doors.

The Great Change in China: A Third Trip, 1998

Fuerstenau: I had many proposed visits, many invitations to go again to China, almost annually, but never did until three years ago when Guy Harris and I were invited to go. The main purpose was to visit the Huainan Institute of Technology in a coal-producing region, where my last Berkeley graduate student, Renhe Jia, had studied. The dean of engineering and now vice president, Zhang, was a close friend of his and Jia arranged for the invitation. So first we were in Beijing and then in Huainan and finally Shanghai. We gave lectures in the Beijing Mining Research Institute and at the Nonferrous Metals Research Institute in Beijing, and then a series of them in the Huainan Institute of Technology, both Guy Harris and myself. A year ago, they made me an honorary professor at Huainan Institute, and Guy also.

In Beijing, we were nicely taken care of by several people. One who looked after us was Professor Wang Dianzuo, who had succeeded Lo Lao as president of the Central South University of Technology and was honorary president of the Nonferrous Metals Research Institute in Beijing.

I want to talk a little bit about the changes in two decades. Today in Beijing you can see about a block or two, and then buildings disappear in the haze and smog. I told you how in '79 that one day we were there it was just like being up in the Sierras. If you're in the Forbidden City now, even looking across that courtyard starts to get hazy. It's that bad. In '79, there was virtually nothing but bicycles in Peking, and maybe some horses and a bus or two. Now it's just a parking lot of cars. And there's a freeway, virtually, out to the Great Wall, a nice highway. Out there, it's totally built up in contrast to 1979.

Swent: Souvenir vendors.

Fuerstenau: Souvenirs. I'll tell you! You know that. For about two blocks, souvenir heaven. And now, the Great Wall—we were there on a Sunday, a lovely day out there, out of the smog, and it was absolutely jammed with people and tourists, the tourists being local Chinese. I remember just lots of nice-looking Chinese girls in high heels and beautiful dresses, whereas in '79 the only people there were soldiers, who were there as tourists, not to guard the wall. You could hardly move on this Sunday afternoon on the Great Wall.

Swent: A big change.

Fuerstenau: We saw a few American tourists, but not many. It was mainly local. Just a complete change in fifteen years. And Beijing is, you know, modern skyscrapers, and Shanghai, lots of them, and you can see more of them being built in any direction you look. Smog and pollution is really a pathetic thing that they're going to have to solve, if they ever can. Because, you know, in China only about 15 percent of the land is habitable; 85 percent is mountains and desert. So the billion people must live in this limited area. And low-grade coal provides most of the energy. But we were, again, super well treated. I told Renhe Jia at some earlier point that I had not had Peking duck on other trips, and on this last trip we were taken to the number-one, first, fancy Peking duck restaurant in Beijing, and we had the true Peking duck.

My only comment is, what a change! The cities are like a parking lot. You can almost make better time walking than driving. Another change taking place in 1998 was that all of the universities, research institutes, et cetera, had just initiated a program to get out of the housing business and sell the staff apartments directly to their employees. Before that, every university, institute, factory, and mine provided all housing for everyone working for them. We were told this in Beijing and in Huainan. In Huainan, Guy Harris and I visited a coal-cleaning plant and they were also selling apartments back to the workers.

This trip also had a little tourist payoff, in that we went up and spent one night on Yellow Mountain, Huang Shan, where the clouds drift in like a sea and small peaks look like islands. Beautiful. I've been asked many times to go back to China, and I don't know whether I'll ever get back again, but we'll see.

An Early Visit to Korea, 1979

Fuerstenau: [Added by Douglas Fuerstenau during editing] A significant visit that I had not commented on but would like to include because I consider it to have important historical aspects, namely South Korea. In the spring of 1979, I was invited by the Korean Institute of Science and Technology (KIST) under a program of bringing visiting scientists to Korea for a week. I remember that Milt Wadsworth and my colleague Gareth Thomas had also been there under this program. During that trip, I gave a seminar at Seoul National University and discussed curricula in metallurgy. I also visited the Korean Advanced Institute for Science and KIST for research discussions. Later with Dr. Lee, head of the extractive metallurgy group at KIST, we drove down to Pusan, where I had a thorough tour of the huge operation of Pohang Steel Company. This modern, new plant utilized direct casting instead of the old ingot method of rolling steel. Pollution control was such that the streets inside the steel mill were lined with healthy pine trees. Another day we visited the site where a huge copper smelter based on the Outokumpu process was being constructed. Interestingly, South Korea has no copper ore, but concentrates were to be

imported from Chile. Korea is basically a country made up mostly of low mountains with fairly narrow valleys where people live and work—very little flat land.

In Seoul, I met Dr. Hyung-sup Choi, whom I had briefly met years earlier when he was working on a PhD at the University of Minnesota. Choi told me that he had gone to Notre Dame to study physical metallurgy, and then decided processing would be more important for Korea, and so after getting his MS degree, he transferred to Minnesota to study steelmaking. It turned out that he was awarded research assistant support on iron ore flotation research and moved in that direction. After returning to Korea, he taught a bit and became involved in research leadership. He was an early teacher of Ken Han, and two other Korean students of mine, Jong Minh Wie and Kwang Soon Moon. After becoming head of the Atomic Energy Research Institute, he was asked by South Korean President Park Chung Hee to mastermind a plan to develop the country's science and technology base and transform the still agrarian South Korea into a high-tech export nation. He established KIST and several other technical organizations and universities to do this. From 1971 to 1978 he was the minister of science and technology under President Park. Dr. Choi told me that when he set about to establish Pohang Steel, he brought over retired Japanese classmates who had worked in the Japanese steel industry to do the job. Choi had graduated from Tohoku University in Japan during World War II. Throughout Asia, he became a prominent adviser on science and technology policy. Those of us in the world of flotation chemistry know of the many papers on iron oxide mineral flotation published in the *AIME Transactions* by Iwasaki, Cooke, and Choi. For his PhD thesis, Choi's research was exactly along the lines of what I had initiated at MIT with Modi. In several review papers, I used to use Choi's flotation curves because they were so good. Over the years, I made three or four later trips to Korea and always had discussions and lunch with Dr. Choi. Each time, Dr. Choi would say, "All I did was copy your ideas." A couple of these trips were on behalf of my former student Dr. Moon, who had returned to Korea to establish a private research institute, the Korea Interfacial Science and Engineering Institute, KISEI. For several years I served on KISEI's advisory board.

I still feel that it is most unfortunate that Dr. Choi was never made a foreign associate of NAE—and this was due to ineptness by certain senior people here in the nomination process. He clearly deserved that recognition, in my opinion, since very few had ever done much more in bringing technology to his country than did Dr. Choi. In 1996, after retiring from the University of Minnesota and returning to Japan to work for Mitsubishi Materials Company, Dr. Iwao Iwasaki was elected a foreign associate of NAE. Iwasaki has since come back to Minnesota at the University branch in Coleraine.]

Cooperative AID Program with Egypt

Fuerstenau: I'd like to also tell you about an AID program in Egypt that I was involved with. This started in 1978 as a joint program between the American Chemical Society and the National Research Center of Egypt.

[Tape 37, Side A]

Fuerstenau: This was a joint program to discuss research problems, okay? There were four or five different groups of three delegates each, as I recall—one of the groups was on extractive metallurgy, mineral processing, and that included Milt Wadsworth, Roshan Bhappu, and myself. There was another group on pharmaceuticals, also with three people involved with pharmaceuticals. There were three people on corrosion, and a group of three people related to weaving cloth and fibers, and a final one of three people on disease. You know, when they dammed up the Nile, the little tributaries became stagnant and these parasites [schistosoma] thrived. People get infected when they wash their clothes or bathe in these stagnant streams. This problem did not exist in the days when the Nile flooded and washed out all the side streams each year.

Initially there was about a week of meetings. Obviously, each group had their counterparts at the National Research Center or National Research Council in Cairo. Our team of Wadsworth, Roshan Bapphu, and myself discussed some of the problems that faced Egyptian process metallurgy with our Egyptian counterparts.

Their iron ore, for example, had been a porous material and the pores in it were filled with salt, sodium chloride. You think you put salt in water and you easily dissolve it, but try to wash out dissolved salt in pores, as a process. It just doesn't go. So there was a lot of corrosion in their iron-making, steel plants because of all the chlorine that was there—probably at high temperatures the sodium chloride evaporated in the blast furnace. That, I remember, was one of the problems. That wasn't the one I dealt with. My discussions related to the processing of phosphate ore for fertilizer production, and how to upgrade their minerals, et cetera. And I'll be back to that in a few minutes.

After the meetings in Cairo, there were field excursions to different places for these different groups to look at facilities. Bhappu and Wadsworth and I went to Nag Hammadi, which is where the big aluminum plant is, one of the world's greatest aluminum plants. It was built because of the power produced by the Aswan Dam. The plant was financed and built by the Russians, and so you can see all this Russian equipment with Russian control boxes and controllers, all looking big and rather massive and crude—you know, at the time.

When I made that trip out to Samarkand, the Intourist guide had said she had spent five years in Egypt. Obviously, she was down there when the Russians were building that plant. What was interesting was: the alumina came from Australia. They don't mine any bauxite in Egypt. We were told that something like half of the aluminum goes back to Australia to pay for the alumina, and a quarter of it went to Russia to pay for the plant, and then a quarter of it for Egyptians to use and to sell. Something like that was the economics of this huge operation.

It's only about one hour by car from Nag Hammadi to Luxor, so we went to Luxor and visited the Luxor Temple and then crossed the river and to visit the Valley of the Kings. This was '78, so it was very interesting to visit—particularly King Tut's tomb but also several others. Some are very massive and elaborate—you've seen pictures in *National Geographic*. One of the most impressive ones, with many paintings of Egyptian gods on the walls, was that of a mayor, a lot lesser person than a king. The walls in King Tut's tomb were bare. I have read that today they don't let people into these tombs very often anymore because of the humidity caused by the tourists inside. So we really had this lovely tour, and after we got back from the Valley of the Kings, we were sitting out late in the afternoon eating lunch when somebody started to talk about Karnak Temple. It wasn't on our agenda, but so a few of us said, well, we must go see this. Karnak is the one with the huge, massive columns that you have probably seen pictures of. We rented a taxi and went there, approaching sunset. That's the place to see, not Luxor Temple.

Swent: One of them was moved, wasn't it?

Fuerstenau: No, those were all in the Aswan Dam area. Those were moved because of the high dam that would eventually flood them. I never got down to Aswan. However, at the front of Luxor Temple on one side is a big obelisk, and on the other side, the obelisk is gone, and that's the one that's in Paris in Place de la Concorde, that Napoleon took.

But there is a little museum in Luxor that's got hardly anything in it related to King Tut, and you go back to Cairo and—have you been in Cairo?

Swent: I have not.

Fuerstenau: That museum in Cairo in '78 was just like a warehouse. It had everything you can imagine, but it just kind of looked like a warehouse, including this great room full of King Tut stuff. There's four of about everything in the King Tut part of the Cairo Museum. They ought to put some of it down there at Luxor.

But anyway, when I was there, there would be a little card saying this item is in transport to the U.S., and that item, and so on. I remember there was a King Tut display that we went to see in LA, and we also went to see it here at the de

Young Museum. It was beautifully done, and they used the money for upgrading the Cairo museum.

We also had an excursion that went to a lot of different pyramids. You know, the oldest pyramids are at Sakhara south of Cairo. They're kind of big, step pyramids. Our guide led us down with a candle, underneath. As I say, this was '78 so it was before they really built up for tourism. Of course, what really amazes one is how—the Nile Valley is only a mile, two miles wide, where it's green and—sometimes it widens out a little bit—and then on either side it's just sand. The kind of pictures we see of the three great pyramids, you think they're way out in the desert, but they're right on the edge of where the green stops, just on the edge of the Nile Valley.

I remember flying in there, looking out the airplane, and seeing all this light brown area, with what looked just like a shoelace or a string, black string. I was wondering what that was. Of course, that's as we were coming in, high. It was the Nile Valley. And all those people live in just that little strip of land. Of course, it fans out into the delta. But I think there were, in 1978, thirty-eight million people and something like seventy-eight million are there now. They've got a terrible population control problem. Obviously a poor land overall.

Anyway, out of that came an AID joint project that I had with Cairo University researchers, so I then made at least three other trips to Egypt. AID is U.S. money that comes from payback of loans, but they don't pay it back. It is used for internal projects that may require American equipment, assistance, et cetera. I was involved with several professors, one of whom was my own former student, Abouzeid, Abdel Abouzeid, a professor at Cairo University. Another was, Desuki El Gillani, who did his PhD with my brother, Maurie, at Colorado School of Mines. Very bright, able soul. The project involved working on processing a low-grade phosphate ore that Egypt had in large quantity.

- Swent: What do they mine in Egypt?
- Fuerstenau: Well, iron ore and phosphate.
- Swent: That surprises me.
- Fuerstenau: There may be—
- Swent: Stone quarrying, of course!
- Fuerstenau: Right. I'll find out if there are others.
- Swent: There were ancient mines.

Fuerstenau: Oh, they had ancient gold mines. They had underground gold mines, which must have been terrible, mined by slaves, you know, 2000 BC. In the Sinai area there's some mining. Historically, copper was mined there.

But anyway, the phosphate that we were concerned with was formed when Egypt was covered by the sea. The apatite came from fish bones that were hollow and pyrite filled the hollow fish bones. This meant that the pyrite is about five microns in diameter, which means you can't grind it and separate it out. And when they try to make fertilizer or phosphoric acid out of it, it's black, and the farmers want to buy beige-colored fertilizer, like that which comes from Morocco and Florida. So our efforts were to try to remove the pyrite, which is iron sulfide precipitated inside the pores of the fish bones. This potential resource of phosphate is huge and is located out in the desert at a place called Abu Tatur. It occurs in a huge mesa that has a billion tons or more of phosphate in it, which is why the Egyptians were working on it. But this mesa apparently is about five hundred feet or more high, and it's flat on top, and then about one hundred feet down from the top is the layer of phosphate about fifty feet thick, and there's a billion tons of that phosphate.

They had tried to mine this as an experimental mine—I wasn't connected with it, but during that time, it caved in and killed about thirty people or so. You can see what a tough problem that would be to develop a mining method for that kind of deposit.

Abouzeid told me they had actually built a railroad about fifteen years ago all the way to Abu Tatur, and now they've just given up because they're not able to sell this product. The Egyptians built a railroad from the seacoast to this mesa, a several-hundred-mile-long railroad, clearly without full planning. There is some sort of lesson, I think, to be learned there. But they had international support for it.

Swent: Were you working with producers or with students or—?

Fuerstenau: I worked with professors who were the principal investigators on the project. I assume these professors had some grad students to do some of the work. We were actually trying to determine how to make some of the separations. They do have some good phosphate but that ore is quite high in dolomite, which is a calcium magnesium carbonate mixed with the apatite. And so we were also involved on studying flotation behavior of dolomite and apatite as part of this program. That was really quite fundamental, and it's also something I'd been doing here in Berkeley. Some success was achieved by working at low pHs where there is a surface transformation between dolomite and apatite. That was really an interesting program based on detailed solubility calculations that El Gillani had carried out. That related closely to some of our own research here in Berkeley. I was there two additional times.

I'll come back to the first time. It was '78 because Abdel Abouzeid, who was my student, always to this day sends me a birthday card. He and his wife, Gamila, had me and Wadsworth to dinner, which was a full-fledged birthday party for me. It was my fiftieth birthday. That same evening other people went out and ate at various places, including the wife of a vice president of a pharmaceutical company. They drank some crushed sugar cane juice and that put some of them in bed for a couple of days with dysentery. I wouldn't drink sugar cane juice anyway, but some people did. So I missed going with the group where a few people got sick because of Abouzeid's having this wonderful birthday party for me at his house.

So anyway, this kind of program was interesting, along with the technical discussions.

Swent: Did you think that the AID program was worthwhile?

Fuerstenau: Oh yes, I think so. Particularly the results of research on separating dolomite and apatite by flotation, which of course is also a problem for the Florida phosphate industry.

And then another time, Peg went along with me, and we again made the same trip to Nag Hammadi to visit the aluminum plant and to visit Luxor. This was enough years later that you could see the change in the Cairo Museum.

I was impressed with the general manager of this aluminum plant. He had ideas—such as trying to grow trees and plants in the desert. Apparently there's a tremendous flow of fresh water under the sand. The plant was up on the sand away from the River, but you could pump out fresh water from wells. He was doing that to try to create agricultural land around where the aluminum plant was. I could tell this manager was first rank. Then I learned, about a year later—he had a farm on the edge the Nile Delta—west of Alexandria. He went up to take a look at his farm, and there were a lot of squatters on there, and he wanted to get the squatters off of his land, and they shot him, and about thirty days later, in the hospital, he died from an infection. He was fifty-ish in age, but you could tell he was a good leader. Probably a little aggressive, which led to his demise.

Anyway, these international cooperations are quite useful, in my opinion.

An NSF United States/Italy Cooperative Research Program

Swent: The one in Italy was not AID, was it?

Fuerstenau: No, no, Italy was one of these NSF international cooperative workshops. I believe that holding a U.S.-Italy cooperative workshop was suggested to me by the NSF international program director. I prepared a joint proposal that was

submitted to NSF with me as the American chair and Giorgio Rinelli, whom I mentioned earlier, the Italian chair. I think we had about twelve or fifteen American participants—no more than that—and then maybe an equal number from Italy. First we split our group into a couple of subgroups to go visit a few institutes, and so on, in Italy. I was in Rome. Maurie, my brother, was there too. Some went to Sardinia to visit facilities in Cagliari and another group to Turin. Then our workshop was held in a small hotel at Lago Maggiore, near Milan. I remember looking out from our room right across to a little island. There was only forty feet of water, maybe, between the island and the shore, but right there was Arturo Toscanini's house. We've all probably seen pictures of his Italian summer place.

We met for a week to work out potential cooperative programs, and then proposals were eventually written, all related to mineral processing. I had a cooperative program on flotation with the CNR group, of which Rinelli was the director. And two of my grad students on different occasions spent four to six months in Italy at that institute, conducting research. This would be related to flotation chemicals, and one, Jim Hansen, actually used the research he did there for his MS thesis here at Berkeley. And another one, Jim Gebhardt, who afterwards worked for the Bureau of Mines and now has got part of a small company in Salt Lake—was in Rome there for four or five months on that. When Gebhardt worked for the Bureau of Mines, he spent a half year or year again in Rome on the equivalent of a sabbatical. NSF funding was used to pay for the Americans' expenses during their time spent in Italy. And then the Italian researcher from that institute, Dr. Maria Barbaro—came here for a few months and her expenses were then paid by the Italian component.

Swent: What do you see is the advantage of this?

Fuerstenau: Exchange really, exchange of ideas. Certainly growth for the students, to be able to go live in another country for a few months.

Swent: Is the quality of work comparable?

Fuerstenau: Yes. Yes, it would be because the person who became the director of this CNR Mineral Research Institute in Rome, Professor Anna Marrabini, was really a leading person in developing new flotation collectors and other reagents. She's now retired—but is recognized as a leading person in developing new reagents, flotation reagents. In fact, she's a foreign associate of the U.S. National Academy of Engineering. She had good ideas, quite related to things I had worked on, so it was, I'd say, to mutual benefit exchanging ideas. When she became director of that institute, she really moved it up a great notch in getting new equipment, better equipment, and that sort of thing. So I looked at it as a good thing for the grad students who went there. In 1995, I had a CNR fellowship under which Peg and I spent a month in Rome, giving a series of lectures and participating in discussions. Other commitments held our stay down to one month, unfortunately.

U.S.-India Workshops

Fuerstenau: I also participated in two such workshops in India. The very first one was in '79. That was supported by AID money also. That workshop had to do with resources and environment. It was concerned with more than minerals. Delegates were there to talk about food, energy, climate, environment, and so on. We were in Bombay, and I remember Somasundaran was the American leader, and Professor Malikarginan was the Indian leader. We met in Bombay at a large hotel near the airport for the general presentations and discussions and then we moved to the Indian Institute of Technology in Bombay for more detailed discussions in smaller groups. All our expenses were paid by AID money, and Soma had to go with a briefcase to a bank to fill his briefcase with Indian rupees. He said that coming back to the hotel, sitting in a taxi, he was totally scared; only he knew that his briefcase was completely full of Indian money, to pay all of the American delegates' internal expenses.

That workshop involved a complete variety of people since it also had to do with climate and agriculture. There I met Professor Ronald Scott from Caltech, a soil mechanics person, and if you remember about 1975 we saw pictures of Mars where a scoop shoveled Martian soil for the first time—well, he made the scoop.

Swent: Oh really?

Fuerstenau: And designed it all; it was his project with the Jet Propulsion Lab at Caltech. I didn't even know him then, but we did get well acquainted during the meeting. The final output of that workshop was a two-volume proceedings with the presentations and discussion.

Afterwards I traveled for a week with Tom Meloy, also with Ron Scott for a couple of days. We went to Aurangabad to see the caves at Ajanta and Allora. In a deep valley are about two dozen large caves carved into the valley wall. Inside each cave is a large Buddha, carved out of the rock in place inside the caves. Incredible. Tom and I went on to Udaipur, Jaipur, and Agra to the Taj Mahal before returning to Delhi for the trip home.

Then, a few years later, John Herbst and Jan Miller from University of Utah organized an NSF international cooperative U.S.-India workshop that was all related to minerals and metallurgical processing. I had made the suggestion that we meet in Udaipur, and at first the Indians wanted to meet in Delhi so that they could have all sorts of people participate. It took Herbst a lot of effort to get them to reduce their numbers since this is supposed to be an exchange workshop. You know, if you have a hundred in one group and only twenty, say, in the other, you can't have real exchange discussion. So we got it down to a reasonable number of Indian participants. Also John Herbst

prevailed in holding the meeting in Udaipur, which is a beautiful city. I had been there and suggested that as the venue. We organized a wonderful banquet at the Lake Palace Hotel on an island there.

After meeting in Delhi and going to Agra, our delegation of about fifteen or twenty American participants was split into three groups to make different site visits before going to Udaipur. My group visited the large Regional Research Laboratory at Bhubaneswar, the Department of Atomic Energy at Hyderabad, where I gave a seminar to an audience of about two hundred people, and to the chemical engineering labs at the Indian Institute of Technology at Madras, The Germans had built IIT Madras, and the chemical engineering laboratories looked exactly like those of Professor Rumpf in Karlsruhe, since he had led the design of those lab facilities years earlier.

Hindustan Zinc Company has a huge zinc plant and mine nearby Udaipur. One night at a dinner I sat next to the president of Hindustan Zinc and he told me that if they did not lose electric power for about three hours every day, they would have been very profitable that year. Electricity demand in India exceeds production such that power is shut off for something like three hours daily. They have diesel generators to continue power underground, but the electrolytic zinc plant shuts down for those daily periods.

The objective of the workshop was to work out specific exchange research projects in mineral processing, hydrometallurgy, and pyrometallurgy—smelting. That was a very good meeting, where we met and planned different projects. You know, the U.S. part all got approved fairly early, and then about three years later, the Indian Scientific Research Council was still thinking and waffling and couldn't come to decisions.

On the short run, that sort of thing died, or just never got going. But eventually there have been a number of U.S.-India exchanges. But the final outcome was that Indian professors in the U.S. set up exchange programs with Indian professors in India. That makes a nice way to make a trip home. That's how I read it.

[Tape 37, Side B]

Swent: The Indian students used this as a means—

Fuerstenau: That's how I read it. Of course, we're now talking about good people professionally. But it doesn't give quite the benefit you would get in both directions if the participants represented a broader scale.

I did make one additional trip to India. That was for an international symposium in honor of the retirement of Prakash Kapur as professor of metallurgical engineering at IIT Kanpur in 1995. In India, retirement is mandatory at age sixty, even for teachers and researchers of renown. Kapur

told me that the director of IIT Kanpur even approached the minister of education about delaying his retirement. After that, we went to Pune where I gave a plenary lecture at an excellent Engineering Foundation Conference organized by my former student Pradip and Brij Moudgil from the University of Florida. For two weeks prior to the Kapur symposium Peg and I toured northern India.

You know, there are different kinds of exchange programs between various countries. For example there used to be what I called “iron curtain exchanges,” because I had several people that came from Eastern Europe under the IREX program. I don’t know what those letters stand for, but under this program senior researchers would come and spend various time periods here, ranging from a year down to a few months or even one week or so traveling to a number of places. Some were doing research but others were lecturing. Persons that I recall who came to Berkeley under the IREX program were Vladimir Nebera from Russia, Frantisek Spaldon from Czechoslovakia, George Georgescu from Romania, and Heinrich Schubert from East Germany. I never had an exchange program that took me into Eastern Europe, but I know Somasundaran had one for quite a while with people in Bulgaria, and Bulgarians would come to Columbia, and he’d go to Sofia. These exchange programs are useful in my opinion. There have been quite a few in the mineral area in past years.

Flotation Course at the University of San Luís Potosí, Mexico, 1985

- Fuerstenau: In 1985, my brother and I gave a series of lectures on flotation in Mexico. My graduate student, Alejandro Valdivieso-López who had done his MS with Maurie in South Dakota, organized a course on the principles of flotation at the University of San Luís Potosí. This was a three-day course of lectures given by Maurie and me in English, plus a couple of lectures by Alejandro in Spanish. A fairly large attendance. That was my first real trip into Mexico. There is a lot of mining and smelting activity around San Luís Potosí. Is San Luis Mining Company where you and Langan were nearby?
- Swent: Oh no. San Luís Potosí is near Mexico City. The San Luis mine was about one hundred miles south of Copper Canyon. That mine was in the bottom of a canyon with mountains on both sides, and flying in and out was adventurous.
- Fuerstenau: Just a short while ago, Peg and I went on an Oakland Museum excursion to Baja and Copper Canyon. Copper Canyon is really quite spectacular and different from around San Luís Potosí. Anyway, there was an audience of a hundred or more—maybe two hundred people—not students but engineers from the area. Afterwards, Alejandro drove us via Guanajuato, the old gold and silver mining city, on the way back to Mexico City. He gave me a great tour of Mexico City and the pyramids north of the city. I remember staying totally healthy on that trip, but Alejandro thought he was still a Mexican and

must not have paid attention to what he ate or drank. As I was leaving, he asked me for all of my various pills et cetera because he had gotten Mexican visitors' problems. After getting his PhD degree from Cal, Alejandro returned to Mexico as a professor in the University of San Luís Potosí, and has established the first PhD program in mineral processing in Mexico. He was recently given the Mexican national award in metallurgy. Excellent!

Swent: Wonderful.

International Surface Chemical Meetings

Fuerstenau: I also want to briefly comment on some other types of international activities. You know, I also am in the field of surface chemistry and I've always done a lot of very fundamental work, so I've been invited many times to straight surface chemical meetings, by chemists, not minerals people. And two or three times I was invited to participate and lecture at what's called Summer School for Surface Chemistry, in Yugoslavia. They always were for two weeks with a relaxing weekend in between. The first one may have been 1970-ish, and another one maybe eight years ago or so. These are great—two weeks right on the Adriatic. The two that I went to were in Rovinj, which is just a few miles south of Trieste at the Italian border—you know, beautiful coast, clear water. Lectures in the morning and then the afternoons were free. The Yugoslavs did it right. The evening session was, like, five to seven, so then you didn't eat till afterwards and that made it nice. We were there at least two different times, and there might have been two, three other Americans there each time. Major European surface colloid chemists were regular participants. It was organized by the Yugoslav group of surface chemists, which used to be very good, in Zagreb, initially by Professor Bozo Tezak. Several professional friends grew out of those. I remember one afternoon we went to a city, oh, maybe a half hour or more away by car, called Pula, which had a beautiful Roman coliseum. You know, there's a lot of Roman stuff that's in good shape along there, that you can visit, in contrast to Italy, where a lot of it got smashed.

I told you I had been invited to the IUPAC congress in '69 in Australia. I've been invited two different times to what are called Discussions of the Faraday Society in England: one of them on oxide/water surface chemistry; and another one on surfactant adsorption. Everybody there were basically physical chemists, other than myself, except that I was doing that sort of work. In addition, a couple of times I have participated in the International Surface and Interface Science Symposium, held every four years, and one of the last ones was in Compiegne in France, '91. That was a big international meeting, again mostly surface chemists, that included Tom Healy.

Swent: You do choose lovely places.

Fuerstenau: Oh hey, yes. They're not bad. You're right. Yes, Compiègne is where the replica railroad car is, where the World War I treaty was signed and where Hitler made the French surrender to him. Then they took the car to Germany, and I guess it got destroyed, so what's there now is a replica car; right there in the forest, right on the edge of Compiègne.

You know, another area that I've dealt a lot with is particle science and technology, so I've been to many of the international particle technology meetings, mostly in Germany, some in the U.S. In Nuremberg there have been several particle conferences—also in Amsterdam. That's a different group of people, and that comes out of the research that we've done on pelletizing or on particle transport, mixing of solids, comminution, things like that. These are not mineral processing people but are scientists and engineers working on characterizing and processing solid particles. So many things are handled in particulate form. Dr. Reg Davies from DuPont said that something like 85 percent of all the materials they deal with at DuPont, in some stage, are particulates. So that's why the engineering of particles is so important. Many pharmaceuticals are particulates that have been tableted. A former student works for one of the pharmaceutical companies and he has been doing research on pelletizing some of their products. One area where they used to do a lot of research work is grinding of flour. You know how fine cake flour is. Well, cake flour came about as a result of a lot of research on how do you grind wheat to a fine size. Cement, of course, is one of the world's major particulate materials, and it is ground twice in production. Similarly, chocolate is ground, and the finer it is the better it is. So there are international meetings on particle science and engineering—how do you measure the particles, how do you measure size, shape, size distribution. Of course, we do that in the mineral field, but generally at a somewhat coarser size with sieves. Today, automatic particle size measurement is needed for process control. In some cases, you've got to make accurate particle size measurements in microns, and that gets very tough. Through the years, I have participated in a lot of these meetings. The newest hot field involving particulates is that of nanomaterials, which brings producing and characterizing particles far below the micron scale.

Europeans have regularly held the European Comminution Symposiums, starting with the first in 1961. I've had a paper in every one of them, starting with the second, which was held in Amsterdam in 1966. Professor [Hans] Rumpf from Karlsruhe and Klaus Schoenert were the two leaders of that symposium. I still recall Schoenert from Germany and Tanaka from Japan having an animated discussion in English up at the podium. I just got a notice accepting a paper I had proposed for the Tenth European Comminution Symposium, which will be held in Heidelberg in September 2002.

Swent: How nice.

Fuerstenau: I plan on going to that one. The notification just came days ago. So I know quite well the people who have dealt with crushing and grinding, particularly in Europe, but participants also come from other parts of the world, such as Japan, Australia, and India. So here at this stage in life I still want to continue to stay active. I can make a long, long list of the world's best-known people in all of these fields that I know personally, some of them really as close friends, not just professional friends. And that has been a great thing.

[tape interruption]

Swent: You were speaking of the satisfactions of this worldwide network.

Fuerstenau: Yes. I have gone to enough of those meetings of different types, outside of the mineral processing meetings that I always take for granted that I might well accept an invitation to participate when approached. Coming back to the surface chemical meetings, I really got to know well and personally many of the world's giants, let's say, in surface colloid chemistry, like Overbeek we've talked about before. I've had considerable contact with Theo Overbeek on numerous occasions since my last year of being a grad student at MIT. Ron Ottewill at Bristol is the major colloid chemist in England. Joe Kitchener was considered the leading surface chemist in England, one who has contributed extensively to flotation also since he was in the Mineral Technology Department at Imperial College. The previous generation of surface chemists in this country that I know or knew well included Egon Matijevic at Clarkson University, Al Zetlemoyer at Lehigh, and Art Adamson at USC. Adamson wrote the surface chemistry textbook that I used in my course on surface properties of materials. Several of my research results have been included in his text. I became well acquainted professionally and personally with a number of the leading European engineers in particle science and technology, including Hans Rumpf, Klaus Schoenert, Kurt Leschonski, Otto Molerus, Horace Rose, to name a few. The number of friends in the mineral world is of course very large.

Another thing of great importance is whenever we've had visitors, I often invited them to our house for dinner. When I retired, our children made some kind of poetic presentation. One of the verses was what they learned—as part of their upbringing—was having those many foreign visitors to our house for dinner. I've heard each of them say what that meant to them, particularly our son, Steve, who is very good at talking with people. I think he acquired this due to his interest when he was still in high school. Obviously, it has meant a lot to our children, and they recognized it. So that's just another plus of the kind of life that this has been.

Two different families who became really close family friends are the Healys and Schoenerts. Healys back to 1960 and Schoenerts to 1969. Annalene died a year ago, but we've done so many things together in years past. In fact, with

the Schoenerts we've exchanged children. Ours went to Germany, and their son came here. Great.

Swent: That's wonderful.

Fuerstenau: It's been a good thing.

Sixtieth Birthday International Symposium, Berkeley, 1988

[Added by Douglas Fuerstenau during editing: I don't know how I could have forgotten about the most important international symposium that I participated in, namely the SME International Symposium held in Berkeley in recognition of my sixtieth birthday in 1988. Former students, mineral processing researchers, and professional friends from around the world presented papers at this three-day symposium. Papers from the symposium were published in a book entitled, *Challenges in Mineral Processing*, edited by K. V. S. Sastry and M. C. Fuerstenau, comprised of forty-four chapters and 767 pages. Many of these were absolutely excellent papers, but unfortunately SME did not print enough copies of the book to have it available for a longer period of time. However, if you want a copy, you can now order a special copy on the Internet for something like \$275.

A second aspect of this occasion was that at the banquet, SME President Roshan Bhappu announced that I had been elected an honorary member of AIME. There can only be fifty living honorary members, and I was the 195th person elected since 1872. Out of interest, some names that you might know are Joseph LeConte (1895), Andrew Carnegie (1906), Rossiter Raymond and Robert H. Richards (1911), Herbert Hoover (1917), T.A. Rickard (1934), Henry Krumb (1939), Donald McLaughlin (1957), Antoine M. Gaudin (1971), Frank W. McQuiston (1977), Simon Strauss (1980), John F. Havard (1984), Plato Malozemoff (1985), just to name a few of the first 195 honorary members. More recently, my friend Frank Aplan was also elected an honorary member of AIME.]

Some Additional Comments on Organizational Structure in China

[Interview 16: October 26, 2001]

[Tape 38, Side A]

Swent: You had said that there were a few things more that you wanted to say about the organization in China, so let's just pick up with that today.

Fuerstenau: The last time I mentioned that in the early eighties we had a number of delegations come through here, and in those days there were a lot of so-called visiting scholars. One of the visiting scholars in the mineral processing area named Liu spent, oh, maybe six months here, working not with me but with Professor Sastry. Some months later—either he had gone back to China or it's while he was here—the minister of building materials of China came through, and he wanted me to meet him. Most of the trip of that delegation was in Canada, I believe in relation to purchasing asbestos—as you know, Canada, Quebec has big asbestos mines. Perhaps visiting Berkeley was on the last leg of their trip back. We had a very nice dinner at a Chinese restaurant in San Francisco.

I remember asking the minister, probably through an interpreter, how many employees he had that worked for him, and he said, "Ten million." [laughter] He had ten million that were in the Building Ministry, of which he was responsible for all.

Swent: And this was just an agency.

Fuerstenau: Yes, there's a Coal Ministry, Nonferrous Metals Ministry—on our first trip, we had dinner in Beijing—in those days, Peking—with the minister of nonferrous metallurgy in '79. But anyway, we learned a little bit more about the organization of industry on this occasion. In China, everything was—then and until really quite recently—organized vertically. There were universities for building materials. He had under him two or three medical schools for doctors for workers in the building materials industry, et cetera. The entire school system was organized vertically with respect to building materials workers. They were also responsible for all the housing for all the employees. This was the structure of all these ministries.

Swent: So it wasn't just procurement.

Fuerstenau: No. Their main object, of course, was to produce cement and get rock, and whatever else would be involved, but the ministry was involved with all aspects of the life of workers and their families.

And by the way, I remember also in our discussions in '79—many people—let's say, a husband and wife, and in some cases I know they were both engineers—were assigned to go to work, maybe a thousand miles apart, and the only two weeks' vacation per year would they be able to get together, and that was very typical of people we talked to. Really kind of a sad state of existence, wasn't it?

Swent: Dehumanizing.

Fuerstenau: Right, right. Our last trip, which was about three years ago—that was paid for by the Coal Ministry—we learned that the Coal Ministry, again was organized the same way. There were universities for teaching coal mining, coal cleaning, et cetera, and at Huainan Institute was a medical school, and again, that medical school was to train doctors for dealing with patients working in the coal mine industry. But about three years ago, this situation of ministries, universities and research institutes owning the housing for everyone—faculty, staff, all employees changed. They were starting a program of selling all apartments off to the individuals. That was just beginning—in other words, of a national program to divest that activity.

On the last trip in Beijing, I did give a lecture at the university related to coal mining, but we also spent a day at the Beijing Research Institute for Mining and Metallurgy. I was very impressed with those people, and that's where I first heard that they were just beginning the act of divesting their housing to the employees. Now, how that would work in the long run, I have no idea because housing obviously cost the employees just a few yuan, a few dollars a month, because everything had been subsidized.

In Beijing we also spent time in more of a consulting discussion time with a large group at the General Research Institute for Nonferrous Metallurgy—which the director there had said, was the largest or second largest research institute in China. The previous president of this institute was Wang Dianzuo, who I already knew as an outstanding mineral processing researcher. He had done a lot of very good work on flotation, so I knew of him technically and had met him in Changsha and in the U.S. He had been president of the Central South Institute of Mining and Metallurgy, which is now Central South University of Technology at Changsha. Through our efforts, he was elected a foreign associate of the National Academy of Engineering. When that happened, obviously his stature in China mushroomed, and he became the director of this huge nonferrous metals institute. When we were there three years ago, he was honorary president. In other words, he had retired. With him we drove in two big black Audi limousines—up to the Great Wall. We had a wonderful dinner with him.

Swent: You said that you were very impressed with him. In what way? What impressed you about them, the institute?

Fuerstenau: He had a very solid knowledge technically in the field, and obviously he was a good director, good leader, a fairly mild and quiet sort of individual. His successor, who hosted a dinner in what had been an informal part of the Imperial Palace, was more outgoing. He had obtained his PhD in physics from University of Bristol in England, and had come back to China. I could see that those years that he would have spent in England gave him a more outward approach to management and dealing with people. I gathered that he is leading the institute more in the direction of research on advanced materials. And now, as of about two years ago, Wang is now the vice president of the Chinese Academy of Engineering. One can see the good that came to him out of his election as a foreign associate of the U.S. National Academy of Engineering.

Swent: Very prestigious.

Fuerstenau: Oh yes. That gave him absolutely outstanding prestige in China. I assume there are probably a couple of other foreign members that are Chinese, but it's a good illustration of how that works.

[Added by Douglas Fuerstenau during editing: There are several mineral processing engineers who are foreign associates of NAE: Klaus Schoenert of Germany, Anna Marabini of Italy, Dianzuo Wang of China, Eric Forssberg of Sweden, Robin Batterham of Australia, and Iwao Iwasaki when he returned to Japan after retiring from the University of Minnesota. There are no mining engineers who are foreign associates of NAE. Most recently, it pleases me very much that Tom Healy was elected as a foreign associate of NAE. In 1977 Hans Rumpf was elected to NAE but unfortunately died very shortly after the election.]

But I still think that it is interesting to just comment on the vertical organization in China twenty years ago and the fact that the minister said, "I have ten million employees."

Swent: [laughs] So different from our system.

Fuerstenau: Right, right. I'm assuming things have changed. It would be interesting to find out whether—how they've accomplished this change.

For example, near Huainan Guy Harris and I visited a coal preparation plant, a big one, and the manager and assistant manager—I remember sitting with them after we'd gone through the plant. I was very impressed with them. You knew you were dealing with people that were right on top of what they were doing—it would be like dealing with the mine manager of an American mine. He told me, though, that they were going to have to lay off, within a few months, something like seven hundred employees. In other words, China was changing from complete support of everybody to having not run everything at

a loss. I never heard anything about what went on, but he did tell us that that would be coming about in very short order there.

Swent: That would be a big change.

XXIV DIRECTOR OF HOMESTAKE MINING COMPANY, 1977 TO 1999

- Swent: Do you want to move on now to capitalism? We were going to talk today about your experience at Homestake as a director. That's kind of a jump from the ministry in China.
- Fuerstenau: Less employees than ten million. That's true. Yes, I was asked in the early—either late 1976 or early 1977 about being proposed to join the Homestake board.
- Swent: How did this invitation come?
- Fuerstenau: It came, I think, from a telephone call or personal visit in my office from Paul Henshaw, who was then the chairman of Homestake. I never did ask the details of how that happened, but I have a feeling it may have come through Don McLaughlin and Dee [Dimitri] Vedensky. I had told you about the day the College of Engineering had what they called distinguished alumni meetings in all the departments, and both McLaughlin and Vedensky were extremely impressed with the talk I had put together. I told you Vedensky told me he had gone home that night and changed his will to leave stock for scholarships. You remember Vedensky, of course. He was a little older, possibly, than [Plato] Malozemoff, but he too came from Russia, White Russian. He came alone with no relatives in this country in the 1920s. He enrolled in the University of California and graduated from Cal in metallurgy in the late twenties, 1928 perhaps, and had risen to being the technology vice president of Hanna [Mining Company], vice president of research, and was on the board of Homestake. He certainly was well aware of my professional accomplishments since he had recommended support by Hanna Mining Company of my proposed pelletizing research program. It's my guess, without ever asking anybody, although he had retired from the Homestake Board, Vedensky perhaps had proposed me as a replacement for someone who had just resigned. Anyway, that's what I think was the course of events.
- Swent: Did you go on the board at the time that he went off?
- Fuerstenau: No. I read in the 1976 annual report that I replaced Alvin Rockwell who had resigned after eleven years of service. But when I was first on the board, they had three or four retired people who were called consultants to the board, and who came to every meeting, and one was Vedensky, and another was Howard Vesper, who you must know, must have known, who had been president of California Chemical—or Chevron Research. Another was John Kiely, who had retired as the president of Bechtel Power Company. Kiely I always consider to have been really one of the top civil engineers of his generation. He was an early member of NAE. And John Gustafson, who had been the previous president-chairman of Homestake. Gustafson had come from Hanna,

where he had been chief geologist and an early protégé of Don McLaughlin from Harvard. That's my guess as to how Vedensky had become a Homestake board member some years back. Things tie together, as you know. For maybe a couple of years or three years, these so-called consultants to the board would sit at the board table. Obviously they did not vote, but would participate in discussions because of their long experience as previous board members.

Six Inside Directors, Six Outside, on a Twelve-Member Board

Fuerstenau: What I found interesting in thinking back about it: at that time there were eleven, and soon twelve, board members, of which six were in-house and five outside, and a short time later six were outside directors.

[tape interruption]

Swent: The phone rang then, and you were just talking about inside directors versus outside directors. That means those who were employed by the company.

Fuerstenau: Right, right. When I joined the board February 7th, 1977, the inside directors were: CEO, and company president Paul Henshaw; and Harry Conger³⁸, who was vice president for the Missouri lead activities and who a few months later succeeded Henshaw³⁹ as president; Frank Corbin, who was finance vice president; and Don Delicate, who was vice president of mining, I think.

Swent: He was manager at Lead.

Fuerstenau: And manager at Lead, but I think he also had broader responsibilities that included overseeing the Bulldog Mine in Colorado—and Don McLaughlin, who at that point would have been eighty-six years old; and Dick Stoehr, who was senior vice president.

The outside directors were Cliff[ord] Heimbucher, who was an early member of our mining oral history advisory board. As you well recall, he participated in the mining oral history organization discussions at your home. Ted Marks, who was vice president of Castle & Cook—you know, Dole Pineapple, et cetera, in Hawaii. And Berne Schepman, who at that point was president of Envirotech. Berne had been, I think, a member of the board since about '73. Ted Marks had come on board the year before I.

³⁸ Harry M. Conger, *Mining Career with ASARCO, Kaiser Steel, Consolidated Coal, Homestake, 1955-1995: Junior Engineer to Chairman of the Board*, 2001

³⁹ Helen R. Henshaw, *Recollections of a Life with Paul Henshaw: Latin America, Homestake Mining Company*, 1988

One other outside and interesting director was Jim Salisbury (O.J. Salisbury III) who I guess had been on the board since the late forties. I used to sit down next to him. He would sit at the end of the table and smoke a pipe almost constantly. Smoking was allowed then. His grandfather (O.J. Salisbury I) had a freight-hauling company in Wyoming and in the Dakota Territory, and had hauled all the early equipment into Homestake and I guess was paid off in stock. So Salisbury was a significant stockholder of Homestake due to this ancient history of his grandfather's freight company. You probably know more about that story than I know.

Swent: I did know that. You didn't identify Heimbucher. He was an accountant, wasn't he?

Fuerstenau: Yes, and apparently very good, because he had been president of the national association involved with accounting, and he was the finance vice president of Varian. He had had a long career in accounting and apparently he was an outstanding person in his field.

So that made a total board of twelve, with 50:50 split. A short time later on in 1977, Wally [Wallace] Macgregor joined the board. You probably knew Wally from way back because I think he had spent one year as vice president of Homestake in the fifties or maybe about 1960. After that, he was with Amax or Climax and then Kaiser Aluminum. I think he was executive vice president of Kaiser. This was after my day with Kaiser. Just at the time he joined the board, he was serving for a year or two as the president of Peabody Coal. You may have learned a little of this in your interview with Frank Joklik. Peabody Coal had been bought by Kennecott after they had been kicked out of Chile. With the money from Chile, Kennecott bought Peabody Coal, and then the wisdom of our people in Washington at the time said that Kennecott had to divest themselves of Peabody Coal because antitrust laws came into play since Kennecott already owned a small coal mine in Utah. While Peabody was in limbo, for an interim couple of years Wally Macgregor was the president. I think upon retiring from Peabody, he considered himself a mineral financial consultant and worked professionally in that capacity. So Wally became a board member a few months after I did in 1977.

I would like to pass on a short bit about my joining the Homestake Board. I'm here at the University of California, and you are too, so you've gained a little bit of how things are approached here; that is, there's complete freedom of speech, and people can say what they want and write what they want, I guess as long as it's PC, politically correct, because if it's not PC, you can't say what it is.

I'd like to just read to you a short letter to the editor of the *Daily Californian*, dated April 13, 1977. There is a now-retired professor of physics, Charles Schwartz, who almost on any controversial or on any matter expressed his opinion. He was very outspoken during the Vietnam War and I think a major

thorn in the side of the director of LBL [Lawrence Berkeley Laboratory], et cetera. But this letter, which I think is important—the headline was “Hearsts” and the letter is addressed to The Icebox. “Charter Day Brought Honors”—I’ll give you a copy, but I’ll read it.

“Charter Day brought honors to Donald McLaughlin, former UC Dean of Engineering, UC Regent, and president and chairman of Homestake Mining Company. Friday’s *Daily Californian* article also mentions that he was a friend of the Hearst family, apparently unaware that Homestake Mine (now a \$130 million-a-year corporation) was the original source of the Hearst family fortune. This bit—”

[Tape 38, Side B]

- Fuerstenau: “This bit on UC ancient history is linked up today. Homestake Mining Co. just announced a new addition to its board of directors: Professor Douglas Fuerstenau, chairman of Berkeley’s Department of Materials Science and Engineering. Charter Day is a time when we can all pause, recall our rich heritage, and ask anew, ‘Whose university is this?’” Charles Schwartz, Professor of Physics.
- Swent: What was his point, I wonder?
- Fuerstenau: That the Hearsts controlled the university in some way, I guess.
- Swent: I see. Okay.
- Fuerstenau: Probably any time there was any demonstration down at Sproul Hall or Sproul Plaza, generally there was Charlie Schwartz at the microphone.
- Swent: So he thought you were a tool of the Hearsts.
- Fuerstenau: Tool of the Hearsts. Recently I did see in a book related to the copper wars in Butte, Montana, that George Hearst did own 50 percent of the Homestake Mine, himself, and that he owned 39 percent of Anaconda, and when he died, his wife, Phoebe Apperson Hearst, sold Anaconda. That was obviously one big mistake on her part at the time. But the fortune that started with them in mining was obviously very major. One reads that when Phoebe Hearst came to a meeting of the Regents, she came with her checkbook. At the time, she provided the full support of the Anthropology Department. She built Hearst Mining Building as a memorial to her husband a century ago, a building considered to be the most outstanding one architecturally in the University of California.
- Swent: I was trying to recall—the Patty Hearst abduction was earlier than this, of course, wasn’t it? That was the late sixties.

Fuerstenau: Yes, I guess so. And, you know, her mother was a regent of the university. I remember seeing her on one occasion because once in a while they [the regents] would come through Hearst Mining Building. One time, Don McLaughlin brought Mrs. Hearst, then a regent, to see the bust of Phoebe Hearst, which always had been in the lobby of Hearst Mining Building, only the bust was in the broom closet at that time for some reason!

Swent: Oh no! [laughter]

Fuerstenau: Well, I'll tell you, Phoebe's bust was back out in the lobby shortly thereafter. And there was a lady janitor, not too much later, that was very industrious and at night had decided that Phoebe needed polishing, and so she took sandpaper and sandpapered off the patina on this bust, bronze casting of Phoebe Hearst. Well, Buildings and Grounds agreed to pay for having Phoebe re-patinaed. And then there was a huge casting, bronze casting of George Hearst on the wall, and she had started to sandpaper the copper oxide from it, and down at one corner for a few inches you can still see where she had decided to clean this up. [laughs]

More on Homestake Mining Company during My First Years on the Board

Fuerstenau: When I joined the board in 1977, about 25 percent of Homestake's earnings were from gold, which then sold for \$147 per ounce. Uranium, at \$47 per pound, accounted for about half the company's earnings. Most of the rest was from lead. At that time, the company's holdings included the Homestake Mine in South Dakota, the Bulldog Silver Mine in Colorado, half ownership of the Buick lead mine and smelter in Missouri, the partnership for uranium operations in New Mexico, the Madrigal Mine in Peru, and 48 percent ownership of the Mt. Charlotte Gold Mine in Kalgoorlie in Western Australia. Black Hills forest products contributed a small amount.

Towards the end of 1977, Harry Conger became president and chief executive officer of Homestake, with Paul Henshaw moving up to chairman, an arrangement that continued until 1981 when Conger became chairman, president, and CEO upon Henshaw's retirement. This management organization stayed in place until '86 when David Fagin joined Homestake as president and chief operating officer.

So back in the '77 era, gold was about a quarter of the revenues of Homestake. Uranium was major, and lead, and I know—

Swent: Did they have the iron in Australia?

Fuerstenau: You know, that was before my time, I guess. Oh yes, I guess when they brought in Gustafson as president, because then gold was only \$35 an ounce,

with the idea that Homestake had to be diversified. Gustafson was familiar with iron ore through his years with Hanna and also with the geology of Australia. That probably led to Homestake's venture into Australian iron ore—but all of that was before my day. Also at that time they were drilling for copper up in the Michigan peninsula, and I remember Wally Macgregor one time saying some years later, "Boy, we're lucky we never hit any copper." [laughs]

Swent: They even had the brick plant in Port Costa; they did all kinds of things.

Fuerstenau: They had to. With \$35 gold, you had to. And forest products up there in the Black Hills contributed. Of course timber was needed for the underground mine.

Swent: So when you came on, they were still not just a gold company. Were they beginning to concentrate on gold?

Fuerstenau: Well, there were discussions about increasing the gold because there's always a multiplier on the value of the stock, based on gold, which unfortunately is the other way around today because of the lag in the gold price. But Homestake also had other international interests, such as the Madrigal Mine down in Peru—did Langan⁴⁰ go down to Peru?

Swent: Yes, a lot.

Fuerstenau: He must have been involved with that because that needed a lot of attention, I gather. This was a marginal copper-lead-zinc mine that essentially sustained itself and did not require additional capital from Homestake. Don McLaughlin liked the Madrigal Mine because he figured there was great potential in the region. I'm speaking geologically. Eventually Homestake sold that in '86. But early on, a lot more discussion took place over KMA, Kalgoorlie Mining Associates, over the 50 percent ownership that Homestake had in the Mt. Charlotte Mine. In those days—again with now only \$100 gold—it was my impression that this was kind of a baling-wire operation then that required lots of discussion as to whether any more money be spent there, et cetera. That eventually turned out to be a magnificent investment.

The first expansion in Australia was reopening the old Fimiston Mine in Kalgoorlie. That led to the concept of developing the Super Pit to mine out the entire area that had been the original famous Golden Mile. Homestake owned half of this venture. Just before I retired, Homestake acquired Plutonic Resources which had several well-known mines and properties. I assume that

⁴⁰ Langan W. Swent, *Working for Safety and Health in Underground Mines; San Luis and Homestake Mining Companies; 1946-1988*, two volumes, 1996

Harry Conger would have discussed the corporate developments that Homestake had in Australia.

Swent: Yes.

Board Committees

Swent: What particular expertise did you bring to the board?

Fuerstenau: Paul Henshaw, as I remember, said, “You’re not being asked to come on the board technically but for your overall wisdom, general input, and advice and judgment on board matters.” So I naturally did comment on technical matters, but I think after a lot of years around the university and government committees, and dealing with people and solving problems and assessing approaches, I could apply the same thoughts to plans, corporate strategy, and so on, that came up to the Homestake board.

Committees that I served on were, oh, for quite a while, the compensation committee, which really determined compensation levels for the two top people—that’s the real concern of that committee—and approved general executive compensation matters. Usually compensation consultants were there whenever there was an annual change in plans. With the passage of time, the concept of employment contracts required compensation committee action before board approval.

In later years, the environmental committee was established, for example, and that became very important for a mining company. Most of these committee members were board members who were engineers. All during my years with Homestake, their handling of environmental matters was always outstanding. The tailings dam at Lead had already been constructed, but we were involved with plans for lifting the height of the dam on two different occasions.

Later on, a couple of years after I was on the board, Bob Jaedicke, who was professor of accounting at Stanford, was elected to the board. This probably took place with the retirement of Cliff Heimbucher. About the time that he was announced to be a board member, he also became dean of the business school at Stanford, and so his broad expertise covered the accounting and finance. He was chairman of the audit committee, et cetera. I know he had that role and also served on the finance committee. Board members who may have been president of another company would serve on the finance committee, maybe audit committees, and so on. Another board member with whom I interacted a lot with was Stuart Peeler, who was chairman of the board of Statex Petroleum and who served on the Calmat board with Harry Conger. Peeler was also on the board of the Getty Museum in Los Angeles, and I recall a number of discussions with him about the planning and building of the new Getty Museum.

[Added by Douglas Fuerstenau during editing: Later Bob Jaedicke was often in the national press because he was chairman of the audit committee at Enron at the time of their demise.]

Swent: Compensation committee was your principal assignment?

Fuerstenau: And also the nominating committee, which was the committee that looked at bringing in new board members. For a long time, Homestake had a fairly young board, after Conger became chairman. At that point, a policy was instituted, which I think is a good policy, that board members should retire at age seventy. We had board members and consultants to the board who were getting into their high eighties and low eighties, et cetera, and after the retirement policy was adopted, it was my duty to go around and tell each of them that they're going to be retired from the board. I had to go discuss this with Don McLaughlin, for example. It was part of my role as chairman of the nominating committee.

During my latter years, only the president and chairman of Homestake were members of the board, and essentially all the rest were outsiders. For quite a number of years, a large number of board members were, let's say, in their early fifties, so several persons were invited to be on the board upon their retirement from other companies. This was kind of a nice luxury that Homestake had for a few years. One such board member was Bob Reininger, who had retired as chairman of Rosario Mining Company. Here we had the opportunity of having him serve on the board for five years, between his being sixty-five to seventy. Another such person was "Bull" Durham, who had been head of Phelps-Dodge, and I remember Bull telling me that when he retired as chairman of Phelps-Dodge, they wanted him to stay on the board but he said no, he was going to leave. He wanted to turn—which I think is the right way to do it—to turn everything over to the new management and not have him as the former CEO looking over their shoulders, or having people coming to him when they should really be going to the new CEO. As the years began to pass, various people aged, and so that luxury began to disappear because all of a sudden, in the last handful of years, board members reached the retirement age. Let's say the last half dozen years had one or two retirements every year, because of this aging phenomenon. At that point, one would have to consider younger persons as new board members. The only other older board member was Hadley Case, who had come with the acquisition of Case-Pomeroy, when Homestake took over Felmont Oil Company. Hadley was well into his eighties, but when you owned a large percentage of the company, you were on the board. And Hadley was still contributing very well up until he finally decided to step down in his late eighties.

Let me comment on another aspect of the Homestake board, namely that there be some representation from geographic areas where Homestake operated. Since a very significant part of Homestake was in South Dakota, you always wanted to have somebody from South Dakota on the board. And early on,

with uranium in Colorado and silver-lead at the Bulldog Mine in Colorado, board input of somebody from that general area was important. So for a number of years, this was filled by Don Delicate who lived in South Dakota, oversaw the Bulldog Mine, and had spent earlier years in New Mexico at the uranium operations. But when he decided to retire, he stepped down from the board. After that, Chuck Undlin from the Norwest Bank in Rapid City joined the board for a number of years. Later he was transferred by Norwest to Omaha, and being in Omaha didn't serve the purpose of South Dakota representation anymore, so he stepped down.

Another person who turned out to be a very good board member was Glenn Ryland, who was chairman of Frontier Airlines and headquartered in Denver. He was a good contact person for the Colorado region. This was particularly important at the time because of the environmental aspects going on with the development of the uranium property at the Pitch Mine. I'm assuming Ryland was helpful in making contacts with the governor, et cetera, concerning establishing this mine. But I read in the newspaper that Frontier had many problems with unions and so on, and eventually Ryland and Frontier parted ways and he moved back here to the Bay Area. He stayed on the board. He was a good board member. Eventually Frontier Airlines went under, or was taken over by somebody else.

But coming on back to South Dakota, some years ago—I had had some dealings, through my brother, with one of his former students who had devised a device as an accurate tool for ophthalmologists making cataract operations. They formed a small company, called Magnum Diamond and both Maurie and I invested some money in it. They hired an excellent, fairly young woman, originally from South Dakota, named Carol Rae, as the president of the company. This small company was sold to Chiron before it had a chance to grow simply because the technical founder constantly interfered with running the company and could not let go of trying to manage his venture.

Anyway, from my dealings when this took place, I realized that Carol Rae was a very capable woman. Although, she came from Rapid City, I think then she was living in Tucson or Phoenix but came back to be head of Magnum Diamond. I think she also was an officer at the national level, on the Chamber of Commerce in Washington, too. One day it occurred to me that here would be somebody in South Dakota, a very capable person, and a woman who would be an excellent board candidate. I made the suggestion. I guess I was away on a trip when she came out and interviewed various board members and so on, and they all were impressed, and she was elected to the board and has served very admirably.

Swent: You broke that one open.

Fuerstenau: Oh yes. That idea just hit me one morning. That action took care of: one, getting a woman on the board, and two, taking care of having a South Dakotan

on the board. Carol Rae apparently also had had much interaction with the governor of South Dakota and knew her way around the state. She was a great addition.

Some Major Corporate Actions

Fuerstenau: But Homestake eventually moved more and more to becoming a gold company. Of course, the increasing price of gold helped give the incentive for doing that.

Swent: You were on the board when the Corona acquisition came.

Fuerstenau: Oh yes. That was a major acquisition. Some years earlier, I had read in newspapers about the suit between Corona and Lac over the Hemlo dispute. I guess that Corona had been given the option to the mine by the owner, but somehow she turned around after signing some papers and gave it to Lac Minerals, and all of that ended up in a trial that lasted many years. And eventually Corona won that. So Lac had already put in the mine and maybe was building the mill or had already built a mill when the trial was over. So Corona got the property and then paid to Lac the money that Lac had already put into the development of it. I gather a Corona officer that was one of their main witnesses was Peter Steen, who, at the time Corona was taken over, was president of Corona. But as I recall, Corona was in really very bad financial shape, had considerable debt, and so Homestake was able to acquire them because they were close to imploding due to the debt. And from that came the wonderful Hemlo mines. Homestake then had 50 percent ownership of the Williams and the David Bell mines there. The David Bell mined about 300,000 tons of ore annually with a grade a little more than 0.4 ounces of gold per ton, and the Williams annually mined about 1,300,000 tons of ore at a grade of about 0.2 ounces of gold per ton. Both were underground mines.

Also, with Corona came the Eskay Creek deposit. You know, Eskay Creek is the kind of operation that you would love to have lots of. I remember that the ore ran about eighty ounces of silver, two-plus ounces of gold, and a few percent zinc, and maybe a little trace of lead per ton. Basically it was a sulfide, and there was lots of board discussion over whether to mill it or build a mill, which would probably have been a hydrometallurgical plant, or to just direct-ship the ore. Finally the decision was made to just ship it without processing, and half of it went to Japan and maybe the other half, in round numbers, to the Noranda smelter in eastern Canada. By doing that, as you can well imagine, you're not ending up with an environmental problem, et cetera, so it turns out that you don't have to invest in a mill and find a way to handle tailings—

Swent: That solved that problem.

Fuerstenau: It also solved the problem of capital, and solved what might have been a long-range environmental problem. Imagine having tailings that are basically residual from an ore that's virtually all sulfide.

One of my former students, Mauricio Hoover, worked for Exxon Minerals, and you may remember years ago, Exxon found in Wisconsin a huge deposit—I forget what they called it—it ran something like 6 percent zinc, from what I remember reading. I think it was basically a zinc deposit, but all of the tailings were pyrite, and what would you do with these pyrite tailings? There may have been some copper, but I vaguely remember it was mostly zinc, but a rich deposit, and huge. Anyway, it was all put to bed, the decision made not to mine it because how would you handle the environmental aspects of tailings that would be completely pyrite. Maybe generations from now they'll have to go to a deposit like that, but as for now it has been put aside.

But Eskay Creek was a jewel; it truly was. Eskay Creek was not a large operation, in that about one hundred thousand tons of ore was mined annually, but the grade was nearly four equivalent gold ounces per ton.

Swent: You were on the board when they voted to acquire that, then.

Fuerstenau: Oh yes. Right. That was 1992.

Swent: Did that bring in new board members?

Fuerstenau: It brought in two new board members, two good ones. Peter Steen as president of Corona became a Homestake board member. And also Norman Anderson, who I think was functioning as—I may be wrong—but I think he was functioning as the chairman, probably non-CEO chairman of Corona. I think that's how Norm Anderson came in. He had been chairman, I think, of Cominco at one time and was, of course, a solid mining engineer and a conservative financial mining executive—

Swent: Did the fact that he was Canadian make any difference? Peter Steen was Canadian also—well, actually, Peter Steen was South African, wasn't he?

Fuerstenau: Peter Steen was originally a South African and he went to Cambourne School of Mines. I just know that from reading. And then whether he was a Canadian citizen—

Swent: Would it make any difference in their attitudes?

Fuerstenau: It didn't seem to. I think, if I remember now from talking with Norm Anderson, his father or grandfather was American, but working in Canada maybe as an engineer.

Swent: It's a very porous border.

Fuerstenau: Yes, whereas Anderson, himself, was born in Canada and native of—he I believe ran the Cominco operation at the Buick Mine in Missouri. You know, Cominco had an adjacent mine on the same ore body there. I don't know whether Harry Conger knew him at that point or not. Perhaps he did since during that time Harry was Homestake's liaison with the Buick operations, managed by Amax.

Swent: And he, of course, had worked in British Columbia.

Fuerstenau: True enough. So it's an international situation.

Swent: That was one of the big events. What was another?

Fuerstenau: Well, there are two or three. A huge event that took a lot of board time was the development of the McLaughlin Mine, but you have covered that in great detail with the oral histories on the Knoxville district. The peaking in the price of gold at that time really helped push the development of the McLaughlin deposit. As a metallurgist, I was pleased at the success of a major processing innovation, namely the autoclave oxidation of gold-bearing pyrite in so-called refractory gold ores. Homestake essentially pioneered this approach to refractory gold ore processing. The environmental restoration there is particularly impressive.

Of course, the Felmont acquisition was major, and changed the mix of Homestake production. I have no recollection of how that began. I think Hadley Case wanted to sell Felmont, which in addition to oil and gas production, they owned one-quarter of the big open pit mine in Nevada, Round Mountain. That, perhaps, interested Homestake management considerably.

To me, the Round Mountain operation is very interesting. As I recall, some fifty thousand tons of ore were mined daily and crushed to about a half inch or so in size. Then, the crushed ore was heaped on pads and leached for about a hundred days with cyanide solution, after which the ore was removed from the pads as waste. Because of the scale, the leached rock could be moved to the final dumps for about ten cents per ton so that the pads could be reused. Interestingly, the grade of the ore was only 0.03 to 0.04 ounces of gold per ton and the gold recovery by heap leaching was about 70 percent. Because of scale and the low costs of open-pit mining and heap leaching, that is a very economic operation.

Another aspect of the Felmont relationship was that Don McLaughlin had consulted at length for Hadley Cases's father in Kalgoorlie, or in Australia, decades earlier. The contact did come out of gold, but Felmont brought, of course, oil with it, and so for some years Homestake was in the oil production business. Hadley Case joined the Homestake board, as well as Bob Clark, his son-in-law.

[Tape 39, Side A]

Fuerstenau: David Fagan was a good person as president in those years. His undergraduate degree was in petroleum engineering, and he had been, I believe, president of Rosario under Bob Reininger, and so he had extensive experience in both mining and petroleum production. Of course, when you have oil production as part of your company, you really want to make sure you have somebody inside that understands that part of the business, right? So Fagan, I think, served that role well.

With Felmont came another board member, who was in direct charge of the petroleum production, Lynn Walker. Walker after a couple of years retired. I mean, he just up and sort of quit. So that's where Fagan's background and expertise was important.

Eventually, Homestake exited from oil production. I don't recall if the Felmont connection played any role in it, but in 1988 Homestake took a one-sixth interest in the development of a large Frasch sulfur deposit, called Main Pass, offshore in the Gulf of Mexico. Unfortunately, the price of sulfur decreased steadily through the years, in large measure due to low-priced imports of by-product sulfur from Canadian gas and oil production. When I retired from the board in 1999, there still was some income from sulfur.

Having grown up in South Dakota, the Homestake Mine in Lead always interested me, and I would like to comment on some of my recollections there. During my years on the board, the only major change at the Homestake Mine in Lead was the initiation of surface mining in the old "open-cut" and the construction of facilities to handle the ore. This added considerably to the total production at Lead since much of the underground was becoming ever deeper—down to eight thousand feet. For a few years, there was a severe financial crunch because the State of South Dakota changed the severance tax from one based on operating earnings to 6 percent of the gross—which was very punitive to overall costs during that period.

One thing that I was against at the time was Homestake's selling the manager's mansion after Al Winters decided to live outside of Lead when he became general manager of the mine. I guess it was a symbol to me. I was the only board member to speak against selling the house.

Some time before I joined the Homestake Board, the Grizzly Gulch dam was built to impound the mill tailings. For a century the tailings had been discharged into Whitewood Creek, and I assume some of them may have reached the Gulf of Mexico. Some problems relating to tailings deposits along the Whitewood Creek banks, et cetera, had to be dealt with at various times. All of that was finally settled by the time I retired. In recent times, discharging tailings into rivers, lakes and oceans has led to real problems, you know. Off the top of my head, some examples include the Silver Bay taconite tailings

being discharged into Lake Superior, the ilmenite tailings from Titania into a Norwegian fjord, the tin mining tailings into the sea off Cornwall, and particularly the Freeport-McMoran⁴¹ tailings in Indonesia which has received a lot of very negative attention from environmentalists.

There was one technical achievement at Lead that always impressed me very much. As you know, the Homestake Mine produced excess water that had to be discharged into Whitewood Creek, and at one point the state of South Dakota imposed extremely strict limits on the cyanide content of that water. Trying to reduce the residual cyanide by chemical reaction such as ozone treatment and iron addition to make ferrocyanide, et cetera, were not able to reduce the cyanide level to the parts per billion demanded. I know that my brother Maurie consulted with them on trying to develop chemical methods. Terry Mudder, a School of Mines grad working for Homestake, discovered bacteria on the edge of the tailings pond that ate cyanide. After successfully culturing these bacteria, a new cyanide destruction process was developed where the discharge water is contacted with these bacteria to convert the cyanide in solution to a nitrate and followed with a second type of bacteria that converts the ammonia in solution to nitrate that can be discharged. At the end of the whole system where the treated water is being discharged is a small glass tank with healthy trout swimming in it. I remember visiting that a couple of times. Did you ever visit that facility?

Swent: Yes. It was absolutely fabulous.

Fuerstenau: That is a beautiful example of what mining company research can achieve. Going on a little bit to uranium, development of the Pitch Mine near Gunnison, Colorado, by Homestake got underway in 1978. A lot of effort was expended to obtain permits for building a mill. As I remember, the design for the mill included dry tailings disposal—something that would have been quite innovative at the time. Eventually the decision was made not to build a mill but to ship high-grade ore directly to Grants in New Mexico for milling. Though Homestake still had long-term contracts for selling uranium in the middle \$40 per pound, the spot price fell to \$17 per pound in 1982. In '81, Homestake took full ownership of the New Mexico partnership. By 1989, the contracts had expired and uranium had dropped to \$9 per pound, so mining was suspended and milling stopped shortly thereafter. After that, full reclamation was undertaken at Grants. As you probably knew from Langan, uranium in the effluent plume from the tailings dam was a technical problem that had to be faced and was nicely solved with injection wells.

[Added by Douglas Fuerstenau during editing: Because of energy and greenhouse gas concerns and renewed interest in nuclear energy, in 2007 the

⁴¹ Paul Schipke, *Mining and Environmental Engineer for Utah-BHP Company, 1972-1997*, 2004

price of uranium has risen to \$100 per pound with expectations to go much higher. The Olympic Dam mine in Australia, discovered by Roy Woodall and described in his oral history has the world's largest known uranium reserves, something like 29 percent.]

A good history of Homestake during my twenty-two years on the board can be gotten by reading the annual reports. You know, I still have copies of each of those issued during my years on the board.

Averaging Six Percent Expenditures for Exploration during Twenty-Two Years

Fuerstenau: Before I joined the Homestake board a decision had been made to increase exploration.

Swent: You had done some interesting compilations of figures on exploration.

Fuerstenau: Yes. My whole time on the board was twenty-two years—so I looked at data in annual reports over quite a long period. Probably Berne Schepman, a couple of years older than I am, may have been there a total of twenty-three, but that was, as you can imagine, a long service.

Just out of general interest, looking at the annual reports during my tenure, in 1977 the revenue of the company was \$159 million, and it grew fairly steadily, reaching something like \$520 million in 1987. The high point of \$998 million was reached in 1996, and decreasing to \$748 million in 1999, my last year on the board. The total revenues during my years on the board were \$12.3 billion. I also tabulated from the annual reports, just out of general interest, the Homestake expenditures for exploration. Because a decision had been made to increase exploration quite significantly, Jim Anderson had been hired as vice president for exploration shortly before I got there. In 1977 the budget for exploration was \$10 million; increased to \$22 million in 1981; for more than a dozen years, exploration expenditures were running something like \$40 and \$50 million; and in 1997, something like \$68 million, according to the annual reports.

When you sum all of these, in those twenty-two years, exploration totaled \$709 million, which, in round numbers, was about 6 percent of revenues. I always find interesting, tabulations of what different industries spend on research. Let's say, the metallurgical industry spends 1 percent; and the pharmaceutical companies about 10 or 20 percent; and maybe the chemical industry, 3 or 4 percent. Summaries of research expenditures are regularly tabulated in financial journals and in *C&E News* [*Chemical & Engineering News*] of the American Chemical Society.

Well, if one considers exploration by a mining company as research—in other words, finding new ore deposits is like research in pharmaceuticals for finding, let's say, a new drug—then Homestake spent approximately 6 percent, quote unquote, on research. Of course, that's different from thinking only in terms of research carried out to develop new or improve existing metallurgical processes or to work on ground control problems in a mine. So, to me, that exploration expenditure was quite a significant number.

Swent: Do you have any idea how that compares with other mining companies?

Fuerstenau: Not really, except that years ago, Malozemoff was visiting our department here at Cal, and that was before I had any relationship with him, I remember he said that at that time, when Homestake may have spent \$17 or \$18 million on exploration, that Newmont that year had budgeted \$22 or \$23 million for exploration. He commented that almost all of it that year was spent in Nevada. In other words, they were spending their money in the geographic area that they knew, and on land adjacent to their existing mines. I think that exploration geologists like to explore for extensions of known deposits.

These are just general observations on my part. Of course, I found very fascinating reading David Lowell's oral history. It's an interesting perspective on exploration expenditures and what the accomplishments can be to read an oral history of somebody who devoted his whole life very successfully to that very point.

Swent: Yes, indeed.

Annual Board Visits to Various Homestake Operations

Fuerstenau: Homestake every year had an annual board trip to visit different operations of the company. I found these to be very useful, but these trips were probably even more important towards educating the nontechnical board members about the business of the company. One year we went up to Lead [South Dakota], underground in Lead; another year to see the uranium operations and tailings situation at Grants, New Mexico; another time to Missouri, in the mine, in the Buick lead smelter, et cetera. To see the huge veins of galena in the underground Buick Mine was almost an unbelievable thing. Those veins were horizontal and maybe sixty, eighty feet high, sometimes solid galena, virtually. You know, the mining there involved big trucks driving underground—mining that deposit could not be compared to the stope mining in Lead. Obviously, in the Buick Mine major pillars of ore were needed to hold up such openings underground. This involves pretty fantastic mining engineering, in my opinion. The final step in such a mine is to recover those pillars by kind of letting the ground down as they back out and so on. This requires technical understanding of rock behavior.

Another one of the trips was out to the oil platforms in the Gulf of Mexico. We went to Lafayette, had a nice cruise on a river—dinner on a boat with whatever that music is that they play.

Swent: Cajun?

Fuerstenau: Cajun music, yes. That was great. Then the next day, we helicoptered out to the platforms, and we stopped on two or three of them and had complete tours of the facilities, and of course a fabulous lunch on one of them. I noticed that around the legs of each of these oil platforms the water was teeming with fish. In other words, any organic material that was there was great fish food. I gather some of the workers liked to fish. They would just have to go down onto the lower platform where there were all sorts of fish: big fish, medium fish, and so on.

On another occasion, we visited the Hemlo mines in Ontario. There one could see different methods of mining. In the Williams Mine, there were fairly wide almost vertical mineralized veins, where they ran tunnels alongside the ore vein and then drifts into the vein, from which they would drill and blast. But because of the instability of this fairly wide vein, removing blasted ore was all done by loaders that were run remotely. So here was a guy dressed up, looking like somebody on the moon, with little joysticks and a controller, running this loader into the stope to load it up and run it back out. This was because the potential for rock falling was huge. To mine an ore body like that obviously took special design as to how to plan doing it. Afterwards they filled the mined out stope with concrete. A lot of the filling was done with concrete made with rock produced for that purpose from a small open-pit mine on the surface. The purpose of the small open-pit was purely to obtain rock for back filling. Those are really good-quality mines in that district, but their life is limited.

We visited Kalgoorlie early on, and we made two board trips to Kalgoorlie. I had been there once before, as we had discussed. The first board trip was to Mt. Charlotte, and the Super Pit wasn't underway yet. There was some open-pit mining, but we went underground in the Mt. Charlotte Mine and also in the old Fimiston Mine, and I'll tell you, we crawled around rocks and boards and whatnot down in there. You know, it's pretty hard to believe, but they were actually mining residual ore in the upper areas of this old mine. The ore body at Mt. Charlotte is a huge massive plug with defined limits. As I told you, they drill and blast underground once a year. When they do, the whole town waits, shakes and everyone hears and feels the blast. Anyway, for that first board trip, I went a day early to Perth because I was invited to give a seminar at the University of Western Australia, to their metallurgy department and the CSIRO research group on the campus. I had written to Dr. Len Warren of CSIRO that I was going to be there, and he invited me to come a day early.

We had been told that the board members were going to have dinner with Sir Arvi Parbo, the head of Western Mining who was hosting the dinner, and that we should wear a dark suit for that occasion. We were always nicely informed for board trips what kind of dress would be appropriate. Anyway, I got to Kalgoorlie, opened my suitcase, and I had nothin'. I had no jacket; I had no suit. They were hanging on the door of my closet at home!

Swent: Oh!

Fuerstenau: I arrived on a Friday, so Saturday morning—and the stores are only open mornings on Saturday—I went to a department store in downtown Perth. This was in Perth, because that's where Western Mining had a divisional headquarters and where the university was. I went to the main department store and they had a sale on—[looks at jacket he is wearing] Wait! This is it! And I bought this off the rack. Australia sizes are a little different from ours—you've got to get a bigger size there. But it fit without any alteration—this very jacket I'm wearing right now is the one I bought to go—

Swent: Weren't you lucky!

Fuerstenau: But I'll tell you, what a shock to open the suitcase and see that you left your main clothes hanging on the closet door. I didn't even have a sports jacket. I did too much last-minute rushing.

That, as I said, was before the Super Pit came into the picture. So we went underground there, and went through the old mill. In the original mill, they recovered the gold-containing pyrite by flotation. Kalgoorlie is famous for having a lot of gold telluride, and that responds to flotation very similar to pyrite. As I recall, they recovered about 90 percent of the gold in the pyrite concentrate, which was roasted and then leached with cyanide for gold recovery. Everything was gold then. Some years later we came there again for another board meeting. Then the open pit was already the Super Pit. There was a new mill with a big change all around. All of the old head frames were gone along the stretch formerly known as the Golden Mile, because the Super Pit took in that ground.

On the second board trip to Australia we also visited a small operation a couple hundred miles north of Kalgoorlie that looked like it was going to be a good mine but never did amount to much and was eventually sold. We flew up there in little airplanes. That mine consisted of a couple of small open pits and a mill. And there everybody worked twelve-hour shifts for two weeks, and then flew out for two weeks. Everyone there lived really in very small, compact—you can't call them apartments—bedrooms that were practically—well, not much more room than for a bed and a closet in each one. That's kind of standard, I think, for working in the outback of Australia. After two weeks, the next crew comes in. Anyway, we had an interesting day there and then flew in the small planes down to Kalgoorlie.

Then, in an attempt to diversify, fifteen years ago or less, Homestake bought a sixth of a sulfur dome, called Main Pass. Homestake was one-sixth owner. Fifty-some percent was owned by Freeport, who operated it, and International Minerals and Chemical Company had the other part. This looked like a very good investment at the start, but sulfur prices dropped due to imports from Canada. Main Pass was really a huge operation out, again, in the Gulf of Mexico south of New Orleans. And we visited the operation on one of the board trips. You know, I think maybe a total of \$4 billion or something like that was spent on the construction of this well and its recovery facilities. There was a long causeway about a half mile long where vehicles could drive between producing wells and new sites where they were drilling for sulfur. I remember talking with one of the engineers, where they were drilling at the time. They've learned to direct drill from one spot several hundred different, long drill holes that fan out to get all of the sulfur.

When molten, sulfur is a very nonpolar liquid due to its molecular structure and will not wet rock, which is sort of like flotation in reverse. The sulfur is inside the pores in the rock. I don't know that I ever learned about the geology of the formation of sulfur deposits, but to start a sulfur mine, for several weeks or months they pump down steam, hot water, to heat the rock to melt the sulfur. Because sulfur molecules are nonpolar, now all you have to do is to pump hot water down into the well, and the water will wet the rock and displace the sulfur in the pores, and so the liquid sulfur then can be pumped out of the well. You get almost full recovery of the sulfur this way, because liquid sulfur is a very hydrophobic material, and the rock is a hydrophilic material. The rock surface likes water, and not molten sulfur. This mining method is called the Frasch process after its inventor. So this is a beautiful way to recover the sulfur, which is then put in barges and taken to shore. And it's used mostly for sulfuric acid for fertilizer production. But the price of sulfur went south every year, and some years ago Homestake sold out their share to—I forget, either to IMC or to Freeport. Of course, part of the dealings were always with the head of Freeport. I don't know whether Milt Ward was involved with any of that or not, but he was president of Freeport at one time, so he may have been. That was one of our very enlightening board trips—all new to me.

Another trip was up to Eskay Creek a few hundred miles north of Vancouver. Of course, that was something to see, that small but beautiful operation where they mined about three hundred tons per day of very high-grade ore. We flew up there in small airplanes, looking out at the grand mountains of British Columbia. We stayed in the dormitory-like facilities there overnight. Our visit there included going underground, which really meant just going in an adit. We were given presentations about the exploration program underway to find possible extensions of the ore body. Some additional reserves were found, but nothing extensive.

By the way, we made a second trip to the Missouri lead operations after the Doe Run Company was formed when Amax dropped out of the scene. I recall a tour of the Herculaneum lead smelter just south of St. Louis. We wore enclosed helmets and were dressed like we were going to walk on the moon. What a contrast to how we dressed with the simple small mask that I wore when loading the lead roast charge cars at the Bunker Hill Smelter forty years earlier—an incredible difference.

Those trips always gave board members a real good understanding about the nature of the business that Homestake was involved with—and that was especially important for the nontechnical board members like Bob Jaedicke, Ted Marks, Carol Rae, and Bob Clark, who had come with the Felmont acquisition, et cetera. Those with a non-mining background could see what the issues are at the mining properties and at least acquire some feeling for them.

I would like to tell you about one additional board member with whom I talked quite a bit on our first trip to Kalgoorlie—a board member who had been a congressman from Nevada, Jim Santini. He had been a Democrat, and he switched to being a Republican. After a few years, he decided to run for the Senate, so he stepped down from the Homestake board. When he ran for the Senate, he got more votes than any Republican in the country, but he still lost, and he said, “I carried seventeen of eighteen counties of Nevada, but of course I lost”—is it Douglas County that Las Vegas is in? He lost the county, anyway, where Las Vegas is, but he won all the other seventeen. Unfortunately, he failed to make the Senate, and he said it was tough because his opponent was [Harry] Reid, who still is a good senator. He said everything that Reid was running on were the things that he, Santini, had proposed when he was in the House. Maybe his successor in the House was Reid. So he said, you know, “I had no issue because what Reid was running on were my own ideas.” Anyway, I always regretted that Santini never became a senator because it would have been wonderful to have had somebody with that kind of mining interest in the Senate that one knew personally. We would have had somebody to talk to about mineral research matters. I’ve seldom given political money to candidates, but I did make a good contribution when he ran for the Senate. The only other senator I ever gave money to was Harrison Schmitt—you know, the geologist from New Mexico who walked on the moon, someone whom I had met one time at an AIME/SME annual meeting in Washington when he was a senator.

You asked earlier about any other kinds of input I had to Homestake. In addition to board activities, I did a certain amount of consulting for the company also. Anyway, first I’ll just tell you about one kind of input that I recall. I noticed right early on that the geology people would talk about exploration and maybe deposits that they were working on. There was a big silver deposit right next to Creede [Colorado] called Batchelor Mountain, a huge prospect that maybe ran something like five ounces of silver. Of course, with today’s silver prices, that was too low grade to mine. But back in the

Hunt days, that might have been viable. I noticed that everything was presented in terms of grade, tons of ore, maybe mineralogy. For decades, the previous top management at Homestake were geologists, which would of course had been McLaughlin and John Gustafson, and then Paul Henshaw, all PhD geologists. I guess to a geologist what matters most is the mineralogy, the grade, and the potential tonnage. So I commented, “You know, what’s extremely important is metallurgical recovery.” At the same time, I saw that the operating reports given at board meetings did not give metallurgical recoveries from the operating mines, and I said, “Well, you know, I really think it’s important that we be given the information on what the metallurgical recovery is at operations since the expenses include all the ore that had been mined.” And from then on, recoveries were always given in reports. You would maybe know from your young days, from your father and all your years with Langan, that metallurgical recovery is a big key. Sometimes—let’s say over there in Australia—recoveries might run 90 percent, but sometimes they dropped to 89, 88 or less. Okay, all that gold has been mined and what has happened? The cost per recovered ounce of gold has gone up proportionately to recovery. And so by managers seeing monthly recoveries in their own internal reports, efforts would be made to get those recoveries back up. At Lead, they had beautiful recovery of gold—it always was around 93 percent or more, just wonderful.

At times I did participate as a consultant to Homestake, not as a consultant to the board, but to the company. For example, I spent some time involved with the early development of the flowsheet for the McLaughlin operation, the planning for it and the design, and met with the people over there in Denver. And at that time, the test work was being carried out at Hazen where they had run some pilot-scale tests. Before that some initial bench-scale tests had been run by, I think, A. H. Ross, a gold consultant in Toronto. All of this pointed out that without autoclaving the ore, the recovery would only have been something like 50, 60 percent, because the gold was very finely encased within the pyrite and not available to the cyanide leaching solution. But by autoclaving it, oxidizing the pyrite, you expose that gold. So I participated in discussions over this potential process there in Denver with the Homestake people. Homestake’s head development metallurgist at the time was John Ransone. A major consultant on the project was Oscar Tangel, who had retired as vice president of metallurgy from Newmont. He would have followed [Frank] McQuiston, or maybe worked for McQuiston at Newmont. He had been at Montana School of Mines when Malozemoff also was at Butte. Tangel had been involved with the construction of lots of plants for Newmont, and so on, so he really delved into the McLaughlin autoclaving process. By the way, I first met Tangel when he was at Battelle, when I visited them over a project that they were working on with Union Carbide.

And then, of course, Langan [Swent] was VP of engineering when I first joined the company, the board, but Langan moved—had he retired yet? No, he hadn’t retired yet.

Swent: He moved into the environmental—

Fuerstenau: He was environmental VP, right, and environmental became very big, as you well know. Rex Guinivere was then hired as vice president of engineering.

So then a decision was made to test the potential process further because the autoclaving tests were giving recoveries of, like, 90 percent. Of course, this was now oxidation autoclaving—quite new in those days. The decision was to go to Sherritt-Gordon, who had put in the first autoclaves for treating copper-nickel-cobalt sulfide concentrates up near Edmonton, and so we flew up there, several of us, to discuss conducting a detailed pilot-plant test program on autoclaving the ore. Sherritt-Gordon undertook a year-long pilot-plant testing program on refining conditions for oxidizing the McLaughlin ore by autoclaving it with oxygen. This autoclave process for treating refractory gold ores, as you well know, was an innovation that was first brought into real production at the McLaughlin Mine. Except for certain glitches, it really worked like an absolute charm. It was a very major innovation in gold milling. So, as I said, I participated in discussions and planning of progress on that development. That would be separate from board kinds of activity.

On another occasion, plans were underway to expand and redesign the crushing and grinding circuit at the Fimiston mill that processed ore from the Super Pit, and also ore from the Mt. Charlotte Mine. On that trip, I visited the Argyle Diamond Mine of CRA, now Rio Tinto, near Kuninuura up north near Darwin, where they had a Schoenert high-pressure roll mill and were in the process of installing a second one. The two roll mills are part of their secondary or tertiary crushing circuit there. At Argyle thirty-five thousand tons of diamond ore are processed a day. This ore is extremely hard and CRA had done a lot of research on the design and composition of the steel used for the shoes that encased the rolls. These shoes took the wear and could be replaced fairly rapidly. The manager told me that 7 percent of the Argyle diamonds are gem quality, and another 15 percent are small gem quality stones that they sell to India for jewelry making, while all the rest are simply industrial diamonds. The gem-quality stones virtually pay all the costs of the operation. The manager told me that they were extremely pleased with the performance and flexibility that the roll mills gave them in their processing. By the way, I was able to go through that part of the plant as far as where they used heavy media vessels and cyclones to separate out the heavier fraction in the crushed ore, but they would not let me tour the section of the plant where final diamond recovery takes place.

As part of that trip, I also went to Kalgoorlie where a pilot plant high-pressure roll mill was being tested on the ore there. They had a pilot unit from KHD that put out a huge cloud of dust when ore was being ground. I think that the dust cloud turned them off. Anyway, the decision was made by the technical staff in Kalgoorlie not to go the high-pressure roll mill route. Instead they installed the largest SAG mill built up to that time, one that had troubles with

bearings for quite some time. By the way, today, KHD solved the wear problem on high-pressure roll mills by attaching tungsten carbide studs to the roll surface. Between the studs, ground ore packs in tightly and that acts as the roll surface. I would like to add that because of the greater energy efficiency of high-pressure roll mills, they are now being installed in many mining operations around the world. Over the last fifteen years, we have published a number of research papers showing the energy savings that can be achieved with high-pressure roll mills, compared to ball milling, especially with the roll mills in combination with ball mill grinding.

There was one other occasion in Homestake where high-pressure roll mills were considered. The grinding system at McLaughlin involved a SAG mill followed by a ball mill. In most SAG mill systems, very hard pebbles accumulate because they simply are not crushed or broken in the mill. So these must be removed from the system and crushed separately. Rex Guinivere arranged for a test to be done by Polysius with one of their portable pilot high-pressure roll mills, one that had rolls about two feet in diameter. The hard, rounded SAG mill pebbles were about two or three inches in diameter. When the test was run, the pebbles shot out of the mill about ten or twenty feet straight up, and we all walked away in a minute or so. It was absolutely silly of the Polysius people from Atlanta to try to use that small-scale mill on large round pebbles, because there is no way such pebbles could ever be nipped and crushed with rolls of such small diameter. By the way, today Cleveland Cliffs successfully uses high-pressure roll mills to crush SAG mill pebbles in their iron ore plants.

Sued by the Oglala Sioux

Fuerstenau: Another board incident that I'd be interested in telling you about is [looks through papers]—I don't know whether I mentioned it, but I ran onto this—I got sued for \$500 million. The Homestake board was sued for \$6 billion by the Sioux Indians. This was 1982 [continues to go through papers]. Here's—I found a copy given—[reads]: “The United States District Court, Northern District, California. The Oglala Sioux tribe of Pine Ridge Indian Reservation is the plaintiff, versus Homestake Mining Company and Paul Henshaw, Harry Conger, O. J. Salisbury, Clifford Heimbucher, Wallace Macgregor, Leonard Marks, Berne Schepman, Douglas W. Fuerstenau, John Kiely, Don Delicate, and Donald McLaughlin, in their capacities of said company.”

Anyway, the suit was here in California, and transferred back to South Dakota, where fortunately it was eventually thrown out. But the suit was over the fact that—

Swent: What was the date of the suit?

Fuerstenau: The date was January 28, 1982. Yes, filed under Clerk of Court stamped with that date.

Anyway, as you know, the Black Hills had been ceded to the Indians under treaty, because they considered it, as we always heard living in the Black Hills, holy ground to them. As you well know, Bear Butte was one of their really holy symbols, and still is.

Swent: Paha Sapa. [Sioux word for holy place]

Fuerstenau: True, true.

[Tape 39, Side B]

Fuerstenau: In the Fort Laramie Treaty of 1868, the federal government made the Black Hills a permanent part of the Great Sioux Reservation, but, you know, in 1874 [George Armstrong] Custer made his expedition through the Black Hills, discovered gold, and because of this Congress changed its mind and took the land back. Before 1980, there was a Supreme Court decision for paying I think \$105 million to the Sioux Indians for the Black Hills, but the Oglala Sioux disputed it and didn't go along with it, so they split off from the other seven Sioux tribes and refused the money. Now they were renewing the part of this suit, for land that they considered, as we said earlier, sacred. Remember, they wanted—I see here \$10 billion. They wanted the Black Hills back. How to get the Black Hills back, you know, is an impossibility.

Swent: How did you feel about it?

Fuerstenau: Well, you know, in my old age, I really feel more in line with the Sioux Indians than I would have a lot of years ago. Maybe twenty-five years ago we spent a half a day driving around Custer Battlefield, and the whole thing was oriented towards the white man. I have not been back since, but I gather the battlefield memorial has been redone to show that there are two American sides to the issue, and so it has completely changed. I don't understand why the Sioux Indians don't say, "Hey, any of the land in the Black Hills that's U.S. federal land, such as Wind Cave National Park, should be ours." To me, that would be legitimate. Instead, you go down to Pine Ridge and see that wonderful—actually pitiful—land down there that they've given them for a reservation. You know, there's a lot of the Black Hills that's actually national forest and so on. Why isn't that given back to the Indians?

[Added by Douglas Fuerstenau during editing: Recently there was a long article in the *Smithsonian* magazine that commented about how the Black Hills was an area going way back as sacred to the Indians. However, I think that the Sioux lived in Georgia and moved west after that area was settled. So their time in the region seems more like three hundred years and not a thousand years.]

Swent: What was the disposition of this case?

[tape interruption]

Fuerstenau: Now, in a newspaper article it says [reads], "The end came when justices, without comment, refused to hear an appeal from a lower court ruling that limited the Indians to cash compensation rather than the land itself." So that was the end of the case. But it had to be handled as a real legal case. But anyway, I can say I was sued for five hundred million dollars! [Laughs]

Swent: Of course, as a board member you're protected, aren't you? You can't be personally liable for that.

Fuerstenau: Right, and there's board member insurance that all board members have because you can be liable to suits for your decisions. And this is where difficult matters have to be handled with full consideration. You know, I kept records of my notes of compensation committee meetings in case anything came up on these matters. I've destroyed them all upon retirement, but I remember Wally Macgregor saying that he had been on the board of Natomas, which started with gold dredges and then found oil in Indonesia, as you know. Anyway, through their Indonesian oil find, Natomas became a fairly big oil company, which was later taken over by Diamond Shamrock. So Macgregor was then on the Diamond Shamrock board, and Wally told me that somebody else took over Diamond Shamrock, in Texas, and Macgregor was flying almost every week to Dallas or Houston on court matters in a suit regarding the takeover, and particularly I think on compensation committee matters.

So anyway, he said he had to go look up his own notes of what he said and what his opinion was all during the merger negotiations. So I always just hung onto some of these in case somebody brought a stockholder suit. Anyway, as I said, I destroyed all that when I retired.

But you probably learned from your discussion with Harry Conger, the flurry that came when—[T.] Boone Pickens sent a letter to him about proposing to do a takeover of Homestake, and, of course, that created a flurry of meetings with investment bankers. The decisions that are made and the discussions that go on during such times really become critical if things end up in court, so I kept that in mind, especially after listening to Macgregor.

Swent: Were there any decisions that you made that you would make differently today, or that you regretted?

Fuerstenau: No, not really. No. I'll tell you, I'm glad that I had retired and didn't have to be there for discussions about shutting down the Homestake Mine. During my latter years the price of gold was in the \$280 range, and that entered into all considerations. You naturally have to look at operating the mine as a business and say, "Look, it just no longer is economical and there's just no way to go

ahead.” But when something was such a status or symbol for the whole western part of South Dakota, where one grew up, it was difficult to have it come to an end. I’d hate to have participated in discussions over closing the Homestake Mine.

But, you know, the Homestake board is a very congenial environment. Discussions were never heated. I remember when I first got on the board, Ted Marks said, “You know, the thing about being a board member is that you have a discussion, you can bring up different viewpoints, but this should be done with no animosity,” et cetera. This is exactly right. Finally everything is reached as a consensus. Sometimes issues were sent back for further study by management and brought back to the next meeting or something, so the board as a whole did have that kind of input on matters. And sometimes a certain person might have to abstain just because of a slight conflict or former conflict. That hardly ever happened, but of course could.

On all major items consensus was reached, but it was reached after discussion. The input of some people was obviously larger than that of others. As an example, one board member who always had a lot to say, and with good advice backed by lots of experience, was Berne Schepman, who you know. He was an engineer who had been president of Envirotech, which manufactured and built waste water treatment facilities and dewatering systems for mineral processing plants. Eimco was a subsidiary of Envirotech as you know.

An acquisition during my last months on the board was Argentine Gold, a small Canadian company that had an interesting gold prospect on the Argentina-Chile border. Barrick had an adjacent prospect. As everything developed, that led to the merger of Homestake into Barrick, and the disappearance of the great mining name of Homestake, and I am glad that I had retired so as not to have been involved in voting on those matters.

Let me just say finally that I was, like, forty-eight when I joined the board, and some time after Harry Conger became CEO, the board established a rule that board members retire at age seventy. Finally all of a sudden I hit the seventy. Bull Durham and I both hit seventy that year, and several people proposed that they raise—which I’m very pleased that they weren’t looking at getting rid of me—that they raise the retirement age to seventy-two. Board members voted on this, I gather, individually. But I think the correct thing was arrived at that, namely that retirement at age seventy is really a good idea, and so I was put to pasture. But some of my colleagues, our colleagues, wanted to raise that a couple of more years. I took that as a compliment.

Swent: But you had served a long time.

Fuerstenau: Oh, a long, long time, right. By the way, during many of my years with Homestake, I learned a lot about the Homestake Mine itself from a very

intelligent nephew, Fred Raubach, who worked there as a miner when I joined the board. Probably once or twice a year, I stopped through Rapid City to visit my parents. My dad died in 1989 and my mother in 1995—both had long lives. Fred had always been close to my parents, so probably each time I was in Rapid City, he came down from Lead. For many years he was a contract miner and I talked a lot with him about mining. As I recall, there were three or four miners on each crew, and two crews. The miners were exceptionally careful about who they added to a crew, not only their own but the other crew who worked in the same stope on the other shift. They had to be certain that each knew his job well.

One time two experienced miners were killed when a huge slab from the roof fell on them. We had heard about this at the board meeting. Some time later I was in Rapid City, and Fred said that the miners had talked at length amongst themselves about what had happened, and they concluded that the two miners themselves were at fault simply because they had not thoroughly examined and barred down the roof of the stope.

I also learned a lot from Fred about how the miners viewed the competence and general approach of the upper level mine management. Each manager would have his approach to handling people and problems in relation to his personality and background, as you well know. Although contract mining paid well, it was extremely hard work, and eventually Fred transferred to the maintenance department and eventually to mine safety.

One summer Peg and I were in the Black Hills, and we had a superb underground tour of the Homestake Mine with the assistant general manager, the underground superintendent, and my nephew Fred. We visited a couple of operating stopes and got down to the 7,200-foot level where the huge refrigeration units were installed to cool the mine. At that level the rock temperature is 134 degrees Fahrenheit. There were drill holes down to the next level, capped with a cover. You could really feel how hot the rock was when you put your hand down into that hole after the cover was removed.

And I just want to say one more thing. My years with Homestake really helped create a whole second aspect to one's career. Well, I had chosen being an academic as my main career since I wanted to stay active with students, research, et cetera. I, as I told you earlier, turned down positions as dean at other institutions and research director in companies to have continuing involvement with students and research, patterned after the career of Professor Gaudin, whom I admired so much. However, I did have extensive involvement on government panels and committees, professional society activities, editorial boards, consulting—all of which generally involved advising, decision making, and solving one problem after another.

To me, my association with Homestake was a great add-on to my life. I value the many friendships that resulted from my years on the board. It provided an

opportunity for close association with people at the corporate level and involvement with important decisions in the mining industry. I used the same experience and approach for discussing and helping advise on problems at Homestake that I did in all my other activities, and I mean more than technical matters. As you know, most companies will have an academic or two on their board, although they're often a university president—but I know of several who are not but are very distinguished faculty members. An academic can bring other approaches to the table.

In my opinion, one really has got to do homework for a board meeting, and I did, and I think everybody else did. That involved study of reams of material. But earlier I told you that I always spent hours preparing lectures for classes—the same preparation was extended to preparing for a board meeting. Since board members must do their homework ahead of time, I often wonder about some of these people that are professional board members, like Frank Carlucci, who used to be defense secretary under President Reagan. I read that he's on fifteen boards or something, or Vernon Jordan, who has just written a book, I saw an interview on TV, called *You Can Read* or *Vernon Can Read*. Just out. I haven't read it, but Vernon Jordan's also a career board member, a dozen boards. The *New York Times* had an article about the woman who was president of the CUNY, City University of New York. She used to be head of the California State University system. They fired her, and then she went to CUNY. She was on seven boards while being head of the City University of New York. How can you do that and your job too—given what I saw and the homework I would do for this. I think some companies now limit their presidents to a couple of boards because of the time involved in such service.

Swent: I'm sure you were an extra-conscientious board member.

[tape interruption]

Swent: You might just kind of review the progression of the board in your tenure there.

Fuerstenau: I recall very early on that Don Delicate actually got up and discussed the need for two new Xerox machines at Lead, so it shows you the kind of thing that might have entered the discussion way back then when gold was selling for something like \$40 an ounce. But as the years went by, board matters rapidly changed more and more to strategic planning issues, broad issues, and the financial authority of the CEO went up with the progression of time, to where he could make a decision of \$5 million, without needing board preapproval, or something like that, towards the end of my time. But at first practically every issue came to the board. To me, that talk about the need for two Xerox machines did show you the closeness with which things must have been looked at in 1977.

Swent: Yes, compared to twenty-some years later.

Fuerstenau: Oh yes, yes. Everything just grew, and particularly so as Homestake grew by acquisitions. Management reports discussed not only performance but longer-range issues. Always at the top of the list on operation reports was safety—to Homestake, safety was of major concern. Environmental matters were always presented and discussed. Of course, another topic of broad interest involved exploration, which is the bread and butter for maintaining and increasing ore reserves in any mining company, something that we talked about at length earlier.

Swent: And now the company is being sold.

Fuerstenau: And now it is being sold. Again, I am glad that I had retired and did not have to participate in that decision. In one sense, the combination of Homestake with Barrick has apparently some real pluses. I did ask Jack Thompson, current CEO of Homestake, whether there would be any way the name could live on, and he said he had actually discussed this with the Barrick people. As you know, there's Exxon-Mobil and there's now the newly merged company Chevron-Texaco is now finally approved. So could there be a Barrick-Homestake? And frankly, I would think that Homestake in the name would be a big plus, but Jack said that it appeared that that wouldn't be the case, that the Barrick people are so happy with what they have accomplished in these few years that they just want the name Barrick.

[Added by Douglas Fuerstenau during editing: Over the years between the time of my being a student at the South Dakota School of Mines and Technology and today, I was either aware of or involved with many changes that occurred at the Homestake Mine over a third or nearly half of its 125-year history. When I toured the Homestake mill in 1948 with Frank Aplan, stamp mills were still the final stage of comminution. Homestake introduced the carbon-in-pulp process for gold recovery from the fine-ground fraction after a rod mill/ball mill circuit replaced the stamp mills. During my years on the board, mining went through conversion to bulk mining underground and then to more selective mining as the price of gold dropped. Mercury amalgamation was used to capture the coarser gold particles, but the use of mercury had to be discontinued. Shortly thereafter the total recovery of gold decreased because the coarser gold particles were now not fully dissolved in the leaching vats. The mill metallurgists then proposed and incorporated a gravity system of Reichert cones into the milling circuit to recover the heavy particles, followed by passing the heavy concentrate over a shaking table. The band of gold on the shaking table was great to see. Something like 52 percent of the gold in the ore was recovered in this manner. A major change took place in the early 1970s when the tailings could no longer be discharged into Whitewood Creek, but were impounded in a dam at Grizzly Gulch. Since the mine produced excess water, water had to be discharged and this let a certain amount of cyanide go out in the stream, a real problem. The development of the bacterial system for cyanide destruction was a major innovation that solved that problem. The final development at Lead during my time on the

board was the planning and decision to mine the open-cut. That contributed significantly to the operating life of the Homestake Mine during those final years of low gold prices. Interestingly, at one board meeting, Wally Macgregor made the comment that that deep hole in the ground should eventually have some value. With the establishment of the Deep Underground Scientific and Engineering Laboratory at the Homestake Mine, Macgregor's comments may come to be.]

So regrettably the company's name will disappear, but hopefully the name Homestake will apply to that deep physics research facility that has been proposed at Lead. Are you on the list of that South Dakota School of Mines alumni publication? From the alumni director, Tim Votterro?

Swent: I get some things from there, yes.

Fuerstenau: Well, there was something that he sent about Homestake; I'll tell him to put your e-mail address on his list since this was an e-mail. Apparently Thompson met with [Senator Tom] Daschle, in Daschle's office, just days ago, and apparently Homestake or Barrick finally signed off on things related to Barrick's environmental liability. There was something to the effect they will finance liability up to \$75 million for past environmental potential problems. That was in the Rapid City paper. The state of South Dakota is interested in this project and is becoming actively involved with a financial commitment. Of course, the long-range success will be determined by the willingness of the National Science Foundation to support such a facility. When everything does proceed, I trust they'll continue the name as the Homestake Deep Laboratory or Homestake National something.

Swent: Oh, I hope so. I hope so.

[Added by Douglas Fuerstenau during editing: On July 10, 2007, the Homestake mine was approved by the National Science Foundation to be a national underground deep laboratory. This means that this great old mine will have a new life mining scientific data. The principal investigator for the Homestake project with regard to NSF is Dr. Kevin Lesko of LBNL here in Berkeley. This is the Deep Underground Science and Engineering Laboratory, with the acronym of DUSEL. In addition, to the funds from the State of South Dakota and initial study funding from NSF, T. Denny Sanford, the philanthropist from Sioux Falls, gave a major grant of \$70 million towards establishment of the laboratory, and the specific facility has been named the Sanford Underground Science and Engineering Laboratory. In 2007, my nephew Fred Raubach was part of a group who went underground and he commented that the shaft and the mine is in really quite good shape after this extensive period of not being maintained.]

[tape interruption]

Swent: All right. So they did stick with the seventy-year limit, and you went off the board.

Fuerstenau: Yes. And so I—well, I keep my interests there, but basically—

Swent: And we're hoping that they can keep the name Homestake for the neutrino laboratory. That's where we were [before the tape interruption]. We had just gotten up to that. Now we're concerned about their papers.

Fuerstenau: Their papers.

Swent: Their papers, yes, yes.

Fuerstenau: My papers are piles. [laughs heartily] You wanted to talk about family.

[Added by Douglas Fuerstenau during editing: Homestake Mining Company had a quieter ending than it did for its beginning. In a letter dated May 28, 1876, from Lead City, Dakota Territory, to one of his two partners, J.B. Haggin in San Francisco, George Hearst wrote: "The survey made when I first looked into the matter aroused my suspicion that there must be something wrong, and I commenced inquiry and finally dropped on a man that of course the survey is wrong—if we succeed in finding out the fraud and maintain our rights there would be more squealing than ever was heard of before. And it is quite possible that I may get killed, but if I should, I can't but lose a few years and all I ask you is to see that my wife and child gets all that is due them from all sources and I am not buried in this place."]

XXV

FAMILY AND OUTSIDE INTERESTS

Margaret Pellett Fuerstenau

Swent: I think maybe now it's time to move on, to make some personal observations. Peggy [Margaret], of course, went on a lot of these board trips with you. Not just those trips but a lot of the others too.

Fuerstenau: Oh yes, she has traveled a lot. On some of the board trips—the one to Australia was one they had all the wives, including VP wives; Langan had retired by then, unfortunately, right? I remember Ray Beebe and his wife were there, and so on. While waiting in the airport in Los Angeles to fly to Sydney, I found it kind of interesting that when you looked at the people—I'm speaking of all these board members—for riding on the airplane, they were basically dressed about like they might have been farmers or loggers. Now you can never tell by their dress on an airplane who a fellow traveler might be, because often people today just travel in very leisurely outfits.

Swent: That's right. Times have changed.

Fuerstenau: But anyway, Peg has gone on many, many of the trips with me. Three times we have been on extended trips to China, twice to Japan, and also Brazil, Egypt, India, and South Africa. Plus we've lived overseas—well, in England but several times in Germany and in Rome for a bit too. So that's been obviously to her a very broadening experience and a good part of her life. She graduated from Radcliffe with a major in American history, got a master's in library science, I may have said, from Simmons [College] in Boston. Never worked other than one year; my last year at MIT, she worked at Watertown Public Library as a reference librarian. I say, she never worked. [laughs] But she has been a very good mother to all our offspring and a real mate to me.

She has spent much time in volunteer work. For over twenty years was a docent at the Oakland Museum [of California], both natural history and California history, two different areas. And she has been involved with church committees and some Berkeley programs—not Berkeley politics but certain things—and a faculty wives association called the Section Club. In fact, she's I guess next year going to be treasurer, and that, I understand, is a busy job. She's serving as the assistant treasurer this year. We've now been married forty-eight years as of this year.

As you may have gathered, I am one of those who works almost all of the time—maybe not all of the time but a lot of it. One can never say too much about a wife who understands and puts up with that. She has contributed greatly to my life, both personal and professional. The time that she spent on behalf of students and professional visitors must add up to months. I really am

fortunate that Peg married me—our life together has been really great—outside of the tragedy around our daughter Linda.

Lucy Ann Fuerstenau Cliby, Grandson Cole Winston

Fuerstenau: Our three offspring all live in California. Our oldest daughter, Lucy, is a nurse and very outgoing and very up. Boy, you talk to her on the telephone and she's the most enthusiastic person you ever heard. But unfortunately, she's got MS [multiple sclerosis], and I guess she gets tired from it, and so on. [sighs] She has three horses and loves horseback riding. A year or so ago, she found that she was numb in her legs, and that made riding a horse not very easy, but she still works as a nurse, but it's—

Swent: Wasn't she doing emergency nursing?

Fuerstenau: She's in emergency. She likes that. She's a take-charge person. You take care of the patient, and they're gone, whereas for a year or something she worked in a burn ward, and she said that's terrible. You see the same painful people every day. It gets on you. In the emergency room, you take care of what's going on and that person goes, and you're on to the next one. It's not busy all the time, but at times it's hectic, I guess.

Lucy always had an interest in becoming a nurse. She went one year to UC San Diego when she decided to pursue nursing. She then came back to Berkeley and took the nursing prerequisite courses at Merritt Community College—chemistry, biology, et cetera. After two years there, she was accepted into five state university nursing programs, and decided to accept going to Fresno State—so she has BS degree in nursing from California State University at Fresno.

As I say, we have one grandchild, who's now seven, which is her son. She and her husband Paul live in Coarsegold, about twenty-five miles north of Fresno. I once mentioned Coarsegold to Jim Anderson. He said, "You know, nearby is a Finegold." I've never been there, but on the map nearby is Finegold. That region was the very south end of the Mother Lode: Coarsegold and Finegold. Mariposa is another thirty miles north, maybe thirty-five, from where they are. Their house is in the Sierra foothills on several acres of live-oak covered hills. It works out quite well with their horses.

[Tape 40, Side A]

Swent: What is Lucy's name?

Fuerstenau: Lucy Ann Fuerstenau, now Lucy Cliby. And her husband is Paul. He is a fireman in Fresno. And her son is Cole Winston, named after me, she said.

Not the Cole, but the Winston. My middle name. Douglas Winston. She made up the name Cole from somewhere.

[Added by Douglas Fuerstenau during editing: Paul and Lucy are now divorced but Lucy still lives in her Coarsegold home.]

Sarah Jane Fuerstenau

Fuerstenau: Our second daughter, Sarah, who has a BA degree in landscape architecture from Cal—

Swent: Is that with an “h” or not?

Fuerstenau: Yes, she’s got the old-fashioned Sarah, Sarah Jane. She went to the University of Oregon to do a master’s in landscape architecture. For five years we were paying for it. [chuckles] That program she never did finish, but she got a bachelor of landscape architecture from Oregon. Something is wrong there with the program and the faculty. I was actually going to write to the president and say, “Hey, these people are not doing it right,” because Sarah said a very few ever completed the master’s program, due to the faculty. She was working on a thesis topic, and one person, one member would say, “Do this,” and she would go to the next committee member: “Oh, you don’t do that. You do this.” It ended up she never did finish it. She went down one summer to Long Beach to get some help in the final writing of her thesis. Sarah had gone one year to Santa Cruz, and her freshman roommate later had gotten a master’s in some kind of writing at USC. So Sarah was getting some writing help from her.

She took a part-time job in the accounting department of Pioneer Entertainment, a Japanese company, and she found all sorts of errors that the accountants had been making. She was filling in while they were on vacation. Her boss said, “I want to hire a new accountant. The ad says two years’ experience, but I want you to apply.” Well, she got the job, so here’s somebody with no business background—a landscape architect. She still loves plants in a big way. After a while, I know she had an MBA working for her, and she was a little upset that he was making more money than she was at the time. I suppose that was standard. Anyway, she moved on up in the system and became manager, I think, of Karaoke—Karaoke is that Japanese singing along with DVDs on a television set. She’s not a musician, but would select music, write the contracts necessary for these, and negotiate contracts, things like that. Then I think she became manager of budgeting, and now she’s post-production manager for DVD disks for music for them. She’s very bright. Works hard. I mean, she really is dedicated. She’s taken, I think she said, eight extension courses at night on entertainment economics, stuff like that at UCLA. She has done that through the years, so that she obviously knows her field. She would like to come back to the Bay Area, but she’s in the kind of

field where the main activity is down in Los Angeles, where the entertainment capital is. Looking back, somebody turned her off in about the fourth grade on math, and she had no use for it—to me, it’s interesting how she got started in accounting without any background in it.

[Added by Douglas Fuerstenau during editing: In 2004, Sarah bought a house in Glen Ellen, near Sonoma, and found a position in Petaluma. About two years ago, she met Scott Walker and they were married August 25, 2006. Sarah’s neighbor, an architect who worked on the remodeling of Hearst Mining Building, is a volunteer fireman in Glen Ellen and introduced her to Scott, a fellow volunteer fireman there.]

Stephen Douglas Fuerstenau

Fuerstenau:

Our son, who was the youngest, Steve, Stephen Douglas, the old-fashioned spelling of Stephen. Before our first daughter was born, we had picked two names, Linda Margaret and Stephen Douglas. So when Steve was born, his name was already there. When Steve approached going to college, he was interested in engineering, and I suggested to him that chemical engineering might be the broadest field to study, and he did this and majored in Chem. E. down at UC San Diego. He took a lot of German in high school and we, as I told you, had spent a sabbatical in Germany and had put our offspring into German schools. Steve had become quite fluent in German. Anyway, he told me as a freshman he signed up to take German II, and I said, “Hey, you’re nuts. You should take German I.” I think he got an A in German II as a freshman. So in the end, he had a full minor in German literature, as a chemical engineer. He spent his third year abroad at Göttingen, which was the UC campus abroad for Germany. Obviously he became completely fluent in German then.

When he was in his final semester at San Diego, somebody from Yale gave a seminar, and Steve asked a lot of questions. This person (Juan de la Mora) was talking about particles, and, of course, Steve knew about particles from me and from his experiences in Germany, so the person asked an assistant professor at San Diego that Steve was working for part time who this young guy was. So later, de la Mora came by and talked to Steve, and said, “Do you have a job?” And Steve said, “Well, no, I haven’t even started looking”—he was a senior. He hadn’t even started to think about it.

So he said, “Why don’t you come back and work for me for a while, the summer?” So Steve went back to Yale and worked for him as an employee, as a lab assistant on some research project. He told me he was auditing a couple of the graduate chemical engineering courses and doing the homework, and that he was even writing the midterms, the exams. And he said he was doing okay. And I said, “Well, hey, there’s your meal ticket.” And so that next fall, with the not very good GPA that he had because he didn’t study very hard at

San Diego, he was admitted to the Yale PhD program with a full fellowship. But he did it by the route I said. So he eventually got a PhD in mechanical engineering and chemical engineering. It's a fairly small engineering program there at Yale. So he ended up working with a very senior, elderly, retired professor as his nominal research advisor, named John Fenn, who had developed a field called electrospray. Electrospray involves spaying a solution into a flame after it has passed through a very fine nozzle to which a very high potential is applied, five thousand volts. So you charge the little droplets as they form a spray. And then, let's say, if you've got something dissolved in the solution and it evaporates, a lot of those charges are left on the little crystals that are left behind. By passing the dried particles or dried molecule fragments into a mass spectrometer, you can accurately determine their weight. So Steve, if I recall, worked with DNA fragments and so on, and did this for his PhD thesis. And when he finished at Yale, he got a postdoc up here at LBL for a couple of years, where he continued working on this sort of thing. And he's becoming quite an expert in aerosols and mass spectroscopy, and has given a number of papers on this at technical meetings.

[Added by Douglas Fuerstenau during editing: John Fenn, at age eighty-five, was awarded the 2002 Nobel Prize in chemistry for inventing the electrospray technique that could be used for accurate determination of the molecular weights of large organic molecules such as proteins.]

Then Steve went to JPL, Jet Propulsion Lab[oratory] in Pasadena, where he's been involved with mass spectroscopy and design of apparatus for Mars missions—you know, related to dust and things like that, adherence of dust, and so on. His most recent work has been involved with the development of a device to measure the electrical charge on dust particles. A PhD geologist from the University of Nevada had studied dust devils for his thesis and pointed out from photographs that there are huge dust devils on Mars. So part of Steve's research involved chasing dust devils on a desert lake bed near Las Vegas to capture the dust particles and measure their charge characteristics. This might help explain their origin and their effect on apparatus they encounter.

Something else that I would like to comment on concerns the two rovers that are still operating on Mars, called Opportunity and Spirit. One of them reached the edge of a huge crater, and there were decisions to be made as to whether the rover could go down into the crater and climb out. To test this, the head geologist at JPL proposed building a sixteen by sixteen foot concrete slab that could be tilted. Instead of having to wait for the concrete to harden and not knowing the roughness of such a slab, Steve suggested that they go to Home Depot and buy a truckload of artificial garden stone, which they did, and put the stone slabs and a little sand onto the sixteen by sixteen foot frame, tilted it to different angles and tested the performance with the third rover which they had there at JPL. They had a positive answer in short order. Of

course, any publicity of this did not mention Steve's common-sense contribution.

One time Steve commented about differences between LBL and JPL. According to him, LBL seems to be basically more run by physicists, whereas JPL is run by engineers, and he felt that this basically leads to a different work environment that you're in. For several years I was a member of the Scientific and Education Advisory Committee on Lawrence Berkeley Laboratory, reporting to the UC president's office. So I have been quite aware of the operating philosophy at LBL. That provided me with a comparison of my observation of LBL with Steve's comments about JPL, which is mainly a mission-oriented facility.

The Cabin at Pinecrest for Play and Hard Work

Fuerstenau: We have a cabin that we bought in 1965 at Pinecrest, which is on Sonora Pass, about thirty miles above Sonora. At Pinecrest there are about four hundred cabins on land leased from the Forest Service. So we own the cabin but the property is on recreational forest lease land. Camp Blue, Camp Gold, which are large Cal alumni camps, are right there across the highway nearby. Pinecrest Lake is a PG&E dam, quite small, about five miles to walk around it. A beautiful place. Elevation: 5,600 feet, so just lovely climate. Only a couple of miles on up the mountain is a ski region called Dodge Ridge. Oh—and as for weather, sometimes there may be six feet of snow around the cabin—picture-book snow. We always paid to have snow plowed so that we can park our car. We've gone there regularly all year round. I work all the time, or used to, and I consider Pinecrest being really great because it was great relaxation for me. Sometimes early on it would almost blow my wife's mind when I'd take not one but two briefcases, but then I might not even open them. But most important was that I played games with my kids there. Lucy was only six and Steve only two when we bought the place. If I were here at Berkeley all of the time, I probably wouldn't have done that much with them, and so Pinecrest has really been a big thing in our lives.

Steve was always out looking for things and doing things. He told Peg one time early on, "I can just smell the different seasons up here." That's when he was little. And he'd catch tree frogs, small snakes, butterflies, and so on.

Swent: That's wonderful to have that.

Fuerstenau: Oh, it really was. Early on, my wife would say, "I don't know why we come up here. All you do is work." And I'm now speaking not about professional work, but about carpentry, et cetera. When winter would bring heavy snow like that, there can be a lot of damage so in the summer I would be reshingling or doing this and that. You know, I could physically be up there on the roof pounding nails and cursing the dean.

- Swent: [laughs]
- Fuerstenau: That is true. [laughs]
- Swent: Good therapy.
- Fuerstenau: Good therapy, whereas if I'd go down and lie on the beach—then my stomach would kind of turn.
- Swent: Yes.
- Fuerstenau: Whereas Peg'd rather lie on the beach then. I always fish a lot. I don't know whether Peg really likes it or not, but we fish in our boat and troll. Peg usually goes with me on that. My son, Steve, is a very ardent fly fisherman, but I'm too intense to enjoy fly fishing. I can't just gently do the fly fish casting. But still after the nearly forty years that we've had Pinecrest, we still go there a lot. But, you know, I don't think we've hardly ever been there more than a week or so at a time in the summer. That is the dumbest thing I ever heard of. When I was traveling, Peg and the kids were there a lot.
- Swent: But you go up for short periods.
- Fuerstenau: Through the years, every three weeks, roughly, we'd be up for the weekend or more.
- Swent: And your children still go, too?
- Fuerstenau: Yes. And, you know, that is very calming because the kind of life I've led, always the tension builds up, and I could go up there and knock it down. Both in summer and winter we would go there—well, summer we'd be there more, but every three or four weeks all year round we would go to Pinecrest, maybe stay there an extra day. We would spend Thanksgiving here in Berkeley but go up to Pinecrest on Friday and be there for the rest of the long weekend. And then usually right after Christmas we'd be there for a week or ten days.
- Swent: Are you skiers?
- Fuerstenau: I quit downhill a long time ago; I started skiing too old in life. One year I never fell down. If you don't fall down, that means you're too cautious. But the last time I went downhill skiing, my son was with me, Steve. He was only twelve at the time, I think. I fell down, and he stopped to see me, and I said, "I'm getting up. No problem." And a second or two later, I heard somebody crying. He'd gotten thirty feet away and turned incorrectly somehow and had a spiral break on his leg. The doctor said if he had been even six months older, he'd have been six months in a cast. Now Steve's a fearless skier. He will go out with mountain cross-country skis. That means cross country skis that can

act as downhill skis, like a fool. He'll go out—you know, with a mountain tent, go skiing all day, and stay out overnight. I can't believe that that is fun.

So we reverted to cross-country skiing. I haven't done it now in a couple of years. Peg, with her hip replacement, probably will never go again. But we were skiing on little forest roads. They're snow covered, so they make nice places for cross-country skiing. You can see that Pinecrest has been a big, big plus to our life, and our kids still go there—I think Sarah enjoys it the most. But they're down there in Long Beach, and Steve is down in Pasadena, so it's a long drive for them up to there. They've got to plan being there for longer time periods. Lucy will spend a week or something there with the kids in the summer. We've had Thanksgiving there twice, with everybody, two or three times in later years.

Another aspect of Pinecrest is that each fall for a weekend I have invited all of my grad students there for a weekend. The first time I thought that we would have seminar discussions. But that never happened. Great times were spent sometimes hiking higher up in the Sierras, fishing, playing volleyball, etc. Often Sunday mornings were spent doing some chores related to wood gathering, rock piling, digging trenches, etc. All meals were planned by a few of the students as a committee. Many of my former graduate students still talk of their weekends at Pinecrest being one of the highlights of their years at Berkeley. Also, many of my postdoctoral researchers and visiting scientists, et cetera, spent time with us up at Pinecrest. With many of them, we spent a day making the circle route over Sonora Pass and Tioga Pass into Yosemite and back to Pinecrest. The drive up to Sonora Pass is probably the most beautiful one over the Sierra. The elevation at the pass is 9,624 feet. On several occasions with visitors we drove over Sonora Pass to Bodie to visit the state park made of this early gold mining town, where cyanidation had its beginnings through the early efforts of Charles Merrill. He worked at Bodie before going to Homestake.

Pinecrest also offered interaction with other cabin owners, although our place fortunately is not very close to adjacent cabins. Through the years, we maintained close friendship with Tom and Doris Everhart. Tom had been an electrical engineering professor at Berkeley for twenty-one years and we got to know them because their children were all close friends of each of ours. They bought a cabin at Pinecrest and still have it. Everhart left Berkeley to be dean of engineering at Cornell, then chancellor of the University of Illinois at Champaign-Urbana, and then president of Caltech. All during that time, we would interact with them, often during the Christmas break and in the summer. Tom, who also liked to do maintenance work like I did, must have found Pinecrest to be relaxing similar my experience.

Photography, Mine Lamps, Opera, Theater, Woodworking

Fuerstenau: Photography was my first real hobby as an adult. I bought my first 35-mm camera the first summer that I worked in Chicago. I've now got thousands of slides. I've seen the world through the lens of a camera. I really don't know what's eventually going to happen to those slides.

Swent: That's a problem, what to do with all those slides.

Fuerstenau: Yes, yes. Isn't that right? And I've got so many Nikon cameras that I could set up a store in Nikon equipment. I decided to quit taking slides, and went to using a little point-and-shoot camera, and then I started getting prints, boxes of prints. Now I just bought a new toy, which is a digital camera. So I guess photography will continue to be with me.

I also started another hobby a few years ago, collecting antique mining lamps—oil lamps, candlesticks, and carbide lamps. Even my wife says maybe we better go and look in this antique store. I have old oil lamps and mining antiques, some a hundred, two hundred years old from Europe, lots of candlesticks, old carbide lamps. Carbide came into existence at the beginning of the twentieth century. By the way, the manufacturer of calcium carbide was Union Carbide. Before the invention of carbide lamps, candles were used to light mines in this country and in Cornwall. In Germany and other parts of Europe, they used oil lamps often called frog lamps because of their shape, that burned rape seed oil to light the mines. I've seen something from an old report of Homestake that one year in the 1890s forty-two million candles were bought for use in the mine at Lead—you know, which was at the time when they were using candlesticks. I've got one or two Homestake candlesticks. I have a nephew, Fred Raubach, who was a miner there at Homestake, and he said one time after he had transferred to the mechanical division on the surface that they came upon something like three wooden boxes full of candlesticks. And they all disappeared somewhere. I'd sure like to have gotten my hands on something like that—he had no interest in them. Ted Jordan, a professor at Montana Tech found three unused candlesticks at an antique store in Butte and bought them for me. I must have 150 different antique oil lamps, carbide lamps and candlesticks from all over the world. I don't know what's going to happen to them in the long run either.

Swent: What about your cartooning? Have you done any of that lately? You have a lot of talent.

Fuerstenau: Just on birthday cards. That really started with my sending away for a cartooning course when I was in the sixth grade. It cost maybe \$20 with the person behind it sketching improvements on drawings that you mailed in. I still have those original materials. My old lecture notes are full of cartoons that depended on the quality of the lecture. If the lecturer was very good,

there's not much there, but if it was a little bit dull, my notebooks were full of all sorts of cartoons.

Another great thing about living here in the Bay Area is the availability of theater and musical events—but I don't think very many of my colleagues take advantage of it to the extent that we do. We for years have had season tickets for Berkeley Repertory Theater, which has really grown to be very good, as you know. And there's a little theater called the Aurora Theater that we have subscription tickets to. By the way, they're moving this week into their new building. We're talking about half a dozen or eight plays that we go to each year, and they're right nearby.

And then we also go to San Francisco Symphony, on a short series, not a weekly one but a series of four or five concerts a year. But we do have the full season series for the San Francisco Opera, and we've done that for many years. Most of the time it's beautiful music done by wonderful singers. You can see that in the fall, when the opera season is on, our schedule can get pretty full. Then I'm saying, "What? We've got another one to go to?" Last Wednesday, this past Wednesday, we went to *Die Meistersinger*. We left here at four o'clock for dinner in San Francisco. The opera started at six-thirty and *Die Meistersinger* is long, five hours. It was over at about twenty-five minutes to midnight, and we got home at about quarter after midnight. It was beautiful. Before we went, I said, "Oh, it's going to be five hours."

Swent: But it was worth it.

Fuerstenau: Oh yes. And on top of all of that, I've got two almost complete woodworking shops, one here in Berkeley and the other which is needed up in the mountains at Pinecrest, to take care of things. So practically any tool there is, I got one or more of them. I can do the carpentry and I do some of the plumbing. Once in a while you get freezing problems at Pinecrest. I can't do anything major, but I do the rewiring at times, so I play electrician a bit.

[tape interruption]

Fuerstenau: We're about to finish up here.

A Look into the Future

Swent: I remember from your retirement dinner how impressed I was that you said that you had made good decisions and that you had followed the path that you had wanted to follow, which I thought was wonderful. But I wonder if there are things now that you wish you had made a different decision or done in a different way.

Fuerstenau: I guess my only disappointment is really the direction that I see this department going. Maybe those changes of directions are taking place all over because of the change in industry. I think that changes take place in most every field, probably.

Swent: You built the department up from practically nothing to a worldwide leader.

Fuerstenau: At least in my processing area, it is true. In the last years, we've had roughly a group of four people that have been in the overall processing side: Jim Evans in electrometallurgy and pyrometallurgy, high-temperature metallurgy; Fiona Doyle, who is the Donald McLaughlin Professor of Mineral Engineering, and who has basically been involved with hydrometallurgy and the aqueous processing minerals and materials, and Kal Sastry, who is in the mineral processing area, mostly involved with process modeling and computer simulations, that sort of thing. Plus myself, who has covered quite a number of the areas in mineral processing and particle technology and surface chemistry. Unfortunately Sastry has just never developed an ongoing research program. Over the years he had a few students complete master's degrees and a total of five their PhD degrees; the last person finished his PhD with him in 1989. However, Sastry does have an international reputation as being a leader in agglomeration and pelletizing iron ores and other metals. Unfortunately our mineral processing program will disappear at Berkeley with his retirement. Without an active program with a number of students, there's no way you can justify a replacement for anyone in the mineral processing area.

[Added by Douglas Fuerstenau during editing: In 2005, Fiona Doyle was appointed to be associate executive dean and associate dean for academic affairs of the College of Engineering. Prior to that she had very ably served three years as chair of the department. In November 2006, she was named acting dean of the College of Engineering upon the serious illness of Dean Richard Newton.]

But in contrast even in these days, there are several examples of successful ongoing programs. At Columbia University—Somasundaran, my former student, has built up a good program within a not very active situation at Columbia. He is a leader in the applications of surfactant science. Jan Miller, at the University of Utah, has a very strong program on mineral liberation, some hydrometallurgy, flotation, and surface chemistry, which is a continuation of what my brother really had got started there. Plus another former student of mine, John Herbst, was there for several years and established the Comminution Center at the University of Utah. Brij Moudgil at the University of Florida has this huge NSF Particle Engineering Research Center. At Virginia Tech, there is an exceptionally strong program started by Roe-Hoan Yoon with a group of about four mineral processing professors, one being Greg Adel who did his DEng. with Sastry here at Berkeley and had been an undergraduate at South Dakota School of Mines and Technology. Another example is Komar Kawatra, an Indian at Michigan Tech, who

obtained his PhD in Australia and came over here and has a strong ongoing program in mineral processing. Success in developing a leading program in specialized areas is absolutely dependent on the strength of the individuals appointed as faculty members.

But here, unfortunately, there's now very little. I continue to have one or two students, but I don't know how many years I will continue to do that not only because it takes energy and effort to finance research programs but also it is not good educationally if there are only one or two students in a research group. When I decide to take on no more students, then I think the program here will fully die.

[Added by Douglas Fuerstenau during editing: My last graduate student was Renhe Jia, who completed his PhD degree in 2003. He is doing exceptionally well working for Applied Materials. I concluded that a small group of one or two students is not really effective and decided not to take on any new students even though I continue to receive regular inquiries about coming here for graduate studies in mineral processing. With the move back into Hearst Mining Building, what had been an outstanding mineral processing laboratory and surface chemistry laboratory totally disappeared. Some of the surface chemical apparatus was acquired by others in the department, and most of the mineral processing research equipment was donated to the University of Utah and the University of Nevada. What wasn't taken was discarded.]

Swent: Is there no other institution in the state that has any comparable program?

Fuerstenau: None, none. You know, about twenty-five or thirty years ago, Stanford had cut out their program in extractive metallurgy. In fact, a very able person was Bob Bartlett in the extractive metallurgy area—in fact, superb. He was associate professor, but they made the decision not to give him tenure because they were going to phase the program out. A senior professor in pyrometallurgy, Norman Parlee, stayed but when he died or retired, they phased that program out. And another able person there in my general area was George Parks, a Berkeley undergrad and a PhD student of Phil deBruyn at MIT, who remained at Stanford where he continued to work on mineral-water interfacial phenomena as a professor of geochemistry and also environmental engineering. So the answer to that is: there's very little. Yet California is, production-wise, I think the biggest producer of minerals in the country, to this day, in the value of its minerals.

Swent: Of course, Maurie—

Fuerstenau: Well, my brother in Nevada. Yes, Maurie is a very good person, does excellent work. Very good, very dynamic. He has done many good things during his years at the University of Nevada and formerly at the South Dakota School of Mines and Technology—and a lot of great work when he was at the Colorado School of Mines. Jan Miller received his PhD under Maurie at

Colorado School of Mines. Maurie made a wide range of innovative contributions to flotation chemistry, solution chemistry, and to hydrometallurgy. I'm very pleased with his abilities and what he has accomplished.

[Tape 40, Side B]

Fuerstenau: Some years ago—and I think it was on the telephone that I had a conversation with Nat Arbiter, Professor Arbiter, who was born in 1911, which means he's ninety today. If you talk to him, he's still got that same old enthusiasm. I remember him saying, just two or three years ago, something to the effect that "I think we have lived through the heyday of our field." Which really means the twentieth century, because that's when all the real developments in modern mineral processing took place. And his role in it—active role probably was started in 1930, when he worked with Taggart at Columbia. Let's say roughly 1930 or so, till probably approaching 1980, because he was still active as a consultant then. So he covered a long part of it. By the way, the first U.S. flotation plant was installed near Butte, Montana in 1911.

[Added by Douglas Fuerstenau during editing: After I was elected to the National Academy of Engineering in 1976, the first thing that I did was prepare a nomination of Nat Arbiter for membership in NAE. At the same time, I also prepared a nomination of Hans Rumpf, who also had been born in 1911, for foreign associate of NAE. Both were elected, but unfortunately Professor Rumpf died that same year from a heart operation. It has always been interesting to me that Professor Gaudin, a founding member of NAE, never nominated any academic to the academy. Professor John Chipman of MIT who is considered the father of the physical chemistry of steelmaking was never elected to NAE. Interestingly, I have noticed that none of the MIT metallurgists and ceramic engineers were elected to NAE until after Gaudin had died. The great Professor Morris Cohen became an NAE member only in 1973, just the year before Professor Gaudin died. A prize grad student of Gaudin's at Butte was Reinhardt Schuhmann, Jr., and he was elected to NAE, only in the same year that I was, 1976.]

And my activities in flotation research were really intense during the last half century. I started as a grad student at MIT in 1950. Actually, my beginning in flotation research was in Butte in 1949. During my half century, I had a lot to do with bringing physical chemistry into the study of flotation, understanding the surface chemistry and the role of the electrical double layer in oxide mineral flotation and the electrochemistry of sulfide mineral flotation, and predicting conditions for differentiating the flotation behavior of different minerals. I think that my role was very major in that. I am very proud of my many graduate students who have gone on and made additional contributions to flotation, surface chemistry, and other aspects of processing.

And as we had talked about earlier, much of my efforts also were involved with particle technology and applications to mineral processing. For all these years, I kept a significant research program aimed at simulation and understanding comminution phenomena, grinding. I started that because it was easy to do experimentally and I always kept one foot in the door on that, and followed that right on through and still have a bit going on, mainly with studies of high-pressure roll mill grinding which had been initiated by Klaus Schoenert in Germany. Another field we brought engineering science into was pelletizing iron ores, and many people consider those early papers to be classic.

At Berkeley, in carrying out this research, for some thirty years I generally had a group that ranged from fifteen to twenty persons, including graduate students, postdoctorals, sometimes undergrads, a technician, and a secretary. I had no interest in organizing a large center or institute, as some persons do in the university and around the country. My support for all of this came from a steady flow of grants and contracts, most from government agencies but also from industry.

But anyway, one final sentence is: I think everybody ought to take a look at whether the world is better off that you passed through it? I think it is.

Swent: You've certainly left it a lot better. A lot better. You said Gaudin did the first half of the century; you did the second half; and Arbiter, I guess, bridged. But the first half was Gaudin.

Fuerstenau: Gaudin really was in my opinion the father of systematic flotation research, which he initiated at the University of Utah in the middle twenties. He was the first to conduct seminal research on the flotation of pure minerals with pure reagents, using a small flotation cell. After what were extensive flotation studies on the flotation chemistry of very pure minerals, he was invited in 1929 by President Francis Thompson to come to Montana School of Mines in Butte in 1929 as a professor. After he moved to Butte, he became mostly interested in the nature of adsorbed films of flotation reagents on minerals. A 1935 master's thesis by Schuhmann on collector coatings on chalcocite remains classic to this day. So Gaudin's contributions to flotation occurred in the twenties, thirties, and of course forties, fifties, until his retirement. I'm always amazed how good his work was, when you read back. He applied good science and precise reasoning not only to flotation but also to problems in mineral processing. He had insatiable curiosity about what was happening and the ability to transfer that curiosity to students working with him. You know, it's the thought process that's important. To me, it amazes me to go back and read some of the ideas that he and the other two giants of that era—Ian Wark at Melbourne and Arthur Taggart at Columbia—had. Those people were quite phenomenal.

Swent: A lot of people will be saying the same about you.

[tape interruption]

Swent: I think probably a lot of people have said, "Well, we've seen the greatest period." Don't you imagine that might be sort of universal in a sense? Then somebody else comes along with new materials and new ideas, and the field moves on.

Fuerstenau: That is really right. You know, there's something like around the turn of the [twentieth] century, I think the head of patent office said that everything worth patenting has been invented, and I also recall that some major person in physics somewhere in the 1900s said that all of the major physics has been done. They were wrong.

But I can see how in a field where you think everything is known, someone comes along and moves it ahead in a major way, both experimentally and theoretically. A good example, let's say, is dealing with contact angles in flotation. The contact angle is a measure of the strength of the attachment between a bubble and a mineral particle. Measurement and data analysis of contact angles started with Langmuir in 1920, followed by Taggart in 1930, and then used extensively by Wark in the 1930s and forties. This was followed by W.A. Zisman at the Naval Research Laboratory who spent a career in applying contact angles to all sorts of wetting problems in the 1950s and sixties. After that, to name a few, Rulin Johnson of DuPont wrote a lot of papers on contact angles, on both theoretical and experimental aspects of contact angles. Ron Ottewill at the University of Bristol developed a simple method for measuring contact angle hysteresis. One would have thought that all of this was the last word in contact angles. Then in the 1980s, Wilhelm Neumann at the University of Toronto carried all of this a major step forward. Here was something that I thought had all been solved decades earlier and was really simple but kept moving along, with new theoretical concepts and new instrumentation. Of course, there are several others who contributed along the way. And so I think that this sort of thing is true in many, many fields. New instrumentation and new process models will continue to lead to greater understanding and new developments in mineral processing. In the last few years, the invention of the atomic force microscope has allowed researchers to directly measure forces between bubbles and particles and to actually look at molecules at the surface. If I had not retired, and maybe if my labs had not been dismantled because of the move in and out of Hearst Mining Building because of its renovation, perhaps I would be involved with using that technique for looking at molecular details in systems that I have worked on. But even though I still have lots of ideas there really no longer is a way for my undertaking new research projects.

Of course, there is often re-invention of the wheel out there. I would like to urge researchers in processing to look at some of the early published papers because some of those early researchers had great intuition for explaining phenomena without having the modern tools available to them. During the

past century, much of the research that led to furthering basic understanding of mineral processing phenomena and to new processing methods was carried out in the United States. In Russia and other East European countries were several mineral processing research institutes that were very active and I found some of their contributions to be very interesting and useful. Now, I think that much of the future research in mineral processing will be carried out in Australia. As you know, mineral production is the chief industry there. As an example, my friend Tom Healy has been part of a group that has been successful in establishing a mineral processing research institute at four universities: Melbourne, Queensland, South Australia, and Newcastle. This is called the Australian Mineral Science Research Institute with funding from the federal government, their states, their universities, and industry as a five-year program for a total of \$20 million. This is a far cry from the situation here where NSF some years ago said that they would no longer support mineral processing research.

Surely, our field is not going to disappear because resources are going to be needed. Now, I've been preparing some material for a lecture that I am to give in Sweden concerning broad issues in mineral resources. You know, the U.S. has 5 percent of the world's people in the twentieth century and used up 30 percent of the resources. Now what if you take some other countries—China, India in particular—where there are masses of people, bring them up even partway to our consumption, and now you see what problems the world will face on raw materials and of course on energy.

Swent: Processing is going to be very important.

Fuerstenau: It's going to be important. Mining with total resource utilization, if you can achieve that, is an important objective. The world will have to come back to devoting attention to processing technology. In fact, I read in the NAE magazine, the latest issue, a summary of talks that I had heard when I was in Washington, that they're now starting to talk about uranium again as a major source of energy. One of the sources that somebody is speculating about—and I'm speaking about remarks by John Holdren—is getting uranium from sea water. Now, you can imagine that's going to be a whole new processing scheme. In fact, all such problems of environment, energy sources, raw materials, and so on have got to be solved by engineers. So someday we'll get back to that. [laughs]

[Added by Douglas Fuerstenau during editing: I would like to illustrate the evolution and/or demise of our industry by summarizing what has happened to all of the mining and metallurgical companies that once were major where I lived, worked, or consulted in these past decades. First of all, during my year at Montana School of Mines in 1949–50, the Anaconda Copper Mining Company completely dominated Butte and perhaps all of Montana. All of the mines were going full blast. Some years ago, ARCO bought Anaconda and now Anaconda no longer exists. The great South Works of Carnegie-Illinois

Steel Corporation of U.S. Steel in Chicago no longer exists. At one time twenty-thousand people were employed there. The Bunker Hill Mining and Smelting Company in Idaho [lead and zinc] no longer exists, as is the case with the Howe Sound Company. The Electrometallurgical Company of Union Carbide [ferro-alloys] and Union Carbide itself no longer exist. Companies that I had long-term consulting activities with also no longer exist, either by incorporation into other companies or exiting the business: Foote Mineral Company [lithium], AMAX, Exxon Minerals, Kennecott Copper [the Bingham Canyon operations became part of Rio Tinto], Stauffer Chemical Company [nacohlite or soda ash from Green River, Wyoming], Hanna Mining Company, which exited iron ore mining and moved into plastics. Kaiser Aluminum and Chemical Company, which once was major in aluminum and refractories production, is now much more of a minor player in the aluminum industry. To me, regrettably the great old gold mining company, Homestake Mining Company, also no longer exists, having been absorbed into Barrick. Interestingly, one company that I consulted for a number of years was Marathon Oil Corporation, which later became large due to their being the discoverer of petroleum in Libya—but twenty-five years ago, U.S. Steel bought Marathon—the reverse of the action of many oil companies in the mining world some decades ago.]

I would like to close with a few comments about how the probability of life's decisions and events influenced the course of my personal and professional life, any or all of which may have changed the whole direction of my life. The first was my parents leaving the farm in 1934 and their eventual move to Rapid City in 1943 during my high school years. My friend Frank Marion's plans to graduate at the end of his high school junior year gave me the idea to do the same and start a year early at the South Dakota School of Mines and Technology. I may not have majored in metallurgical engineering if a great chemistry class taught by Earl Prunty had not been my last science course in high school. My decision to go to Montana School of Mines led to my entering MIT in 1950 and not in 1949. Ed Samuel's becoming my long-term roommate at the MIT Grad House in January 1951 eventually led to my meeting Peg. Also because my graduate program was pushed back one year later, I had the great benefit of Professor Overbeek's year at MIT during my final grad student year—which really gave me my background in surface and colloid chemistry. A departmental colloquium during my last semester probably had a significant part in my being appointed as an assistant professor at MIT. I have always thought that Lysle Shaffer, Berkeley professor of mining whom I had gotten to know quite well in my senior year in South Dakota, had some role in initiating the invitation for me to come to the University of California. And there was the chain of events that led to my association with Homestake Mining Company. Of course, the final outcome of all of these events was in my hands.

[End of Interview]

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End of Interview

Interview with KLAUS SCHOENERT

INTRODUCTION by Douglas Fuerstenau

Because of the key role of comminution in the mineral industry and because of Klaus Schoenert's seminal contributions to comminution fundamentals that led to the exciting new technology of high-pressure grinding rolls and because of my own activities in comminution research, a brief interview giving his back ground and experiences is included as part of this oral history. Our longtime relationship started from our mutual interests in comminution.

Comminution has a central role in the processing of ores, not only because it controls mineral liberation and product size distributions, but also because of its huge energy consumption. About 80 percent of the energy consumed in a flotation plant is for comminution. CSIRO recently reported that nearly 15 percent of the electrical energy of Australia is consumed in processing minerals. Hans Rumpf estimated that about 4 percent of the world's electrical energy was expended for comminution. When Rumpf established his institute in Karlsruhe in 1957, he concluded that the most important topic that they could work on would be comminution. In 1957, Schoenert agreed to be an assistant to Rumpf to study comminution fundamentals.

In the ensuing years, Klaus Schoenert became the world's foremost engineering scientist in the field of comminution. He conducted seminal experimental and theoretical studies on the fracture and breakage of single particles ranging in size down to 10 microns or finer. In what he called fracture physics, Schoenert investigated the energetics and probability of breakage, and how cracks initiate, propagate and bifurcate to make smaller particles. After extensive studies of the breakage of single particles, he investigated the comminution of beds of particles. Although less energetically efficient than breaking single particles, he noted that the comminution of beds of particles was still more energetically efficient than ball milling. He conceived of a process in which beds of particles are comminuted at high-pressure followed by deagglomeration of the caked product. To carry out his process continuously, he designed the high-pressure grinding roll (HPGR) system. This truly outstanding development had its beginnings in his detailed work on the first principles of particle breakage. Much of the earlier progress on comminution machines was simply the brute force design of larger equipment. Several hundred HPGRs, ranging from 1.0 to 2.5-meters diameter, are in operation—mostly in the cement industry. Schoenert commented that he thought that the high cost of electricity in Germany was the driving force behind the rapid incorporation of HPGRs into the comminution circuits of cement plants. Late in 2006, at Cerro Verde in Peru, Freeport-McMoran commissioned their new 125,000 tons-per-day concentrator that uses four 2.4-meter diameter HPGRs instead of two 12.2-meter diameter SAG (semi-autogenous grinding) mills. This is the first base-metal application of HPGRs. About that, John Marsden and his colleagues wrote, "Since SAG milling is a mature technology, limited gains or improvements are expected in the future. HPGR, being a relatively new technology, is expected to offer excellent opportunities for operational and maintenance improvements." The comminution energy consumed by their HPGR/ball mill system is only 64 percent of that required for a traditional SAG/ball mill system! Frank Aplan has stated, "In my opinion, in terms of advancement in processing, Schoenert's high-pressure grinding rolls are equal to what flotation did a century ago. What is so impressive to me is that Schoenert's success came from a careful study of the first principles of particle breakage."

I first met Klaus Schoenert and Hans Rumpf in 1966 at an Engineering Foundation Conference in Milwaukee. Later, Schoenert came to Berkeley as a visiting professor and I spent three sabbatical

leaves in Germany. Through Schoenert, I was awarded a Humboldt Senior American Scientist Award to spend one year in Clausthal, which led to my own interest in HPGR comminution. For forty years, we have all been close family friends. The path that led to Klaus Schoenert's pinnacle role in comminution science and technology is given briefly in the interview that follows.

Interview with Klaus Schoenert
Interviewed by: Eleanor Swent
Transcriber: Julie Allen
[Interview: January 23, 2003]

Swent: He's here visiting from Clausthal, Germany. And he's a world expert on comminution and we're going to talk about his early life and education and then some of the—we'll see what we have time for. Let's begin, if you don't mind, I make notes of the words for the transcribers.

Schoenert: Sure, sure. Yes.

Swent: Let's begin with where and when you were born and something about your family and your education.

Schoenert: I was born in 1927, on June 18 in Saxonia in Döbeln. Döbeln is a small city, maybe when you look at the map, it's 60 kilometers west of Dresden. About that.

Swent: How do you spell the name of the town?

Schoenert: Döbeln. The o umlaut. Yes?

Swent: Yes, like that.

Schoenert: Like that. With an l here. [writes]

Swent: Ok, Döbeln. And it was east, you said?

Schoenert: West.

Swent: West of Dresden, 60 kilometers.

Schoenert: About 50 kilometers, about that. My father was the director of a—what is it, dairy? A plant that makes milk and butter?

Swent: Dairy.

Schoenert: Dairy! Yes. Dairy.

Swent: Oh, that probably is a big business in Germany.

Schoenert: Yes, but this was a small plant, because it was owned by the farmers. It was a kind of cooperation. That was usual in Germany.

Swent: A cooperative, I guess.

- Schoenert: That's right.
- Swent: Did this give you any interest in machinery or anything of that sort?
- Schoenert: No. At this time I was interested—actually all during the years when I grew up I was interested in electricity and electronic things. I tried to make a few radios and such things. I was always interested in electronics.
- Swent: Radio was just beginning.
- Schoenert: Then during the later war time I had to leave school, as a young man, a young kid, to join the army, for a short time.
- Swent: Let's see, Hitler came in in 1933?
- Schoenert: In '33 yes. The war was '39. I was drafted in '45, just at the end of the war. It was in February. We had a very short training and then we were sent to Austria to go to the battle line in the east. However, we were very lucky, we never reached the battle line. The war stopped before we came to the battle line. Then we moved westwards. We wanted to be captured by the American troops, not by the Russian troops. [laughter] Then we were prisoners of war for three months and then released.
- Swent: Let's not go quite so fast here. I have some questions I'd like to ask. What kind of schooling did you have then in the thirties?
- Schoenert: First I entered what was called the Kleinschule, and then high school. In Germany we call this gymnasium. Gymnasium.
- Swent: Sure. How was it affected by the war?
- Schoenert: We had maybe an unusual situation when I was in the gymnasium. What happened is that the young teachers were drafted to the war so we then got old teachers. They had already been pensioned, some, before they were brought back to teach us. But I couldn't finish the school at that time because I was drafted.
- [Added by Douglas Fuerstenau during editing: Younger male teachers were drafted into the army in the United States also. When I was in the seventh grade and eighth grade three different teachers were drafted midyear.]
- Swent: I understand.
- Schoenert: I finished my school after I came back home.
- Swent: Were you a member of any youth organization at all?

- Schoenert: Yes, we all were members of the youth organization. What we called Jugend, the Hitler Youth Organization.
- Swent: You had to belong to that.
- Schoenert: Yes, that was normal.
- Swent: Was you town of Döbeln bombed or anything like that?
- Schoenert: No. It was not bombed. It was too small.
- Swent: Was there a shortage of materials? Did it affect your father's business?
- Schoenert: No. No, we had no bombs, no fights there. I think, yes, we always had food there until the Russians came to our town. This was after the end of the war, when they occupied us. In this time I was a prisoner of war in an American camp.
- Swent: When was the terrible bombing of Dresden?
- Schoenert: Oh yes, this was, I think '44 or the beginning of '45. It was around that time, yes.
- Swent: But you by then were not there.
- Schoenert: I was not in Dresden, no. When they were bombed, we could see, in the night a kind of light, a red light on the sky. I don't remember when, however.
- Swent: You mentioned radio. Did you have a radio in your home that you listened to?
- Schoenert: Yes.
- Swent: Could you get news of the war, for instance?
- Schoenert: Yes. Well, my father tried to hear the Swiss radio station, Beromünster.
- Swent: What was it?
- Schoenert: Beromünster. This was a radio station in Switzerland. Beromünster. This was a radio station which the people tried to listen to to get the real news, but it was forbidden to do that, yes? So you could never say it, that you listened to this station.
- [Added by Douglas Fuerstenau during editing: Klaus Schoenert told me that when his father heard the news in 1941 that the German Army had invaded Russia, his father said that the war was lost.]
- Swent: Was it short wave?

- Schoenert: It was long wave.
- Swent: Long wave, oh.
- Schoenert: Because you couldn't get it over long distances.
- Swent: But you couldn't talk about it?
- Schoenert: No no, you couldn't talk about it. I think—I know the BBC had news for Germany. But I think my father didn't listen to BBC. He liked to listen to the Swiss radio station. I don't know why.
- Swent: I'm going to adjust the volume here. What were the conditions like when you were in the prison camp?
- Schoenert: Pardon me?
- Swent: The prison camp, what were the condition like? You said you were captured by Americans.
- Schoenert: Yes.
- Swent: What were the conditions like in that camp?
- Schoenert: Not good and not bad. [laughter] We survived ok. We got something to eat, and then after three months we were—they had had a large number of soldiers living on the field when we arrived there, and we had some tents to be protected.
- Swent: Where was it?
- Schoenert: It was south, in Austria somewhere. It was in southwestern Austria because when we were released we drove north by car to Germany. If we had been farther east, driving north would have brought us to the southern border of Czechoslovakia. Dresden is across the northern border of Czechoslovakia.
- Swent: You were about eighteen years old?
- Schoenert: Oh no, I became eighteen as a prisoner of war, yes? I was drafted when I was seventeen. Very young. I can't imagine it.
- Swent: So then you went back home?
- Schoenert: Not at once, because there was a border between the western part of Germany and the Russian part. My friend and I drove to Bamberg. Then we took several trains in the direction of Darmstadt and a car to a nearby village called Niedermörsdorf. We went there because the family of my friend had friends

there. We worked at a farm for several weeks where I could relax and to gain weight. The farms in Germany needed people to work there, because all the men were somewhere, in camps. We worked about two or three months on the farm, so we had something to eat. Then we decided, we both, my friend and me, to go home to Saxonia, to the Russian zone, and we did it. Then I came home.

Swent: How did you get there?

Schoenert: Oh, there was a border, but you could cross the border. The border was not really dense, it was possible. It was not really a problem.

Swent: Did you walk?

Schoenert: We walked. Yes.

Swent: You walked.

Schoenert: We walked. Sometimes we had a train, but all the train system was destroyed. There were only short distances with the train, then you had to walk, then after a while you could find a train again. It was possible. It was not a problem. We felt so happy because we had survived! [laughter] We traveled east to the limit of the Russian zone, passed it at night, and continued on to a small village called Erlau, where an aunt lived. There also a cousin of mine worked. After a couple of days we drove further to the city of Waldheim in the middle of Saxonia, which was the city of my youth. Then on to Döbeln.

[Added by Douglas Fuerstenau during editing: From what Schoenert told us a few years ago, things were really difficult for him by the time that they reached Erlau. In 1997 after the IMPC in Aachen, where Schoenert received the International Mineral Processing Congress Lifetime Achievement Award, we spent a week visiting ten or more different chateaus in the Loire Valley. Annelene Schonert had made all the reservations for our trip, starting in Angiers and ending at Chambord. After that we drove to a town near Lake Konstanz for the wedding of the nephew of Klaus's older cousin. This was the cousin who had played a special role in Klaus's life in 1945. The arduous travel from Darmstadt without much food left him in a very weak condition by the time that he arrived at the house of his aunt, who said that she could not take care of him because they hardly had enough food for her own family to survive. At that point, his cousin said that she would take care of him. Klaus told us that he doesn't think that he would have survived if it had not been for this cousin.]

Swent: Yes. Did your own family know where you were?

Schoenert: No. They didn't know that. When I came home, my mother—my mother was just overwhelmed when she saw me, and I said, "I'm here!" But before that

she didn't know where I was, because the mail, the post system was completely destroyed.

Swent: Oh, it must have been a very touching homecoming. Your mother and father were both there?

Schoenert: Yes, yes.

Swent: Did you have brothers and sisters?

Schoenert: No, I have a sister. My sister is five years older and she was married already, and she had a baby, and was living in our hometown.

Swent: A happy day when you came back.

Schoenert: Yes, yes. But all was changed.

Swent: So then you picked up your life again. What did you do then?

Schoenert: Ok, after then, I went to school again, to finish my school, with exams which we call Abitur—it's the name of the exam at the end of Gymnasium.

Swent: How do you spell that?

Schoenert: Abitur. Abitur is what you need for studying, to go to a university, you need abitur. It is the final examination of Gymnasium. Then a great change took place. The communist party took over everything. My parents were arrested by the communists and brought to prison because their decision that all big farmers, all owners from companies would be captured. They arrested them and took them away. A dairy was interesting because you have milk, you have butter, you have cheese—it's very interesting—the people of the communist party in our town were intent on getting this position. So they took off my family. I wasn't at home at this time, so I could escape. Of course, they always took the entire family. I escaped to relatives in a village near Weimar in Thuringen. My father was born in Thuringen and had a farm near that village. I then went to Erlau, nearby to Sutil. where I passed the final gymnasium examination, the Abitur. After a while my parents were released because the Russians said, you do not belong to this group of people that we want to remove, so you can go home." However, the communists in our city said, "No, you cannot come again into your apartment," where they lived, "You are not allowed to come to our town; you are banned from our town."

Swent: Oh!

Schoenert: So, near to our town my mother owned a small farm, a market garden of two hectares, near the city of Leisnig near Waldheim. My parents went to this small farm and then I came back from Thuringia and joined them there again.

- Swent: How were you communicating at this time?
- Schoenert: This was complicated. You know, a lot of people were traveling around, and I got messages that my parents were at home by a link of people who gave me the message. Not with a letter, a letter was impossible at this time.
- Swent: And no telephone.
- Schoenert: No! No, no telephone. It happened that they came back in the spring of '46. In the spring I also came back to my parents. I tried to go to university, to study electronics. But I wasn't allowed to go to university. At this time they only allowed youngsters from workers and from small farmers to go to university.
- Swent: This is the Soviet Russians?
- Schoenert: Yes, yes. I tried then to get entry to a Western university. So I wrote letters to Hannover, Darmstadt, Karlsruhe and to Munich. This was a university dealing with engineering. At this time, however, we had limited access to university because the universities were destroyed in a number of places.
- Swent: Those universities were in West Germany, also, what we called West Germany.
- Schoenert: Yes, yes.
- Swent: But you might have been—
- Schoenert: They selected the people with respect to their age. It was reasonable that the older people would be those who came from the war, from the prisons. They had survived, so they should be able to study first and the younger could wait.
- Swent: I see.
- Schoenert: Living in Leisnig for two years from '46 to '48, I was trained as a technician in electronics to repair radios. I did some work. In April '48, I got information from Karlsruhe, that I have to show up there for an examination which we had to pass for admission in the university. Then I took the train to Karlsruhe. This time, again, it was what we called the closed border, yes? You had to find a way to do it, and it was always possible. It was always possible. It was not legal but it was possible. At the border I walked by foot at night and it was very lonely. I came to a farm house and was so happy being told that I have reached West Germany. I then went on to Karlsruhe.
- Then I took the examination. I passed the examination, so now I could start my studies. However, at this time in Karlsruhe after passing your examination for becoming a student, you had to work for one term to help clean the university because it was destroyed from bombing. Only after this term could

you start your studies. It was in April when the summer term began that I had to work, and in the winter term I could begin my studies.

Swent: Did you have to pay?

Schoenert: No, I didn't pay. In Germany you don't pay—ok, let me tell you about the money, which was an interesting thing. I had a bundle of money from my father, but in these times there was a rumor around that they would have a change in money, a devaluation, yes?

Swent: Right.

Schoenert: I thought maybe, if they do that, all my old money is useless. I need something, because my parents are far away—I need something to do. I joined the big company Siemens that had a plant in Karlsruhe. I was a trained technician, so I could get work there. This was my luck, in the middle of June suddenly, we had new currency. But I was in the company and was paid at this time, so I could live with my new money. My old money was gone, yes. Because of that I could not start my study that same year, because my devalued money was too small. I had to save some money. For two years until 1950, I worked for Siemens and then I went to university and began my studies.

Swent: Now what was the name of the university?

Schoenert: University of Karlsruhe.

Swent: University of Karlsruhe.

Schoenert: I decided to study physics, not electronics, not electrical engineering. But I was always interested, after my study, to go back to this kind of work—electronics was my real interest.

Swent: I was trying to think, what was the state of electronics in 1950?

Schoenert: Oh, very simple!

Swent: Had the transistor been developed yet?

Schoenert: No, the transistor I think came later. We had the tubes, the old tubes. We had some transistors, only for—what is that, kleisrifter—

Swent: Kleistron tubes?

Schoenert: It transforms the alternating voltage, AC and DC. Ok. This was a kind of transistor.

- Swent: There is something called the Kleistron tube.
- Schoenert: Maybe.
- Swent: What was Siemens manufacturing?
- Schoenert: Oh many things. I was in a department where we manufactured instruments, equipment for medicine. Then I changed to a department where we made equipment for radio stations, for studios. This was my main business then.
- Swent: I see.
- Schoenert: At this time, everything was big—big things, big equipment, big amplifiers were what you had to make—today it's all very small.
- Swent: Everything is small.
- Schoenert: At this time we had all these amplifiers equipped with tubes, not with transistors.
- Swent: This was just on the brink.
- Schoenert: Yes, yes, came later.
- Swent: On the brink of great change.
- Schoenert: Yes, great change.
- Swent: When you entered the university you stopped working at Siemens?
- Schoenert: No, no, I had always to work, I needed the money. I was always working between the terms. Once in the summer and once in the spring. We have a term from—the winter term starts in October and goes to February, and the summer term starts in April and goes to July. So in the mean time I had to work because I needed the money. I had no financial resources. Scholarships weren't at this time possible in Germany. Germany was poor. It was a great time! [laughter] We had a lot of fun.
- Swent: The students still managed?
- Schoenert: The war was over, I could study, we had fun. It was a wonderful time.
- Swent: Where is Karlsruhe?
- Schoenert: Karlsruhe is here—here's Heidelberg, maybe 60 kilometers south of Heidelberg. Here on this corner here. Karlsruhe is in the Rhine Valley very

close to the northeast corner of France. On the west the city is at the Rhine and on the east it is at edge of the Schwarzwald, the Black Forest.

Swent: Ok. Well, students always manage to have some fun.

Schoenert: Yes! Why not? Why not?

Swent: You finished then, and you received a degree in physics?

Schoenert: Yes, my intention was to finish the degree and go back to Siemens although they had already offered me a job because they knew me. However, my physics professor asked me at the end of my studies, if I was interested in going for a PhD. I was really astonished, because I was thinking that I belonged not to this group of people for PhDs—I was a simple physicist, that's enough, yes? Oh, it was interesting to me that he thought that I could go for a PhD. However, a couple of weeks after this he died. Suddenly he died. So I had finished, I had my final degree, he died, and now I was in the mood that I would like to go for a PhD, something that I had never thought before.

[Added by Douglas Fuerstenau during editing: Schoenert refers to a doctorate as a PhD. In science and engineering, German universities do not give a doctor of philosophy degree. In fields such as chemistry and physics, the degree is Dr. rer. Nat. (Doktor der Naturwissenschaften) and in engineering the degree is Dr.-Ing. (Doktor-Ingenieurs). In Sweden, the engineering doctorate is Tekn. Dr. In the United States, the PhD degree is usual for engineers, but at MIT the degree is ScD. At Berkeley, most engineers get a PhD degree, but they can elect to get a D.Eng. degree. In Germany, the undergraduate degree is called a Diplom. Klaus Schoenert received the degree of Diplom-Physiker in 1957.]

Swent: What was his name?

Schoenert: The name? Professor Gerthsen. Yes.

Swent: You were left without a mentor?

Schoenert: Yes, that was the problem. At this time, Professor Rumpf—

Swent: I've heard—Professor Fuerstenau has told me of him.

Schoenert: Yes, yes. Rumpf came to Karlsruhe and he founded a new institute. He wanted one physicist as his assistant and he asked in the physics department, if there were some people available. I was told about it and I went to talk with Rumpf about his program. The institute was focused to—in Germany, say, Mechanische Verfahrenstechnik—shall I write it? [writes in Swent's notes] Ah, you do well! [to Swent]

Swent: Mechanical—

Schoenert: Mechanical technology—but this is not a good translation. You do chemical engineering. Chemical engineering normally focused on heat and mass transfer. Now, this is the usual basis of chemical engineering. In Germany, it is called Thermische Verfahrenstechnik. Chemical engineers in the past were generally concerned only with fluids, such as in petroleum refining. But you also have many processes in the chemical industry, or in other industries, where you process solids and often as particles. Processing particles was taught mainly in mining programs, in mineral processing, maybe for cement-making people, cement plants, and also in food departments. Grinding wheat to make the flour is a large industry. Now if you bring together all these different experiences and fields into one institute, in one discipline, then in German you call this Mechanische Verfahrenstechnik. Here in your country most of this work was done in mineral processing. That is the reason that our partners in the United States were mineral people, Doug Fuerstenau and Frank Aplan and all these men coming from the mineral business, yes?

Swent: Mm-hmm.

Schoenert: The first thing that we looked at and investigated was particulate science analysis—the characterization of particles. Then our work was directed towards comminution, then classification, mixing of powders, and agglomeration of powders. These were the four main fields. The very important thing is categorization of the behavior of these systems, which is broadly called particle analysis. In this time, I had no idea about this. I didn't know what is meant by chemical engineering. I was not interested. However, Hans Rumpf the man, impressed me so strongly that I changed my mind at once and joined him. It was a decision based on how the person defined and approached his field. I had no idea what I would do, I thought, "This is such an interesting and challenging man. I would like to work with him." By accident I came to the field.

Swent: Because of his personality.

Schoenert: Yes, because of his personality. I loved that.

Swent: Is he a German?

Schoenert: He is German, yes.

Swent: He was born in Germany?

Schoenert: Yes yes, he was born in Germany. He was born in 1911.

Swent: He's a lot older than you.

Schoenert: Yes, yes, for sure. He came from industry. He was a scientist, he was a young scientist. Now, with this group of people and with Professor Rumpf, founded the institute we had—we needed a new building, we had to buy all the equipment. It was a wonderful time.

[Added by Douglas Fuerstenau during editing: Professor Hans Rumpf is such an important person in particle technology that I would like to fill in some details of his background. In 1935 he received his Dipl.-Ing. degree in mechanical engineering from the Technische Hochschule in Darmstadt and his Dr.-Ing. degree in mechanical engineering from TH Karlsruhe in 1939. From 1936 until 1942, he worked for I.G. Farbenindustrie on the development of an air classifier for making precise fine particle separations and building a high-pressure laboratory for acetylene chemistry. From 1942 to 1955, he worked for Alpine on classifier design and then as general manager in charge of manufacturing machinery for comminution, classification and dust collection. When I was at Union Carbide, our pilot plant had an Alpine Mikroplex air classifier, designed by Rumpf. From 1955 to 1957, he worked for Bayer setting up a group for development of a powder technology program. In 1957, he was invited to the Technische Hochschule Karlsruhe as a mechanical engineering professor and established the research institute in particle technology (Mechanische Verfahrenstechnik). To me, it is interesting that at age 46, he came from industry and initiated so much seminal research and published so many valuable papers on comminution fundamentals, classification, agglomerate strength, powder flow, mixing of particles, adhesion, etc. In my opinion, during Rumpf's day this was the foremost research institute in particle processing worldwide. When I spent a sabbatical there in 1973, the personnel totaled at least or more 200 persons, including faculty, researchers, staff and graduate students. From 1966 to 1968 Rumpf was Rector of the University of Karlsruhe and from 1968 to 1971, he was President of the Westdeutsche Rektorenkonferenz, which represented all West German universities in Bonn at that time. In 1969, he led the formation of the chemical engineering department in Karlsruhe. In 1976, he was elected a Foreign Associate of the National Academy of Engineering, but died that same year from a heart operation. He was going to spend a semester in Berkeley upon his retirement.]

Swent: It must have been very exciting.

Schoenert: Like pioneers, yes. It was a great time.

Swent: So you were working with mining people, and also did you have food people coming in also?

Schoenert: Yes, later on Rumpf had a big project on grinding chocolate. But not much early on. We had more contact with chemical industry because Rumpf came from the chemical industry. Then we began to have a lot of contact with the people from cement plants, making cement by grinding clinker. Rumpf

considered comminution to be biggest problem in particle production, so it was in this field that I started comminution research with Rumpf. Fundamental research into comminution. We did not investigate grinding in mills. Instead we investigated the behavior of single particles. If you load them in a testing machine, we wanted to know how the particles will be deformed, how the particles break. We looked at cracking and breakage phenomena, what I call fracture physics. Interesting fundamental work, far away from mills.

Swent: Far away from mills, yes. What were you working with? With rocks? Pieces of rocks?

Schoenert: Yes, later. But first we started with simple materials, we started with glass balls, glass spheres.

Swent: Glass spheres, glass balls, hmm!

Schoenert: Because with glass balls, we had particles of spherical shape. Later we worked with particles of calcite, limestone, quartz, and cement clinker. A lot of our work was with cement clinker because we had good contact with cement companies. Our specialty was to investigate what happens with very fine particles. And the biggest particle we investigated would be a couple of millimeters in size. The smallest were ten microns. We had the wires in a device where we could compress a ten-micron particle and measure the force of displacement—displacement force.

Swent: Force displacement?

Schoenert: The force-displacement curve.

Swent: Curve?

Schoenert: Maybe I can explain. If you have a small rubber ball and squeeze it between your fingers, the ball will flatten as you apply more force with your fingers. Plotting how much the ball flattens as a function of the force that you apply with your fingers would be a force- displacement curve. What we were determining was the curve for a ten micron particle.

Swent: Oh my!

Schoenert: The thing was that comminution is difficult in the fine size range, not in the coarse size.

Swent: Was anybody else doing this kind of research anywhere else?

Schoenert: No. No, no. All people dealing with single particle breakage took big particles, not small particles. We were the only ones doing fine particle size.

- Swent: You were the only people in the world at that time.
- Schoenert: Yes! It was very important for us because the production of fine particles takes a lot of energy. Comminution is a very old—an ancient technology. Nowadays, about 3.5percent of the electrical energy goes to comminution. It's a high number, 3.5percent of the produced electrical energy in the world is used for comminution purposes.
- Swent: Wow!
- Schoenert: Yes. You can see that this is really a big number. The main consumer of comminution energy is the cement industry, with I think about 0.6percent. Then comes the mineral processing plants. Then another big one is the wood industry where you make small pieces of wood for paper and so on. That takes a lot of energy. The high energy consumption in comminution is commensurate with the fineness of the product material. When you have—you know, cement powder, that powder is a fine material, yes?
- Swent: Yes.
- Schoenert: The maximum particle size in cement is about 60 microns. The clinker which we get from the kiln is about ten centimeters, 20 centimeters, in size, so you have to mill it down. You mill it down to one millimeter. You can forget it, it's very simple. But from one millimeter down to 60 microns, 50 microns, this is the problem. One reason is that energy is required to produce new surface and the amount of new surface increases sharply as the particle size is reduced. Grinding down in the small size range is difficult for other reasons also. We thought this size range was important, we were not worried about the big sizes. That was easy.
- Swent: What were you—what was the result of your investigation?
- Schoenert: Pardon?
- Swent: What was the result of your investigations?
- Schoenert: Ok. We wanted only to know what was going on. [laughter] How is a particle formed? How will it break? And what is the size of it? Then we measured for that, the energy which is needed to break it. We could determine the probability of particles breaking for different energies. We measured in some cases the fracture pattern, how the pattern goes through the particle. Then we measured the fragment size distribution, after breakage. We measured the newly created surface, and we got fundamental data for all of this.
- Swent: You were using microscopes, I presume?

- Schoenert: Yes, yes. One of our most sensitive equipment was, with very very fine particles we had two parallel sapphire plates—as a compression platen, you know sapphires, yes?
- Swent: Yes.
- Schoenert: Artificial sapphire, yes. They were placed below a microscope so that we could put particles in the right position—when you have a 20 micron particle and the gap is 20 microns. In those days it was not easy to measure deformation. Now it is much easier. And we had to break it, and we had a displacement of ten microns, so at least we had a gap of ten microns. You want to measure accurately the force-displacement curve—this was really possible, not a problem.
- Swent: You were doing this mechanically?
- Schoenert: Yes, mechanically.
- Swent: With your fingers?
- Schoenert: Yes.
- Swent: Oh my!
- Schoenert: Yes! It was mechanical. For measuring the displacement you had at that time, probably today what we call inductive gauges. You can buy them. To measure the force, this was more of a problem because it is so small. But it was not a real problem. Now, with this data, we could calculate how much energy is really needed to mill a material let's say from ten-millimeter size down to 60 microns. We do all these experiments with single particles of different sizes. We have all the measurements, and you can imagine that with this data you can calculate the energy required to comminute any mill feed. Say you start with ten-millimeter feed particles and you want to produce a powder of 60 microns, how much energy is needed? In breaking a single particle in a testing machine, we bring energy directly to the particle without any lost. This is the ideal comminution process. No machine can do better than that. Now, with this data we can define the efficiency of a mill. We were interested in what is the efficiency of an actual mill. We found that the efficiency of a mill is on the order of 5 to 10 percent. This means the energy lost is maybe twenty or ten times of the energy required to break the particle.
- Swent: Is wasted.
- Schoenert: It's wasted. Now back to these figures I gave you, the power figures, the electricity consumption. How interesting it would be to reduce the energy for comminution. Then we tried to develop a new process that we would reduce the energy needed for industrial comminution. There are some limitations. In

principle, you can do it by inducing the breakage of chemical bonds that hold the particle crystal together. It perhaps isn't possible. When you have a mill for cement production, the mill has a capacity of 100, 200 tons per hour, a big stream, and you can do it by grinding in standard machines, yes? We developed a new process in which we could save one half of the energy, and this was already a success, one half.

Swent: How was that different from what they had done before?

Schoenert: It was very simple. [laughter] Before that and also today, we use a ball mill for comminution of such kind of materials. You know what a ball mill is?

Swent: Oh yes.

Schoenert: Surely, you come from a mining background, yes?

Swent: Right.

Schoenert: After we studied the breakage of single particles, we investigated the breakage of beds of particles. Breakage of particles in a confined bed is less efficient than single particle breakage, but still far more efficient than comminution in ball mills. In our new process to comminute particles continuously in a bed, we used a high-pressure roll, this means we had two counter-rotating rolls, like the old—you know the old roll crusher used in processing ores. Roll crushers?

Swent: It was sort of like the wringer on a clothes washing machine.

Schoenert: Now we feed the bed of particles into the gap between the two rolls. When the feed particles pass through the gap, they are compacted with a very high pressure. Most of the material will be agglomerated. The material will come out as an agglomerate, like a cake. But as the material is being squeezed into an agglomerate, many of the particles are broken. Now what did we have to do? Instead of making small particles, we were making agglomerates! It turns out that we simply had to de-agglomerate the material and to classify it. In the de-agglomerated material from the roll mill, we had about 20 percent of product material.

Swent: Of what?

Schoenert: Twenty percent of the fine powder you want to have. We had to separate out all this 20 percent or 10 percent, it depends, and to recirculate the coarse material back to the mill, which is what you normally do in comminution plants. This is not new. With this new high-pressure process, we could save energy. We only have energy lost by internal friction during the compaction of the particle bed.

Swent: Internal what?

Schoenert: Internal friction. Friction between particles as the bed is compressed.

Swent: Friction, yes.

Schoenert: We supply the energy directly to the material by compression—directly to the material. Let's consider a ball mill. In the case of a ball mill, you have a huge tube filled up about 35 percent with balls, for example. You rotate the tube, so that you can get energy to the balls. The balls are lifted as the mill rotates. Now you put the feed material into the mill, and the balls will hit the particles by accident. This is a highly stochastic process.

Swent: Highly what?

Schoenert: Highly stochastic. There is only a certain probability that a particle might be struck by a ball in a mill. Because of that the process is not highly effective. The power draft—the power for a ball mill—is nearly the same independent of whether you have feed material inside the mill or not.

Energy for comminution is taken from the motion of the balls, and the ball filling. The trick is now to operate the ball mill in a way that most of the energy should go to impacting particles between two balls as they crash down into the charge of particles. That cannot be controlled. Ball milling is a highly stochastic process—it's a hit-or-miss kind of process. Now in the high pressure rolls—we call this a high pressure roll mill—we have a deterministic process. We really supply the energy directly to the bed of particles. This is the reason that it is more efficient. It's very simple. [laughter]

Swent: Once you discover it.

Schoenert: Yes, sure. This kind of process cannot be used all ways. For example, we cannot have wet milling.

Swent: Wet, no.

Schoenert: For wet milling we still need the ball mill. We can only use this machine for brittle materials, which is important for ores and cement clinker. I've given you the figure for energy required for cement. Cement plants and the mineral processing plants take most of the energy for comminution. I think that more than 75 percent of the energy in a mineral processing plant goes for comminution.

Swent: Because it's brittle?

Schoenert: Because it's brittle, yes. And plants have a big capacity. You know in mineral processing plants they treat more than a thousand tons per hour. This material

is cheap. The value per ton of a low-grade ore can be very low. Energy costs also should be small, otherwise the material cannot carry the cost of comminution. Now, when you have high-value materials, maybe pharmaceuticals or other such things, they are so expensive that those people do not worry about comminution costs.

Swent: Doug told me that they were particularly used by DeBeers for diamonds?

Schoenert: Yes, DeBeers, sure.

Swent: They use the roller mills.

Schoenert: Yes!

Swent: Yours, yes. Do you want to tell us?

Schoenert: The reason wasn't energy, the reason was liberation. You can get better liberation of the diamonds from the matrix rock when the diamond ore is crushed in the roller mill. You have heard about it?

Swent: Doug told me.

Schoenert: Oh, Doug told you.

Swent: Otherwise I would not have heard. Yes, he said DeBeers got—well, you tell it.

Schoenert: The point is you can explain in terms of physics, yes, by the high compression of a bed you cause shear forces between the gangue material and the diamonds. The shear forces liberate the diamonds. You pass the diamond ore between the two rollers, compress it very highly, and then you only have to put the product cake in water and the diamonds are free. So the point was they could increase the diamond liberation and they could avoid breaking the big diamonds. You know that in some diamond mines the profit is made from only a few very big diamonds. Diamonds smaller than one millimeter are sold as industrial diamonds. They don't worry about them, only about big diamonds. The old process of liberating diamonds did not avoid completely that one big diamond could be broken. Now when you have this roller mill you know you have to be careful of the gap between the two rolls. The gap should be a little bit bigger than the biggest diamond you expect in the wall. Then the big diamond will not be broken. For two reasons. One reason is the liberation is better, the liberation of diamonds. They never really told me the exact figure, but I guess they have an increase of 20 or 30 percent of diamond liberation. The very important thing is that the big diamonds are not broken, this is very important. This was the reason that they installed roller mills.

Swent: How did you make your contact with DeBeers?

Schoenert: No, no, I had the contract with the manufacturer. The patent is cancelled, Unfortunately it's out.

Swent: You patented it?

Schoenert: Yes, it was my patent and I gave a license to the German company Polysius and to the German company KHD, now it's gone. The patent lasted until 1986, about, now it's open. My patent was for a process not for a machine. The patent was for a process to compress a bed of particles to a certain pressure and to deagglomerate the product. Not for the roll mill, which of course was the device to carry out the high pressure compression.

[Added by Douglas Fuerstenau during editing: Klaus Schoenert's original German patent is Deutsches Patent DE- 2 708 053 (issued in 1977), Verfahren zur Fein- und Feinstzerkleinern von Materialien sproeden Stoffverhaltens, (Method of fine and very fine comminution of materials having brittle behavior). An essentially identical patent was issued in the United States in 1982, U.S. Patent 4357287.]

Swent: I think you misunderstood, I didn't say contract, I said contact.

Schoenert: Oh contact.

Swent: I was wondering how did you—

Schoenert: This was a funny thing. I gave a lecture in Australia in Melbourne—an invited lecture about the fundamentals of comminution. In this lecture also I mentioned the effect of high pressure on a bed of particles, not only on comminution but also on agglomeration. The agglomeration effects were considered as unusual, because normally the people who comminute do not like agglomeration. I described the mechanics of what is going on in a bed of particles under high pressure, the mechanics of that, of the stress field, the shear field, and so on. A paper from this lecture was written, and a researcher at DeBeers in Johannesburg got the idea that the high shear forces must be good for liberation. He contacted me and invited me to visit him for discussion. So I went to Johannesburg to DeBeers as a consultant and we discussed it. We said that there was a good chance that it would happen. Then he did some experimental work with artificial bodies that behave like diamonds. You cannot make that experiment with real diamond ore because the quantity of diamonds is so small and you don't know how many diamonds were inside before. They had to have quantitative measurements, so they made artificial bodies with the strength, with the behavior, the mechanical behavior of diamonds, and mixed it up and made an artificial diamond ore from it. Then he did some preliminary research on this artificial material and got good results. Then they started to contact Polysius and they developed the first high-pressure roller mill for diamond ores. It was a huge mill. It had a

diameter of 2.80 meters. Oh, it was a beauty that they built. Ok, after this was successful they ordered more and more mills. This was the way.

Swent: The spelling of this Polysius—the company that manufactured the mills, what was the name?

Schoenert: Polysius. Now its name is Krupp Polysius. Polysius was their own company before and then was taken over by Krupp and now it is Krupp Polysius. The other company was KHD. This stands for Kloeckner-Humboldt-Deutz, KHD. KHD is a well-known name in equipment manufacturing.

Swent: When you were developing this, you did a pilot, a little model in your laboratory at your institute?

Schoenert: Yes, yes. We did the first research with a piston stamp. You can do this with a piston stamp, to measure all things. What it is, it's high pressure. It doesn't matter how you make it, you need high pressure. You need high pressure and then you need to test for agglomeration. That's all that you need. You can do it very simply in the lab. Then we thought about how we do this in a machine—there are different possibilities. You can design different machines in which a bed of particles is continuously pressed with high pressure, you can do it. The simplest design is rotating rolls. Because the flow of the force as the bed is compressed between two rolls is so simple that you can have a simple design. Then you make a small lab scale machine to investigate operating variables on running this roller and so on, and determine the performance of the system. After we had found the same results as those results we had predicted from our experiment with the pistons, from the piston press, then I wrote a patent, I applied for a patent. After that I contacted companies and asked them whether or not they wanted to try and make a pilot machine and investigate how it would work in an industrial machine. I approached Polysius because I had contact with Polysius before this time. They agreed and then it started. It took about three years. After I applied for the patent, I started at once the negotiations. It took about six months to come to an agreement. After three years maybe we had the first industrial application. But three years is a very short time.

Swent: Ah, is it? What else is it used for, besides diamonds and cement? Is it used for anything else?

Schoenert: Ok, the first time the main application was in the cement industry, particularly in Germany where electric power is very expensive. Then we got some application in minerals, not very much. Then we had diamonds. Now people are trying to introduce this machine more and more into mineral processing. We had one problem in the application, it's wear, the wear is very high—you need some ideas of how to solve the wear problem—the problem of wear is not that we have wear, but that we have to shut down the machine and replace the ring of material on the roller, and it takes time and is expensive. Actually,

the wear per ton of material processed is like a gram or so—far less than the steel wear in crushers and ball mills that can almost be a kilogram per ton with minerals.

Swent: What are the rollers made of?

Schoenert: Ordinary steel, and then you have a lining around the roller, yes, from hard steel. Actually, different methods have been used to handle wear on the roll surface. In cement plants, the roll mill could be bypassed and a weld bead could be used to renew the roll surface. Polysius designed steel shoes that could be bolted to the roll surface and replaced fairly rapidly to minimize shutdown time. Now, KHD engineers came up with the idea of starting with a hard steel plate to make the roll surface, and in this plate they drilled holes in which very hard tungsten carbide pins were inserted. It's a very hard material and it's expensive. But when material is fed to the roll mill, comminuted particles pack tightly between the pins. The pins do not wear and the packed cake of particles protect the steel roll surface from being worn. A very good idea, but not from me from the companies. Ok, that is just one point, the wear. You see in cement, it's already hard, but compared to quartz it is soft.
[laughter]

Swent: Right.

Schoenert: And most ores contain quartz, and quartz makes the wear.

[Added by Douglas Fuerstenau: I have already mentioned my trip to CRA's (now called Rio Tinto) Argyle Diamond Mine in Australia, where two Polysius high-pressure roll mills are installed. The roll surface consisted of steel shoes that were attached to the rolls. At that time, CRA metallurgists had spent considerable effort on the nature of the steel used for the shoes. For example, so that wear would be more or less even across the roll, they used a softer steel near the edge of the rolls where wear was less. They had an electronic laser scanner that monitored roll wear automatically.]

Swent: Did Professor Rumpf, was he involved in this?

Schoenert: No, he died before.

Swent: I see.

Schoenert: Yes, it was my idea.

Swent: It was you alone.

Schoenert: Yes. It's a pity that he couldn't see this. He would be very glad about this.

Swent: I'm sure.

Schoenert: But he died before.

Swent: How long were you there at the institute?

Schoenert: How long? I was with Rumpf—

Swent: At the institute with him, yes?

Schoenert: I started I guess—let me say in '57. Yes, '57 I started. I got my PhD in '66. This was a long time because we were mostly at work with building up the building up the institute and other things. Only a little time for PhD work. But we were full paid though, we were full paid. Then I continued to work there, '69 to '70 I was here in Berkeley, invited from Doug Fuerstenau for half a year with my family. In '71 I made my—what we call in German Habilitation. This is a higher doctorate degree that you normally take when you want to enter an academic area. Then I became a professor in Karlsruhe. Then Rumpf died. I was at the head of the institute for four years. Then I got a call to go to Clausthal and I moved to Clausthal in '81. From '57 to '81 I was in Karlsruhe with the institute. Starting as a PhD—as an assistant and then as a professor. Yes.

[Added by Douglas Fuerstenau during editing: In 1966, Schoenert received the degree of Doktor-Ingenieurs from the Faculty of Maschinenbau (Mechanical Engineering) und Verfahrenstechnik of the Technische Hochschule Karlsruhe for his dissertation, "Einzelkorn-Druckzerkleinerung und Zerkleinerungskinetik. Untersuchungen an Kalkstein, Quarz, und Zementklinkerkoernen des Groessenbereiches 0.1 bis 3 mm." (Single Particle Compression and Comminution Dynamics. Experiments with Limestone, Quartz, and Cement Clinker Particles 0.1 to 3 mm in Size). Schoenert's Habilitation dissertation was entitled, "Mathematische Simulation von Zerkleinerungsprozessen in kontinuierlich betriebenen Muehlen" (Mathematical Simulation of Comminution Processes in Continuous Mills.)]

Swent: Were you teaching then? You were teaching as well as doing research?

Schoenert: Yes, sure. Always. In Germany we are obliged to teach as professors. We have to teach at least six hours a week, always. Let's say between four and eight hours, that is the normal load of teaching.

Swent: At Clausthal, from, let's see, '81—

Schoenert: From '81 to my retirement.

Swent: You were teaching and continuing research as well?

Schoenert: Yes. In Clausthal—in Karlsruhe I was the department of chemical engineering and in Clausthal I joined the department of mining engineers. I had to teach

mineral processing, which I had not taught in Karlsruhe, only the comminution part. I taught mineral processing in Clausthal. They had an institute of mineral processing—in German, Institut fuer Aufbereitungs Technik. I was also doing research in other fields than I did in Karlsruhe. Comminution was the main field, but in Karlsruhe I had research in classification, agglomeration, all these. Here in Clausthal I started research in—what is it—electrostatic separation, magnetic separation, density separation, and in classification. [laughter] I naturally had comminution—it's clear, yes? Then I started in density separation—density.

Swent: Density separation, ok.

Schoenert: Electrical separation, and magnetic separation. This was my research in Clausthal.

Swent: I see. Quite different from comminution.

Schoenert: Yes, these were physical, not chemical.

Swent: The separation.

Schoenert: The very important field of flotation, yes, in mineral processing—this was the research of my colleague, we were two professors at the institute in Clausthal. Professor Albert Bahr, my colleague and me. We both were the professors in the institute of mineral processing.

Swent: Ok.

Schoenert: He was a big man in flotation, who did excellent research on flotation chemistry and in the design of new flotation machines. Doug Fuerstenau knows him.

Swent: I know that name. I had forgotten that you had been here at Berkeley for a year.

Schoenert: Half a year.

Swent: Yes. Were you lecturing?

Schoenert: Lecturing, yes. They invited me to give his course 141, I know this number still. [laughter] This was a general course dealing with particle technology. He wanted that I should teach it in the Karlsruhe manner. Doug wanted to get the information from the Karlsruhe school, so I did that. This was the main reason that I came over. I was doing a little work in research related to agglomeration, and then Doug asked me to give some seminars about fracture physics, because we had done some nice research in fracture physics. He told me, "Ok, you tell what you have done in Karlsruhe, my students would like

that.” Two days later he came up and said, “I think maybe now we should give credit for this as a course. The students will get credit for a graduate course.” I didn’t say I can’t do it. I had no idea what happened. He announced the course with credit, I don’t know how much, but I lectured for two hours once a week. I started this course—I entered the classroom and I was shocked. The classroom was full of 20 people, 25 people, all American PhD students and three professors. I can tell you that I never ever worked so hard as I did to prepare those lectures.

[begin disc 2]

Swent: All right. You were going to tell just a little bit about when you came here.

Schoenert: Yes. I think it was about ‘65, I have to look. About that. Rumpf and I flew to America to attend an Engineering Foundation Conference -- Engineering Foundation meeting in Milwaukee. This was that kind of meeting with only about 100 people, staying together at a school, you sleep there, you eat there. You have a session in the morning, the afternoon is free for discussion and maybe recreation. Then we have dinner, and after dinner an evening session, and after the evening session, the whiskey session. [laughter]

Swent: [laughter]

Schoenert: That was the first time we were here. There was a meeting, a conference, on particulate material. This was the first time we met all these people, Tom Meloy, Doug Fuerstenau, Frank Aplan, and others who was there—from Australia, Denis Kelsall and Alban Lynch -- all the well-known people, mineral processing people. We gave two lectures, and Rumpf was always discussing, always discussing! I remember on the third day, Tom Meloy was the chairman and after the lecture, he said, “Now, Professor Rumpf, you, no question?” [laughter] We became good friends. From this time we had regular contact with these people. They came to visit us in Germany, and we came over to America. Then Doug invited me to come here to Berkeley and we became good friends. It was nice.

Swent: You stayed in his home, as I recall?

Schoenert: No, when we arrived, we rented a home from a professor—he was on leave, sabbatical leave. His home was in El Sobrante, the north. We had three children, and this professor also had three children. The same age about, so they had some toys for them, it was very nice. It was very nice.

Swent: A nice experience for your family.

Schoenert: Oh yes, it was a great time.

Swent: This was '69, you said.

Schoenert: It was '69, yes. We came in August '69 and we left I think in January or February, maybe late February '70.

Swent: Were you aware of the Free Speech movement at that time?

Schoenert: Yes, yes. But it was over already, a little bit. The main time was '68, yes? 1969 was later. You could see it, maybe, the People's Park and other things. Something had changed, Doug told us, yes. Telegraph Avenue, yes.

Swent: How did it compare with your students in Germany?

Schoenert: It is not easy to compare. I can say that the students in my class, or the students in the lab that I know, they are the same. Maybe in Germany in the labs the students do more of the experimental work. Here the students have liked to do more modeling with the computer. That was very popular here. Our philosophy was we had to do experiments. We had maybe better equipment, made by our own workshop, and they do all their own experiments. Maybe this was the main difference, nothing else.

[Added by Douglas Fuerstenau during editing: The research instruments and machines in the German laboratories of Rumpf, Schoenert and Leschonski impressed me greatly. I believe that this was influenced by Rumpf's background in mechanical engineering. They designed most of their own research apparatus, and had outstanding machinists and technicians to construct them. In Berkeley, we had a laboratory high-pressure roll mill. The rolls, their housing, and gear drive for for that machine were specially constructed in Schoenert's workshop in Clausthal. All of the rest was put together by our machine shop in Berkeley, led by my graduate student Oliver Gutesche.]

Swent: Were there women students at that time?

Schoenert: Oh, I don't think so. Not in mineral processing, but yes, a lot of women students around, but in mineral processing we had none. Doug maybe had two or three women students later.

Swent: In Germany did you have any women students?

Schoenert: In engineering, only a few women. But you see, it was in Karlsruhe and Karlsruhe is a university for engineering, yes, not a traditional university —no law, no medicine, no history. Only engineering. Probably 80 percent of the students were men and maybe only 20 percent women. We had women maybe in the school of pharmacy. This was the resource for the boys. [laughter] The pharmacy, female students, yes.

- Swent: In Clausthal, are there women students?
- Schoenert: In Clausthal, ok, this was '81, this was much later, yes? When I joined Clausthal University, in mineral processing I had two female students. There were two female students, among ten male students, about that. Twenty percent, yes. And now in the eighties, you had, in engineering, many more female students than before, all over the campuses in Germany.
- Swent: Is that still true today?
- Schoenert: Pardon me?
- Swent: Still today?
- Schoenert: I think so. I don't know the figure, but I think so. Maybe more than before. More than in the eighties.
- Swent: You're retired now, are you?
- Schoenert: Yes! Since ten years.
- Swent: Are there still mining students, many mining students, in Germany?
- Schoenert: No, no, not in mining. Mining is gone. We have no mines. In Germany, around Clausthal and Freiberg metal ores were being mined since the Middle Ages, some maybe back to Roman times. Some of the old mines shut down after I came to Clausthal. What we have are coal mines and they are subsidized. Always we are talking about closing them. Then we have some open pit mines. They have salt mines—what is it—oh, I don't know the English name, what is it? Cadium chloride?
- Swent: Calcium chloride.
- Schoenert: Calcium, no kalium, not calcium. K, not c.
- Swent: Clay? Kaolin?
- Schoenert: No, KCl.
- Swent: Potassium?
- Schoenert: Potassium, yes. Yes yes. You need it for fertilizer. That we have. That is a huge mine. We have no other mining.
- Swent: So mining is on the decline there as well as here.
- Schoenert: The young people have no interest in going into mining.

- Swent: No. That's a problem.
- Schoenert: It may—over here in the United States, when we were here, in '69 and '70, the general class for mineral processing had no American students, at that time. The people in the lab were PhD students of Doug Fuerstenau, I think only one half of them, or maybe one third of them were American. The others came from foreign countries. Many Indians, many South Americans.
- Swent: That's where the mines are.
- Schoenert: Yes, yes, sure.
- Swent: Is this true in Germany also? Are there students from foreign countries that go there to study?
- Schoenert: Yes. In Clausthal we had always in the department of mining, we had always a good number of foreign students. On the average about 30 percent.
- Swent: Where were they from?
- Schoenert: There were some from Turkey and from the eastern part of Europe and from South America, we had a very good contact with South America. Not from Australia, no contact. From South America, Turkey, Egypt some, and eastern Europe, but most from South America. Clausthal in former times was a very well known in the mining area.
- Swent: Oh yes.
- Schoenert: Now there still is Freiberg, but not much.
- Swent: People from all over the world—
- Schoenert: Now not much. But what can you do? When we have no mines in our nation, we cannot expect the young people to study it.
- Swent: That's right. Well, I think you've answered all of my questions. Is there anything else you'd like to say?
- Schoenert: No.
- Swent: No? This has been very nice.
- Schoenert: I hope you can understand what I have said. My English is not so good.
- Swent: Oh, well—
- Schoenert: Maybe it's good enough that they can get my message [laughter]

BRIEF CURRICULUM VITA OF KLAUS SCHOENERT

June 18, 1927 Born in DoebeIn, Saxonia, Germany

- 1957-1966 Scientific Assistant to Prof. Dr. Hans Rumpf, Institut fuer Mechanische Verfahrenstechnik, Technische Hochschule Karlsruhe
- 1966-1981 Supervisor, Comminution Division, at the Institut fuer Mechanische Verfahrenstechnik, Technical University Karlsruhe (TH)
- 1969- 1970 Visiting Associate Professor, Dept. of MSE, University of California, Berkeley
- 1972-1981 Member of the Directorium of the Institute, TU Karlsruhe
- 1973-1981 C3 Professor, TU Karlsruhe
- 1977-1981 Speaker of the Directorium of the Institute, TU Karlsruhe
- 1981-1992 C4 Professor, Director of the Institute for Mineral Processing, TU Clausthal
- 1989-1991 Dean, Faculty for Mining and Raw Materials, TU Clausthal
- 1975-1993 Chairman, German Working Party on Comminution, of the GVC
- 1977-1994 Chairman, European Working Party on Comminution, Agglomeration, Classification, of the European Federation of Chemical Engineering.

AWARDS AND RECOGNITION OF PROF. DR. KLAUS SCHOENERT

- 1970 Arnold-Euken-Preis, DECHEMA (Gesellschaft fuer Chemische Technik), Frankfurt
- 1971 Venia Lengendi "Mechanische Verfahrenstechnik", TH Karlsruhe
- 1988 Antoine M. Gaudin Award, Society of Mining Engineers, AIME (USA)
- 1991 Hans-Rumpf-Medaille, DVCV (Deutschen Vereinigung fuer Chemie und Verfahrenstechnik)
- 1991 Foreign Associate, National Academy of Engineering (USA)
- 1994 Distinguished Service Award, International Comminution Research Association, ICRA
- 1996 Frank F. Aplan Award, The United Engineering Foundation (USA)
- 1997 Cemex Diploma, Monterrey, Mexico
- 1997 International Mineral Processing Lifetime Achievement Award, International Mineral Processing Congresses

2002 Ehrenmedaille, VDI (Gesellschaft Verfahrenstechnik und Chemieingenieurwesen)

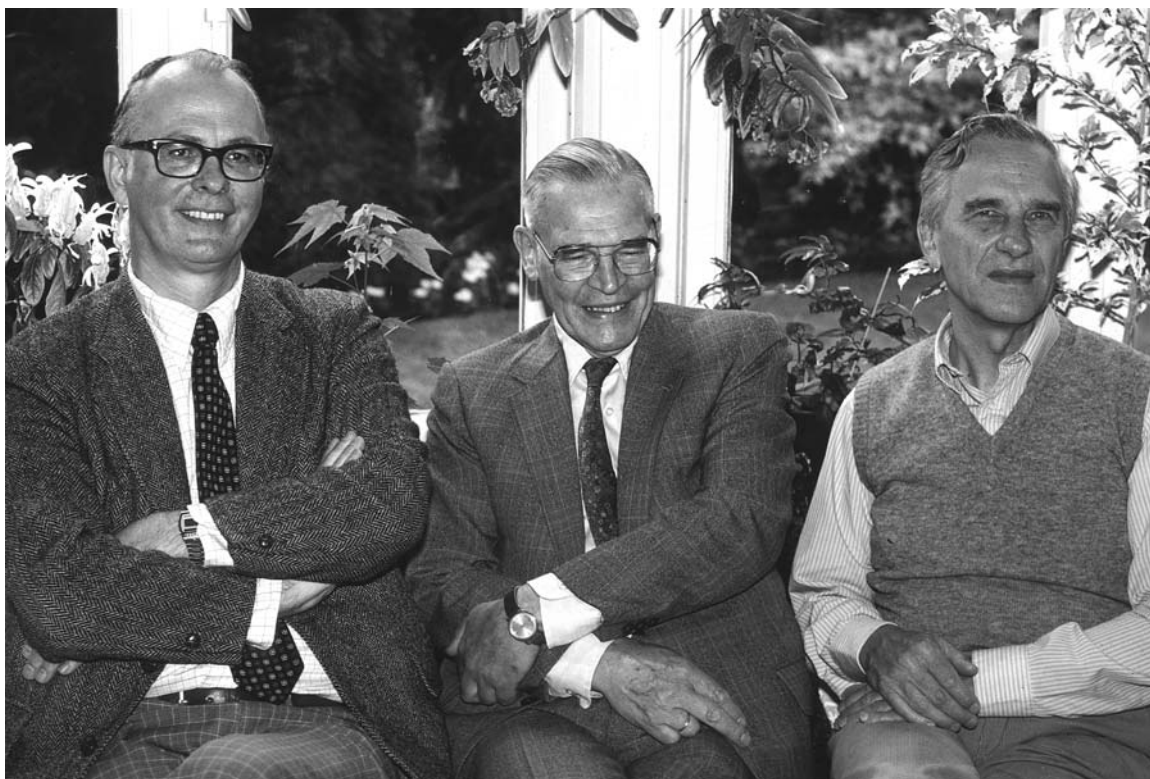
2004 Doktor-Ingenieur Honoris Causa, TU Bergakademie Freiberg

Klaus Schoenert died on September 24, 2011 in Karlsruhe, Germany.

In March 2012 in Bad Dürkheim, the ProcessNet-Fachgruppe Zerkleinern und Klassieren of Dechema held a Gedenkkolloquium in honor of Prof. Dr.-Ing. Klaus Schoenert and announced their establishment of the Klaus Schoenert-Preis in recognition of his tremendous contributions and stature in research and education.



Our family in front of our cabin at Pinecrest in 2001. From the left, DWF, Peg, Sarah, Steve, Lucy and our one grandson, Cole Cliby.



At the home of Theo Overbeek in 1984 together with Phil deBruyn, who had moved to the University of Utrecht as Professor of Chemistry. Overbeek was one of the all-time greats in the field of colloid chemistry and is the O in the DLVO theory of colloid interaction.



The farm near Hazel, South Dakota, where I spent the first six years of my life. When my parents left the farm in 1934, this was in the midst of the dust bowl and along the fences were drifts of fine dirt, like snow drifts in the winter. (Photo taken in 1972).



Photograph of Professor Gaudin and DWF taken in Berkeley in 1965, at the time of his retirement as Richards Professor of Mineral Engineering at MIT. Gaudin was one of the 25 founding members of the National Academy of Engineering in 1964.



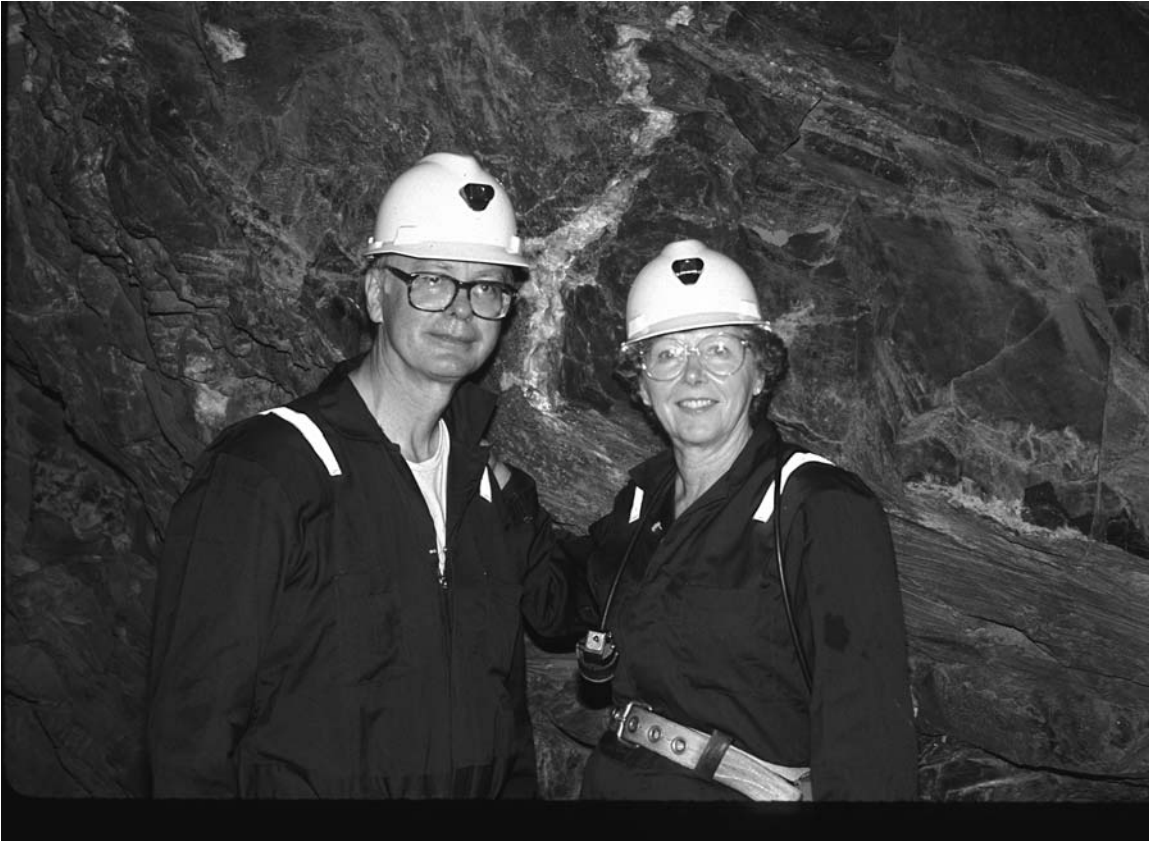
Photograph of my brother, Maurie, and me with Professor Gaudin, taken at a reception during a symposium at the Colorado School of Mines in March 1974, just months before Gaudin died of a stroke in August 1974.



Receiving the first Robert Lansing Hardy Gold Medal from Charles W. Hart, Chairman of the Niagara Frontier Section of AIME in 1957. Second from the left is Barney Field, Manager of the Metals Research Laboratory of the Electro Metallurgical Company, and on the right is Rush Spedden, Manager of the Chemical and Mineral Engineering Group of the Metals Research Laboratory. Subsequently, all awards of the Hardy Medal have been given at the Annual Awards Ceremony of TMS/AIME.



Photograph taken in August 1953 when Professor J. Th. G. Overbeek was leaving MIT to return to Utrecht in the Netherlands. Starting from the left, Harsukh Modi (my first doctoral student at MIT), Richard J. Charles, DWF, Profesor Overbeek, Assistant Professor Phillip L. deBruyn, Dirk Stigter (who spent the year as a researcher at MIT while writing his Utrecht Ph.D. dissertation, and had a long career with Agricultural Research Station in Albany, CA), Dr. Horacio Bergna (post doctoral researcher, who later had a distinguished career at DuPont as an expert on silica colloid chemistry), and graduate students Jean Tournesac and Kenneth Larson.



Peg and I down at the 7200-foot level in the Homestake Mine in Lead, South Dakota, in 1990. Rock temperature at that depth is 134 degrees Fahrenheit, which required extensive refrigeration in the ventilation system.



Photograph (2003) of Klaus Schoenert and DWF with the Clausthal-Berkeley laboratory-scale high-pressure roll mill (now being more generally referred to as high-pressure grinding rolls, HPGR). The counter-rotating rolls and their gear drive were manufactured in Schoenert's Institute at the Technical University of Clausthal, Germany, with rest being constructed and assembled in Berkeley.



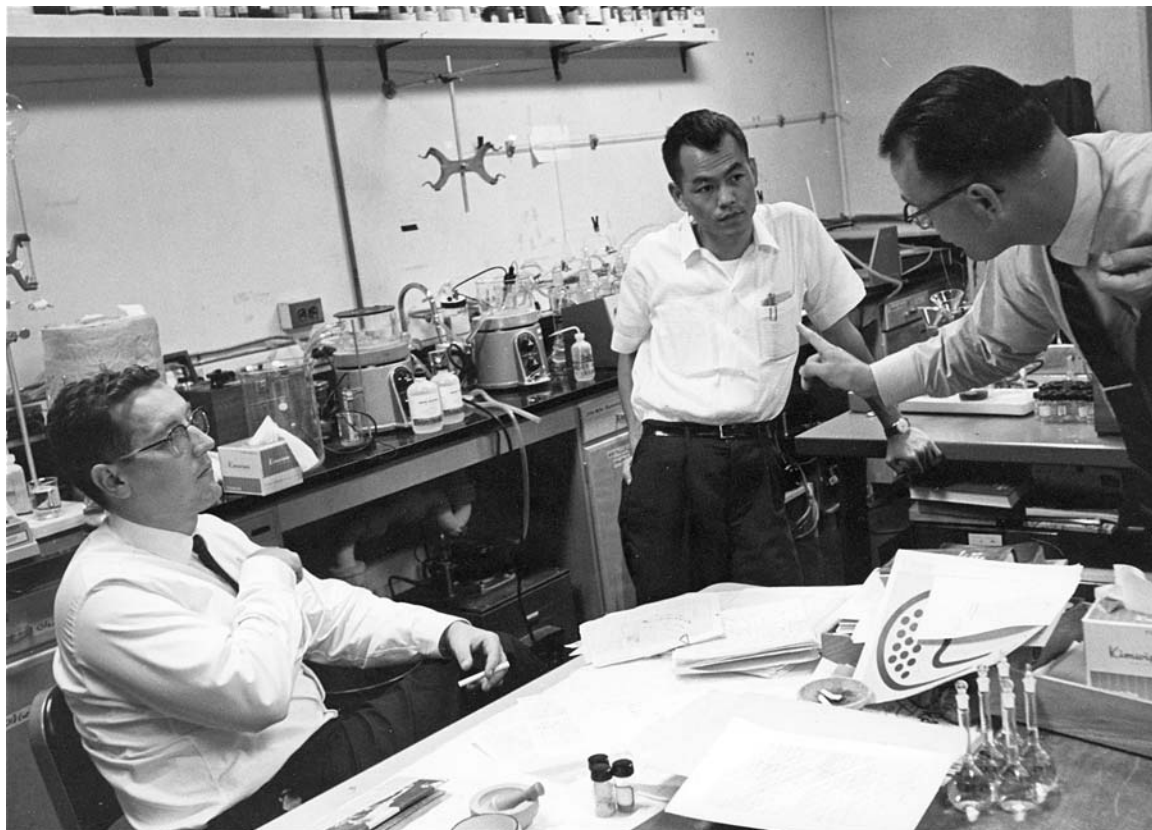
At our home in Berkeley in 1972, with Professor Dr. Hans Rumpf of the Institut fuer Mechanische Verfahrenstechnik, the University of Karlsruhe, Germany. Rumpf was one of the world premier leaders in particle technology, particularly in comminution, classification and agglomeration.



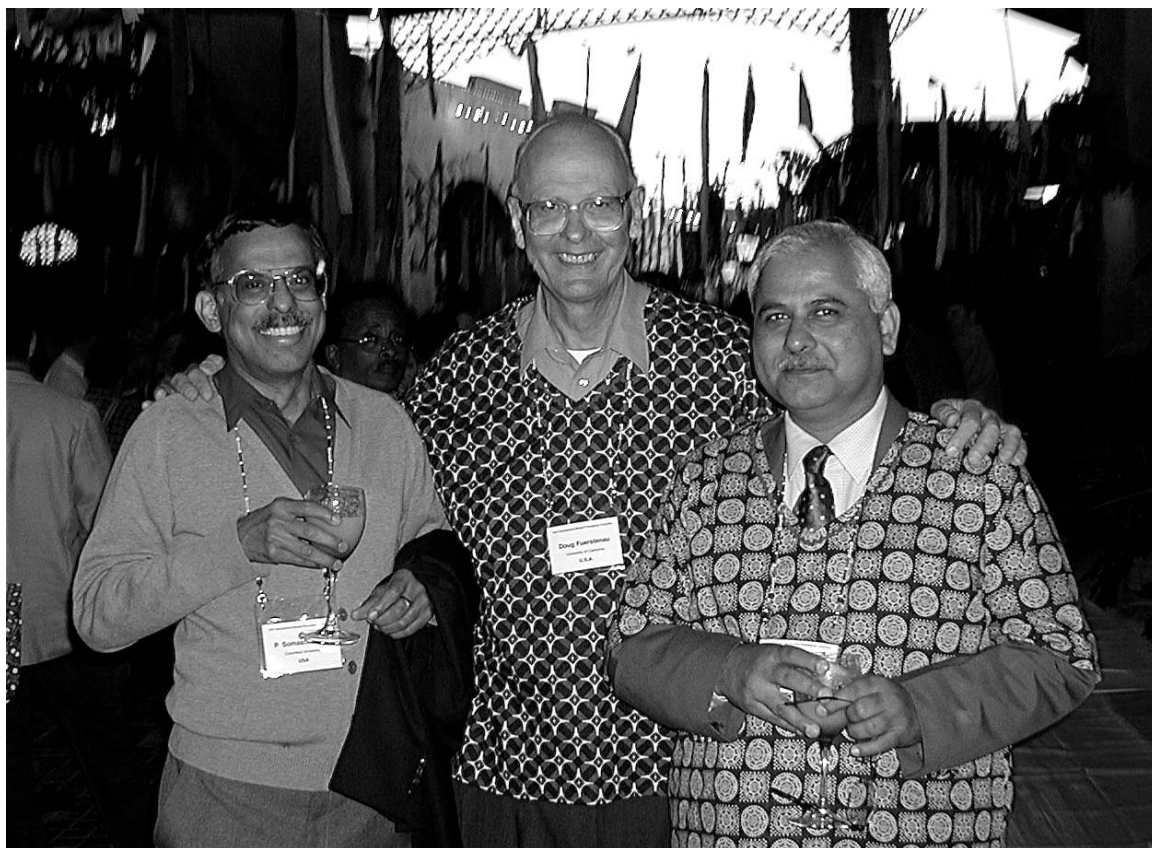
With Sir Ian Wark at the Wark Symposium in Adelaide, Australia, in 1983. Ian Wark was one of the three giants who laid the foundations of modern flotation chemistry (Gaudin, Taggart and Wark) in the 1930s and 1940s.



Photograph with Richard Hogg (M.S. 1965 and Ph.D. 1970, UC Berkeley) and Tom Healy taken in Sydney, Australia, at the 1993 International Mineral Processing Congress. Dick Hogg's M.S. thesis was the basis of the seminal paper, "Mutual Coagulation of Colloidal Dispersions" *Transactions of the Faraday Society*, Vol. 62, pp. 1638–1651 (1966), authored by R. Hogg, T.W. Healy and D.W. Fuerstenau. This very widely referenced paper presents the theory for the interaction of dissimilar particles, often referred to as the HHF theory, which has had wide theoretical and practical application in assessing and controlling colloidal stability in multicomponent processing systems. Tom Healy was elected as a Foreign Associate to the National Academy of Engineering (NAE) in 2008 and Dick Hogg as a Member in 2012.



Discussing research results in the flotation surface chemistry laboratory in Hearst Mining Building with Visiting Associate Professor Thomas W. Healy from the University of Melbourne and graduate student Ralph Lai (Ph.D. 1970, UC Berkeley). Tom Healy was one of four visiting faculty in the mineral processing group during the year that I had a Miller Professorship. The others during that very productive year were Klaus Schoenert from Karlsruhe, Ron Ottewill from Bristol, and Sam Levine from Manchester.



Photograph taken in 2003 at the African Cultural Gala at the International Mineral Processing Congress in Cape Town, South Africa, with Ponisseril Somasundaran (M.S. 1963, Ph.D. 1964, UC Berkeley) and Pradip (M.S. 1977 and Ph.D. 1981, UC Berkeley). Somasundaran, Krumb Professor at Columbia University, was elected to NAE in 1985. Pradip, Chief Scientist and Head of the Process Engineering Innovation Laboratory, Tata Research Development and Design Centre, Pune, India, is President of the 2012 International Mineral Processing Congress in Delhi and was elected as a Foreign Associate of NAE in 2012.

BIO-BIBLIOGRAPHY OF DOUGLAS WINSTON FUERSTENAU

Birth December 6, 1928, at Hazel, South Dakota

Education BS, Metallurgical Engineering, South Dakota School of Mines and Technology, 1949
 MS, Mineral Engineering, Montana School of Mines, 1950
 ScD, Metallurgy (Mineral Engineering), Massachusetts Institute of Technology, 1953

Professional Experience

- 1953-56 Assistant Professor, Department of Metallurgy, Massachusetts Institute of Technology
- 1956-58 Section Leader, Metals Research Laboratories, Union Carbide Metals Company, Niagara Falls, New York
- 1958-59 Manager, Mineral Engineering Laboratory, Kaiser Aluminum and Chemical Corporation, Permanente, California
- 1959-62 Associate Professor of Metallurgy, Department of Materials Science and Mineral Engineering, University of California, Berkeley
- 1962-92 Professor of Metallurgy, Department of Materials Science and Mineral Engineering, University of California, Berkeley
- 1966 Guest Professor, Royal School of Mines, Imperial College of Science and Technology, University of London
- 1970-78 Chairman, Department of Materials Science and Mineral Engineering, University of California, Berkeley
- 1973 Guest Professor, Institut fuer Mechanische Verfahrenstechnik, University of Karlsruhe, W. Germany
- 1978-93 Director, California Institute for Mining and Minerals Resources Research, University of California, Berkeley
- 1984, 87 Guest Professor, Technical University of Clausthal, W. Germany
- 1987-92 P. Malozemoff Professor of Mineral Engineering, Dept. of Materials Science and Mineral Engineering, University of California, Berkeley
- 1993- P. Malozemoff Professor of Mineral Engineering Emeritus, Department of Materials Science and Mineral Engineering, Univ. of California, Berkeley
- 1994- Professor in the Graduate School, Department of Materials Science and Mineral Engineering, University of California, Berkeley

DOUGLAS W. FUERSTENAU
AWARDS, HONORS, AND RECOGNITION

Robert Lansing Hardy Gold Medal, the Metallurgical Society, AIME (1956)

To a person under the age of 30 in recognition of exceptional promise for a successful career in metallurgy

Rossiter W. Raymond Award, AIME (1961)

For the best paper published in the AIME by an author under the age of thirty-three.
 "Retention Time in Continuous Vibratory Ball Milling," *Transactions AIME*, Vol. 214, pp. 1238-1242 (1959)

Mill Man of Distinction Award, Mineral Processing Division, Society of Mining Engineers (1967)

For service as Chairman of the Mineral Processing Division of SME, 1966-1967

Mineral Engineer Degree (h.c.), Montana College of Mineral Science and Technology (1968)

Miller Research Professor, University of California at Berkeley (1969)

Distinguished Teaching Award, University of California at Berkeley (1974)

Best Paper Presentation Award, Society of Mining Engineers, AIME (1975)

For the presentation of the paper, "On the Natural Floatability of Molybdenite,"
 AIME Annual Meeting, New York, 1971

Fellow, Institution of Mining and Metallurgy, London (1975)

Robert H. Richards Award, AIME (1975)

Citation: "In recognition of extensive contributions to the literature, skill as a teacher, and leadership in innovative education in mineral processing"

Distinguished Member, Society of Mining Engineers, AIME (1975)

Member, National Academy of Engineering (1976)

Citation: "For contributions to utilization of low-grade mineral resources and to the processing of solid materials"

Antoine M. Gaudin Award, Society of Mining Engineers, AIME (1978)

Citation: "For exceptional creativity displayed in the development and experimental verification of the electrostatic model of flotation"

Guy E. March Outstanding Alumnus Award, South Dakota School of Mines and Tech. (1979)

Citation: "For his excellence as an educator, his extensive contributions to the field of mineral engineering, and his leadership, both at home and abroad, in assuring proper development and maximal utilization of mineral resources"

Mineral Industry Education Award, AIME (1983)

Citation: “In recognition of his excellence in teaching, creative research in mineral processing, personal interest in his many students, and contributions to the mineral industry”

Humboldt Senior American Scientist Award, Alexander von Humboldt Foundation, Federal Republic of Germany (1984)**Centennial 100 Award, South Dakota School of Mines and Technology (1985)****P. Malozemoff Chair in Mineral Engineering, University of California, Berkeley (1987)****Society of Mining Engineers International Symposium Honoring Douglas W. Fuerstenau on his Sixtieth Birthday. (1988).**

Challenges in Mineral Processing, Proceedings, Edited by K.V.S. Sastry and M.C. Fuerstenau, forty-four chapters, 767 pp. (1989)

Honorary Member, AIME, American Institute of Mining, Metallurgical and Petroleum Engineers (1989). (The 195th member since 1872. Honorary membership in AIME is limited to fifty living persons.)

Citation: “In recognition of his distinguished service as a recognized world leader in mineral processing education and research, his long service to the Society of Mining Engineers and many contributions to its technical literature; and for his important service to government”

Henry Krumb Lecturer, AIME (1989)**Doctor Honoris Causa, University of Liege, Belgium (1989)**

Citation: “This mark of exceptional esteem will bring the testimony of our University’s admiration for yourself, your work, and the services which you have rendered to science”

Frank F. Aplan Award, the Engineering Foundation (1990)

Citation: “For unparalleled technical accomplishments in mineral beneficiation processes, particularly flotation, comminution and agglomeration”

The Berkeley Citation, University of California at Berkeley (1993)

For distinguished achievement and notable service to the University

JSPS Fellowship, the Japan Society for the Promotion of Science, Kyoto University (1993)**Foreign Member, the Russian Federation Academy of Natural Sciences, Mining and Metallurgy Section (1993)**

Citation: “For outstanding contributions to the theory and practice of processing ores for resource recovery and the extraction of metals from raw materials”

International Mineral Processing Lifetime Achievement Award, International Mineral Processing Congresses (1995)

Citation: “For unparalleled contributions to education and research in the field of mineral processing”

Proceedings of the XIX International Mineral Processing Congress, 4 Volumes, SME/AIME (1995)

Dedication: “These volumes are dedicated to Professor Douglas W. Fuerstenau, outstanding educator, researcher, mentor, and recipient of the first International Mineral Processing Lifetime Achievement Award”

Legion of Honor, Society for Mining, Metallurgy and Exploration of AIME (1996)

In recognition of 50 years of service to the Society

CNR Fellowship, Consiglio Nazionale delle Ricerche, Istituto Trattamento Minerali, Rome (1996)

Douglas W. Fuerstenau Professorship, Established by the Department of Metallurgical and Materials Engineering at the South Dakota School of Mines and Technology, (1998)

Distinguished Alumnus Award, Montana Tech–University of Montana (2000)

Honorary Professor, Huainan Institute of Technology, Anhui, P. R. China (2000)

Tekn. Dr. (Honoris Causa), Luleå University of Technology, Sweden (2001)

Citation: “For his unique achievements in research and development in mineral engineering during a period spanning many years, and for being a source of inspiration for University research in mineral engineering”

Foreign Fellow, Australian Academy of Technological Sciences and Engineering (2001)

Citation: “In recognition of his seminal contributions to Australian mineral processing research and innovation”

Foreign Fellow, Indian National Academy of Engineering (2002)

Citation: “For his many contributions to research and education in the processing of minerals and particulate materials, including applications of interfacial science and engineering to particulate systems”

Distinguished Alumnus Award, South Dakota School of Mines and Technology (2002)

Special Issue, *International Journal of Mineral Processing*, to Honor Professor Douglas W. Fuerstenau, Volume 72, 500 pp, 85 authors (2003)

Member, The Berkeley Fellows, An Honorific Society of the Distinguished Friends of the Berkeley Campus, University of California (2004)

Certificate of Recognition, in Appreciation for Fifty Years of Service, ACS Board of Directors, **the American Chemical Society** (2005)

Recognition as ‘Living Legend’ at the Centenary of Flotation Symposium, along with others, Australian Inst. of Mining and Metallurgy [AusIM] and the Soc. for Mining, Metallurgy, and Exploration [SME] (2005)

Citation: “In recognition of your distinguished leadership and service to the science, technology, and application of the flotation process for separation of mineral over many years”

South Dakota Hall of Fame Inductee, Education and Cultural Affairs Section (2005)

Particle Technology Forum Lifetime Achievement Award, Am. Inst. of Chemical Engineers (2006)

Citation: In recognition of his distinguished career, contributions to particle technology research and scholarship, and for outstanding leadership to the Particle Technology community worldwide.

IMPC Council Award, International Mineral Processing Congress (2008)

For outstanding and noteworthy contributions to the programs and role of the IMPC Council and its various activities in mineral processing.

Honorary Professor, Central South University, Changsha, China (2008)

DOUGLAS W. FUERSTENAU

PROFESSIONAL ENGINEERING

Registered Professional Metallurgical Engineer, State of California

Chartered Engineer, Great Britain

Member, Board of Directors, Homestake Mining Company (1977-1999)

Compensation Committee, Member and Chairman

Nominating Committee, Chairman

Environmental, Health and Safety Committee, Member

Director Affairs Committee, Chairman

Committee of the Whole, Member

DOUGLAS W. FUERSTENAU**PROFESSIONAL SOCIETY SERVICE**

Member:

National Academy of Engineering
 American Institute of Mining and Metallurgical Engineers; both SME and TMS
 American Institute of Chemical Engineers
 American Chemical Society
 American Ceramic Society
 The Institution for Mining and Metallurgy
 Sigma Xi
 Fine Particle Society
 International Association for Comminution Research
 International Association of Surface and Colloid Scientists
 Chairman, Basic Science Committee, Minerals Beneficiation Division, AIME (1961-1962)
 Second Regional Vice-Chairman, Minerals Beneficiation Division, AIME (1964)
 First Regional Vice Chairman, Minerals Beneficiation Division, AIME (1965)
 Chairman, Minerals Beneficiation Division, AIME (1966)
 Chairman, Crushing and Grinding Committee, AIChE (1964-1969)
 Robert Lansing Hardy Award Committee, AIME, Member (1963-1967); Chairman (1967)
 Member, Board of Directors, Society of Mining Engineers, AIME (1967-1970)
 Member, Nominating Committee, Minerals Beneficiation Division, AIME (1967-1971); Chairman (1971)
 Member, Nominating Committee, Society of Mining Engineers (1970-1972)
 Member of the LaMer Award Committee of the American Chemical Society (1972-1974); Chair (1974)
 Member, Materials Processing Committee, AIChE (1972)
 Member, Solid Flow and Mechanical Separations Committee, AIChE (1973-1974)
 Member, Richards Award Committee, AIME (1976-1979); (1982-1985)
 Co-Chairman, Session on Advances in Coal Desulfurization, AIChE National Meeting, Philadelphia (1978)
 Councilor, Fine Particle Society (1979-1981)
 American Member, Scientific Committee, International Mineral Processing Congresses (1978-1998)
 American Member, Steering Committee, International Mineral Processing Congresses (1978-1998).
 Member, Rand Award Committee, AIME (1978-1981)
 Co-Organizer, Winter Course on Flotation, South African Institute of Mining and Metallurgy, Johannesburg (1978)
 Member, Gaudin Award Committee, AIME (1979-1984; 1987-1991)
 Chairman, Frank Aplan Award Committee, the Engineering Foundation (1990-1994)
 Chairman, Education Session, AIME Pacific Southwest Minerals Conference, San Francisco (1979)
 Member, Organizing Committee, International Conference on Particle Technology, Institution of Chemical Engineers (1980)
 Member, Organizing Committee, International Symposium on Fine Particles Processing, AIME, Las Vegas (1980)

Co-chairman, Symposium on Surface Chemistry of Flotation, 56th National Colloid and Surface Science Symposium, Blacksburg, VA (1982)

Member, ad hoc Committee to Establish a Surface and Colloid Science Journal (Langmuir), American Chemical Society (1983-1984)

Member, Program Committee, Engineering Foundation Research Conference on the Science and Technology of Processing Fine Coal, New Hampshire (1985)

Member, Program Committee, Engineering Foundation Research Conference on Comminution, Kona, Hawaii (1985)

Member, Organizing Committee, Arbiter Symposium on Advances in Mineral Processing, AIME, New Orleans (1986)

Member, Fundamentals Committee, Minerals and Metallurgical Processing Division, Society of Mining Engineers (1985-1990)

Member, Organizing Committee, International Symposium on Particle Science and Tech., Kyoto, Japan (1988-1990).

Member, International Advisory Committee, International Conference on Extractive Metallurgy of Gold and Base Metals, Australasian Institute of Mining and Metallurgy, Kalgoorlie (1992).

Member, ad hoc Committee to establish the Particle Forum, AIChE (1992)

Member, Scientific Committee, First International Forum on Particle Technology, 1994 Denver (1993-

Member, International Organising Committee, 8th International Conference on Surface and Colloid Science, 1994 Adelaide, IACIS, IUPAC (1992-1993).

Member, Organizing Committee and Chairman, Program Committee, XIX International Mineral Processing Congress, 1995 San Francisco (1991-1995).

Member, Fundamentals Committee, Mineral Processing Division, SME (1986-1990)

Co-Chairman, Symposium on Comminution, SME (1988)

Member, SME Research Council, Society for Mining, Metallurgy and Exploration (1988-1996)

Member, Steering Group of the Subcommittee on Technology and Mining Research, American Mining Congress (1988-1995)

Member, International Advisory Committee, International Conference on Mineral Processing: Recent Advances and Future Trends, Kanpur, India (1994-1995)

Member, International Advisory Committee, Engineering Foundation Conference, Particle Science and Technology in the 21st Century, Pune India (1995)

Organizing Chair, Comminution and Attrition Session, 5th World Congress on Chemical Engineering, San Diego (1996)

Member, Organizing Committee, SME Symposium on Advances in Flotation Technology (The Sixty-fifth Birthday Symposium in Honor of M.C. Fuerstenau), Denver (1998-1999)

Member, International Advisory Committee, SIS 2000, Symposium on Surfactants and Polymers at Interfaces, Gainesville, FL (1999-2000)

Member, International Advisory Committee, International Seminar on Mineral Processing Technology MPT-2002, Bangalore, India (2001-2002)

Member, International Scientific Committee, Second International Conference on The Iron Ore Industry: Today and Tomorrow, Kiruna, Sweden (2001-2002)

Member, Organising Committee, Centenary of Flotation Symposium, June 2005, Brisbane, Australia, (2002-2005)

Member, International Advisory Committee, International Seminar on Mineral Processing Technology (MPT 2006), Chennai, India (2005-2006)

Member, International Advisory Committee, International Workshop on Bioprocessing of Minerals, Changsha, China (2008)

DOUGLAS W. FUERSTENAU**UNIVERSITY OF CALIFORNIA SERVICE**

Member, Berkeley Campus Committee on Prizes (1960-1965), Chairman (1962-1965)
 Berkeley Representative, State-Wide University Representative Assembly of the Academic Senate (1963-1965)
 Vice-Chairman, Department of Materials Science and Mineral Engineering (1964)
 Member, Campus Committee on Budget and Interdepartmental Relations (1966-1969)
 Member, College of Engineering Undergraduate Study Committee, (1961-1964), Chair (1962-1964)
 Chairman, Statewide University Committee on Prizes, (1964-1965)
 Member, Chancellor's Advisory Committee on Science Policy (1970-1972)
 Co-Chairman, College of Engineering Mineral Resources Committee (1975-1977)
 Member, College of Engineering Building Committee (1980-81), Chairman (1982-1983)
 Member, College of Engineering Oceanographic Committee (1965-1969)
 Chairman, College of Engineering Jane Lewis Fellowship Committee (1970-1982)
 Member, College of Engineering Ocean Engineering (1980-1981)
 Chairman, Department of Materials Science and Mineral Engineering (1970-1978)
 Member, Berkeley Campus Committee on Committees (1970-1972), (1985-1987)
 Member, Chancellor's Advisory Committee on Science Research Policy, (1970-1973)
 Member, Phoebe Apperson Hearst Foundation Advisory Board (1978-)
 Chairman, HEW Domestic Mining and Mineral Resources Fellowship Program (1977-1982)
 Acting Chairman, Dept. of Materials Science and Mineral Engineering (1981)
 Chairman, Faculty of the College of Engineering, (1982-1984)
 Member, Board of Directors, College of Engineering Alumni Association (1982-1984)
 Chairman, Berkeley Campus Committee on Research (1984-1986)
 Member, Administrative Committee for Review of Positive Financial Disclosures (1983-1986)
 Member, Science and Education Advisory Committee on the Lawrence Berkeley Laboratory, President's Office of the University of California (1984-1992)
 Advisory Committee Chairman and Principal Investigator, Oral History of Western Mining in the Twentieth, Regional Oral History Office, Bancroft Library (1985-)
 Member, Chair, College of Engineering Building Committee, (1987-1991)

Chair, Graduate Review, Department of Chemical Engineering, (1988-1990)

Member, College of Engineering Rock Mechanics Committee (1989-1993)

Member, the Graduate Council, Berkeley Campus (1992-1993)

Member, ad hoc Committee on the Organization of the Berkeley Engineering Fund, College of Engineering (1992-1993)

Member, the Graduate Council, Academic Senate (1991-1993)

DOUGLAS W. FUERSTENAU**GOVERNMENT PANEL SERVICE**

Member, Panel on Solid Processing of the Ad Hoc Committee on the Ceramic Processing Program of the Materials Advisory Board, National Academy of Sciences (1965-1966)

Panel Member, Workshop on International Mineral Supply, National Commission on Materials Policy (1972)

Invited Participant, NSF Workshop on Extractive Metallurgy, Philadelphia (1974)

Co-Chairman, NSF Workshop on Research Needs in Mineral Processing, New York (1975)

Panel Chairman, U.S. Bureau of Mines/Battelle Colloquium on Foreign Materials Technology Assessment (1975)

Member, Advisory Panel of the Engineering Chemistry and Energetics Section, Engineering Division, National Science Foundation (1975-1979)

Member, Board on Mineral Resources/Board of the National Academy of Sciences/National Academy of Engineering (1975-1977)

Member, Mineral Technology Committee, Board on Mineral Resources, National Research Council (1976-1978)

Member, Committee on Accessory Elements, Board on Mineral Resources, National Research Council (1976-1979)

Member, Panel on Marine Technology, Assembly of Engineering, National Research Council (1976-1977)

Member, U.S. Delegation, Bilateral Workshop on International Technology Exchange, Amer. Chemical Society/Egypt National Research Centre, A.I.D., Cairo (1977)

Member, Committee on Comminution and Energy Consumption, National Materials Advisory Board, National Research Council (1978-1981)

Chairman, Panel on Evaluation of Egyptian Phosphate Ores and Phosphate Fertilizer Production, National Research Council, NAS and Academy of Scientific Research and Technology, Egypt (1979-1981)

Unit Coordinator, Indo-U.S. Workshop on Mineral Processing and Chemical Metallurgy, Udaipur, India (1981)

Invited Participant, Workshop on Cyanide and Organic Effluents from Mineral Processing, National Science Foundation and U.S. Bureau of Mines (1981-1982)

Co-Chairman, Italy-U.S. Workshop on Mineral Processing Research Needs, Palanza, Italy (1982)

Member, Panel on Energy/Resources and Environmental Systems Research, Engineering Research Board, National Research Council (1984-1986)

Panelist, DOE Workshop of Coal Surface Science (1987)

Invited Participant, NMAB Committee on Mineral Engineering Research, Washington, DC.
(1989)

Member, Shipboard Pollution Control Committee, Naval Studies Board, National Research
Council (1994-1995)

Member, EPSCOR Panel, National Science Foundation (1997)

Delegate, U.S.-South Africa (NSF-Engineering Association of South Africa) International
Workshop on Multiphase Systems in Mineral and Chemical Processing (1997)

DOUGLAS W. FUERSTENAU

SERVICE TO TECHNICAL FOUNDATIONS, INSTITUTES, UNIVERSITIES

Chairman, Engineering Foundation Research Conference on Comminution (1963)
 Chairman, Engineering Foundation Interdisciplinary Committee on Comminution (1963-1965)
 Chairman, Engineering Found. Interdisciplinary Committee on Particulate Materials (1967)
 Member, Advisory Board of the School of Earth Sciences, Stanford University (1970-1973)
 Chairman, Engineering Foundation Research Conference on Particulate Systems (1970)
 American Representative, General Scientific Committee, International Mineral Processing Congress, Italy (1975)
 Metallurgical Society, and also the Society of Mining Engineers, AIME, ABET Accreditation ad hoc visitor (1976-1988)
 External Visiting Examiner for the Graduate School, Department of Civil and Mineral Engineering, University of Minnesota (1977)
 Member, Mineral Education Committee, National Association of State Universities and Land Grant Colleges (1977-)
 Member, Special Delegation for Mineral Processing Lectures and Research Consultation, Central South University of Technology, Changsha; Ministry of Nonferrous Metallurgy, People's Republic of China (1979).
 Chairman, AMID, Association of the Directors of the State Mining and Mineral Resources Institutes (1982-1993)
 Member, Visiting Committee, Dept. of Materials Science and Engineering, M.I.T. (1981-1984)
 Member, Peer Committee for Mining, Metallurgy and Ceramics, National Academy of Engineering (1981-1984)
 Member, Team to Review the Graduate Program in Metallurgical Engineering, University of Utah, Advisory to Graduate Dean (1983)
 Member, Capital Campaign Cabinet, Montana College of Mineral Science and Technology (1981-1983)
 Member, Team to Review the Henry Krumb School of Mines, Columbia University (1989)
 Member, Advisory Board, Korea Interfacial Science and Engineering Institute (1991-)
 Member, Advisory Board, South Dakota School of Mines and Tech. Foundation (1994-)
 Trustee, South Dakota School of Mines and Technology Foundation (1997-)
 National Co-Chair, Vision-2000, National Campaign, South Dakota School of Mines and Technology Foundation (1996-2000)
 Chairman, Scientific Advisory Board, Engineering Research Center on Particle Science and Technology, University of Florida (1997-)

External Examiner of PhD Dissertations, and DSc Theses

(In some cases, several different candidates were examined)

- University of Queensland (Australia)
- McGill University (Canada)
- University of New South Wales (Australia)
- University of Melbourne (Australia)
- University of Sydney (Australia)
- University of British Columbia (Canada)
- University of Cape Town (South Africa)

Andhra University (India)
Sri Venhateswara University (India)
Indian Institute of Technology, Kharagpur (India)
Jadavpur University (India)

Supervisor of Diplom-Ingenieur Theses (Research conducted in Berkeley)

Karl Werner, Maschinenbau , University of Karlsruhe
Oliver Gutsche, Mechanische Verfahrenstechnik, University of Karlsruhe
Rainer Bunge, Aufbereitungstechnik, Technical University of Clausthal
Uwe Sander, Aufbereitungstechnik, Technical University of Clausthal

DOUGLAS W. FUERSTENAU

EDITORIAL BOARD SERVICE

Chairman, Editorial Committee, Commemorative Volume for the 50th Anniversary of Froth Flotation, AIME (1958-1962)

Member, Publications Board of the Society of Mining Engineers of AIME (1961-1965)

Member, Editorial Board, J. of Colloid and Interface Science (1968-1971)

Member, Transactions Committee, Society of Mining Engineers (1967-1970)

Member, Editorial Board, Advances in Particle Science and Technology (1972-1978)

Regional Editor for the Americas (1975-1977), Editor-in-Chief (1977-1998),
Honorary Editor-in-Chief (1999-), International Journal of Mineral Processing

Editorial Advisory Board, Mineral Science and Engineering (1973-1981)

Advisory Editor, Elsevier Monograph Series on Advances in Mineral Processing (1974-1999)

Member, Editorial Board, Marine Mining (1978-1981)

Member, Editorial Board, Colloids and Surfaces (1979-1995)

Member, Editorial Board, Mineral Processing Technology and Review (1983-)

Member, Editorial Board, Particle Characterization (1984-)

Member, Editorial Board, Coal Preparation-An International Journal (1992-)

Member, Editorial Board for the Americas, KONA-Particle Technology (1994-); Chairman (1997-2003)

FORMER GRADUATE STUDENTS OF PROFESSOR D. W. FUERSTENAU**NOW IN THE ACADEMIC PROFESSION**

- P. C. Kapur, Professor, Department of Metallurgical Engineering, Indian Institute of Technology, Kanpur
- P. Somasundaran, Professor and Chairman, Henry Krumb School of Mines, Columbia University, New York
- A. L. Mular, Professor, Department of Mining and Mineral Process Engineering, University of British Columbia, Vancouver
- R. Hogg, Professor, Mineral Processing Section, Department of Mineral Engineering, Pennsylvania State University, University Park
- J. A. Herbst, Professor, Department of Metallurgical Engineering, University of Utah, Salt Lake City
- T. S. Goldstick, Professor, Department of Chemical Engineering, Northwestern University
- D. C. Yang, Adjunct Professor, West Virginia University, Morgantown
- K. V. S. Sastry, Professor, Department of Materials Science and Mineral Engineering, University of California, Berkeley
- A. Z. Abouzeid, Professor, Department of Mining Engineering, Cairo University
- K. N. Han, Professor and Head, Department of Metallurgy, South Dakota School of Mines and Technology
- K. Osseo-Asare, Professor, Metallurgy Section, Department of Materials Science, Pennsylvania State University, University Park
- S. Raghavan, Professor, Department of Materials Science and Engineering, University of Arizona, Tucson
- S. Chander, Professor, Mineral Processing Section, Department of Mineral Engineering, Pennsylvania State University, University Park
- G. Y. Onoda, Professor, Department of Materials Science and Engineering, University of Florida, Gainesville
- M. Pritzker, Associate Professor, Department of Chemical Engineering, University of Waterloo, Waterloo, Canada

H. M. Jang, Professor, Dept. of Metallurgy and Materials Science, Pohang Institute of Science and Technology, Pohang, South Korea

R. Herrera-Urbina, Professor, Universidad de Sonora, Hermosillo, Mexico

A. López-Valdivieso, Professor, Instituto de Metalurgia, San Luis Potosí, Mexico

G. C. C. Yang, Institute of Environmental Engineering, National Sun Yat-Sen University, Kaohsiung, Taiwan

Y. S. You, Professor, Dept. of Electronics, Information and Communication, Sun Moon University, Korea

R. Bunge, Professor, Umwelttechnik, Hochschule Rapperswill, Switzerland

K. S. Moon, Adjunct Professor, Dept. of Materials Science and Engineering, Seoul National University, Korea

M. C. Williams, Adjunct Professor, Dept. of Mineral Processing, West Virginia University; also Adjunct Professor, National Fuel Cell Research Center, University of California at Irvine

In addition, two other former students had held assistant professor positions for a number of years:

B. Ball, Department of Metallurgical Engineering, Colorado School of Mines

T. S. Mika, Department of Materials Science and Engineering, University of California, Berkeley.

**GRADUATE STUDENTS WHO COMPLETED MASTERS' DEGREES UNDER THE
SUPERVISION OF PROFESSOR D. W. FUERSTENAU**

Master of Science (MS)

1. M. M. Turkanis (MIT)	1955	34. R. K. Mishra	1975
2. H. L. Miaw (MIT)	1956	35. B. M. Bilimoria	1976
3. R. S. Kinasevich	1962	36. A. P. Mitra	1977
4. G. Y. Onoda	1962	37. I. Harris	1977
5. M. Robinson	1962	38. D. Manmohan	1977
6. P. Somasundaran	1962	39. Pradip	1977
7. D. A. Sullivan, Jr.	1963	40. M. D. Pritzker	1978
8. J. A. Yopps	1963	41. L. J. Judy Lin	1978
9. D. S. Cahn	1964	43. J.E. Gebhardt	1978
10. M. J. Yokota	1964	44. S. H. R. Swaroop	1979
11. P. C. Kapur	1964	45. P. Krishnaswamy	1979
12. R. H. Hogg	1965	46. W. R. Wahl	1980
13. L. M. Berlioz	1966	47. J. M. Rosenbaum	1980
14. Harman	1966	48. K. S. Venkataraman	1981
15. J. C. Woodfill	1966	49. A. S. Osoka	1982
16. M. P. Hoover	1967	50. C. H. Li	1982
17. P. K. Chaudhuri	1968	51. K. L. Chang	1984
18. J. A. Herbst	1968	52. F. J. Sotillo	1985
19. B. Ball	1968	53. J. S. Hanson	1985
20. G. A. Grandy	1969	54. A. S. Ibrado	1986
21. P. K. Aggarwal	1969	55. J. Diao	1987
22. S. Chander	1970	56. P. B. Phatak	1990
23. A. -Z. M. Abouzeid	1970	57. M. Colic	1991
24. S. K. Doss	1972	58. R. C. Bunge	1991
25. K. Osseo-Asare	1972	59. O. W. Gutsche	1991
26. S. C. Panigrahy	1972	60. R. Ravikumar	1992
27. M. W. Lai	1972	61. E. Vandeburgt	1993
28. J. M. Wie	1972	62. P. Huang	1994
29. S. Raghavan	1973	63. J. Vazquez-Favela	1995
30. D. L. Gefvert	1973	64. J. J. Lutch	1997
31. D. A. Griffiths	1973	65. R. Jia	1999
32. V. K. Karra	1973		
33. V. N. Seetharama	1974		

(Except for the two at M.I.T., all were at the University of California at Berkeley)

**GRADUATE STUDENTS WHO COMPLETED DOCTORAL DEGREES UNDER
THE SUPERVISION OF PROFESSOR D. W. FUERSTENAU**

Doctor of Philosophy (or DEng or ScD)

1. G. M. Mao (ScD, MIT)	1955	34. S. R. H. Swaroop	1982
2. H. J. Modi (ScD, MIT)	1956	35. Y. S. You	1983
3. P. Somasundaran	1964	36. J. E. Gebhardt	1983
4. C. R. Guerra	1965	37. A. S. Osoka	1983
5. D. S. Cahn (DEng)	1966	38. G. C. C. Yang	1983
6. P. C. Kapur	1968	39. K. S. Venkataraman	1984
7. D. C. Yang	1969	40. M. C. Williams	1985
8. R. H. Hogg	1970	41. K. S. Moon	1985
9. R. W. M. Lai	1970	42. R. H. Urbina	1985
10. K. V. S. Sastry	1970	43. H. M. Jang	1986
11. B. Ball	1970	44. C. L. Li	1986
12. T. S. Mika (DEng)	1970	45. D. M. Deason	1987
13. K. N. Han	1971	46. J. S. Hanson	1987
14. J. A. Herbst (DEng)	1971	47. B. V. Velamakanni	1988
15. M. P. Hoover	1972	48. A. L. Valdivieso	1989
16. G. Kar	1972	49. K. S. Narayanan	1989
17. A.-Z. M. Abouzeid	1973	50. J. Diao	1990
18. S. Chander	1973	51. A. S. Ibrado	1992
19. G. L. Hubred	1973	52. R. C. Bunge	1993
20. S. W. Clark (DEng)	1974	53. O. W. Gustsche	1993
21. V. S. Gupta	1974	54. M. Colic	1994
22. S. K. Doss	1975	55. A. K. De	1995
23. G. A. Grandy (DEng)	1975	56. F. J. Sotillo	1995
24. K. Osseo-Asare	1975	57. P. Huang	1996
25. C. C.-Y. Chiao	1976	58. R. Ravikumar	1996
26. S. G. Malghan	1976	59. M. A. McGraw	1996
27. S. Raghavan	1976	60. R. Jia	2003
28. J. M. Wie	1976		
29. V. K. Karra	1976		
30. D. Manmohan	1979		
31. Pradip	1981		
32. J. M. Rosenbaum	1981		
33. P. Krishnaswamy	1981		

(Except for the two at MIT, all were at the University of California at Berkeley)

**POSTDOCTORAL RESEARCHERS, VISITING RESEARCHERS, VISITING
FACULTY WHO WORKED WITH PROFESSOR D. W. FUERSTENAU**

Vladimir Nebera, (1960-61), Visiting Scholar (CSc, Metals and Alloys Institute, Moscow)

Andrew L. Mular, (1962-64), Junior Research Engineer, Associate Specialist; (MS, Montana School of Mines, previously at Minerals Research Institute, Michigan Tech)

Thomas W. Healy, (1963-65), Assistant Research Engineer (PhD Columbia University) (1970) Visiting Associate Professor (University of Melbourne)

Ponisseril Somasundaran, (1964), Postdoctoral Researcher (PhD, UC Berkeley)

Roger Burns, (1965), Assistant Specialist (PhD Geology, UC Berkeley)

Horace E. Rose, (short term, 1965), Visiting Scholar (Professor of Powder Technology, Kings College, University of London)

Jan Leja, (1965), Visiting Research Engineer (Professor, University of Alberta)

Hamza El-Ketanni, (1965), Conference Board of Associated Research Councils Fellow (Professor, Universite Mohamed V, Rabat, Morocco)

Takahide Wakamatsu, (1966-68). Post-Graduate Research Engineer (Lecturer, Kyoto University, Japan; later appointed professor)

Jahan Fouladi, (1966), Junior Specialist (MS, Mech. Engineering, UC Berkeley)

Prodyot Roy, (1967-68), Assistant Specialist (PhD [Hultgren], UC Berkeley)

Guntbert Mempel, (1967-68), Post Graduate Research Engineer (Dr.-Ing., Technical University of Munich, Germany)

Johann Kerl, (1968-69) NATO Postdoctoral Fellow (Dr.-Ing., Technical University of Clausthal, Germany)

George B. Georgescu, (1969-70) IREX Exchange Visitor (Head, Mineral Processing Dept., Institute for Geological Research, Bucharest, Rumania)

Frantisek Spaldon, (1969), IREX Exchange Visitor (Professor and Rector, Technical University of Kosice, Czechoslovakia)

William T. Foreman, (1969), NSF Summer Visiting Program (Associate Professor, VPI)

Samuel Levine, (1969), Visiting Professor (Reader in Mathematics, University of Manchester, England)

Klaus Schoenert, (1969-70), Visiting Associate Professor (Tech. University of Karlsruhe) (1991)
Visiting Professor (Tech. University of Clausthal, Germany)

Ronald H. Ottewill, (1970), Visiting Professor (Reader in Chemistry, University of Bristol)

Kalanadh V. S. Sastry, (1970-73), Assistant Research Engineer (PhD, UC Berkeley)

Subhash Chander, (1973-74), Assistant Research Engineer (PhD, UC Berkeley), (1981-83)
Associate Research Engineer (from IIT Kanpur)

Gitimoy Kar, (1972-73), Post-Graduate Research Engineer (PhD, UC Berkeley)

Klaus Steier, (1973-75), NATO Postdoctoral Fellow (Dr.-Ing., Technical University of Karlsruhe, Germany)

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(1978-80), Visiting Assistant Research Engineer (Cairo University)
(1983-84), Visiting Associate Research Engineer (Cairo University)
(1988), Visiting Research Engineer (Cairo University, Egypt)
(1991), Visiting Research Engineer (Cairo University)
(1997), Visiting Research Engineer (Cairo University)
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Srinivasan Raghavan, (1975-77), Assistant Research Engineer (Lecturer, PhD, UC Berkeley)

Marilyn McGregor, (1977-78), Post-Graduate Research Scientist (PhD, University of Queensland, Australia)

Latif Khan, (1977-78), Post-Graduate Research Engineer (Dr.-Ing., University of Clausthal, Germany); (1989), Visiting Research Engineer (Illinois Geological Survey)

Jorge Rubio, (1978-79), Post-Graduate Research Engineer (PhD, Imperial College, London, previously Postdoctoral at Clarkson University)

Jan Drzymala, (1978-79), Post-Graduate Research Engineer (PhD Technical University of Wroclaw, Poland); (1990), Visiting Assistant Research Engineer (Assistant Professor, Technical University of Wroclaw)

Peter Kerlin, (1978-79), Junior Specialist (Dipl. Ing., University of Braunschweig)

Robert J. Hunter, (1979), Visiting Research Engineer, Visiting Professor (Associate Professor, Chemistry Dept., Univ. of Sydney, Australia)

Kenneth N. Han, (1974), Visiting Lecturer, Visiting Research Engineer (Lecturer, Monash University, Australia); (1979), Visiting Associate Professor; (Sr. Lecturer, Monash Univ.)

Rapur Natarajan, (1979-80), International Atomic Energy Agency Fellow (Bhaba Atomic Energy Establishment, India)

Mohamed R. Moharam, (1979-80), Egyptian Missions Department Fellow (Associate Professor, Alazhar University, Egypt)

Jungi Shibata, (1979-80), Post-Graduate Research Engineer (PhD, Kyoto University Lecturer, Kansai University, Japan); (1988-89), Exchange Visitor (Associate Professor, Kansai University)

Geoffrey D. Parfitt, (1980), Visiting Professor (previously at Tioxide International)

Pradip, (1980-81), Post-Graduate Engineer (PhD, UC Berkeley); Visiting Research Engineer (Tata Research Development & Design Centre, Pune, India); (2000), Visiting Research Engineer (TRDDC)

Vikhram P. Mehrotra, (1980-81), Assistant Research Engineer (PhD [Sastry], UC Berkeley)

Janusz Laskowski, (1981-82), Visiting Professor, Visiting Research Engineer (Professor, Technical University of Wroclaw, Poland)

Heinrich Schubert, (short term 1982), IREX Exchange Visitor (Prof., Bergakademie Freiberg, DDR)

Weibai Hu, (1982-83), Visiting Professor (Central South University of Technology, Changsha, China)

Ha Ngoc Le (L Cevy), (1982-83), Post Graduate Research Engineer (PhD, University of Bern, Switzerland)

Ernest Peters, (1971), Visiting Professor (Professor, University of British Columbia); (1983), Visiting Professor (UBC, Vancouver)

Dirk Stigter, (1984-85), Visiting Research Engineer; (retired US Dept. of Agriculture)

Sureshan K. Moothedath, (1985), Post Graduate Research Engineer (PhD, Indian Institute of Technology, Madras)

Maria Barbaro, (1985), Visiting Research Engineer (CNR, Head, Flotation Research, Istituto Trattamento Minerali, Rome)

Mark C. Williams, (1984-85), Assistant Research Engineer (PhD, UC Berkeley)

Stanislav Kmet, (1985), IREX Exchange Visitor (Professor, Technical University of Kosice, Czechoslovakia)

J. Th. G. Overbeek, (1985), Visiting Professor (University of Utrecht, the Netherlands)

Ronaldo Herrera-Urbina

(1985), Post-Graduate Research Engineer (PhD, UC Berkeley)

(1986-88), Associate Specialist

(1989), Visiting Assistant Research Engineer (Professor, Instituto Tecnológico de Saltillo, Mexico)

(1990), Visiting Assistant Research Engineer National Polytechnic Institute, Mexico City)

(1995), Visiting Research Engineer (Professor, University of Sonora)

Prakash C. Kapur

(1986), Visiting Research Engineer (Professor, IIT Kanpur, India)

(1987-88), Visiting Research Engineer

(1990), Visiting Research Engineer

(1991-93), Visiting Research Engineer

Oleg Tikhanov, 1987-88), Visiting Fulbright Scholar (Professor, Leningrad Mining Institute, USSR)

James S. Hanson, (1988-92), Assistant Research Engineer (PhD, UC Berkeley)

Guy H. Harris, (1988-2001), Visiting Research Engineer (retired from Dow Chemical Co.)

K. S. Narayanan, (1989-90), Assistant Research Engineer (PhD, U C Berkeley)

Kwang Soon Moon, (1990), Visiting Research Engineer (Sabbatical Leave, Mineral Processing Group, CANMET, Ottawa)

Jianli L Diao, (1990-95), Associate Specialist (PhD, UC Berkeley)

Asoke De, (1994-97), Post-Graduate Research Engineer (PhD, UC Berkeley)

Paul Verbeiren, (1999-2001), Visiting Post-Graduate Research Scientist (PhD, Free University of Brussels)

PUBLICATIONS OF D. W. FUERSTENAU

1. "Streaming Potential Studies--Quartz Flotation with Anionic Collectors," A. M. Gaudin and D. W. Fuerstenau, Transactions AIME (American Institute of Mining, Metallurgical, and Petroleum Engineers), Vol. 202, pp. 66-72 (1955).
2. "Streaming Potential Studies--Quartz Flotation with Cationic Collectors," A. M. Gaudin and D. W. Fuerstenau, Transactions AIME, Vol. 202, pp. 958-962 (1955).
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4. "Measuring Zeta Potentials by Streaming Potential Techniques," D. W. Fuerstenau, Transactions AIME, Vol. 205, pp. 834-835 (1956).
5. "Streaming Potential Studies on Quartz in Solutions of Ammonium Acetates in Relation to the Formation of Hemimicelles at the Quartz-Solution Interface," D. W. Fuerstenau, Journal of Physical Chemistry, Vol. 60, pp. 981-985 (1956).
6. "Activation and Deactivation of Sphalerite with Ag and CN Ions," A. M. Gaudin, D. W. Fuerstenau and M. M. Turkanis, Transactions AIME, Vol. 203, pp. 65-69 (1957).
7. "Modified Hallimond Tube for Flotation Testing," D. W. Fuerstenau, P. H. Metzger and G. D. Seele, Engineering and Mining Journal, Vol. 158, pp. 93-95 (1957).
8. "Streaming Potential Studies on Corundum in Aqueous Solutions of Inorganic Electrolytes," H. J. Modi and D. W. Fuerstenau, J. Physical Chemistry, Vol. 61, pp. 640-643 (1957).
9. "Correlation of Contact Angles, Adsorption Density, Zeta Potentials, and Flotation Rate," D. W. Fuerstenau, Transactions AIME, Vol. 208, pp. 1365-1367 (1957).
10. "Effect of Chemical Reagents on the Motion of Single Air Bubbles in Water," D. W. Fuerstenau and C. H. Wayman, Trans. AIME, Vol. 212, pp. 694-699 (1958).
11. "Iron Oxide Slime Coatings in Flotation," D. W. Fuerstenau, A. M. Gaudin and H. L. Miaw, Transactions AIME, Vol. 212, pp. 792-795 (1958).
12. "Activation and Deactivation Studies with Copper on Sphalerite," A. M. Gaudin D. W. Fuerstenau and G. W. Mao, Transactions AIME, Vol. 214, pp. 430-436 (1959).
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14. "Retention Time in Continuous Vibratory Ball Milling," D. W. Fuerstenau, Transactions AIME, Vol. 214, pp. 1238-1243 (1959).
15. "Activation of Sphalerite with Lead Ions in the Presence of Zinc Salts," D. W. Fuerstenau and P. H. Metzger, Trans. AIME, Vol. 217, pp. 119-123 (1960).
16. "The Flotation of Corundum--An Electrochemical Interpretation," H. J. Modi and D. W. Fuerstenau, Trans. AIME, Vol. 217, pp. 381-387 (1960).
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The Exploration Geologist



The Mining Engineer



The Mineral Processing Engineer

Cartoons drawn for the 1950 college annual, the **MAGMA**, to represent the various academic degree programs of Montana School of Mines.



The Process Metallurgist



The Petroleum Engineer