

Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

University History Series

Thomas H. Pigford

BUILDING THE FIELDS OF NUCLEAR ENERGY AND NUCLEAR WASTE MANAGEMENT,
1950-1999

With Introductions by
Richard D. Smyser
and
Elizabeth Pigford

Interviews Conducted by
Carl Wilmsen
in 1999

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Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of northern California, the West, and the nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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Thomas H. Pigford

Professor of Nuclear Engineering

BUILDING THE FIELDS OF NUCLEAR ENERGY AND NUCLEAR WASTE MANAGEMENT, 1950-1999, 2001, xviii, 340 pp.

Family and youth in Mississippi; background in chemical engineering, graduate student at MIT, wartime service at Oak Ridge National Laboratory; co-founding the Department of Nuclear Engineering Education at MIT with Manson Benedict in 1952, developing the first nuclear engineering textbook, nuclear reactor designs and government policy in the 1950s; starting the Nuclear Engineering Department at the University of California, Berkeley, in 1959, reflections on citizen and student activism in the 1960s, environmental concerns of the 1960s and 1970s; service on the National Atomic Energy Safety Licensing Boards, 1963-1974, reviewing the Diablo Canyon, Fermi-1, and General Electric reactors; investigating the accidents at Three Mile Island and Chernobyl, member of delegations to the Soviet Union, 1988, and the Ukraine, 1994; changing research focus to nuclear waste management in the 1970s, theoretical breakthroughs in the transport of radioactive materials through geologic media, consultant to the WIPP and Yucca Mountain projects, reviewing the Swedish program, 1986-1991, site characteristics of Yucca Mountain, dissenting opinion on Yucca Mountain Standards, 1995, working to deter proliferation of nuclear weapons in the 1990s; role of family in career.

Introductions by Richard D. Smyser, brother-in-law and newspaper editor, Oak Ridge, Tennessee, and Elizabeth Pigford, wife.

Interviewed 1999 by Carl Wilmsen for the University History Series, Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

ACKNOWLEDGMENT

This oral history has turned out to be a more interesting project than I could have imagined. Several individuals deserve credit and have my appreciation for their contributions to the project. Carl Wilmsen, the interviewer from the Regional Oral History Office, thoughtfully and carefully framed the discussion for the story told herein. Dick Smyser, husband of my late sister Mary and astute commentator, wrote the first introduction. Elizabeth Pigford wrote the second introduction and also helped as an editorial assistant and critic; it is my pleasure to dedicate this oral history to her. Shannon Page, production manager of the Oral History Office and her colleagues, efficiently coordinated the publication process. And it was Willa Baum, former director of the Regional Oral History Office, whose idea launched this oral history project. My heartfelt thanks to them all.

The expertise, fellowship and cooperation of the faculties and staffs of the Nuclear Engineering Departments at Berkeley and, before that, at M.I.T. have contributed to my academic and professional career in more ways than I can enumerate here. The outstanding accomplishments of these colleagues and their support and excellence in teaching and research have laid the foundation for quality education in nuclear engineering. Nuclear engineering draws on a multitude of disciplines. To have been associated with many fine scientists, engineers, and others in so many fields, in this country and abroad, has been an inspirational, life-long learning experience.

I wish to extend my thanks to the Mitsubishi Corporation for their long-standing support of my work and for making this oral history possible.

It is indeed an honor to have been selected to be a subject of an oral history. I am grateful to all who have made it possible.

TABLE OF CONTENTS--Thomas Pigford

PREFACE	I
INTRODUCTION--BY RICHARD D. SMYSER	IV
INTRODUCTION--BY ELIZABETH PIGFORD	XII
INTERVIEW HISTORY--BY CARL WILMSEN	XV
BIOGRAPHICAL INFORMATION	XIX
I EARLY INFLUENCES, EDUCATION, AND CAREER CHOICES	1
Family Background, Early Influences, and Pre-Graduate Education.....	1
A Temporary Deferment from the Military at MIT.....	16
A Choice of Wartime Service, and Influence of World War II on Career.....	18
An Assignment at Oak Ridge National Laboratory Leads to a Career in Nuclear Engineering ##.....	21
Solving Design Problems at Oak Ridge.....	23
Focusing on Civilian Applications but Working on a Nuclear Reactor for Military Aircraft.....	29
II THE EARLY DAYS OF NUCLEAR POWER: THE DEPARTMENT AT MIT, WORK IN INDUSTRY, AND GOVERNMENT PROGRAMS IN NUCLEAR POWER	34
Starting the Department of Nuclear Engineering Education at MIT.....	34
<i>Developing Instructional Materials</i>	35
<i>Being Encouraged to Consult</i>	36
<i>Government Assistance in Building Nuclear Research Reactors</i>	38
The Savannah River Plant.....	40
Manson Benedict: Mentor.....	42
1950s Government Policy and Early Career.....	43
Costs of Constructing Nuclear Power Plants in the 1960s.....	45
General Atomic, 1957-1959: Strong Physics, Weak Engineering.....	48
Many Different Nuclear Reactor Designs Developed in the 1950s.....	52
Industry and Government Response to Developing a Helium-cooled Graphite Reactor at General Atomic.....	59
Alternatives to Nuclear Power, and Environmental Concerns in the 1960s and 1970s.....	64
III THE UNIVERSITY OF CALIFORNIA: STARTING AND CHAIRING A NEW NUCLEAR ENGINEERING DEPARTMENT, 1959-1964	72
An Invitation to Start the Nuclear Engineering Department at the University of California at Berkeley, 1959.....	72
Establishing the New Department: Opportunities and Challenges, and the Need for a Research Reactor.....	74
Funding, Designing, and Using the Research Reactor.....	79
Working with Chancellor Seaborg, and Relations with Other Academic Departments.....	83

Major Research Programs in the Department at Cal, and the Founding of the American Nuclear Society.....	85
Collaboration on Research, Edward Teller, Competing for Grants, and the Cold War.....	91
The Nuclear Engineering Curriculum at Cal.....	96
Reflections on Citizen Activism at Bodega Bay, and Student Activism in the 1960s.....	100
IV EARLY INVOLVEMENT IN NUCLEAR SAFETY, THE ECONOMICS OF NUCLEAR POWER, AND A CHANGE IN RESEARCH FOCUS.....	105
Member of the National Atomic Energy Safety Licensing Boards, 1963-1974.....	105
<i>The Origin of the Boards</i>	105
<i>Fermi-1</i>	107
<i>Making an Independent Finding of Safety at Diablo Canyon, Circa 1966</i>	110
<i>Meeting the Provisions of the Atomic Energy Act: A General Electric Case</i>	112
<i>Making Technical Improvements: A Condition of Licensing</i>	114
<i>Determining an Acceptably Low Hazard</i>	115
<i>The Need for Better Public Involvement</i>	116
The Economics of Nuclear Power, and Shifting Research Focus to Nuclear Waste Management.....	119
Two More Terms as Department Chair, 1974-1979, and 1984-1988.....	122
<i>Restimulating the Department</i>	122
<i>New Research Directions</i>	125
Quantifying Safety Standards, Economic Issues, Fuel Reprocessing, and Concerns about Weapons Proliferation in the late 1970s.....	129
The Politicization of Public Concern Over Nuclear Power.....	134
V ACCIDENTS, SAFETY, CONFLICT, AND ARMS CONTROL.....	141
The Accident at Three Mile Island, 1979: Implications for Safety, and Industry's Response ##.....	141
Management Blunders in the Construction of the Diablo Canyon Nuclear Power Plant.....	152
Reflections on Resolving Conflicts between the Nuclear Industry and Its Opponents.....	160
Safety Reviews of the River Bend, Louisiana, and Rancho Seco, California Nuclear Power Plants.....	163
The SP-100 Project: Nuclear Power for Space Applications, 1986-1990.....	166
Investigating the Chernobyl Reactor Accident, 1986: Implications for the N Reactor at Hanford, Washington, and Reactors in the Former Soviet Union.....	169
Member of the Delegation to the Soviet Union for Cooperation on Nuclear Reactor Safety, 1988.....	174
Member of a Delegation on Arms Control to the Ukraine, 1994.....	179

VI GEOLOGIC REPOSITORIES FOR NUCLEAR WASTE: PREDICTING PERFORMANCE AND DEVELOPING SAFETY STANDARDS182
Developing a Research Program on the Transport of Nuclear Waste Through Geologic Media182
Origins of the Recommendation for a Geologic Repository Program.....187
A Theoretical Breakthrough in Predicting the Rate of Appearance of Radioactive Materials in the Environment, 1988189
The Waste Isolation System Study, and Brother's Role in Developing the Theory.....194
Outside Reviewer for the Swedish Program, 1986-1991196
Waste Transport, and the Government's Program to Select a High-level Nuclear Waste Repository.....198
Setting Safety Criteria for the Nuclear Regulatory Commission, 1977.203
Technical Difficulties, Delays, and Shrinking Rooms at WIPP, 1978-1985205
Problems with the EPA Standards for Dose Limits208
Dissenting Opinion on Yucca Mountain Standards, 1995212

VII LOOKING FOWARD, LOOKING BACK: PLANNING FOR FUTURE PUBLIC SAFETY, AND REFLECTING ON CAREER, FAMILY, AND THE NUCLEAR INDUSTRY AS A WHOLE...217
Dealing with Possible Conflicts of Interest in Committee Work.....217
Scientific Master to the Court on a Lawsuit Concerning Wartime Releases of Radioactivity at Hanford, Washington, 1994218
Site Characteristics of Yucca Mountain, and Designing for Future Public Safety.....222
 Seismicity and Volcanism222
 Ground Water, Climate Change, and Long-term Warning Systems224
 Making the Repository Watertight, and Reducing the Solubility of Uranium227
Interim Above-ground Storage of Nuclear Waste to Deter Proliferation of Nuclear Weapons.....232
Efforts to Stop Overly Lenient Standards in Proposed Legislation...238
More Caution Needed: Reflections on the Rapid Development of Nuclear Power.....241
More Reflections on the Department at Cal.....243
 Hans Mark: Department Chair, the Free Speech Movement, and National Defence243
 Few Women and Minorities in the Department246
 Committee Work on Campus246
Sibling Rivalry, Music, and Family Support.....249

APPENDIX A255
TRIVIAL PURSUITS:255
A COLLECTION OF CHRONICLES255

APPENDIX B299
COMMENTS FROM COLLEAGUES299

APPENDIX C302
CITATIONS302

APPENDIX D305
CURRICULUM VITAE FOR.....305

APPENDIX E313
LIST OF PUBLICATIONS313

APPENDIX F339
INTERVIEWS ON THE HISTORY OF THE UNIVERSITY OF CALIFORNIA.....339

INDEX355

PREFACE

When President Robert Gordon Sproul proposed that the Regents of the University of California establish a Regional Oral History Office, he was eager to have the office document both the University's history and its impact on the state. The Regents established the office in 1954, "to tape record the memoirs of persons who have contributed significantly to the history of California and the West," thus embracing President Sproul's vision and expanding its scope.

Administratively, the new program at Berkeley was placed within the library, but the budget line was direct to the Office of the President. An Academic Senate committee served as executive. In the four decades that have followed, the program has grown in scope and personnel, and the office has taken its place as a division of The Bancroft Library, the University's manuscript and rare books library. The essential purpose of the Regional Oral History Office, however, remains the same: to document the movers and shakers of California and the West, and to give special attention to those who have strong and continuing links to the University of California.

The Regional Oral History Office at Berkeley is the oldest oral history program within the University system, and the University History Series is the Regional Oral History Office's longest established and most diverse series of memoirs. This series documents the institutional history of the University, through memoirs with leading professors and administrators. At the same time, by tracing the contributions of graduates, faculty members, officers, and staff to a broad array of economic, social, and political institutions, it provides a record of the impact of the University on the wider community of state and nation.

The oral history approach captures the flavor of incidents, events, and personalities and provides details that formal records cannot reach. For faculty, staff, and alumni, these memoirs serve as reminders of the work of predecessors and foster a sense of responsibility toward those who will join the University in years to come. Thus, they bind together University participants from many eras and specialties, reminding them of interests in common. For those who are interviewed, the memoirs present a chance to express perceptions about the University, its role and lasting influences, and to offer their own legacy of memories to the

University itself.

The University History Series over the years has enjoyed financial support from a variety of sources. These include alumni groups and individuals, campus departments, administrative units, and special groups as well as grants and private gifts. For instance, the Women's Faculty Club supported a series on the club and its members in order to preserve insights into the role of women on campus. The Alumni Association supported a number of interviews, including those with Ida Sproul, wife of the President, and athletic coaches Clint Evans and Brutus Hamilton

Their own academic units, often supplemented with contributions from colleagues, have contributed for memoirs with Dean Ewald T. Grether, Business Administration; Professor Garff Wilson, Public Ceremonies; Deans Morrrough P. O'Brien and John Whinnery, Engineering; and Dean Milton Stern, UC Extension. The Office of the Berkeley Chancellor has supported oral history memoirs with Chancellors Edward W. Strong and Albert H. Bowker.

To illustrate the University/community connection, many memoirs of important University figures have in turn inspired, enriched, or grown out of broader series documenting a variety of significant California issues. For example, the Water Resources Center-sponsored interviews of Professors Percy H. McGaughey, Sidney T. Harding, and Wilfred Langelier have led to an ongoing series of oral histories on California water issues. The California Wine Industry Series originated with an interview of University enologist William V. Cruess and now has grown to a fifty-nine-interview series of California's premier winemakers. California Democratic Committeewoman Elinor Heller was interviewed in a series on California Women Political Leaders, with support from the National Endowment for the Humanities; her oral history was expanded to include an extensive discussion of her years as a Regent of the University through interviews funded by her family's gift to The Bancroft Library.

To further the documentation of the University's impact on state and nation, Berkeley's Class of 1931, as their class gift on the occasion of their fiftieth anniversary, endowed an oral history series titled "The University of California, Source of Community Leaders." The series reflects President Sproul's vision by recording the contributions of the University's alumni, faculty members and administrators. The first oral history focused on President Sproul himself. Interviews with thirty-four key individuals dealt with his career from student years in the early 1900s through his term as the University's eleventh President,

from 1930-1958.

Gifts such as these allow the Regional Oral History Office to continue to document the life of the University and its link with its community. Through these oral history interviews, the University keeps its own history alive, along with the flavor of irreplaceable personal memories, experiences, and perceptions. A full list of completed memoirs and those in process in the series is included following the index of this volume.

September 1994

Regional Oral History Office
University of California
Berkeley, California

Harriet Nathan, Series Director
University History Series

Willa K. Baum, Division Head
Regional Oral History Office

INTRODUCTION--by Richard D. Smyser

I would have known about Thomas Harrington Pigford even if I hadn't married Mary Cochran Pigford, his sister.

Prominent and uniquely controversial as he has been for fifty years within the nuclear establishment--industry, academe--Tom would inevitably have come to my attention as, for most of those same fifty years, I was editor of *The Oak Ridger*, daily newspaper in Oak Ridge, Tennessee, a community preoccupied with things nuclear.

Furthermore, his first significant nuclear engineering experiences were at Oak Ridge National Laboratory. (Tom and his late first wife, Katy, living at the time in Oak Ridge with their beloved female boxer, Nemesis, were first to know of our marriage plans and then two of only five guests at the wedding in May 1950.)

But had it not been my immense good fortune to successfully court Mary, there is much about Thomas of which I would NOT have been aware even after a studied reading of the engrossing 241-page oral history that interviewer Carl Wilmsen has elicited from an often not readily self-revealing person.

As a newsman alert to nuclear issues, I likely would have known Tom to be a widely sought-after expert while also a discerning and often severe critic of virtually every aspect of nuclear science and engineering. Not likely, however, would I have guessed that he rigs a mean rope swing, as he did from backyard trees for both his and Katy's and our two daughters respectively when they were mutually of rope swinging age.

From reporting and reading the news, I probably would have learned how Tom, like few others, has won the confidence of seemingly irreconcilable opposing parties to many of the major nuclear-related debates and court actions of the past half century. Both industry people and environmentalists have coveted him on advisory committees and investigative commissions. But only because of our family ties have I appreciated him also as an enthusiastic patron of his late sister's paintings--oils, watercolors. (Mary, a year and a half older than Tom, died earlier this year.)

Soon after his discharge from navy service as an lieutenant J.G.

during World War II he was named director of the Massachusetts Institute of Technology Graduate School of Engineering Practice located in Oak Ridge. But his two-year Oak Ridge residency notwithstanding, his name would have become familiar to me from my obligatory reading of journals like *Science* and *Nucleonics*, especially as later he has gotten deep into national and international concern for potentially dangerous nuclear weapons and civilian power production radioactive residue.

Absent the in-law connection, I might even have sensed his social consciousness reflected in his populist professional positions that frequently make news. However, but for his brief tolerance of me on a tennis court, I might never have fully grasped his multi-faceted athletic prowess and intimidating competitive drive. (He taught me the tennis term "unforced error" of which, while playing against him, I was guilty of many.)

After leaving Oak Ridge, Tom would return at least once a year for meetings of one of the numerous committees and commissions on which he served. In anticipation of tennis games with real opponents that he would often arrange during these visits, he warehoused a pair of smelly tennis shoes in one of our closets, admonishing us regularly to care for them properly. Talk about safeguarding hazardous materials.

Talk about the compleat nuclear person.

Tom's first practical nuclear work at Oak Ridge National Laboratory was with an experimental reactor with implications for a nuclear-powered aircraft. Not too many years later he was involved with studies on a nuclear-powered merchant ship. Still later he worked on projects relevant to nuclear-powered space travel, including technology with the potential for rocketing spent nuclear fuel to the sun.

The aircraft project died soon after conception, Tom among the early skeptics. The merchant ship became a reality but, as Tom had foreseen, proved useful mostly only as a showpiece. Technology for nuclear-powered space travel--if not yet nuclear-powered space launch--has been demonstrated, but shooting the sun with nuclear wastes is still only an idea.

Tom's principal nuclear role has been as educator. Three times he was named chairman of the Department of Nuclear Engineering at the University of California at Berkeley, this after his own academic training at, first, Georgia Tech (he was first in his Class of 1943) and then MIT.

At Berkeley Tom oversaw the assembly, bringing to criticality and operation of a teaching and research reactor and then, after twenty-five years, the reactor's dismantling and then removal. When the reactor was first planned, Tom had insisted that it be located on the main campus. He believed strongly that neither his students nor their program should be isolated.

There were, of course, protests--demonstrations--this being Berkeley. But with communication aggressively pursued by Tom, the protestors became satisfied that they were being heard while the department's teaching and research was not inhibited. And actually, when the reactor was shut down, some protestors protested its removal. They'd lost a convenient target.

And what broadly based teaching and research it was. In time nuclear engineering education at Berkeley--the curriculum and student and staff research--became as multi-faceted as Tom's personal evolution of interests: power-producing reactor design, the fuel cycle including reprocessing, nuclear power economics and, of course, safety and health effects.

His interest in the all-encompassing nuclear waste issue was first piqued in 1977 by detection of a significant error in some crucial waste calculations. "We later added waste disposal as an emerging technical challenge, though (it was) once said to be a trivial technical problem," Tom tells his oral history interviewer, adding, "We should have added it sooner."

And thus Tom the consultant, the expert court witness and the member of the prestigious Kemeny Commission to investigate the March 1979 accident at the Three Mile Island Nuclear plant in Pennsylvania, to which I related not only as kin by marriage to Tom and editor of a newspaper in a very much nuclear town, but also because York, Pennsylvania, the city of my birth and boyhood, is just seventeen miles south and downstream of the reactor site in the Susquehanna River.

More than any of his other authoritative roles as part of the now mounting debate on the future of nuclear power, Tom's voice and writing for the Three Mile Island investigation established him as "the conscience of the American nuclear industry," as Tom was characterized by Walter Lowenstein of the Electric Power Research Institute at the symposium held in Berkeley in 1989 to mark Tom's retirement.

In a "news analysis" in the *Christian Science Monitor* of November 5, 1979, Tom was described by fellow Kemeny Commission members as "strongly but not blindly pro-nuclear." Another commission member said of their final deliberations on a set of recommendations, "We were trying to spread our sails and there Tom was throwing out anchors." As a result, the *Monitor* reporter (David F. Salisbury) wrote, "the nuclear engineer had an

inordinate effect on the recommendations"--moderating them to be significantly less severe on nuclear technology.

"I had by that time been identified as both a member of the nuclear engineering profession as well as a challenger of some of the things the profession did," Tom says of his appointment to the Three Mile Island investigating group.

"Strongly but not blindly pro-nuclear."

Tom's calculations challenged assumptions on the health effects of the contamination cloud from World War II releases of radiation from plutonium production in reactors at the Hanford, Washington, nuclear site, plutonium that in August 1945 fueled the atomic bomb dropped on Nagasaki, Japan, just three days after the first bomb, fueled with U-235 fabricated by E. O. Lawrence's electromagnetic process at Oak Ridge's Y-12 plant, was dropped on Hiroshima.

Before construction of the Diablo nuclear power plant near San Luis Obispo in California, Tom raised serious questions during the planning--and accompanying court actions.

And most recently he has been a leading dissenter on the Yucca Mountain, Nevada, nuclear waste disposal site, standing firm within oversight groups for more stringent allowable release limits. Safety assurances for the next 10,000 years are not sufficiently forward-looking for Tom. He thinks guarantees are needed for the next million years.

But, significantly, his insistence on stricter Yucca Mountain standards is motivated by his belief in the ultimate integrity of nuclear power. He wants the disposal site to become both economically and politically acceptable and, he believes, this can only happen if the ultimate in safeguards is required. And this hasn't happened yet.

His questioning of these projects is why, in nuclear disputes, he has become acceptable as referee, advisor and/or confidante, or all three simultaneously, to both industry and environmental groups. "I feel free to help each side on the points that I think are worth helping them on," says Tom.

Then there is Tom the negotiator, the peacemaker. "Let's get together for a weekend without the lawyers present and talk to each other," he has proposed to some of the seemingly most antagonistic parties, like the utilities and the Sierra Club in one dispute.

Thomas Harrington Pigford, voice of the people, champion of the common man, advocate, like we news people are staunchly, of freedom of information. His favorite among his fellow Kemeny commissioners was a housewife from Middletown, Pennsylvania, the town closest to the reactor.

In the Diablo Canyon court proceedings, a physics teacher from a Santa Barbara high school sought to offer information suggesting that the reactor was being built too close to an earthquake fault. Utility people tried to bar his testimony, but Tom insisted that the professor be heard, and he was.

Tom feels strongly about something I have felt just as strongly about as a newspaper editor: that the public has a right to be witness to--and an active part of--the planning process for major projects that will affect public life, especially those financed fully or partly with public funds.

Tom states repeatedly that his dissents--his insistence on probing all technical questions, his unwillingness to settle for anything less than the most foolproof of standards, his stand on open proceedings--have one goal: to develop nuclear reactors and nuclear waste disposal procedures that the public will accept.

And thus, instinctively, his latest concern: what he calls "civil plutonium," the large quantities of this highly toxic material produced by nuclear power reactors all over the world that could fall into the hands of terrorists. He's done articles, like one especially with journalist Luther J. Carter for *Issues of Science and Technology*, journal of the National Academy of Sciences. He's been speaking, like here in Oak Ridge in May of last year as part of our Community Lectures Series. And, the responsible citizen, he's been writing to his and other senators and representatives. "I'm getting identified as an activist, which I'm proud of," he says.

Tom the athlete:

Not just the tennis player, but squash, skiing and swimming. He was a lifeguard at Highland Park Pool in his native city, Meridian, Mississippi, during his high school years.

And sailing bigtime. He and his crew aboard his scrupulously cared-for Thistle have won numerous sailing championships in San Francisco Bay Area competition. With a big lead in one regatta event off Palo Alto,

their boat capsized just at race's end. But despite this calamity--the swamped vessel now lying on its side--the crew scrambled back on board and the boat drifted across the finish line a winner.

Tom accompanied Mary and me on a group tour of Australia and New Zealand in November, 1993. One of the New Zealand stops was at the gorge where bungee jumping originated. As Tom watched the jumpers, many of them first timers, you could see the juices flowing. He wanted to try, Mary and I were convinced, and only because the group had to move on did he not.

Tom the linguist:

In preparation for his year at the University of Kyoto, he studied Japanese intently. I visited Tom and Katy there briefly on my way to Mainland China with a newspaper editors group in the spring of 1975. To help me make my way alone from Tokyo, Tom sent me a set of personally crafted flash cards displaying all the basic questions--where's the train, the taxi, the bathroom? They worked wonderfully.

While with them in Kyoto for only about twenty-four hours, the three of us took the train for a midday tour of Hiroshima, the first visit for all of us and a moving experience.

For his Kyoto stay, and in anticipation of future consulting in Japan, Tom wanted to translate his surname into Japanese characters for use on the name cards obligatory in Japanese business and academic culture, but there were difficulties. The Japanese word for pig, "buta," simply was not acceptable, pigs being not well regarded in Japan.

Fortuitously, however, he encountered a Japanese linguistic professor who suggested that he use instead the Japanese word "inoshishi," meaning wild boar, an animal respected for its fierce determination, a quality compatible with Tom. So Tom became Ino Se, Inoshishi shortened as the Japanese are wont to do, plus Se, the Japanese word for shallow river and acceptable for "ford."

Tom the author:

Hundreds of reports and articles for journals and most notably, with MIT colleague Manson Benedict, *Nuclear Chemical Engineering*, THE nuclear engineering text. Tom and Manson taught together early in Tom's career, always in total professional harmony, Tom says with emphasis, to which an

incident at our tiny first Oak Ridge house attests.

Late one night--a warm one, fortunately--there was a knock at the door. There in the dark stood Tom and Professor Benedict, suitcases in hand. Either something had gone amiss with their reservations or they'd never made them, but could they spend the night? Of course, Mary told her brother and his friend, if one would sleep in a jungle hammock hung between trees in our backyard. We had only a single guest bed. Tom graciously took the hammock and all slept well as far as we knew.

I can't count the times that I have been told by nuclear people here in Oak Ridge and elsewhere that the Benedict-Pigford book is the bible for nuclear engineering.

Tom the brother:

After each of his visits to Oak Ridge over the years, Tom would take at least one of Mary's paintings back to Berkeley. That he thought she was a pretty good painter (she eschewed being called an artist) gave Mary great joy. On one of his return trips, Delta Airlines lost a large oil seascape in baggage handling. We contemplated a generous damage claim for an irreplaceable work of art, but after a few days Delta found it.

Mary remembered a family trip to the Mississippi Gulf Coast--overnight in a church campground. Tom, age about five, caught a fish and, out of compassion for its capture, insisted on sleeping with it.

Mary remembered her first day of school. When she got back home there was Tom on the wide front porch of their comfortable house on a shady street in Meridian. In his hand were the Sunday comics which he thrust at her and demanded, "Now read!"

Mary, doing graduate work at Emory University Medical School at the same time Tom was an undergraduate at Georgia Tech, recalls Tom asking her to be his date for one of the big Tech proms. They had a ball, she said, this at the heyday of the Big Band Era.

One evening Mary and I were guests at a party at the home of Janet and H. D. MacPherson, both now deceased. She and Mary had worked closely on League of Women Voters projects. Both of us knew him as a distinguished reactor engineer at ORNL. We had just learned that their son, Robert, mathematician at the Institute for Advanced Studies at Princeton, New Jersey, had been elected to the National Academy of Sciences. Knowing that "Mac," as we all called him, was a member of the National Academy of

Engineering, we remarked that the MacPhersons were likely the only family in Oak Ridge to have two members of these highly respected academies.

Without hesitation, Mac looked directly at Mary, denied the distinction, and reminded her that her two brothers were both Academy members too. (Tom, in his interview, has fully acknowledged Robert as an esteemed chemical engineer, chamber music colleague, and sibling rival.)

As special for Mary and me as having Tom and Katy as two of only five persons at our own wedding in 1950 was our presence at Betty and Tom's wedding in November 1994--the ceremony in an idyllic meadow and blessed by perfect weather, a festive reception following under a pristine white tent surprisingly compatible with the rustic northern California ranch setting where it all occurred. And the following day, a Sunday, a gracious and gala luncheon at the winery of their friends, the Hafners, in Healdsburg nearby.

Tom the poet, and from his "Trivial Pursuits," tales of his boyhood which he says he wrote mostly for his grandchildren:

"The lazy S curve of a fly line being thrown on a trout stream, with the fly landing gently on the water in imitation of a hatch, is beauty to behold."

"Sailing is absolutely beautiful! There is no noise other than the wind on the rigging and water; the interaction of sun, wind, sails, hull and water is pure poetry and music, always changing."

Dick Smyser

Oak Ridge, Tennessee
June, 2001

INTRODUCTION--by Elizabeth Pigford

Tom and I met in September 1993, a year after his wife Katy had died and my first husband and I were divorced; we were married in late 1994. So, perforce, my knowledge and impressions of him are incomplete.

Even in this brief time I have come to realize how extraordinary and many-faceted has been his life. His rhododendrons thrive. His woodworking is exquisite. His photographs are beautifully crafted. His oboe playing has been a source of great pleasure to him and others. He has pursued tennis and sailing with verve and skill. All this in addition to his professional accomplishments.

Professionally, Tom is known for his intelligence, perseverance and independent thinking: he castigated the Nuclear Regulatory Commission in his appendage to the Three Mile Island Commission's report; he has consistently insisted on the most stringent standards for public health protection against possible radioactivity at the proposed geologic waste disposal facility at Yucca Mountain; at Oak Ridge, the home of nuclear fuel reprocessing, Tom spoke out against such reprocessing. The list goes on. This is a man who speaks his mind, declaring his views only after meticulous research and maintaining his positions even in the face of powerful opposition.

Tom has been selected for prominent roles in government and industry for his fair-mindedness relating to difficult issues. In this oral history Tom describes the incident in the mid-1980s (this incident has not received wide attention to date) in which the Board of Directors of northern California's beleaguered utility, Pacific Gas & Electric, hired him to protect the corporation and stockholders against possible legal wrongdoing by the company's management in the construction of the Diablo Canyon nuclear facility.

More recently, in 1993, Tom was chosen as Scientific Master of the Federal District Court, Second Circuit in the suit brought by thousands of individuals against the corporations who ran the plutonium manufacturing operation at Hanford, Washington, during World War II. The release of radioactivity to air and water is alleged to have caused numerous cases of cancer. The litigants and the defense agreed that Tom would make an

astute, fair-minded and objective analysis of the hundreds of thousands of pages of documents submitted by both sides.

On his retirement from the University of California in 1989, an international symposium was organized to recognize his achievements. Unbeknown to him, about sixty former students and associates convened from as far away as Japan, Sweden and France to attend the two day seminar: an event of substance was more to Tom's liking than a social event and his associates recognized this.

But what is the measure of the man? Tom's awards speak for themselves but they don't convey how he is evaluated as a person. "Tom is a national treasure," one of his professional colleagues has said to me. In Japan, there is a group of fans called the Pigford Club. When Tom and I were in Tokyo in 1995, more than twenty-five Japanese friends, colleagues, and admirers attended a social function in his honor.

Not all evaluations have been positive; a controversial fellow faculty member, Charles Schwartz of the Physics Department, assailed Tom as an example of a faculty member's inappropriate consulting, asserting that such consulting was a misuse of a connection with UC. Many universities, MIT for example, encourage this type of involvement, but Tom's critic took a different view and released a news story on the day Tom was leaving the country for a stint as a visiting professor at Kuwait University. Even with extensive coverage in the *San Francisco Chronicle* not much happened because of this incident, but it was a jarring experience.

TOM'S LIGHTER, MORE PERSONAL, LESS PROFESSIONAL SIDE:

A former graduate student has told me that Tom took him and a few of his fellow students to a celebratory luncheon at the acclaimed *Chez Panisse* restaurant. The event was not in recognition of some national award or publication of a paper or testimony to a congressional committee. Rather, the occasion was prompted by his having been named the principal oboist in a local, semi-professional orchestra.

Who would have thought that this brainy professor would be hooked on a soap opera? Where possible, and sometimes his day is planned, around watching *The Bold and the Beautiful*. This soap opera gets to him: when Macey died, Tom misted over. Interrupting him, except during commercials, is inadvisable.

Tom has spoken professionally for decades, often off the cuff; yet, a brief eulogy was almost too much. At the January 2001 service for his beloved sister, Mary, whom Dick has beautifully and aptly described,

speaking was so difficult for him that he wrote out his remarks and wasn't sure that he would be able to pay the tribute in person.

Tom was even selected as a character in a novel. Under the name Thomas P. Harrington, Robert Heinlein parodied Tom's work as an expert nuclear consultant.

A youthful caper: In his younger days, while a student at MIT, Tom and his roommate, both southerners, saw fit to fly a Confederate flag from the window of their room. Since the room overlooked the

Charles River, the flag was highly visible and controversial, even becoming the subject of an item in the *Boston Globe*.

For all Tom's technical and policy-influencing accomplishments, he can be the quintessential absent-minded professor. Frequently, he emerges from his basement workshop with bruises, often bleeding, on his bald pate.

"How did you get that, dear?" almost always brings the response, "I have no idea."

During the relatively brief time that Tom has been a part of my life, he has been caring, loyal and supportive--the way he treats other family members and his colleagues alike.

I am still uncovering the layers of his personality and have learned a great deal about him from reading this oral history. I am grateful to the Regional Oral History Office of the University of California Berkeley's Bancroft Library for undertaking the documentation, to date, of his remarkable life.

Betty Pigford
Oakland, California
June 2001

INTERVIEW HISTORY--By Carl Wilmsen

Thomas H. Pigford

In 1952 Thomas Pigford was asked to help start a new department of nuclear engineering at the Massachusetts Institute of Technology. Mr. Pigford had entered MIT as a graduate student in 1943 after receiving his B.S. in chemical engineering from Georgia Tech. In the spring of 1944 he was commissioned as a radar officer in the U.S. Navy, and when he was discharged two years later he returned to MIT and completed his doctoral research on combustion in 1949 under Professor Hoyt Hottel. In 1950 he was appointed assistant professor in chemical engineering and director of MIT's Graduate School of Engineering Practice at Oak Ridge, Tennessee. One purpose of the practice school was to contribute to the more rapid transfer of the new nuclear science and technology to the private sector for peaceful applications. When his two-year assignment at Oak Ridge was nearing completion, Mr. Pigford jumped at the opportunity to work with Manson Benedict in starting the new graduate program in nuclear engineering at MIT, a field that was yet to be defined and developed.

This beginning set the stage for a long career in which Professor Pigford has devoted himself to finding the truth and openly engaging it with the many challenges of a very complex technology. This career, spanning five decades, included starting the Department of Nuclear Engineering at the University of California at Berkeley in 1959, and staying on to chair the department twice more and teach in it for the next forty years. It also included serving as a consultant to industry, serving on federal atomic safety and power plant licensing boards, and serving on committees which analyzed the accidents at the Three Mile Island and Chernobyl nuclear power plants. In the 1970s Professor Pigford began devoting his energies to the management of nuclear waste, and in the late 1990s he began working on the problem of nuclear proliferation.

Due to his long career in nuclear engineering and the many contributions he has made to the University of California, the deposit of his papers in The Bancroft Library in the fall of 1998 generated interest in conducting and preparing Professor Pigford's oral history as part of the Regional Oral History Office's (ROHO) ongoing University History program. The oral history would be funded by an unrestricted research grant received by Professor Pigford.

With these arrangements made, Willa Baum, then division head of ROHO,

asked me to conduct the oral history. Having no expertise in nuclear engineering, and only a passing familiarity with nuclear power issues, I was hesitant. However, none of the other editors at ROHO had any special knowledge of these subjects either, and since I was an interviewer/editor in the natural resources and the environment project area, where oral histories dealing with nuclear power would seem to belong, I ultimately accepted the assignment.

On a warm and sunny afternoon in August of 1999 I met with Professor Pigford at his home to discuss the topics to cover in the oral history and the general format of the interviews. As I drove up the winding streets lined with modern, custom homes built after the conflagration of 1991 destroyed this Oakland hills neighborhood, I wondered what sort of man Professor Pigford would be. Would he be a booster for the nuclear power industry, espousing views contrary in every respect to the views of opponents of nuclear power? To my interest and delight, I found a man who was much more thoughtful, deliberative, and multi-dimensional than such a flat, dull characterization would allow. During that first meeting Professor Pigford told me of the two instances in which he, himself, opposed nuclear power plants, and related his view that much public opposition to nuclear power was well taken and led to much needed improvements in reactor design.

Our initial meeting also shaped the subsequent interviews in important ways. First, during the meeting Professor Pigford said that he had been interviewed by journalists many times before, and wondered what my interviewing style was. Did I ask questions in rapid succession, expecting only short answers from him, or did I ask fewer, open-ended questions, expecting longer responses? I told him that the latter was more my style, and we followed this format in the interviews.

Another outcome of our initial meeting was Professor Pigford's help in my preparations for the interviews. He loaned me a copy of the introductory textbook he co-authored with Manson Benedict in the 1950s, suggested additional readings, and gave me copies of his recent papers on nuclear waste repositories and nuclear proliferation. Based on these readings, as well as on research I did in Professor Pigford's papers in The Bancroft Library and in other literature, I drew up an interview outline and emailed this to Professor Pigford before the first interview.

Beginning in September and ending in December of 1999, we recorded seven interviews. The interviews were approximately two hours or slightly longer in length, yielding a total of about sixteen hours of recorded tape.

All of the interviews were conducted in the living room of Professor Pigford's home. Normally Professor Pigford and I were the only ones present in the room, although his wife, Elizabeth (Betty), would occasionally work in a room down the hall and would sometimes interrupt us

to serve us tea.

While we had originally intended to do five interviews, and the original budget was set accordingly, it became clear as the fifth interview approached that we would need more time to address additional topics, and expand on others that we had already covered. I thus drew up a list of new topics to cover and gave this to Professor Pigford along with an estimate of how much more a longer oral history would cost. Professor Pigford added to the list of topics and indicated that he would raise additional funds to pay for an expanded oral history.

At one point during the interviews Professor Pigford gave me a copy of a collection of autobiographical vignettes he had written for his grandchildren entitled "Trivial Pursuits." Since this included much detail about his personal life which we did not cover in the interviews, I suggested that we use it as an appendix to the oral history volume.

I began editing the transcript of the first interview as soon as I received it from the transcriber. When I had completed my initial editing, Professor Pigford asked to review it right away rather than follow standard ROHO practice and wait until editing of the entire oral history was complete. I agreed to this, and Professor Pigford shared the draft transcript with his wife. After reading through it, Betty wrote a memo to her husband suggesting many new questions most of which dealt with Professor Pigford's childhood, family life, and his reactions to events in his life. Unfortunately, we ended having too little time to address all of her questions in the interviews. Professor Pigford therefore responded to them in two written inserts he prepared for the transcript during his review of it.

Also during his review of the edited transcript Professor Pigford prepared written revisions of two events he narrated during the interviews.

At his request we have replaced the corresponding portions of the interview transcript with these written revisions. Professor Pigford indicated that his reason for writing out revised accounts of these events was to add more detail to the narrative. The written portions of the text are clearly marked. They begin on pages 134 and 144.

Due to staffing changes at the ROHO, including my own departure to accept another position in the College of Natural Resources, Professor Pigford prepared the index for this volume.

The result of our collaboration on this volume is the oral history you now hold in your hands. It is an oral history which bears out the impression I formulated of Professor Pigford at our very first meeting: that of a man who values the open exchange of ideas in pursuit of the truth, who believes in the efficacy of nuclear science and technology, and

who believes in applying that science and technology as competently as possible to protect the environment and to safeguard public health for present and future generations.

Tapes of the interview sessions are available for listening at The Bancroft Library. Other oral histories in the University History and Higher Education series are also available for use in The Bancroft Library (see the list in the appendix in this volume), as are Professor Pigford's papers.

The Regional Oral History Office was established in 1954 to augment through tape-recorded memoirs the Library's materials on the history of California and the West. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of Richard Cándida Smith, Director, and the administrative direction of Charles B. Faulhaber, James D. Hart Director of The Bancroft Library, University of California, Berkeley.

Carl Wilmsen
Interviewer/Editor

July 19, 2001
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

INTERVIEW WITH THOMAS PIGFORD

I EARLY INFLUENCES, EDUCATION, AND CAREER CHOICES

[Interview 1: September 21, 1999] ##¹

Family Background, Early Influences, and Pre-Graduate Education

Wilmsen: As we were just saying, I wanted to start with your personal and family background, and important influences on you while you were growing up. And just for the record, I want to start with where and when you were born.

Pigford: I was born in Meridian, Mississippi in 1922. In April. April 21.

Wilmsen: And you grew up in Meridian?

Pigford: Yes.

Wilmsen: What did your parents do?

Pigford: My father [Lamar Pigford] worked for the United States Postal Service. At first, he worked in the mail car on the railroad, which would carry mail from one city to another, and he would sort mail along the way. Then after many years of that, he was transferred to a desk job so he did not have to go away and work on the railroad, but worked for the post office in Meridian. Those were the Depression years, at least the years when I grew up, and it was considered very important to have a secure

¹## This symbol indicates that a tape or tape segment has begun or ended. A guide to the tapes follows the transcript.

federal job because you had security of employment as long as you did it reasonably well. We didn't have a lot of money, but at least he knew it was coming in.

My mother [Zula Harrington Pigford] had been a school teacher. She had gone, I think, through either a one year or a two year graduate training program to be a school teacher. Other than that, neither my father nor my mother went to college. But we had school teaching in the background. Next door to where I lived was a house occupied by my two maiden aunts who also were school teachers. And that was the family background.

[One of the aunts, whom we called "Ammie"--her real name was Alma--taught seventh grade arithmetic.² I loved her dearly, but it was with some trepidation when I graduated from Marian Park Elementary School and entered Meridian Junior High where Ammie taught. I was in her arithmetic class. I had to ride to and from school with her, and many of the kids chided me for being the teacher's pet. I was anything but! She let it be known that she expected my good behavior and also expected me to learn something.

It was probably a fortunate environment for me, for I had just fallen in love with an adorable little black-haired girl named Emma Gene. Emma Gene must have liked me, too, for she started appearing on Ammie's doorstep to hitch a ride to school with us. Ammie insisted on keeping my thoughts focused on school work, otherwise I would have daydreamed all day about Emma Gene. That love affair lasted only a few months, after which I turned to tennis, woodworking, and school band to overcome my great grief. Maybe I even learned some arithmetic.

Ammie's older sister was named Cora, but nicknamed "Cowa" by Mother during their early childhood. Cowa had once taught, but when I came along she worked in the junior high school lunchroom. I think that all students got free lunches. I saw her during lunch and recess. I felt a little hemmed in by family.

Another close relative whom I admired was my mother's older brother, Herbert. Uncle Herbert and his family lived on a small

²Professor Pigford added the bracketed material during his review of the draft transcript.

farm seven miles north of Meridian. Mother took us there frequently on Sunday afternoon after church. I had to wear good church clothes, and I was required to sit quietly on a chair and say nothing while the adults talked. I thought their conversation was absolutely boring. I hated that part of family visits. However, after an hour or so of this Uncle Herbert evidently sensed my discomfort and suggested that I might like to go out and play. That there was no one to play with bothered me not one bit. The farm itself was fascinating!

This was before the days of rural electrification. The farm had no electricity and no running water. At night they used kerosene lamps. There was an outside toilet with a proper half-moon silhouette cut into the door. All water was drawn from a well just outside the kitchen. My mother would bring them a bag of ice, from the big city of Meridian, to make ice cream. I got to turn the beater and lick it clean once the ice cream was made. It was absolutely delicious, made from pure cream!

Uncle Herbert and his family grew all their own food and grew cotton to sell. Picking cotton in the fall and hauling it off to the local cotton gin, in a wagon pulled by a couple of big mules, was fun, although picking cotton by hand is back-breaking work. I was young enough to be spared. This kind of work was done by black field hands who lived in some disreputable shacks on the farm. They became my good friends.

The family custom was to slaughter a hog once every year, always in the fall after the first frost. The slaughter terrified me, but I loved the rendered products put up by Uncle Herbert and Aunt Annis. Especially good were pieces of backbone, scrapple, brains, some unnamed things that must have been sweetbreads, pigs knuckles, and the delicious spiced sausage that they stuffed into the cleaned-out intestines. There were also hams, cured for months in the smokehouse.

In early autumn Uncle Herbert crushed sugar cane that he had grown to make molasses. Molasses was a staple part of the diet. The cane was stuffed into a crusher that was turned by a mule walking in circles around the crusher, pulling the crusher spoke. The resulting liquid fed into a large cast iron tub, sitting on some rocks on the ground. A wood fire was kept burning around the bottom of the tub. Slowly the liquid boiled down to the thick viscous brown product that is molasses. It must have taken several days. I got to stoke the fire and taste.

Uncle Herbert poured this valuable product into one-gallon tin cans and gave most of them to favored close relatives and friends. Throughout the year my father's breakfast consisted mainly of grits, biscuits, and molasses.

So, in spite of the torture of the formal gossip sessions, I much enjoyed this side of my family. I always wanted to be invited to stay overnight. I could hardly imagine the strange practices of bathing in a tin washtub, sleeping on a feather mattress, going outside to pee, watching the bats perched in the rafters, and waking up to roosters. Strangely, I was never invited. There was a strange formality to family relations. We never visited except at the regular formal visiting hours. Only rarely did the two families get together for dinner or supper.

I had been told that a few miles further north was the larger farm where my mother and her siblings grew up. Strangely, my mother never wanted to go there, so I never saw it. I did inherit a horn made from a large conch shell, used to call the field hands in from the fields for mid-day dinner.

My father's relatives were not so interesting to me. He grew up on a farm, although his family also ran a country store. During my time all his relatives lived in town. His older brother must have been handsome and suave, because he kept getting married to good-looking women.

My family were strict teetotalers, due, I suspect, to my mother's strict Methodist beliefs. Whiskey was known to be a cardinal sin. It must have been a real strain on mother when, at age five, I became quite ill with pneumonia. There was a lot of fever, and nothing seemed to work, not even the horrible castor oil that I was made to take for illness and, at other times, for punishment. As what was said by my doctor to be a last resort, he administered a jolt of whiskey. Evidently it worked, but my mother never again mentioned my sin.

Many years later I discovered a really fascinating addition to our family history. One day in high school a new girl introduced herself to me. To my surprise I learned that her name was Pigford and that her father was my father's younger brother. His name was Paul. I believe that her name was Dorothy. With much excitement I told my mother about my new relatives. She was so shocked that I expected to be in for another dose of castor oil. I then learned that there was

indeed an Uncle Paul but that he was never mentioned because he was one of the biggest bootleggers in the dry state of Mississippi. He was evidently quite wealthy and was able to evade prosecution by bribing the law officials. Of course, he did not attend our church. His professional activities took him all over the state. It was forbidden to even mention his name in our house, hence I had never even heard of him.

Mother was shocked that I was curious and interested in meeting Uncle Paul. Alas, I never did so.

It was about that time that I began to realize that a lot of hypocrisy was going on about drinking. I would hear testimonials in church about the evils of alcohol but later found many of those same individuals at football games drinking from flasks undoubtedly supplied by Uncle Paul. But, with such upbringing, I was not about to depart from the family strictures. I never touched alcohol until I graduated from college and went to M.I.T.

During the war years, when home for a visit, I discovered a pint of whiskey in my father's bureau drawer. I learned that he was fairly relaxed about drinking and enjoyed an occasional snort with his friends. Undoubtedly Mother had once discovered the bottles when she put away his clean shirts. I expect that she never mentioned that to my father. That was their way of dealing with many problems.]³

I had one brother who is no longer living. He became a scientist and engineer--in fact, a well known engineer.

Wilmsen: What was his name?

Pigford: Robert. He was five years older than me, and he died about seven years ago. And my sister, who was in between, was interested in laboratory science. She became a laboratory technician working in hospitals doing laboratory tests. She went to college, and got a four-year degree as a laboratory technologist. She later became a member of the biomedical research staff at the Oak Ridge National Laboratory in Tennessee. She is an accomplished artist and painter.

Wilmsen: And her name?

³End of inserted material.

Pigford: Mary. She's married to Dick Smyser. They live in Oak Ridge, Tennessee. They have two daughters, Lucy and Katy.

[Mary is only two years older than I--I am the youngest of the three siblings.⁴ Because we are close in age, I identified more with Mary than with Robert when growing up. There were few boys my age in the neighborhood, so I kept trying to engage Mary in boy's play, like cops and robbers. I once had to pay her five cents to join me in roller skating down a neighbor's driveway. We tolerated each other, as many siblings do, but we seldom fought. She did teach me how to draw. But there are vivid memories of frequent battles between Mary and my father. He did not understand why Mary spent so much time primping in the first-floor bathroom that she shared with my parents. Robert and I were lucky to have the upper floor to ourselves. It was freezing in the winter and sweltering in the summer, but we had our own bathroom.

It is strange to me, but a great joy, that Mary and I have finally become close friends after we have grown up, even though we seemed to have had little interest in each other during those early years. The same happened between me and Robert.]⁵

Wilmsen: Oak Ridge is a place that comes up again, or will come up again. Where did your brother work as a scientist?

Pigford: He went through college in Mississippi, at Mississippi State College in Starkville, and from there went to graduate study at the University of Illinois. He studied chemical engineering in his undergraduate and graduate years and he got a master's and Ph.D. in chemical engineering at the University of Illinois. In graduating--this was during the war--he took a job at the duPont Company [E. I. du Pont de Nemours and Company] in Wilmington, Delaware, working in the research laboratory. He was working on problems related to chemical warfare. Then after the war, he took a teaching job at the University of Delaware and worked there through most of his professional career. He was chairman of the Department of Chemical Engineering. It became a very renowned place for teaching chemical engineering.

⁴Professor Pigford added the bracketed material during his review of the draft transcript.

⁵End of inserted material.

He did leave Delaware for some period and was professor at University of California at Berkeley in chemical engineering. We were on the campus together at that same time. The campus rules did not allow brothers to be professors in the same college, I think it is, but chemical engineering at Berkeley is in the College of Chemistry and when I came to Berkeley I was in the College of Engineering. So we were there together as brothers.

Wilmsen: And both with chemical engineering in your background.

Pigford: Yes, because I had majored in chemical engineering in college, also.

Wilmsen: Yes. I want to come back to that. You mentioned the Depression. What sort of influence, if any, did growing up during the Depression years have on you?

Pigford: Well, I mainly remember that everybody was worried about having enough money to live on. And my father did have a secure job. He was not well paid, but he was very frugal and so the whole atmosphere of the family was to save and to not be spendthrifts; however, there was enough money for things that we really needed.

[We lived in a comfortable two-story house in the city. It was on a one-acre lot, with space for a very large vegetable garden, my father's pride and joy.⁶ He worked the garden by himself, with essentially no help from his two sons who were far more interested in their own pursuits. He supplied our family with loads of fresh corn, peas, butterbeans, okra, figs, and some watermelon, and there was enough left over to provide vegetables for many friends and neighbors. This kept my father healthy and happy.

The house was not insulated, so it was cold and drafty in the winter. It did have central heating, supplied by a hand-stoked coal furnace in the basement, a dark and dirty place where coal was dumped from a truck through a window in the basement. In one of his few splurges for comfort, my father had installed an automatic coal stoker, to avoid the many late night

⁶Professor Pigford added the bracketed material during his review of the draft transcript.

trips to keep the fire going. But he soon stopped using it. While it ran off and on all night he would lie awake listening to that machine grind up his precious coal.

When natural gas came along he did not convert to gas-fired central heat. It seemed foolish to waste money on heating rooms except when they were occupied. He had gas-fired space heaters installed in the living room and in two bedrooms. And at night we huddled around a pot-bellied stove on the unheated second floor, where my brother and I lived. The gas heaters were not vented to the outside, so it was not a healthy atmosphere to live in.

There was no air conditioning to relieve the sweltering summer heat. During the hot mid-day hours we all took naps, waiting for the cooler late afternoons. After supper cane-backed rocking chairs were moved outside to the lawn, where we would sit, rock, swat mosquitos, and listen to the katydids. Ammie and Cowa joined us, and, infrequently, one of the neighbors.

The house was built during the First World War. There was an outdoor shed to house a milk cow. The cow disappeared when I was a youngster, probably moved to Uncle Herbert's farm. There was an outdoor laundry area where washable clothes were boiled in a large iron pot, heated by burning wood. This was one of the jobs for the hired black woman, Arabella, who came weekly. Clothes lines strung all over the back lawn were usually filled with drying clothes.

For a while there was even a tough little goat, with big horns, who pulled a child's wagon belonging to my brother. I was terribly afraid of that goat. He would butt me and my sister at every opportunity. He didn't last long, because he smelled terrible. I always wanted a dog, but my parents never let me have one. We did have several cats, all of which I loved. I doubt if they were admitted into the house.]⁷

We didn't buy new clothes very often. I lived on hand-me-downs for many years--most of my early years. But when we needed something for school, it was available. Both my brother and I were active in music in the high school. There the music was devoted to the band--the concert and marching band--and my

⁷End of inserted material.

parents bought good instruments for us. We didn't realize that you were supposed to take music lessons and we didn't have music lessons, but we learned music from the band leader.

[Mr. Howard Lane, the band leader, was an absolute jewel of a teacher.⁸ He once played in a circus band, before the days of canned and amplified music. The circus band musicians were real virtuosos. They had to play a little of everything: operatic, symphonic, jazz, marches, hymns, et cetera. Even though he was a clarinetist, he could play and teach every instrument in the band. He had a remarkable sense of good music. He could make our concert band sound like a well-rehearsed symphony orchestra.

But it was Mr. Lane who decided that I should play the French horn. He didn't tell me that it is the most difficult of all the instruments. My brother had been one of the star clarinetists in the band. In a band the clarinet is the leading soprano voice, the same role as violins in an orchestra. I asked Mr. Lane to let me play the clarinet. He thought soberly and said, "It's a nice instrument, but you are better suited for the French horn. And it will save you money because the school owns a fine instrument that you can use." There was a method in his madness. He needed a second French horn player for a piece that he was preparing for the annual state band contest in Hattisburg. The piece was the "Overture to Oberon", by von Weber. It opens with a cappella French horns for two measures. He knew that no one can be sure what notes will sound from a French horn, or whether they will sound at all, especially if played under pressure to perform. For safety and insurance, he had me double with the first-chair horn player, Oscar Poole. The solo repeats eight measures later. On the fateful day, resplendent in our gaudy band uniforms, Mr. Lane raised his baton, with a look of prayer and hope directed at Oscar and me. On the opening hunting-horn solo Oscar made it, but I bobbled. Our roles reversed on our second entrance of the solo. I was thrilled!]⁹

And to continue that thread, my father's main interest, and I think my mother's too was in the future of their boys, especially that they would get some kind of education that would

⁸Professor Pigford added the bracketed material during his review of the draft transcript.

⁹End of inserted material.

give them a secure job. And that they could, after getting that education, live at home, which was the tradition in the old South, get married and raise families. That was their main goal in life.

Well, it didn't happen that way. My brother was a very outstanding student at Mississippi State College and it was at his own initiative, because he liked learning so much, that he chose to go for higher education at the University of Illinois. My father never had that in mind. My brother got a fellowship to go to Illinois so it did not require any family funds, but my father misunderstood and he concluded that my brother had such a poor education at Mississippi that he could not get a job. Evidently, he needed remedial training.

Wilmsen: Oh. [laughs]

Pigford: So my father grumbled and said okay. But he thought that in a way my brother's education had been a failure. Of course later on, he realized that it was anything but that. My brother was a very outstanding individual.

So when I came along, they were determined not to make the same mistake for me that they did with him, and so they asked around and found that the best engineering school in the South was at Georgia Tech. So they scraped and gave me enough money to go to Georgia Tech with the expectation that engineering was a pathway to a good job: I'd come home and be an engineer. I helped pay expenses by tennis and band scholarships and from part-time work, including building cabinets to house instruments for the physics department. I have always loved fine carpentry and cabinet making.

But my parents made a fundamental mistake in their planning for my future. In this town there was only one engineering job and that was the civil engineer who took care of the city water distribution system. Neither my brother nor I had studied civil engineering, so it was inevitable that my parents were going to be disappointed. But that's how I got to go to Georgia Tech, which was a far better school than Mississippi State College, and then followed the pathway that had been set by my brother, although I didn't feel that I was copying him.

Then I elected to choose graduate study, and went to the best place, in Massachusetts, MIT. My father then roughly concluded that he had another mistake on his hands, and he gave

up. [laughter] But we didn't require any more family funds from him because when you study engineering and go to good schools, you get your way paid completely as a graduate student.

That's part of the family setting that began to mold my career.

Wilmsen: Were there any particular books--or you mentioned music was important--how about books that had an influence on you while you were growing up? Or sports, art, things of that nature?

Pigford: Yes, books. I read a lot, but it was mainly Reader's Digest, which was what my mother read. I don't think my father read very much. I never got much into the classics during the growing up period. I'm sure I read some because they taught some good English courses in high school.

Sports. I departed from the family mold by going into sports. My brother was never into sports, and I don't think my father felt it was anything but a waste of time. But probably because my brother had excelled in high school and I was under his shadow, which I resented, I kept seeking ways of branching out, and I got into tennis. One of the reasons is that, typical of the small towns in the South, the way to get real esteem from your colleagues, your peers, and others, is to play football. And it really was true. I tried that, but I was too thin to play very well and I got hurt in sand lot football. So both the football coach and my parents decided that was not for me. And then I was lucky. I had been dabbling at tennis in some neighborhood courts and then a new history professor was hired at the school, the high school. He also was to be in charge of the school tennis team and he didn't like the look of the juniors and seniors who were the tennis team. So he elected to pick out three kids who were just entering junior high and teach them for years, and eventually they would become his tennis team. I was one of those three.

I hadn't really wanted to spend all that time on it, but I got free tennis balls and they were pretty expensive otherwise. He would work us out in the gymnasium, just learning to stroke, every day throughout the school year. We didn't get to play very much, but we learned some of the fundamentals of tennis and finally became the tennis team for the high school.

That didn't get me a lot of points among my peers because tennis was considered to be a sissy game. But it was still

branching out, and I enjoyed it thoroughly. I enjoyed the competition, and I continued and played tennis at Georgia Tech when I went there, on the college team, although that was somewhat weak because this was during the war and there wasn't enough money and gasoline to take trips much away from home to play other schools. I think the NCAA was not even running during the war years, if my memory is correct.

[Question: How did you become involved in woodwork?¹⁰

Pigford: I seem to have been born with a penchant for drawing and for making things. But I got off to a bad start. When I was about five or six years old, my parents gave me a child's tool box, including a hammer and a little saw. With a gleam in my eye I undertook my first project. Mother had complained about the coffee table rocking instead of resting squarely on the floor. When no one was watching, I proceeded to level the table by sawing off a bit at the end of the longest leg. When that didn't work, I kept at it. Soon the coffee table was an inch or so closer to the floor, and it still rocked. I then pasted some cardboard at the end of some of the legs, and it worked better. My mother was not happy.

This did not discourage me. I found some scrap lumber and made a workbench, which I installed in what had once been the cow shed. Then I created a safe haven where a little boy could be alone and imagine all sorts of glorious adventures. It was a tree house, nestled in a large sweet-gum tree well removed from our house. It had a rainproof cabin, with a little pot-bellied stove for heating and for cooking. The first branches of the tree were too high to reach from the ground without the rope ladder that I would pull up after me when retreating into the cabin. I made tiny fishhooks to catch little "shiners" from a creek behind the Magnolia Cemetery, not far from our home. They made a delicious lunch of fried fish. I once collected a few of these beautiful live shiners in a bucket and proudly presented them to Emma Gene, with whom I was having a short-lived romance. She didn't seem to know what to do with them.

After the collapse of the Emma Gene romance, I retreated into what was then the man's world of woodworking, by enrolling in the junior-high school's course in manual training. The

¹⁰Professor Pigford added the bracketed material during his review of the draft transcript.

instructors, Mr. Thigpen and Mr. Morris, were superb. They taught us the value of hand tools and how to make mortise-and-tenon joints. My crowning achievement was an inlaid cedar chest, now owned by my daughter Cindy.

My enthusiasm must have impressed the instructors. The local Sears Roebuck store sold good workshop tools. They asked Mr. Thigpen to nominate some student who could display the use of those tools during the Christmas holidays. He nominated me. I proudly went for the interview, which turned into tragedy because I was too young to be employed at Sears. I wept bitterly. But I showed them. I bought a little motor-driven table saw and set up a woodworking business in the old cow shed. My junior high science teacher told me that his biology class needed some glass-covered specimen boxes for the student's insect collection. He sketched one, which I copied and got the contract. For thirty-five cents a box I constructed about twenty boxes from white pine salvaged from used apple crates donated by the local grocery store. Each box had a sliding glass lid that slid into a groove machined into the wood. The seven dollars was all profit, the most money I had ever seen. Then I found a design for a cutting board sawed into the outline of a pig, with his curly tail serving as a finger hole. I peddled kitchen cutting boards to the neighbors, especially those who received my father's bountiful summer produce. I sold a few, but I had to buy a cheap jig saw to cut the curves. It must have cost about five dollars, so I may not have broken even on cutting boards. Some of the neighbors gave me jobs refinishing their furniture. I had learned to make a fine hand-rubbed varnish finish (a finish that I no longer like, having learned the virtues of tung oil finishes.)

I soon concluded that I wanted to be a professional cabinet maker for the rest of my life. My father, with his customary patience, warned that there is little outlet for fine cabinet work in Meridian. He encouraged it as a hobby, but suggested a profession with greater potential income, such as engineering. My father had already experienced my fascination with unusual careers. Once the tennis stars Fred Perry and Ellsworth Vines played a professional exhibition match in our school gymnasium.

I was a ball boy. They had such beautiful tennis strokes and looked so suave and elegant in their white tennis outfits, with cable-knit sweaters and long white-flannel pants, that I decided then to pursue tennis as a career, even though it was financially unrewarding to even the top professionals in those years. Our tennis coach suggested that it would be a long hard

road for me to reach Perry's skill, even as an amateur. Then, after playing the french horn solo in Rossini's "Overture to William Tell", the "Hi-Yo Silver!" passage, at the regional band contest in Little Rock, Arkansas, I decided that music was to be my future career. Again, my father was gentle and kind but warned that few professional musicians in the South ever rise to fame and fortune. Our band leader echoed that advice. It was beter to follow tennis and music as hobbies, to be enjoyed through life without the struggle of feeding a family. For me it was good advice. Fortunately, I was able to pursue woodworking, tennis, and music through much of the college years and even now.

Question: What other jobs did you have before going to college?

Pigford: For two summers I got the job of managing the city's public tennis courts. It was hard work to wet, scrape, and roll the clay courts every morning and lay new white lines with slaked-lime powder. From the previous manager I inherited equipment to string tennis racquets. I learned the hard way, by nearly ruining the first racquet brought to me for stringing. It was a fine T.A. Davis laminated frame from Australia, brought to me by an unsuspecting player who asked how much tension I could put on the strings. Having had no experience, I quoted the highest number on my tension gauge, which was eighty pounds. He agreed. That turned out to be an excessive tension, too high for the new laminated racquets. Holding my breath, I strung to eighty pounds and was admiring my work when the frame slowly deformed from its lovely oval shape to an ugly ovate shape cocked well off center. Improvising, I fastened the racquet into a racket press, doused it with steam from my coffee pot, and wedged the frame back into a normal oval. The next day my unsuspecting customer came for the racquet, admired its shape ad especially the high-pitched ping of high tension, and gladly paid. The next day he returned with a sad face. "Look what I have done to your fine work," he moaned. The racquet had returned to its ugly ovate shape. I reassured him that sometimes it happens at high tension and that I would be glad to restring at a lower tension if he liked. He was so happy and relieved. I never again strung at eighty pounds.

During a few months I had a part-time job measuring cotton acreage from aerial photographs of farms, a step in the national program to help the depression economy by reducing cotton acreage. This would reduce the supply of cotton and maintain the price. The federal government would pay farmers according to the acreage that was being plowed up. My teacher, Mr.

Thigpen, had recommended me for the job because I was good at reading fine scales on measuring instruments. I never believed in plowing up good cotton crops, but I was paid thirty-five cents per hour to make the measurements, the standard federal pay at that time. I began to suspect that letting the farmers react to supply, demand, and price might have been healthier. My father agreed; he was a bitter antagonist to such programs brought about by President Franklin D. Roosevelt's New Deal.

Another part-time job was installing new bindings on books. A high school classmate and I had read how to restitch and bind used books. The public schools did not supply textbooks. We got the contract to rebind the school books owned by the local orphanage. We used artsy cloth covers with leather trim. Soon one could see our refurbished books on many student desks throughout the public school. This was better pay than measuring cotton lands.

The worst job was a few weekends driving cattle and hogs in the local stockyards, moving them from one holding pen to another. It was the filthiest job I ever had. Clean work is more appealing.

One of the most boring jobs was lifeguarding at the city municipal pool during the first summer vacation after entering college. I had already passed the swimming and life-saving requirements. My job was to sit on one of the lifeguard towers and watch for any one in trouble. There were only a few instances, other than rowdy play. A few times I had to haul people out of the water, and twice artificial respiration was required. The greatest challenge was to ward off the searing midday heat. But I was already accustomed, from earlier summers spent managing the tennis courts. Happiest was the end of the day, when we closed the pool and sluiced down the paving and changing rooms with fire hoses. But it was not at all like the exotic life portrayed in TV movies with alluring female lifeguards. Our lifeguards were all male, and there were no bikinis.]¹¹

Wilmsen: Now was religion an important factor in your upbringing?

¹¹End of inserted material.

Pigford: It was supposed to be. My mother was very religious and my father went along with it. And they insisted, really, that I get involved in it, going to Sunday school every Sunday and going to church. I didn't like it very much. I considered it boring, but I went along with it.

I don't think I did justice to the religion. It was Methodist. So it was there, and I still continued to go to church. It was really when I went to college that I didn't have to go every week. I would go occasionally. And even during college I got involved a little in the church groups but not much.

Wilmsen: Okay. And so at Georgia Tech you studied engineering.

Pigford: Chemical engineering, yes.

Wilmsen: And you were saying that there were some frustrations that led you to go into chemical engineering?

Pigford: Well, actually that created frustrations because my brother had studied that and he was superb academically, and I often felt I made a mistake by going into the same field. But I liked chemistry and I liked physics, and that led into chemical engineering, and I've never regretted that. I could have chosen physics if I'd grown up in the north. In the South, it was not considered to be much of a career for pure physicists unless you wanted to teach physics. And I had no interest in becoming a teacher. So I was happy to be an engineer--chemical engineering.

A Temporary Deferment from the Military at MIT

Wilmsen: Then you finished at Georgia Tech and went to MIT. What led you to decide to go to MIT?

Pigford: Well, at Georgia Tech I had learned that it is good to make good grades. That began to give me some peer respect. And then along the way I learned to my surprise that it was fun to learn. I had done well in high school, but I was not the top of the high school class. But it was a revelation that just learning was fun. I wanted to learn everything possible. I graduated in 1943 at the top of my class, much to my surprise. So I asked the professors, "Where's the best place in the world to go to

graduate school for engineering?" And they said MIT, so I applied there and got accepted. And I got a financial support; I didn't have to pay tuition, and they paid my living expenses. That's why I went to MIT.

Another reason: this was during the war years. I graduated from Georgia Tech in 1943, and during my undergraduate time, I got a deferment from going into the military because it was considered that the country needed engineering graduates for the war effort, without specifying you had to be in the military or anything like that.

But the deferments were getting harder and harder. There was more pressure that at my age, which was the early twenties, I should go into the military or go into work for defense operation in some way. So when I arrived at MIT, the department chairman told me that the pressure was getting difficult and he was not sure that my deferment would be continued. So to be sure that I got deferred, instead of being just a plain graduate student, he gave me an assignment as an instructor, which was an unexpected elevation in rank. I could be a graduate student at the same time, and I could be an instructor of a class of army students there for the chemical warfare service. And that allowed me to continue my graduate studies. I was glad to accept the national policy that students studying and teaching in fields related to war technology should be allowed to continue, to be ready when called to national needs.

Wilmsen: Was the chair Manson Benedict at that time?

Pigford: No, Manson Benedict was working in the war effort at Oak Ridge at that time. I didn't know him, then.

Wilmsen: Oh, okay. Who was the chair?

Pigford: The chair was Professor Warren K. Lewis, who had studied chemistry in Germany which, during the early 1900s, was the place to study chemistry. He really is the one who initiated the profession of chemical engineering when he came back from Germany, combining some mechanical engineering with chemistry. He had formed that department at MIT which was really world renowned.

I did have a problem continuing with that first teaching job, which may be somewhat amusing. I'd come up from the South and I'd never been in the north before, and of course I had a very deep Southern accent. I knew I had it, but I didn't

realize how difficult it was for people who'd grown up elsewhere to understand what I was saying. At my first class at MIT, I was so proud to be instructor. I wrote my name on the board like you're supposed to, and the name of the course and the units. I had planned for the first lecture to be on the subject of ion exchange. It's a technique you use in purifying water--water softening--and so I said, "Gentlemen, today's subject is ion exchange." And in the South you pronounce the vowels from the back of your throat and so even today to say a hard I, I have to consciously push my mouth much harder to start it. Well, they didn't understand this word "ahn", and they raised their hand, and asked, "You mean, iron?" And I said, "No, it's ahns, ahns." And they looked perplexed, clutching their heads, and worried. So finally I wrote the word on the board, I-O-N, and they were happy.

We got along all right, but of course that problem came up so frequently. I had to spend so much time understanding their problem and then writing the correct spelling on the board that we were all getting far behind. And they had to complete the syllabus for the course, otherwise they had to go to the front line, so they sent an emissary to the department chairman and said, this instructor is a nice guy, but we spend so much time learning how to pronounce--or what he means when he pronounces--that we're not going to survive.

And he called me in and said, "Son, you have a problem." He was from southern Delaware and considered himself to be a Southerner, and he arranged for me to take lessons in diction. So every evening I had a tutor who normally teaches English to foreign immigrants who would teach me how to pronounce.

I managed to survive that course, but in the spring I didn't teach a course; Professor Lewis gave me instead a research job working on the techniques for preparing penicillin, which had just come to this country from England and had an enormous impact on the war effort. Another professor took me over and told me his ideas on how to prepare it in a practical way so it could be a nice, readily dissolved solid that could go into a hypodermic needle. And that was fun, but it was not enough to get me continued deferment.

Pigford: The department chairman told me I should go in and take a job at some place down in Tennessee called Oak Ridge, and he was not allowed to tell me what it was, or else I should go into the military.

And it wasn't so bad. Having a bachelor's degree in engineering, I could immediately become an officer and be assigned to the higher levels of technical management of the radar programs for the navy, and I took that. I wasn't about to go to some place in Tennessee. I was so happy to be out of the South, anyway. I didn't want to go back. In fact, I never wanted to go back.

Wilmsen: Why didn't you want to go back to the South?

Pigford: Well, the whole growing up experience in a little town where the only respect you get was from playing football and that kind of thing, and similar things--you get respect by how much you can drink. And I came from a teetollaling family in those days. I never felt comfortable about the human interactions in the South, even in those days.

Wilmsen: So was it--

Pigford: I was not at all comfortable about the Civil Rights--

Wilmsen: You mean the way blacks were treated?

Pigford: Yes. It was really just another world. For example, my mother did have enough money to hire a black person to come to wash clothes and clean up the house. And I loved her very dearly. I never understood--I asked my mother, "When is that lady coming back?" I can remember the day my mother scolded me terribly for calling a black woman a "lady"=". I probably got another dose of castor oil.

Wilmsen: I see. Did service in World War II have any kind of influence on your career plans?

Pigford: Oh, yes, it's bound to have. First, I learned a whole new field, which was radar. And I was in a group that was supposed to be at the top of the heap to do research. I also supervised training of enlisted men to operate radars and to maintain them and so forth, and so I learned an entirely new field which I had not studied, which was a combination of physics and electronics.

And then I was on a ship for part of the time, serving on a flagship. And I found that to be a very easy and nice life. Fortunately this was just after the armistice, so I didn't have to worry about getting shot at. During the first of it, I went to schools, Princeton and MIT to get more and more specialized training in radar. And I considered for a while joining the regular navy as a career, but I realized then that I would miss the learning because you don't really have to learn very much in the navy. So I began to realize that I wanted to follow a very active life. Even going to MIT, I wasn't sure how dedicated I was to learning and the profession, but the navy made me think about it. So when I got back to MIT after discharge in 1945 I really got into it. I was made an instructor again to help teach the many new people coming in on the G.I. Bill.

Wilmsen: You mentioned that you didn't know what was going on at Oak Ridge. Did you know anything about the Manhattan project?

Pigford: Nothing whatsoever until September of 1945. That's when the bomb was dropped, wasn't it?

Wilmsen: August.

Pigford: August? Oh, yes, you're probably right. I read about it in the newspapers, and I had not had enough physics--that kind of physics, nuclear physics--to even understand it. But obviously it was astounding, and I was very impressed with what had happened. I began to get snatches of it through articles, but I had not really planned on going into the field; I still wanted to stay in chemical engineering. And so it was simply like hearing about another technical area. It was interesting but not for me.

Wilmsen: What aspect of chemical engineering had you been in?

Pigford: Well, you see, I was still a student, even though I was an instructor. And well, I did my doctoral dissertation related to combustion. We called it high-output combustion. Jet engines had just come along in aircraft at the very end of World War II, and here I was back at MIT. Let's see, was the Hiroshima bomb dropped in '45 or '44?

Wilmsen: '45.

Pigford: Okay, so I was back at MIT in '46, then. And so jet engines were the hot subject and the mechanics of burning fuel at this high combustion rate, which is enormous--nothing like you'd find

on a gas stove! This was fascinating to me. And we had one of the world's experts on that subject at MIT in chemical engineering and so I chose to work with him, and that was what my doctoral dissertation was on.

Wilmsen: And who was that?

Pigford: Professor Hottel.

An Assignment at Oak Ridge National Laboratory Leads to a Career in Nuclear Engineering ##

Wilmsen: And how did you get interested in the nuclear field?

Pigford: Well, it was not because of any interest in the nuclear field. But even while I was a doctoral student, the department chairman approached me--with the permission of my dissertation professor --and asked me if I would be interested in a job and that it would become an assistant professor immediately, even though I didn't have my doctoral degree.

And for years MIT's chemical engineering department had a unique optional teaching program for graduate students. It was called the School of Chemical Engineering Practice. MIT established its own research laboratories at various plants throughout the northeast and sent students there--graduate students--to work under an assistant professor who was in residence there, and to solve new technical problems that came up in connection with plant operations. These are problems that the plant's research staff would normally work on, and the plant's research staff would usually take several months, up to a year or so, to study it, and come up with a solution. The MIT graduate students were required to come up with a solution in two weeks, which required very quick assessment of the problem.

That's why it required MIT's own staff and faculty in residence, to visualize this and do it--going out and constructing their own apparatus to get a quick result.

For example, suppose you want to measure the water flow rate through a trough, or a canal. Instead of buying an expensive flow rate meter, you just drop a cork in one end and measure the time it takes to come to the other, from which you can calculate the flow rate. The plant people would never do things like

this, but we would invent quick techniques of getting a quick estimate. Our students would invent the experimental techniques, do the experiment, write a report and present their results to the plant staff, but mainly to the MIT faculty. And we all operated independent of the plant personnel. We had to get their permission to go into the plant and do experiments. And sometimes we frightened them a great deal.

In one of my instructor jobs I had been assigned to one of those practice-school stations as an instructor. Well, MIT was then following the emerging applied nuclear field in the aftermath of World War II and their programs were quite famous in the industry. The people running the facilities at Oak Ridge Tennessee--Union Carbide and Carbon, which was a great chemical company--invited MIT to set up a station, a laboratory there. It would be a new kind of station, open to graduate students in all fields of science and engineering. MIT's motivation was to get in on the ground floor of the new science and technology that had peaceful applications. I was invited to be the person in charge as assistant professor, with an instructor working under me as an assistant. We had access to all three plants. All of the new technology was still classified as secret, unfortunately, so the students had to have secret clearances. But when there, they had free access to all of the plants--something that the plant people themselves, except maybe the plant manager, did not have. Both MIT and the AEC felt that the Engineering Practice School would contribute to more rapid declassification and transfer of this new technology to the private sector.

It was exciting and it was all about problems that arise in making nuclear materials and using them. We got into separation of isotopes, and into designing nuclear power reactors, and running nuclear reactors. I was sent down there without any background knowledge of what they were doing, and I had to learn very fast to keep ahead of the students.

I was there for two years during 1950-52, and still officially a graduate student, as well as an assistant professor. So on the side I had to finish writing my doctoral dissertation, and I managed to finish that in 1952.

Wilmsen: So you got so interested in what was happening at Oak Ridge that your dissertation was not something you really followed up then?

Pigford: Well, I had to work very hard on my dissertation when I was there.

Wilmsen: But I mean once you completed it?

Pigford: Oh, you mean the field of the dissertation.

Wilmsen: Yes.

Pigford: Yes. All I knew then was I liked being an assistant professor and I planned to return to MIT if I could, if I was invited to, to continue on the faculty there. And I thought I had a reasonable chance. The system in place in those days was that the department would hire several assistant professors and on the average only one of them would be invited to stay on and have tenure. And usually the assistant professors would teach for four or five years. And it was considered to be a very noble occupation, because of the experience, even if you didn't get tenure. So when my two years at Oak Ridge were coming up, I was expecting to go back to Cambridge, MIT, and teach chemical engineering again.

I should point out that this station at Oak Ridge, being so multidisciplinary, with a broad range of new physics, chemistry, and so forth, was a laboratory for the entire Institute of Technology at MIT, so it took students from mechanical engineering, physics and so forth. And that was fun.

When my two-year assignment at Oak Ridge was about to finish, MIT told me they wanted to start a graduate program in nuclear engineering specifically, at MIT. They had already hired a more senior person, Manson Benedict, who had a background as a chemical engineer and physical chemist and he would be in charge, and I would be his assistant professor. And that was so good that I jumped at it. I don't think I did it because of any special love for nuclear engineering, but the challenge of really starting a new field and teaching it was the thing that motivated me.

Solving Design Problems at Oak Ridge

Wilmsen: Now backing up to your experience at Oak Ridge, what were some of the design problems? Were you actually involved in the design and construction of that--what was it--an aqueous homogenous nuclear reactor?

Pigford: Yes. I got involved in detail in that. It was for the emerging program to develop commercial nuclear power. In fact, I now remember my dates aren't right. My two years in charge of the laboratory which we called the School of Engineering Practice, were over at the beginning of 1952 in February. That was when I learned that MIT wanted me to start up with Benedict nuclear engineering at Cambridge. They knew I needed more education and background in the field, so they arranged for me to work for six months at one of the research laboratories at Oak Ridge then connected with the design of a new type of nuclear power reactor. That gave me wonderful experience. I worked there for six months as a development engineer. During that time I could also take some courses which were offered at Oak Ridge for plant personnel who had not had formal instruction in the physics of reactor design, so that enabled me to get into that in much more detail than I had been able to in my job of supervising the MIT's graduate laboratory there.

Wilmsen: Did you actually help design that reactor?

Pigford: Yes. In fact, we even got involved in it with my students in the laboratory. My job as the director of the laboratory would be to have many contacts throughout the plant, knowing what was going on and where some new idea needed some quick solution, and so one of the places was that design team. The head of the design team would come to me and say, "Here's something that just came up. We don't know the answer. Can you put an MIT student team on it, research it, and give us a quick answer?" And so to do that I had to learn enough about it to be sure that it might be doable in a reasonable time. The plant never thought it was doable in two weeks, but we enjoyed fooling them and usually did, but not always. If I wanted it, we could get an extension, but I wanted the students to work on many different problems, so that's one reason we didn't give them much time. And so I got them involved a little bit in the design of the homogenous reactor, as well as in the design of the project designing a reactor to propel a military aircraft, which was originated at Oak Ridge. I doubt that that's mentioned in my CV.

Wilmsen: I think it is, actually. I was going to ask you about that. But before I do, can you give some examples of the kinds of problems that came up where people would come and say, "Can you put a student group to work on this particular problem?"

Pigford: All right. Well, probably the most outstanding problem was one that just nearly scared the plant half to death.

There are three plants there. One of the plants deals with isotope separation, originally producing enriched uranium 235 to make bombs, but since those days it was run to make fuel for nuclear power reactors. And so to do that, they have to make a gaseous form of uranium called uranium hexafluoride, UF_6 , and that is gas that's pumped along membranes where the light isotope $^{235}UF_6$ diffuses more rapidly leaving behind the heavy isotope that is not the desired one. And they go through those membranes a few thousand times to finally make 93 percent ^{235}U . To make UF_6 gas, take uranium ore, concentrate and purify it, and then expose it to hydrogen fluoride and fluorine gas which reacts with the uranium to make UF_6 .

Now hydrogen fluoride is somewhat similar chemically to hydrogen chloride, which is an acid and is extremely toxic. And they used a lot of it, and they would buy it from Mallinckrodt Chemical Company. They would store it in one part of the plant in these enormous storage tanks looking like a great big propane cylinder that you see in some small towns. They would store the HF as a liquid under pressure. It easily boils to make a gas. They would draw the gas off to react with the uranium in another part of the plant. Well, one of the most potentially dangerous things in Oak Ridge was not from nuclear reactions, but from chemical reactions. If there had ever developed a large leak in that storage tank, that hydrogen fluoride would pour out as a liquid, quickly become a light gas, and it was expected that it could even form a toxic cloud that could move over the town which was some twenty miles away and kill a lot of people. And of course it could kill people in the plant.

And so they had the idea they can save lives by putting a bed of limestone, calcium carbonate, underneath the tanks, so that when the liquid hydrogen fluoride comes out it quickly reacts with the calcium carbonate, making a solid calcium fluoride which fortunately is solid, keeping the fluoride there. It's the fluorine which is the dangerous part, and the carbonate would decompose into carbon dioxide which is not nearly so dangerous.

Well, they told me they needed to prove that this was safe, and they needed to know how much of the toxic gas HF would still escape and how far it would go as a toxic cloud because you can't expect all of it to be combined chemically. So we undertook a two week project to answer those questions.

First thing we did, we actually went out in a field and put some limestone, calcium carbonate, down and poured some--we were wearing chemical suits with add-on breathing apparatus--liquid hydrogen fluoride on it to thus verify that this would stop the material from becoming volatile. Instead it created a small explosion because the heat of the reaction of the first hydrogen fluoride would volatilize quickly the other hydrogen fluoride even before it got to the calcium carbonate, and it was worse than not doing anything at all. That led to our first recommendation to the plant: "GET RID OF THE CALCIUM CARBONATE."

That was an easy first experiment, but we still had the other part of the assignment. We took movies of the calcium carbonate experiment, made some measurements of temperatures and so forth, and wrote it up as a technical report. The other part of the assignment was to calculate where the toxic gas goes if it forms a cloud. The HF and fluorine were stored right at the edge of the plant, and in some meteorological situations, plant buildings were downwind from this site.

Well, I consulted a meteorologist at the University of Tennessee, and he taught me a little about how you predict the travel of clouds of toxic material--of any kind of material--through the air, over the ground and around buildings and so forth. He said that when it comes to a building, it's very complicated because just like the wind in a city, you have little local drafts that can cause eddies and turbulence. And he said, the only way you'll know is to take some pictures. Well, we couldn't do that with actual HF without killing people. So he gave us the idea of using smoke, which is a traditional technique for meteorologists.

So the plant bought some smoke bombs for us, or maybe the professor had some. And to take the pictures, we needed a helicopter. I went to the plant manager. He said, sure you can get a helicopter from someplace in Ohio, which is part of the emergency standby air protection operation anyway, in case an enemy plane comes. So a helicopter was dispatched, and we got a photographer.

We told the plant manager that on a certain day we were going to set off smoke bombs and we would have a helicopter fly over, and he said he would inform his people. And it turned out, the plant personnel really didn't realize what we were doing. On the day we set off the smoke bombs, this big black cloud went up, wafting around the plant entrances, and over it, and they called a bloody area emergency! We had fortunately

forewarned the plant fire department, so they knew it was not a fire. The main problem was the rest of the plant people working in the buildings didn't know about it. Oh, we got our photographs and wrote our report and gave them the answer.
[laughter]

Wilmsen: Were there any of those kinds of problems that eventually became incorporated into reactor design?

Pigford: Well, that one didn't, because that's not a reactor use, but oh, yes, sure. One was a problem on the aqueous homogenous reactor, which is a solution of uranyl sulfate, a sulfate compound of uranium dissolved in water. Most of the people at Oak Ridge were chemical engineers because they're chemical-type problems, and so they visualized this nuclear reactor as just a solution of uranium in water going through a pipe about eight inches in diameter. To make it into a reactor you just make a bulge in the pipeline about four feet in diameter so there's enough material in that volume to make a critical chain reaction. It takes a certain amount. The hydrogen in the water would moderate the energies of the neutrons formed in fission and make them low in energy, which makes them very efficient as chain carriers to cause further fission. And that was a wonderful concept. Then the hot uranium solution flows very fast out into an external heat exchange, boils some other water to make steam, which then runs a turbine. Very attractive concept.

Wilmsen: That's the basic boiling water reactor design, isn't it?

Pigford: No it's not. The boiling water reactor is using solid fuel. The boiling-water reactor is General Electric's commercial power reactor. It uses the conventional type of solid fuel in contact with boiling water coolant to extract the heat. But the aqueous homogenous reactor would use fluid fuel, and the fuel solution itself is pumped around. It gets hot, and it goes through a heat exchanger.

Well, the trouble is, these things aren't as simple as they look on paper. The uranium fission fragments are highly charged particles moving very fast, traveling maybe ten or twenty microns--about a thousandth of an inch--before they are stopped by collisions with water. But in the process they decompose a lot of water, to make hydrogen and oxygen. And this plant, this reactor, would be generating an explosive mixture of hydrogen and oxygen in the perfect proportions to combine chemically to make water again, which is the most dangerous proportion. This plant would produce enormous quantities of chemically explosive

gas. Well, I drew upon my experiments in chemical combustion on my doctoral dissertation.

Wilmsen: Ah ha.

Pigford: See, they had the idea, which was a good one, of injecting the fresh uranium solution tangentially along the inner edge of the tank so it goes around in a vortex. And that vortex would cause the gas to go inward. It is a vortex like you see when your water flows out of the bathtub--it has a hole in the center as a result of the centrifugal force. So that gas would proceed up that hole and then you'd collect it and do something with it. So it would no longer be explosive.

And they asked me to carry it from there: "What do we want to do with it?" Well, designing a burner to burn hydrogen and oxygen safely was an unusual thing because in those days people didn't have hydrogen fuel. But I designed it based upon what I'd learned about combustion and they used it in the process and it worked.

That had started off as a project done by my students. I was able to define the problem and give them some references. I carefully selected the references so they would quickly arrive at the program I thought was going to be suitable, although officially the students were supposed to generate the approach themselves. Then when I was assigned to work full time with that project in early '52, the Oak Ridge engineers asked me to finish up that design for them. And I did. And I worked on a lot of other things for them as well. So that's an example.

Wilmsen: Now that plant was closed in 1957, wasn't it?

Pigford: That project? No, how did you get '57?

Wilmsen: That was in a book by a guy named Del Sesto who did a history about nuclear bombs and nuclear energy policy.

Pigford: I had thought it was closer to '59, but probably he's right. Let's accept it was closed in '57.

Wilmsen: What were the reasons for it closing?

Pigford: Well, by then, the reactor design for the nuclear navy grew so successful, showing that it was a more reliable technology, partly because it didn't have so much hydrogen generation and its water coolant was not nearly so corrosive as the hot uranium solution of the aqueous homogenous reactor. Pure water is not

so bad. So the experiments in the navy had then caused the commercial nuclear program to be slanted towards solid-fuel, water-cooled reactors. And there were two types. One, the water is pressurized so it's always liquid inside the reactor, generating separate steam outside. The other is G.E.'s commercial type in which the water in the reactor is allowed to boil and that steam goes directly to turbines. And its problem is that it carries some radioactivity with it into the turbines, which makes maintenance more difficult. That's one of the problems. But the aqueous homogenous reactor was no longer competitive.

Focusing on Civilian Applications but Working on a Nuclear Reactor for Military Aircraft

Wilmsen: One thing I wanted to ask you about was in the immediate post-war years. Even until about 1954, most of the emphasis, at least as far as government policy was concerned, was towards military uses of atomic energy.

Pigford: Military applications attracted a lot of money and people.

Wilmsen: Right. So I was wondering what prompted you to continue or to go into kind of the civilian side of things?

Pigford: Well, it was more interesting to me and not classified, although it did have some secret parts because they had to utilize some of the developments for the naval program. But it was clear it was soon going to go without involving secrecy and that made a lot of difference to me. At least you can go to professional meetings at large societies and talk to people about what you're doing. And in a secret program to me that would have been too confining.

Furthermore, I like working on things that are useful to the ordinary people and I thought this would make them electricity.

And they had to be economical and that's the important thing in an engineer's life, is to work on something that has to be economical.

Wilmsen: Shall we talk about the aircraft program, then?

Pigford: Yes. You know, as an aside, I was invited by Japan in, I think it was 1996, or '95, to go there and give several lectures. And one of them was tracing the history of important events of the development of nuclear power. And I wrote that up in a way that was kind of brief, and was not supposed to require very much technical knowledge to read.

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Pigford: But, I can tell you how my association with the program to develop nuclear reactors for propelling military aircraft got started. I was a consultant, while teaching in the new nuclear engineering program at MIT. When I was a graduate student after the war and working on that combustion research, a few of my student colleagues, my peers in chemical engineering, got involved in a summer project run by MIT to evaluate the nation's program to develop a nuclear-propelled military aircraft. And that's all I knew about it. They were not allowed to tell me much about it.

But I was also always curious, what is it supposed to do? What's the purpose? Because the jet planes were so darned good as fighters, I couldn't imagine why you needed nuclear reactors for that. Well, I guess I somehow figured out or was told unofficially that the idea was we needed an aircraft that could fly, well, halfway across the world and back without refueling, to do surveillance and drop bombs. And of course that was the beginning of the cold war with Russia.

And so that sounded interesting. I didn't get involved in it, but when I was there at Oak Ridge as director of the school of engineering practice it was one of the programs there. And in those days most of the programs there were secret, partly because it was difficult to separate the laboratory people working on non-secret things and people working on secret things. That's where the actual technical program to develop a nuclear-propelled airplane originated. And if a high-temperature nuclear power reactor could be developed to propel an airplane, it would surely have commercial applications as a land-based nuclear power plant to generate electricity. That's what interested me.

And as you can see from this aqueous homogenous reactor project, which was for commercial power, Oak Ridge people liked to work on fluid fuel reactor systems. I think it's because the original mission for Oak Ridge was to do chemical engineering

development and isotope separation; it wasn't supposed to be a reactor development laboratory, but it got into it, anyway.

And so the Oak Ridge scientists had the brilliant idea of using a fluid fuel for aircraft propulsion. They couldn't use the aqueous homogenous concept because the reactor would need to operate at a much higher temperature to simulate what a jet engine could do, which operates on high temperature combustion. The aqueous homogenous would require enormous pressure vessels--very thick walls--to operate at such high temperatures because of the volatility of water. But if you used molten salt--say, for example, like molten sodium chloride, that can be liquid at a pretty high temperature without requiring much pressure to contain it. And that's the basic idea. So their chemists worked out a mixture of fluorides of uranium and beryllium and zirconium such that the mixture had a lower melting point than any one of the individual components, which is typical in solids. Mixed solids frequently melt at lower temperatures.

As an aside, in New England they put some kind of salt on the streets in the winter to actually melt the ice because it forms a low temperature melting solid. And I forget the name of the salt--calcium chloride, maybe. Well, back to this. Sorry.

And so this molten salt mixture was to be pumped through pipes and get hot at a bulge in the pipeline--that's the concept--when it becomes critical. And then you pump it out and pipe the hot molten salt out to radiators in the wings, and they heat the air--just like a combustion chamber would in the jet engine. The air gets very hot and expands quickly and gives you a jet-engine propulsion. It's got to be very hot and so the materials to contain that mixture were just unknown, and the material problems were imposing. But they developed very special alloys that had never been developed before. Some were made of niobium, which is a very expensive high-temperature alloy. And the molten salt would come out of the critical part of the reactor in little tiny tubes, thousands of them, which looked like spaghetti carrying molten salt, hot molten salt, out to the wing engines.

On paper it looked great. It was the kind of reactor that I say is typical of being designed by a physicist who has no experience in engineering. It had worlds of engineering problems and they were all fun to work on. The whole thing was so difficult. They couldn't afford to put much weight into shielding the reactor, because this is an airplane! And so they used very expensive shielding made out of mixtures of iron and

special compounds of hydrogen, and finally ended up putting the shielding only like a big umbrella separating the pilot and the reactor. This means if you're on the ground and need maintenance, you can't get near the side of the airplane, it's not shielded. And even if the engine is shut down, the intense radioactivity in the fission products would still require shielding. And even then, they couldn't get enough shielding to reduce the radiation levels reaching the pilots to normal tolerance levels. Finally they had to restrict the pilots to people over a certain age, so that they're not child-bearing. It was fanciful and rather crazy.

And the amount of money spent by the government in that project was mind-boggling. I was working on detailed problems and I didn't have to get much into policy on it, but I continued associating with that project when I went back to Cambridge to teach and study nuclear engineering with Benedict.

And one of my first consulting jobs was to consult for Pratt and Whitney Aircraft [Corporation] in Hartford, Connecticut, which was one of the great--and still is--jet engine builders in the world. And they were the main contractor to develop and build the engine system. They weren't supposed to develop the reactor, but they had to work on reactor problems to try to minimize weight and things like that. But by that time I had developed more of what I felt were intuitive feelings of what's worth working on, and I kept discussing this with the project's engineers and asking them, "wouldn't you solve a lot of problems by going to solid fuels so that your coolant is not so radioactive and corrosive?" That was an enormous departure from the design that Oak Ridge had developed.

I managed to, at least I think I helped, get started a small separate project at Hartford on such a concept. And they worked on it, it began to look better, and I even went to the corporation CEO a few times to get his support. There were just a myriad of fascinating problems on either concept, but there's a concept I helped get started.

It all collapsed, fortunately. And that must have been in the mid-sixties. That project went too long without being shaken down. Did you learn from your book the date it was shut down, the nuclear propelled aircraft?

Wilmsen: I think it was '63, but I don't remember.

Pigford: I think it's not that early, but it may be. You see, the trouble is the air force was the one who made the decisions and managed the money, and it fell in love with almost anything nuclear. And finally what shut it down was the realization that we had an alternative: we had missiles--ICBM's--that could finally fly all the way to Russia and hit a target. You didn't need the nuclear-propelled airplane, which was a low-speed aircraft anyway. It would probably have been shot down easily.

Wilmsen: Were they concerned about nuclear contaminants escaping into the environment in the event of the plane being shot down or an accident or something like that?

Pigford: Oh yes. They didn't get very far into that evaluation of safety. Frankly I thought just the fact that they were so near the threshold of danger in the planning, like the selection of pilots, like ground maintenance, and so forth that they should have realized that it was a very iffy thing in the first place. And I guess that one helped me feel that here there was a great need for more systems engineering in the whole nuclear program, especially the kind of engineering that reassesses the original purpose of the project, the environmental consequences, and the alternatives.

Designing the nuclear bombs, which I know little about, does not require as much engineering. In fact, I'd say it seems very little. The plutonium is the size of a grapefruit and you can spend all the amount of money you care to on machining to a millionth of an inch tolerance--that cost seems to make little difference. This requires a good special lathe and milling machine, that's all--plus careful machining of all the other parts to make the device work. No problems of heat extraction, corrosion, et cetera.

II THE EARLY DAYS OF NUCLEAR POWER: THE DEPARTMENT AT MIT, WORK IN INDUSTRY, AND GOVERNMENT PROGRAMS IN NUCLEAR POWER

Starting the Department of Nuclear Engineering Education at MIT

Wilmsen: Back to helping start the department of nuclear engineering education at MIT. You described a little bit about how you got involved in it. How did you go about starting the department?

Pigford: Well, administratively MIT started it, as I told you, by giving the job to these two professors: I, the assistant professor, and Manson Benedict, my senior professor. And we had authority to create an academic program at the graduate level then. And we described the content like you would in any new academic program, covering physics of nuclear reactions in reactors, nuclear reactor design, covering the entire fuel cycle because both of us had worked in the entire fuel cycle. And that's what we mean, is every operation that has to be carried out to make the whole system work as a, say, commercial power generator. Isotope separation: that's what my senior professor had worked on during the war; chemical separation of fuel after it's been used, which I worked on at Oak Ridge and designing reactors, which I had gotten into more than my senior professor. We later added waste disposal as an emerging technical challenge, though once said to be a trivial technical problem. We should have added it sooner. The fuel cycle, though, originated that way.

And you see, starting from the Oak Ridge engineering practice school experience, I got involved in every part of the whole operation because originally that was our purpose --to involve our students in that. And to involve them, I had to get

involved.

So I tried to institute that. Together we specialized in reactor design and the whole fuel cycle, because we both were chemical engineers. The problem was how to get students, and how to get instructional material developed. That is, how to learn it enough so we could teach it. Clearly my boss could teach isotope separation without a problem: he was the international expert, but that was just one of the subjects. To get students, we learned that Admiral Hyman Rickover--you've heard of that name?

Wilmsen: Yes.

Pigford: Had already started his naval nuclear propulsion program and he was a real bug on education at all levels of his people in the service. He'd already arranged to send some of his officers up to MIT to take a master's degree in physics, and he heard about nuclear engineering and so he offered to send several to us to study nuclear engineering. And those were our first graduate students, together with a little handful of civilians.

Developing Instructional Materials

Pigford: Instructional material. There weren't any textbooks. There were very few unclassified technical reports, but already my boss and I had secret clearances. And the Atomic Energy Commission [AEC] was very supportive of our effort and so it gave us access to an enormous range of secret material. Few people have had it to that extent. I also identified various experts for help on parts of it. There was a physics professor at MIT who knew in detail the physics part of designing reactors, how to make it critical and so forth. Then the AEC set up a system by which we could identify various parts of those reports and nominate them for security declassification, frequently by obliterating certain words or something like that. And that system worked. There it was agonizing because the hardest part was for us to digest, learn, and teach this flood of new technical information. I would get the declassified material just days before our next lecture, have to learn it, and give the next lecture. And that was fun.

Wilmsen: Was that when David Lilienthal was the director of the Atomic Energy Commission?

Pigford: He was the chairman of the Atomic Energy Commission. He left

during that period followed by maybe Lewis Strauss, or maybe there was someone in between there. I never did deal with Lilienthal. Not before we started the nuclear engineering program, I'm pretty sure. I'm sure that my boss, Benedict, dealt with him because for his own training he spent two years working at AEC in Washington, officially setting up techniques to evaluate the various AEC projects--mathematical techniques like an independent check on their performance and things like that. So he must have worked with Lilienthal. And he did during the war, too. So that's how we got started.

The students, the navy students, were all older than I was, and I think smarter, but they were there to learn. We learned together. And then master's theses: some were under my supervision, some under Benedict's supervision.

Wilmsen: Was that the first nuclear engineering program in the country?

Pigford: No. North Carolina State College had the first one, and it started, I think, at the undergraduate level. Two people started it and they had come from Oak Ridge.

The University of Michigan may have started one before we did at MIT. It wasn't much before. In that case, they did not staff it with people who had much contact with the field, and that was a problem.

Being Encouraged to Consult

Pigford: The other thing MIT did, which was very important, besides using its influence to get it started with AEC and Rickover's help in this way--which no other school did--was to encourage us to consult. And quite different from many, many universities and even other engineering schools, MIT's policy is that each of its faculty should consult. It's not so that they can augment their salary, although frequently they do, but MIT feels that the right kind of consulting exposes you to confronting new ideas and having to develop that kind of skill, which is an innovative technical skill, to come up with useful and quick answers--sometimes not so quick if you continue as a consultant. And also, being there, you relate to the needs of your industry which in engineering is your primary customer for your students.

Engineering schools that don't do that really get left

behind. Berkeley does that, but not enough. Stanford does it more than Berkeley--not as much as MIT. And of course, it gets abused sometimes. The field is full of MIT professors who have started their own companies to enhance their consulting, and then let it detract from their work at MIT. Some of them have done that and also created large laboratories at MIT. An example of a famous professor at MIT who served well academia and industry is Harold Edgerton, who developed the strobe lights in photography.

There's a private company, Edgerton, Germeshausen, and Greer, also known as EGG which bears his name and is a major success story, but he was extremely careful never to slight his work at MIT.

I started up a company once because I was getting so many consulting requests. I couldn't take them, so I thought--by this time we had a couple of younger professors in the department who didn't have as much contact--I'll start a company and I'll get those requests and I'll help my colleagues do the work. But it wasn't any fun at all. I spent too much time managing the company and I found it more interesting to do the technical work.

Wilmsen: Oh. Was that when you were still at MIT?

Pigford: Yes. And I could have made a lot of money that way, but it wasn't fun. So we did one job and then I, by mutual agreement, quit.

Wilmsen: Who were the principle people at the Atomic Energy Commission that you dealt with when you were setting up the department at MIT?

Pigford: Larry Hafstad was director of reactor development. And then Lewis Strauss--I didn't have much contact with him, but Benedict did--was the chairman of the Atomic Energy Commission. Willard Libby was on loan, on leave from UCLA. He's a famous chemist and he was a member of the Atomic Energy Commission. Then through the years there emerged other names: Kenneth Davis, who had been a chemical engineer working with Standard Oil of California which is now Chevron. And he had gone to work for the Atomic Energy Commission as head of reactor development following Hafstad, I think it was. And I could rattle off dozens of names, but those are samples.

Wilmsen: Were they working with other schools in developing similar departments, or just MIT?

Pigford: Not in the way that I've described they worked with us. They didn't work with us like that for very long because once we got enough knowledge ourselves, we could then expand that and we started writing our own course material. In fact we were writing about a dozen textbooks at one time and only one of them ever got published. We just found it took too much time just to keep learning and teaching new material. Other schools did not have that help in getting started out.

Government Assistance in Building Nuclear Research Reactors

Pigford: There was another kind of help that was provided by the government, and provided by money from the Atomic Energy Commission, but administered by the National Science Foundation. They had the idea in those days that every school needed a nuclear research reactor, which I think was dumb, but we knew we wanted one at MIT. The funds administered through the National Science Foundation were offered quite readily--too much so in my view--to schools throughout the country. And it's hard, especially for a small school, to turn something like that down. For example, Reed College in Portland, Oregon. Reed is not into high tech engineering. They don't teach engineering there. The physics department there got a small reactor. It has been a pain in the neck. Operating a small reactor is highly regulated for safety, as it should be, by the Nuclear Regulatory Commission, and Reed thought a physics professor could do that. Well, physics professors just by nature aren't interested in such mundane work. Experiments with reactors is simply old physics to physics people.

Stanford got a reactor. Stanford should have known better. It's a first rate engineering school, but they learned sooner than most people that it wasn't for them. They had never committed the resources to faculty and so forth for a solid department like MIT did and as we later did at Berkeley. They got rid of that reactor after a few years, which was good sense for them. And it never was a big enough reactor to do much research.

Wilmsen: How can a department make good use of a reactor?

Pigford: Well, first as training. It's nice to have the students see a chain reaction, which is a rare thing. And you can build a simple reactor that undergoes a chain reaction. They can pull

the control rods out, and see the counter going up, and that's pretty exciting. But that's demonstration. We don't even do many demonstrations like that in a physics laboratory. For example, when I once organized and taught an undergraduate applied physics lab at Berkeley, I assigned the students the task of designing a simple way of measuring interaction at applied electromagnet and electrostatic fields with the quantum spins of nuclei, known as "nuclear magnetic resonance." The students studied the theory, designed and constructed the equipment from readily available crude parts, and measured the resonance conditions of spin flipping of hydrogen nuclei. This is a far more educational approach than taking them to a hospital lab and watching the operator run a "magnetic resonance imaging" machine, known as an "MRI". And that's where you learn things, not by running a product that somebody has made and commercialized.

But the teaching reactors offered by the government were already designed and built by someone else. That's help that the government supplied to lots of schools throughout the country. The reactor at Berkeley which we started working on in either 1962 or '63, I think, cost \$800,000 which was a lot of money in those days. The one at MIT cost about that much. MIT's is still running. Ours is not running. I led the program to shut ours down in the late 1980s. It was no longer used to generate nuclear radiation for student-faculty research, although still actively used by several government contractors. We simply could no longer justify tying up so much expensive on-campus space dedicated to academically related research.

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Pigford: But a large number of schools throughout the country did follow the lead and in one way or another established nuclear engineering at the graduate, or usually undergraduate level. But a lot of those schools don't have strong graduate programs in engineering. That's what it boils down to. Those schools that do good graduate work in engineering frequently have nuclear engineering graduate programs.

Wilmsen: How did what I referred to before, about most of the money in personnel going into the military side of nuclear energy or nuclear applications, effect your developing the department at MIT?

Pigford: Well, it gave us consulting jobs and exposure to many new and challenging technical problems that arise in nuclear reactor applications. I consulted on the nuclear aircraft program. I

consulted during the early part of the cold war. The government built the Savannah River Plant to produce more plutonium for bombs. Very big plant. This was a newer version of the Hanford plant that had been built in Washington during the war. And MIT urged me to do it. In fact, they arranged the very special consulting job to consult there. And I got wonderful technical experience. I consulted, also, for an engineering company in New York, Foster Wheeler, that was working entirely on the commercial applications. These places would look for our students to hire them. Most of our graduates were employed in the commercial nuclear field. I did not consult on the design and construction of nuclear weapons.

I would have been very unhappy if the only job opportunities had been in the military work. In fact, I would have preferred that most of them be in the private sector, and they were, because the private sector was growing very fast all through the 1960s and 1970s.

The Savannah River Plant

Wilmsen: Now the plant in Savannah that you mentioned, was that principally a reprocessing plant?

Pigford: It contained a very large reprocessing plant. Its job was to make plutonium for bombs, so first they had to build some nuclear reactors where they convert uranium 238 into plutonium. That's a solid fuel, which is uranium metal clad with aluminum, cooled with heavy water. Heavy water is water made from a rare isotope of hydrogen. In nature there is only one part of heavy hydrogen [deuterium] in 7000 parts normal hydrogen. Heavy water absorbs only a few neutrons, so you can use it to cool a reactor, and use the extra neutrons to make a lot more plutonium. So they had to build a heavy water production plant, with isotope separation. Beautiful technical problem. A brand new design. After that, the fuel is removed from the reactors, dissolved in acid, and the plutonium is separated, purified, and sent to plants in other locations in Texas or Colorado to be made into metal and made into bomb parts. The heavy water separation and reactor technology later proved to be important to commercial nuclear power, particularly in Canada.

Wilmsen: And at the Savannah River plant, is that where they developed the

Purex process?

Pigford: No, Purex was actually developed at the Hanford plant and at Oak Ridge. During the war they used a crude process where they simply scaled up a laboratory separation that Glenn Seaborg had developed at Gilman Hall. And it did the job: it made plutonium, but it made an awful lot of terrible radioactive waste, just enormous quantities. And we still have the legacy from that.

Well, after the war, General Electric came in to operate Hanford. Du Pont built those plants during the war, but it wanted out after the war. I should point out that du Pont finally agreed to develop Savannah River after arm twisting from the government. They charged one dollar a year to do it. They didn't want to have any more connection with the military program, but they did it under urging that it was an obligation to the national security. General Electric came in and took over that part of the Hanford operations dealing with chemical separation and they installed a Purex process.

The original chemistry was developed at Oak Ridge. In fact, I worked on that at Oak Ridge during the practice school days. My students did. That process was implemented at Hanford, and it was the far superior process that made much cleaner separations and didn't produce nearly the kind of waste that they'd had before. And then with that experience, du Pont implemented that process at Savannah River but with some modifications of equipment. Purex is still the basic process used in reprocessing spent fuel from commercial power reactors throughout the world.

Wilmsen: Do you know why du Pont didn't want to be connected with the military?

Pigford: Well, their business is commercial chemical products, and they can make a lot more money--you only get a fee for working for the government. Westinghouse, which is a company that runs the equivalent operation at Savannah River now, does not make as much money off its commercial investment, it's just the nature of the company. General Electric does, but that's run entirely differently than Westinghouse. Westinghouse sought, and claimed, the contract to run Savannah River when du Pont finally bowed out. Well, it hasn't been so long ago--maybe fifteen years ago. But du Pont, typical of well-run chemical companies, makes a lot more revenue off of its investment. There is not much revenue in simply the fee for running a government operation.

The University of California contracts to operate government

laboratories because it doesn't make any money from operations otherwise, except for its investments in the stock market. So it likes the fee it gets from running the government laboratories at Livermore, Berkeley, and Los Alamos. And sometimes it does a lousy job in running them.

Wilmsen: Okay, well, we can talk about that.

Pigford: There was an excellent op-ed editorial in the Chronicle today. You've heard about these security violations?

Wilmsen: Oh, yes.

Pigford: And it says at last the university is being taken to task, because really it's the operating organization that is responsible for implementing security requirements. And we've been hearing for years that DOE or FBI or CIA screwed up. Yes, they did a little, but the university has the number one responsibility.

Manson Benedict: Mentor

Wilmsen: I see. What was it like working with Manson Benedict?

Pigford: Well, that's a good question. It was a great experience. He is a prince of a man. It's really very difficult to articulate it. He's kind of shy fellow, yet quite confident in his own abilities. Low key, very considerate of other people, ideal.

Every young career man, I'm sure--certainly in the profession of science and engineering--needs a mentor. And I was very lucky.

He was in great demand for consulting work, and he knew enough to keep it in bounds. And it was partly through his influence that I was so lucky to be in on so much exciting technical work.

We were learning together and I grew to respect his knowledge as a scientist, and yet he was able to help rather than instruct me--just the sort of thing I needed. Even when we started writing textbooks, it was amazing how things worked out. I had not done a lot of writing before then. We would divide a

book up into chapters and generally try to rely on what each of us had developed with class notes, but not completely. Remarkably, our chapters just melded; you couldn't tell who wrote them. That probably shows his influence on me. He didn't tell me how to write. In fact he was very kind, too kind: he tried his best to accept what I did write, but would always bring up enough questions so I could see what needed to be done.

Even in the second edition of our book, which came out in 1981, we were then at two different places--he at MIT still and I at Berkeley--and he was no longer department chairman. He had retired from that, so he had time to do the book. I was department chairman at that time, and somewhat pressured, but from all the way across the country, it seemed to work the same way, as if we were in the same place.

He was very, very great. I say "was"--he's still living. But he has a form of Alzheimers that is debilitating to him. He was in the process of writing his memoirs, but now he can't remember enough to write.

Wilmsen: How old is he now?

Pigford: He must be in his mid-eighties. Maybe high eighties, I guess. Yes.

Wilmsen: What was your reaction to Eisenhower's Atoms for Peace speech?

1950s Government Policy and Early Career

Pigford: Oh, I was somewhat thrilled about it. I'd never encountered something that had such a push from government. Of course I'd encountered that during the war, but for a peacetime subject, it was quite unusual.

I couldn't figure out why Eisenhower was doing this, because he didn't know beans about it. And frankly I wasn't sure at the time that he knew enough about the nuclear field to make a judgement, but now I'm sure he did. He was relying upon his advisors and he saw this as a great political opportunity. And I'm sure he believed in it and was happy to be involved in it.

I think it was a little overblown, similar to offering so

much money for training and research reactors to so many schools, but not that distorted. It brought some other countries into the field of commercial nuclear power a little more rapidly than they should have been.

I had a consulting job many years later in Kuwait and they wanted to build some nuclear power reactors. It's a small country, loads of money from loads of oil. I went there and asked them why. You wouldn't normally build a commercial-size electrical generating plant in such a small country anyway. Because they're so small, it's hard to be economical. But they distilled a lot of sea water to make their fresh water, which is a very expensive process. It needed a lot of energy for that.

Well, I told them, "Why don't you just use your gas that you flare up from oil wells, or else use oil?" They said, "Our oil makes so much money for us, we can't afford to use it, and we were told by this British company that we can build a few nuclear power reactors and they will do the job."

Well, that's nonsense. And I didn't know the British company, but I'm convinced they were either ill-informed, or simply riding the coattails of the enthusiasm that got started with those international conventions that Eisenhower initiated. And Kuwait didn't have the capability of making an evaluation. They were subject to the whims of an English engineering company, which to them was bound to have all of the knowledge and reputation it needed. And there's an example of what could have been a very negative fallout from over-enthusiasm.

Wilmsen: I take it they didn't build the reactors?

Pigford: No. I think I earned my consulting fee. [laughter]

Wilmsen: In 1954 Congress passed the second Atomic Energy Act. What kind of an impact did that have on the school at MIT or your work, if any?

Pigford: Well, it did have an impact. It did give a blessing to commercial nuclear energy. At MIT we wanted that kind of national policy support. In short, we didn't want to work just for the military program. We would not have touched it. And that solidified nuclear energy in the minds of many people--parents of students--and we began to get some of the best students that had ever come to MIT.

Wilmsen: Did you testify before Congress or anything like that?

Pigford: Oh, sure. I testified through the years.

Wilmsen: I mean, for that particular act?

Pigford: No. No. My name hadn't risen to such a level that they would want to hear me testify at Congress. And in those days I really hadn't started forming many judgement opinions as to what's worth doing in terms of an overall policy. I loved getting into a project and being presented with technical problems and solving them, but you don't testify in Congress on things like that. They need policy advice, and I wasn't into that, yet. Pratt and Whitney was the first place where I rather carefully, cautiously got into policy advice; many years later.

Costs of Constructing Nuclear Power Plants in the 1960s

Wilmsen: In the 1960s it started to appear that there were really pretty tremendous cost overruns in the building of nuclear power plants, and I was wondering how you reacted to that at that time.

Pigford: You mean commercial plants?

Wilmsen: Yes. The costs actually really far exceeding what estimates had been.

Pigford: Well, in fact, for a long while there were no cost overruns. The main companies supplying commercial nuclear plants were General Electric and Westinghouse, and shortly thereafter also Babcock and Wilcox and Combustion Engineering. To convince their customers that this was real, they had to offer guarantees. Not only did they guarantee the cost of the plant, but they guaranteed the cost of the fuel cycle, which was really not much under their control. How can you guarantee the future cost of your uranium? The plant's supposed to last thirty years and the market may change.

Wilmsen: Yes.

Pigford: They guaranteed it, so there were no cost overruns. They swallowed the extra cost, and I think they swallowed more than they expected to. But the potential difference was large. And it turned out to be very large. One hundred nuclear plants at a

commercial value of a few billion each, you see, that's several hundred billion. And that's big even with today's numbers. As they developed their first customers, which were during the early sixties, they started backing away from those guarantees as quickly as they could. And then the cost overruns began to show up. Diablo Canyon you've heard of.

Wilmsen: Yes.

Pigford: The Diablo Canyon nuclear plant, built and owned by Pacific Gas and Electric Company, is the classic example. I know something about that because I had two official interactions. One was when PG&E applied in the 1960s, maybe 1967, for permits from the Atomic Energy Commission to start construction, which required safety analysis and so forth. I was a member of the three-man Atomic Safety and Licensing Board set up by Congress to review that application and make the first legal decision that it would be safe and competently operated. The other interaction I had was at the very end of that whole series of problems which occurred all up into the 1980s. I got involved as a consultant to a legal firm which was the counsel for the special committee of the board of directors that had been set up, authorized by the full board to act on the behalf of the company against the officers of the company and against the rest of the board because of errors in the management of the Diablo Canyon project. It led to a class-action settlement on behalf of the company and its stockholders, against the officers and directors. And so those were the direct kind of actions I had.

Well, there was an enormous cost overrun that we can get into later for more detail. They got the construction permit, even though I voted against it, myself, which is a different issue.

After several years, construction of the Diablo Canyon nuclear plant was nearing completion. Then it was discovered that the seismic investigation done by the company's consultants to prove adequacy of seismic design was ill founded because they had not recognized the existence of an earthquake fault offshore not so far away. They then had to redesign the plant. And as is frequently said, they had to rebuild it. Not completely, but once you find an error, then you have to go back and check so many other things that had been built, for errors and so forth. It's very expensive.

Then a few years later, they found an error in that reconstruction, and that required the same thing over again. And

the plant ended up costing I think about three times as much as what they had expected to pay.

Wilmsen: Yes. Were those the kinds of problems people were encountering in the 1950s? What caused the cost overruns in the 1950s?

Pigford: There were cost overruns in the 1950s, on government plants where you could see the costs. And the first plant, the Shippingport [Pennsylvania] plant, which was built as a demonstration commercial plant by Admiral Rickover--although it was not commercial--had large cost overruns. His nuclear submarines had large cost overruns, but he performed so well, Congress was not unhappy at all.

I don't know of any government project that didn't have large cost overruns, but the ones that were dramatic, that affected nuclear energy itself--commercial nuclear energy--were commercial projects, and those weren't occurring until the late sixties or early seventies. The early commercial nuclear plants were sold on fixed-price turn-key contracts, so any overruns were absorbed by the suppliers, such as General Electric and Westinghouse. They did this to get the business, but they soon returned to cost-plus financing.

Wilmsen: Oh, okay.

Pigford: Now, I took leave from MIT in 1957 to help start up the General Atomic Company, in La Jolla. This was a new laboratory. And one of our goals was to develop an entirely new type of nuclear power reactor that could do the job better than the water-cooled reactors that had been spawned by the navy program. And that was a fine project and I was in charge of engineering, but we did not have enough engineers on the project. We knew it. We later brought in Bechtel to supply the engineering, and it turned out that our estimates of cost were so bad--enormous cost overruns on that. I had left well before that became apparent, but I may have been responsible for some of it.

Wilmsen: What do you think accounted for improper estimates?

Pigford: Well, General Atomic itself was staffed mainly by physicists, and I was to them more of a physicist than an engineer. They did not have an appreciation for the importance of engineering; I knew we were in trouble. I got Bechtel on board, but Bechtel became so enthusiastic about the new technical features of the project that I think it went overboard. And our customers, the utility companies, became so impressed by the new features that they did

not demand the care and scrutiny that you would normally get from engineering companies.

Wilmsen: What were the new features?

Pigford: It was to be a plant that would turn out steam at high temperature and pressure because that had been the whole trend in the utility industry in this country through the thirties and forties and fifties. You burn coal, generate steam, and the higher the temperature and the pressure, the more efficient it is in making electricity. And that had been their way of obtaining electricity and it worked for them in the 1930s. In the 1940s-- or late thirties--they did try going well beyond the bounds of normal materials into supercritical steam, which is extremely high pressure, and that turned out to be a financial disaster.

But from that background, our customers, the utility companies, loved anything that went to high temperature and pressure if we told them we could do it, whereas the water-cooled reactors had stepped backward. They could operate only 32 or 30 percent efficiency from heat to electricity whereas their modern coal plants could operate at almost 40 percent. It's an enormous difference.

Wilmsen: Yes.

Pigford: And if the plants cost the same, then you always buy the more efficient one. Light-water plants were more economical than coal, in spite of their lower efficiency. Utility companies simply didn't like that. So we came up with a design that was a new kind of reactor that turned out steam at high temperatures and pressures and they were so excited. That's a case of going overboard. Like some places went overboard after Eisenhower's initiative. You just have to be careful. And it's easy to oversell to yourself your project.

General Atomic, 1957-1959: Strong Physics, Weak Engineering

Wilmsen: What prompted you to take that leave of absence from MIT?

Pigford: Wonderful experience. I had gotten such exciting experience at MIT learning and defining a new field and participating in exciting developments and applications. I began to dream of

carrying out even larger projects involving a team of skilled scientists and engineers.

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Pigford: I'd learned how to work with physicists and engineers and so forth; I was given ample commercial funding to assemble a large staff and was put in charge of the engineering projects. A lot of money was available because it was supplied by General Dynamics which was the first big conglomerate in the United States, formed by merger of aircraft and shipbuilding industries. They built nuclear submarines for the navy. And they wanted to be identified with more science, simply because that was the thing to do after the war: add more science into your operations. And they wanted to also have the excitement of an entirely new project in this emerging field of nuclear power.

I was given all the resources I could use, and we moved fast, and we quickly became the successful bidder on one of the lucrative government projects to develop a nuclear-propelled ship with merchant-maritime application, which was a non-military application. That was one of the projects I got started.

And then we introduced this higher-temperature reactor for commercial electricity generation. It was a lot of fun. I was really a world traveler then in engineering, but I wasn't happy in my two years that I dedicated to it because I was spending more time promoting our work in high circles than I really wanted to do. I wanted to get back and do more technical work, so it was very easy for me to decide to go back to teaching.

Wilmsen: In promoting the work in high circles, do you mean in Congress?

Pigford: Yes, as well as in government agencies and industry. We had to argue before Congress to get that contract on the maritime reactor.

Wilmsen: This was for General Atomic?

Pigford: Yes. There were so many complaints from the well-established nuclear industry, by that time, working on light-water reactors, about our claims for the higher-temperature reactor, which were scientifically pretty good. Senator Clinton Anderson, from Arizona or New Mexico, was a very powerful senator on the Joint Committee on Atomic Energy, a powerful entity in those days. He hauled me before a hearing, asking, implying, and even claiming that we had stolen some technical secrets from Los Alamos to go

into the high-temperature reactor. And that was a fun hearing.

Wilmsen: Was it true?

Pigford: Maybe, to a very limited extent, when we adopted--not stole--some published work. But not significantly. But not long after that there was a different project that General Atomic sold to the military. I wasn't involved in it. In fact I was against it completely. This was right after the Sputnik went up, which scared so many scientists in this country. It showed that Russia was far ahead of what we thought. Our laboratory, General Atomic, had some physicists who'd worked at Los Alamos, and Los Alamos has a terrible reputation of coming up with greedy whimsical projects of other things you can do with atomic bombs. And one was to propel a large mass in space by shooting off atomic bombs behind it.

An exploding bomb gives quite a bit of thrust. One of our physicists from Los Alamos worked on that, and he introduced an idea that General Atomic should develop. That led to some proper hard feelings. I was against it because I learned that to equalize the thrust and mass, it doesn't work on a little object like a car or a missile, it has to be a big object like a battleship. The airforce loved it because, on paper, you could have a battleship-sized missile with people living aboard and in the lower hull you'd have a lot of Atlas-sized rockets, each one with a nuclear bomb at its tip, and they'd drop out periodically, fly to the back of the ship, and explode. And there's a big shock absorber there that absorbs the explosion and converts it into a thrust, and that's the way it flies through space.

I was sick when I heard the air force generals had appropriated hundreds of millions of dollars to work on this stupid thing. And it went for a long time--into the late sixties--without being cancelled.

That's an example of what a laboratory like that can do when it had some really terrific physicists, but they did not understand at all the perspective of engineering. I brought engineering perspective, but they thought I was a physicist. But it was a purely physics project, they wouldn't respect the kind of engineering that Bechtel does.

[Interview 2: September 29, 1999] ##

Wilmsen: Today is September 30, 1999, and this is the second interview with Thomas Pigford. When we ended last time, you had just

mentioned a project or a government contract you had gotten when you were at General Atomic to develop a nuclear power reactor for maritime propulsion. And first of all, I was wondering what was the reason for the government being interested in developing reactors for maritime use?

Pigford: Well, that was a part of President Eisenhower's Atoms for Peace initiative. I guess there are two reasons. After the Atoms for Peace project was initiated officially by President Eisenhower, the Atomic Energy Commission was looking for many different applications. And it had been so successful, even shortly thereafter, in the applications to naval vessels--submarines, especially--that they thought it would be logical to consider using nuclear power to propel merchant ships. But that didn't have the unique need as in the case of a submarine to stay under water for months, literally; the only way of doing that is by nuclear power. Like many applications of nuclear power, it's worth doing only if it's done to generate fairly large amounts of power. So the idea of nuclear power for smaller craft and for transportation was just foolish and out of the question in both the expense and the weight.

They settled on propelling a merchant vessel over a long distance, which smacks of the justification for the nuclear propelled airplane. At that time they'd begun getting a lot of oil from the mideast, and the Suez Canal had been blocked by the war between the British and the Egyptians. So for a while it was necessary to transport oil in tankers--very large tankers--around the Cape of South Africa and on up the Atlantic, to the East Coast of the United States. And that appeared on paper to be a long enough voyage to justify using nuclear power. In fact, the oil consumed, if it were propelled by an oil-fired engine would be fairly sizeable, but they were doing that. But oil was getting expensive, so that was the incentive for maritime application. But after a few years elapsed, it began to be evident that the need for that application no longer existed, partly because the Suez Canal was reopened, and secondly, because pipelines to deliver oil to the Mediterranean had been opened.

Well, it was amazing to me that it took the Atomic Energy Commission and Congress that long to see the handwriting on the wall and decide that they just didn't have a unique application for nuclear energy in maritime applications. Meanwhile a demonstration ship had been built by a different contractor to the maritime administration, and that was the USS Savannah, which was officially a cargo ship, but it was used mainly to give prestigious people rides on the first nuclear propelled merchant

ship, and it wasn't used for much of anything else.

The application that my company worked on was strictly to be a large enough power plant to propel something that could be economical, and that was for this merchant application. It led to many interesting technical problems. We came up with a new type of power conversion cycle called a closed-cycle gas turbine system, not based upon generating steam as is done in the naval nuclear power plants, and a new type of helium-cooled reactor which hadn't been built before. It was fun working on it, but as I say, the justification began to be questioned and rightly so, and the project was cancelled sometime in the sixties after I left General Atomic.

Many Different Nuclear Reactor Designs Developed in the 1950s

Wilmsen: Now in the fifties, going over your article on the historical aspects of nuclear power development,¹² it seemed like there was a kind of profusion of efforts to develop different kinds of reactors.

Pigford: Oh, yes. For commercial applications, there were many different kinds for different applications.

Wilmsen: And why was that? Why were there so many efforts at that time?

Pigford: Well, it was partly because it wasn't clear which type of reactor was best suited for economical application in nuclear power. The British had come out in 1957 with the world's first commercial-sized nuclear power plants. Those were enormous gas-cooled plants, cooled with carbon dioxide, and it was evident to the engineers that that's something they can build fairly readily from their experience in plants to produce plutonium, which were different than those used in the United States. But the early British gas-cooled plants were not anywhere close to an efficient and economical design for producing commercial power.

The British thought they were. They held an international

¹²Pigford, Thomas H. "Historical Aspects of Nuclear Energy Utilization in the Half-Century and its Prospect Toward the 21st Century," Journal of Nuclear Science and Technology 33(3): 195-201.

conference in 1957 on that, but their view of the economics was tainted by the fact that they had only just begun using modern economic analysis for costing government-owned power plants. They thought that they could build a capital intensive plant, which nuclear plants are, with a very low yearly charge on capital investment--assuming that it was easy to raise money. Well, that is so far away from what is the going rate of capital investment charges in the United States and other countries where electric power generation is handled by the industry and not by the government that I think that was the fundamental mistake on the part of the British. When we got into the details, looking at it, we concluded that their capital cost was too great, the interest doing construction would sway the economics, and their design had little chance of being economical.

It was the common thread in the United States that different types of nuclear power plants were required to be economical. So Admiral Rickover rose to the challenge. Even though he was supposed to be working strictly on naval applications, he saw the possibility of using that technology--which is pressurized-water reactors where the coolant is high-pressure water going through the reactor--for constructing a commercial nuclear power plant. And he built one--a prototype, not at an economical level--at Shippingport, in Pennsylvania. And it could be said that that led to what is now viewed as the real design for commercial nuclear power plants throughout the world.

The Russians didn't follow that design. They selected the Chernobyl type design which had its unique problems of safety. But it was thought by many scientists and engineers in this country that that could not be as economical as pressurized water technology or some other designs. Russia has since elected to build the American-type pressurized water reactors.

One of the problems was that you want high temperature of coolant to have high percentage conversion to electricity, as I think I must have discussed last time.

Wilmsen: Yes.

Pigford: To get high temperature, you need water at very high pressures because of the boiling point of water. And the cost of the pressure vessel, which is an enormous vessel--it would be in the order of sixteen to twenty feet in diameter, more than that tall, with walls about nine inches thick--some greater and some less. It's an extremely expensive thing to fabricate.

So an alternative idea was to use a reactor coolant--because all of your reactor has to go inside that pressure vessel--that could be operated at high temperature and not high pressure. Liquid sodium is one of the examples. Its boiling point is much much greater than water. There was a parallel project paid for by the government, carried out in Los Angeles by an operating contractor, to develop a sodium cooled reactor for commercial power. And that was one of the contenders.

Another idea that came from a scientific group was to use an organic coolant, a liquid, that does not have such a high vapor pressure as water. A simple analogue in everyday life is antifreeze. Here, the organic was something like two benzene rings attached together. And that was a separate project.

Then the Canadians, as well as several people in this country, saw that another solution is not to use a pressure vessel at all, but to make the reactor very large, consisting of many parallel pipes of zirconium alloy. In between the pipes is heavy water which can moderate the neutrons for criticality. The moderated neutrons then diffuse through the walls of the zirconium pipes and the solid uranium fuel rods on the inside, and those fuel rods have to be cooled with a high pressure coolant to get heat out at high temperature. So the zirconium pipes had to be thick enough to contain the high pressure and yet thin enough to let the neutrons through. And that became the principle design problem. It had no overall pressure vessel at all. The high pressure water coming out through the pipes, through the pressure tubes, would then go to a heat exchanger and generate steam. That survived in Canada. It's a very fine nuclear power design. It turns out to be more expensive in terms of capital costs than the pressurized water reactors, but that was hard to foresee.

In the United States, one of the government laboratories at Hanford and another one at Savannah River did some design work on the heavy-water pressure-tube reactor, as well as the group that I had at General Atomic.

Then another approach to getting a high-temperature coolant without high pressure was to use a gas coolant. Well, that's what the British used in their original design and as I said those were very large, very inefficient. That's because their gas was carbon dioxide and their neutron moderator was graphite. The temperature had to be kept low to avoid reaction of carbon dioxide with graphite to form carbon monoxide. And at high temperatures the carbon dioxide is also corrosive to the metal

tubes that carry the coolant over the fuel rods.

Well, we saw that could be avoided by using an inert gas like helium. Helium is a fairly rare gas, but it is found in gas deposits along with natural gas. And that was a solution, incidentally, back in the 1930s after the Hindenberg explosion. This big zeppelin was buoyed by filling it with hydrogen gas, which is very explosive.

It burst and exploded in New Jersey, and lost a lot of lives. Since then, lighter-than-air craft are filled with helium, and the record of safety is very good. When we see the Good Year blimp going over, that's a helium-filled balloon, or dirigible. So we said, "Let's use helium and generate heat at a very high temperature, and it could flow right through holes in graphite and not react with it." We would put carbon matrix fuel solids in the holes in the graphite and they would get very hot.

There was no metal in the reactor at all. And that became our number one nuclear power reactor design at General Atomic.

And that was very exciting to the electric power industry because we could turn out steam at the high temperature and pressure that they'd grown to love. The efficiency from heat to electrical energy conversion was around 40 percent, which they liked, versus around 30-32 percent for the pressurized-water reactors.

Wilmsen: That was more on the order of the efficiency of a coal-burning plant.

Pigford: That's right. We could have gone to higher temperatures, but the steam part of the plant would have required more expensive alloys, and it was the same thing that limited the steam temperature in the coal-fired plants. And that one became a very attractive power plant. So those were examples of some of the alternatives.

Another example: some people felt that there were not a lot of known resources of uranium fuel at a reasonable cost and that they would have to proceed directly to a breeder reactor which could utilize all of the natural uranium rather than, say, a fraction of one percent of it, which is the present technology for the nuclear fuel cycle. And that led the government to establish a new laboratory of its own in New York called Knolls Atomic Power Laboratory to develop a commercial fast breeder reactor.

At the same time, one utility company so believed in that concept--that was Detroit Edison in Detroit, Michigan--that it decided to start building such a commercial nuclear power plant right away. They wanted to show the industry that it could be done. They had the view that a nuclear reactor was just another heat source and it takes the same kind of skills that they use to build conventional power plants. Well, they were wrong. It takes those skills plus others. And it creates a lot of new problems which they weren't prepared for.

Now that's the story in examples. Well, another example was the aqueous homogenous reactor that was originated before I came to Oak Ridge, that we talked about last time. That was originated mainly because of the wonderful idea of a fluid nuclear fuel that could just go through a pipe, and a large expansion like an onion-shaped bulge in the pipe would make it critical. It would get very hot, and you then pump the liquid fuel out into a heat exchanger and generate steam. The aqueous homogenous project was still going in the 1960s.

And then some people decided--and that was also at Oak Ridge--to adapt the molten-salt, fluid-fuel reactor, which had been originated for the nuclear propelled airplane, to a commercial power plant. And they built a prototype of that and ran it, as they did the aqueous homogenous reactor.

So there's a smattering of examples. I didn't count them, but there were a lot of different concepts, all funded by the government, except here we have the case of Detroit Edison pushing a fast breeder--at its own expense--while the government was pushing it at the Knolls Atomic Power Laboratory.

Wilmsen: Was it, then, just the economics that led to the high-pressure water reactor being--

Pigford: Well, in the long run you can say economics, but the economics occurred as a result of various special technical problems they faced.

Let's see, I should add one more concept. When Glenn Seaborg was chairman of the Atomic Energy Commission, that was beginning I think in 1964--I may have that date wrong--either '63 or '64--at that time the cold war had heated up and they were asking for new and more production of plutonium.¹³ The

¹³Glenn Seaborg was chairman of the Atomic Energy Commission from 1961 to 1971.

commercial nuclear power wave was beginning to blossom, so the Atomic Energy Commission decided to marry both problems and build a commercial power-producing, plutonium-producing reactor at Hanford.

And that was to be a modification of the Hanford reactors that had been built during the war to make plutonium. In those reactors, water from the Columbia River was the coolant. It flowed over the aluminum-clad uranium metal fuel that got hot, but that heat was not converted into power, it was at such low temperature. The water from the Columbia was pumped through the tubes and then dumped back into the Columbia River. So the modification was to have a closed-cycle, water cooling system, which required pressurized water tubes, similar to the Canadian design. But the moderator would be big blocks of graphite, solid graphite, rather than heavy water. Then they would take that hot water coming out of the pressure tubes, put it through a heat exchanger, generate steam, and that steam would then be expanded in a turbine to make electricity. And they actually officially sold that steam as it came out of the plant to a private utility which converted it into the electricity. The two were married physically together, so you didn't have to pipe it very far.

It was a fairly low-temperature operation design, but it made lots of plutonium. It was somewhat similar to the Chernobyl reactors in the Soviet Union. But it did not, fortunately, suffer any accidents like the Chernobyl reactor.

So there's a brief smattering of worlds of different concepts. Oh, there was another one. The army decided they would--they had a unique application, and for a while they liked the idea of the nuclear-propelled tanks and things like this, but they were too clumsy and large. But they were interested in powering a station at the Antarctic, and that's a daunting job. Those stations are normally oil fuelled to generate heat and electricity--mainly heat. And getting the oil in for the winter down there is a very tough problem. A nuclear powered plant would require very little in terms of weight of fuel coming in every year to keep it running, and it could run literally for years without refueling if you wanted it to. The only trouble is they never needed anything as powerful as a normal nuclear power plant, and just the cost of the equipment, shielding and so forth, is so great, you cannot afford to do it for a low power level, and that ruined that project.

Wilmsen: Can you sum up what were the technical features of the high-pressure water reactor that made it a more popular design?

Pigford: Well, of course. Originally, it's because we knew a lot about that technology since they had already succeeded remarkably well in the navy. Why did they succeed? Well, they found some special material of construction--a metal to clad the fuel. It has to keep the water away from the fuel, and it had to be a very corrosive-resistant metal. They developed a new alloy out of zirconium, which was a metal hardly known to the industry before then--the materials industry. They had to separate some contaminants--natural contaminants in it--that absorbed neutrons, and that development was very successful. Some of our students worked on that at Oak Ridge doing the separation.

And being willing to relax the efficiency down to a neighborhood of 30 percent contributed to its popularity. That's a matter, really, in the long run, of economics. In the days of coal-fired plants and nothing else, the economic trend was for higher efficiency, and it was soundly based. But in the days of nuclear power plants, the things that drive the economics are the cost of the high-pressure water system and the reactor vessel, and they drive it the other way. They were willing to do that, and it still became economical. So recognizing and accepting that, which the utility industry didn't want to do, was a major thing.

Wilmsen: What finally brought them around?

Pigford: Seeing the technical problems that were arising in all the other concepts. Just leave out the high temperature and gas coolant reactor because that came up after the water reactors had emerged as the best technology. In the sodium-cooled graphite reactor the sodium is a liquid and it wasn't supposed to leak into the graphite because they had some high temperature alloy to keep the liquid sodium from the graphite. But some did leak; it's hard to look at something with that many miles of thin piping to not expect some leaks. And it reacted with the graphite and absorbed neutrons. The graphite structural strength changed a lot. Sodium fires, mainly external to the reactor, were challenging. Sodium going through the reactor got very radioactive, and if you put that directly into the heat exchanger to generate steam, there's a possibility of sodium leaking into the water. Even tiny leaks would generate hydrogen that could be a fire hazard, and sodium leaking directly into the air will burn.

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Pigford: They had to design new heat exchangers, of a kind they'd never

faced before. And you can make them. It's easy to do it on paper, but the cost of solving the problem of joints and leaks-- there were so many leaks, it was discouraging. We still have leaks in heat exchangers. We have leaks, and we're replacing tubes in heat exchangers for pressurized-water reactors. It's turned out to be one of the most difficult, time-consuming and money-consuming problems in the nuclear power industry. They know how to do it, it just takes down-time to fix them. So what people were sure looked like a mundane problem, turned out to be one that took a lot of effort and never completely worked as well as they thought it would.

In the organic cooler reactor, the organic coolant worked all right. It's not a very efficient coolant, but certainly gave higher temperature and more efficiency than water. But when exposed to the intense radiation inside the reactor vessel-- nuclear radiation will catalyze the polymerization of organic compounds quite readily in terms of pitch, of tar. And this turned out in experiments, to be just a machine to make asphalt.

You were continuously having to separate those high molecular weight, or high melting point tars from the organic. And I think that kind of stuff should have been examined more from that point of view before they ever went very far. It's not just a normal power plant. There are problems that you've never seen before.

Industry and Government Response to Developing a Helium-cooled Graphite Reactor at General Atomic

Wilmsen: Yes, well, where did the impetus come from for developing the helium-cooled graphite reactor that you were working on at General Atomic?

Pigford: I tried to introduce two reactors: the heavy-water reactor like the Canadians used because no one in this country was working on it, and I knew it was a very good concept, and secondly the helium-cooled reactor.

Well, public relations had a great deal to do with it. I was not very much involved in that part of it, but here is a new laboratory trying to make a statement, a scientific statement that shows it can jump over the industry. That was the primary thing that the sponsors wanted.

Wilmsen: Who were the sponsors?

Pigford: General Dynamics. They had made a lot of money building military airplanes and submarines and they were late getting into the nuclear business. They wanted to be in the nuclear business and also to get a special scientific identity. And by that time it had become very difficult for other companies to break into the field of building water reactors. Only four companies made it: Westinghouse, General Electric, Babcock Wilcox, and Combustion Engineering--all of which were well known for building equipment for the electrical utility industry.

Wilmsen: Why did it become hard for other companies to break in?

Pigford: Well, it took a lot of capital. I told you last time that initially these companies had to guarantee their plants' costs, and that took a lot of money to do because you had to guarantee the fuel cycle. Well, by the time they'd established themselves, new companies, to break in, would have had to offer similar things because they were new and untested. To take on a five billion dollar project without enough experience was hard to do.

American Standard, that makes bathroom fixtures and they also make some industrial equipment, tried to break into it but they just didn't have the background in designing and building that kind of equipment, or the connections, or the reputation.

Wilmsen: Now, I had asked you about where the impetus came from for the helium-cooled reactor.

Pigford: Oh, mainly public relations because we knew the utilities were still hungry for a really high temperature steam. They still wanted that.

Wilmsen: Because of that differential in the efficiency?

Pigford: Yes. And we had some novel technical ideas, like no metal in the reactor, and using ceramics entirely. And graphite is a strange material. The graphite would be the structural material: maybe eighteen feet in diameter with holes in it. Helium flows through the holes, and the nuclear fuel is incorporated in some graphite which makes little rods in the holes. Graphite itself gets stronger the higher the temperature. We pointed out that the helium didn't require high pressure, so we avoided the pressure vessel problem.

Those were technical gimmicks that we introduced, and I was

amazed at how quickly many of the utility companies fell in line and poured in money to support the project, which was to build a demonstration plant in Philadelphia, in Pennsylvania.

Wilmsen: Then last time you mentioned that you had to do a lot of promotion of what you were doing because the established companies were complaining. The established companies who were in light-water reactors were complaining about what you were doing. What were their complaints?

Pigford: Well, the complaints from the industrial companies, like General Electric and Westinghouse, were that we didn't know what we were doing. We did not have a strong staff of mechanical engineers and civil engineers. Mainly we had a bunch of physicists and materials people, and a few engineers. And I was in charge of the reactor projects as well as in charge of engineering. Because of that, I recognized the problem and got Bechtel involved, which supplied the other engineering.

But their claims were still valid that we did not have any reputation for engineering like they did. We didn't have any reputation at all. It was a brand new company and it made no products. We didn't have any of the people from the aircraft industry working for us. We did utilize a group from the Electric Boat Company that makes submarines, intending to use them for some of the engineering, and we tried that on the marine propulsion reactor. But I was in charge of that, too. I didn't think very much of their engineering, so we got rid of them. But these were the claims of the competition, and they were valid.

Wilmsen: Did that bear on government support for your project?

Pigford: No, the government supported us wholeheartedly. Let's see, why did they do that? Well, the British had come up by that time with a better design. Their first design of a CO₂-cooled natural uranium reactor was too expensive. They came up with a better design using a higher temperature metal to separate the coolant from the graphite and to clad the fuel. So they upped their temperatures and they went to ceramic fuel, a major development that was done by Rickover's laboratory. And so in the late fifties they came up with a more economical concept called the advanced graphite reactor.

And they had seen the possibility originally that they would be the world leader in nuclear power. Oh, they were initially, but it didn't last very long. They quickly faltered because of the design, but they saw a way of regaining that and it was a

much better design. It wasn't clearly better than the light-water reactors, although they claimed it was.

And I don't think our country understood their economics well enough in those days to understand the importance of how the money is to be raised, the financing costs. But our Congress had this powerful Joint Committee on Atomic Energy--joint between the House and Senate--very powerful. They thought we were missing the boat. All these concepts we were working on, and here the British have their own new one that looks like we ought to be doing that too.

They liked having many different parallel prongs, and so the Congress was about to initiate a project to be carried out by the Atomic Energy Commission on this higher temperature gas-cooled reactor to be built at Oak Ridge, Tennessee, to be a government-owned nuclear power plant.

Well, [pause] you see, of course there had been other projects for government-owned nuclear power plants: the Knolls Atomic Power Laboratory, that first breeder. Well, that had already faulted. They made a fundamental technical error, and then that laboratory was taken over by the naval program--no more breeder work. Then these other projects I've been describing, like with the aqueous homogenous, or the sodium-cooled graphite, were to be developed with government money by a contractor, but then implemented by industry as their own concept to be built on a commercial basis.

Here, Congress was going to leapfrog and build a commercial-scale version of the advanced British reactor. I believe that was to be part of the TVA [Tennessee Valley Authority] system which looks to many people like a commercial generating system--and it operates that way--but it's government owned.

And government can build plants that have much cheaper costs because they can finance at the government rate of interest, which in those days was around 8 percent per year as compared with maybe 16 percent per year for industry. Enormous leverage.

Utility companies were so much antagonistic to this first real government venture into commercial electric power that they were looking for any way to turn it off. They saw our concept, which was much more novel and looked much better on paper than the British concept, and so they supported us even more so because it gave them leverage to turn off the government project, which they did.

I must point out that the high-temperature, helium-cooled concept originated in Britain. And we were very mobile and we hired one of their very top people to come and help us get the project started. So we owed a lot to the British, although we did point out that we had modified the design in several ways that improved on it, which I think was true. The British didn't follow it because they were happy with their advanced gas-cooled reactor, and they just weren't ready for that large a step in technology.

Wilmsen: Was that project subsidized by the government?

Pigford: The high-temperature, gas-cooled reactor? Yes. So did the industry. I think industry subsidized it more than the government. We formed a consortium of different electric utility companies from all over the United States and their input was much larger than the government input. Now, in ensuing years, industry lost faith in it; the government continued. In fact, I think they're still subsidizing it a little bit for different applications, and it may be that their total subsidy grew larger than the industry's. I'm not sure.

Wilmsen: Going back to the question I asked a while ago about the complaints against General Atomic by the industry, their complaints, then, were intended to point out areas where the design could be improved?

Pigford: Oh, no.

Wilmsen: No?

Pigford: Their message was light-water reactor technology is a proved technology and this, what you're working on, is a pie in the sky, and your economics are very uncertain. The engineering is not careful, and so forth, and it won't beat the competition. They were trying to get the utility companies to not put much money into it.

Wilmsen: So there were different industry groups who backed your project?

Pigford: Well, there are two main industry segments. One is a supplier of the equipment, of the reactors, like General Electric, Westinghouse, Combustion Engineering, and Babcock and Wilcox, and they're the ones who feared our competition.

The utility companies were mainly the ones who were

motivated by the threat of government-owned commercial power. There are two different motivations coming into play here. So the utility companies were the ones that wanted us. It may have been for a shallow reason--just to turn off the government project--or maybe some of them really believed in it.

Wilmsen: Yes, I see. Interesting dynamics.

Pigford: Indeed, it is. And everything was going fast. Even though I was not the main promoter of this, I had to do a lot because at practically every place we went they wanted some technical person to show that it had some sense. The chap that we got from England and I were on a road-show circuit for many, many months on that. And us getting that project was a shock to the supply industry. They wanted it very badly because it was to be completely government financed. Whether they believed in the application or not didn't make so much difference; it was the job they wanted.

Wilmsen: Yes, how'd you manage to get it?

Pigford: I guess luck, and the new identity that our company was espousing. And I'd say we really believed in it. We had hired some very good scientists who were impressive--much more so than any of these supply industry companies had--and I think that did a lot to sway the Atomic Energy Commission, which decided on the contractor to go with us.

Alternatives to Nuclear Power, and Environmental Concerns in the 1960s and 1970s

Wilmsen: Another question I had was that I was just thinking about the opportunity costs involved in building nuclear reactors: at that time, in the fifties, was there any thought of developing alternative types of energy, like solar or geothermal or anything like that? Or did that come later?

Pigford: Well, it came along in parallel but in dribbles. Now geothermal, I should be fair, was not a dribble. The geothermal field up near Clear Lake--and I've forgotten the name of that location--turns out a lot of steam. So you sink a pipe into the ground and the steam comes out. And the Union Oil--is that the right name for the petroleum company? Union Oil?

Wilmsen: I think so, yes.

Pigford: Who has laboratories out near Martinez.

Wilmsen: Yes.

Pigford: For some reason, it went in and developed that steam and that steam could be put directly into a turbine, generating electricity. It's low quality steam, but it's free. And Union Oil, I think, bought a lease on the land, or else owned it, and they turned out a lot of electrical generating capacity there. PG&E was impressed by it and they went up and developed similar leases and also became a customer of steam from Union Oil.

They're still running those plants. They haven't expanded much through the years because they're just frankly limited by the amount of water that reaches the underground hot rock to generate steam. They tried pumping the water back in after it had been condensed, after going through the turbine, but it doesn't seem to have really worked well enough. They've tried all sorts of ways of recycling the water. It still is a viable commercial project, but it contributes, in terms of the total generating capacity of the country, a tiny fraction. But it works.

Now solar energy for generating electricity has not gotten to that stage. Many concepts have been worked on, and there's a government laboratory in Colorado that's been working on it for years. They've been much more successful in home heating. The trouble is it costs much more to build a house with solar heating. And you need standby heaters fuelled with oil or gas because of breakdown and things like that, but you see more every year. And it's a nice thing to have if you're willing to spend that much more money for your house.

Photovoltaics, where the light impinges directly on silicon --or something similar to silicon--and generates a voltage is becoming more and more efficient. The other device is simply to heat hot water in coils on your roof. Neither one of those is any where near commercial application for a [power] station for electrical energy production.

Oh, there are designs like focusing the sun's rays on a water boiler, and you can generate high pressure--water and steam--and you know how to do it, but it's just so expensive.

Wilmsen: Back in the fifties were there any efforts to look into those alternatives?

Pigford: Only, in my opinion--and this is oversimplifying it--mainly by the government laboratories.

Now you can look at some of the brochures turned out by PG&E, like annual reports, and they very proudly say they're working on it, but for a central power station they don't believe in it. I think this is public relations. And I know that they would agree with that.

Wilmsen: Also, thinking again about the 1950s, what were your major environmental concerns about nuclear power at that time?

Pigford: Mine or the country's?

Wilmsen: Yours, in the 1950s.

Pigford: Well, of course the safety. We believed that the safety issues could be solved, but they were very important. And we knew that if we stumbled along the way, like having an accident, whether it killed people or not, it would be a major blow to the applications throughout the whole country as well as throughout the whole world.

And there were lots of accidents. The worst one, in terms of killing people in this country, was back in the 1960s when the army's little nuclear power plant for Antarctica had an accident. They had a prototype at a national laboratory in Idaho which was running quite successfully in terms of producing steam and electricity. I can't say successfully in economics; it was too little.

The army sent their people there for training, and this was during a shutdown for refueling when they have the reactor vessel open. There were either two or three operators involved in taking used fuel out and putting new fuel in, and one of them simply made a terrible mistake of effectively pulling out a control absorber when he shouldn't have and the reactor suddenly went critical and with a burst of energy it produced a small steam explosion. It doesn't cause a meltdown. In a situation like that it shuts itself down by boiling the water away. But he was killed by the blast of x-rays--I'm using the term loosely--the nuclear radiation, x-ray-type radiation, that came out. He got a very high dose.

He also was struck by another control absorber that was ejected from the reactor in the steam explosion. Evidently he was leaning over the open tank of water, looking down through the water, which normally would be safe. And I think he got hurt by that. The blast ejected him. It was some force from that steam explosion, and he was found, I think, many yards away.

Wilmsen: Wow.

Pigford: And of course that shocked the industry. And it should have. It was a strong indication that regardless of how safe a thing is, human error can overcome those safety features. Of course, that's what happened at Three Mile Island and that's what happened at Chernobyl.

So safety was always a predominant thing. We spent a lot of time in engineering, in designing and testing safety systems.

Wilmsen: What did you think of the Atomic Energy Commission's radiation standards at that time in the 1950s? It seems to me it's kind of a thing that evolved over time.

Pigford: Yes, they've gotten progressively tighter, and they should have.

Now I really haven't answered your earlier question fully about what did I think about environmental concerns. Should I go back to that?

Wilmsen: Sure, yes.

Pigford: Well, I became more concerned, or equally concerned anyway, about the emissions of radioactive material to the environment that would occur when you operate a nuclear power plant.

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Pigford: I was one of the first in the field to plan out calculations on how important this is. I think I shocked a lot of my colleagues in the industry by my publications on these that started coming out in the early seventies, I think, maybe late sixties. And I've continued that, still am doing that.

And I felt that the reason the nuclear power plants that were operating--some of the very first ones, like for example a water-cooled, boiling-water reactor near Chicago built for the Commonwealth Edison system by General Electric--have emissions to the atmosphere and the water is because it's very expensive to

build something that has zero leakage. It's not impossible. Well, some physicists might challenge it in the long run, but essentially it can be done. And the point of view of the reactor designers, which is a real logical one, is that the government sets the standard on what is the allowable concentration of a radioactive material in the air or in the water, and if you meet that, it's safe.

Radioactivity leaking into the air was a big source of emissions to the environment from boiling-water reactors. It's because, for example, the water does corrode the stainless steel piping and Zircaloy. It's at a very low rate, but it brings some of that corroded metal into the water and it goes through the reactor where it becomes radioactive. You have to continue to remove that with ion exchanges which go to low level waste disposal of solids.

It also generates radioactivity by the interaction of neutrons with hydrogen in the water that makes tritium, which is a triply heavy hydrogen. First there's hydrogen, deuterium, and tritium. It's half-life is thirteen years, so it's pretty radioactive. And it tends to evolve as a gas. You're already decomposing water into hydrogen and oxygen, similar to the process I've described for the aqueous homogenous, but at a much lower rate because fission fragments are not interacting with the water; it's only neutrons and gamma rays. And the reactor produces steam which goes directly to a turbine. Well, these radioactive gases, which are in low concentrations, follow the steam, and they can bleed out of the turbine and require extra maintenance.

But in terms of environmental releases, it happens this way: the steam is condensed at low pressure, much below atmospheric. That means officially you get more work out of it to create electrical energy. That means the condenser operates under vacuum (this is true of all steam power plants) and at a pressure equal to the vapor pressure of water at essentially the cooling water's temperature--like the lake or the ocean or whatever. So air leaks through the condenser, which is a series of metal tubes that has condensing steam on one side and cooling water on the other to cool the condenser. Air leaks through the joints of that heat exchanger where the tubes meet the end--they're spun in with a pressure joint. Leaks run into where the steam is condensing, so you get a lot of air.

Normally in any power plant, you have to have a vacuum pump that continuously removes that air from the condenser, and it's

appreciable. Well, these radioactive gases that came into the condenser with the steam then get mixed with the air and get discharged to the atmosphere. And General Electric found that it had to build a stack about 300 feet high to exhaust the gas--the condenser air and radioactive gases--at a high enough elevation so that it would mix better with air before getting to where human beings are. It was a great embarrassment to them because it looked like that was a stack for a coal-fired power plant and they didn't like that at all. But it was the cheapest way of handling the gas and it met the design requirements, so you can't fault them because that met the government criteria. They exhausted millions of curies a year to the atmosphere of tritium and radioactive noble gases like xenon and krypton.

Well, you said the government standards were evolving and, yes, they were. In the late sixties and early seventies, or even the mid-sixties, the Environmental Protection Agency and the AEC itself began to adopt an additional safety requirement on emissions that not only must you meet those numerical concentration limits, but if you can find you can reduce those by adding some additional recovery equipment and it's cost effective, then do so. They had a whole set of calculations: you add this and it increases the cost so much, and finally there's a knee in the curve where further additions just get to be out of sight in expense. That knee in the curve was called "as low as realistically achievable".

Actually, implementation of that had been done first in Germany on American-type reactors. And they developed themselves a very good process for simply holding up that gas long enough so that it decayed before going out to the atmosphere. It didn't hold up all of it, but it reduced the emissions by factors probably in the neighborhood of several hundred or even maybe a thousand. I was delighted to see that. That was using a novel chemical engineering technique, which is fun to work on.

Wilmsen: They were holding tritium for eleven years or longer?

Pigford: Tritium is the one exception.

Wilmsen: Oh, okay. [laughs]

Pigford: Tritium is not as dangerous radioactively as radioactive xenon. Xenon is a fission product which leaks through some of the holes in the fuel cladding and gets into the coolant, and that's the one that really required the--no, I'm stating it wrong. They did both.

Tritium, what did they do? They passed the gas over a catalytic recombiner like what's used for exhaust of modern cars. And there was enough air there to burn all the tritium. You needed a catalyst because it's such a low concentration. The catalyst was platinum and it recombined with tritium into water and the water could be condensed out. Then it's easy. Then the volume of that is small, and that's treated as low level waste. It doesn't necessarily go directly to the atmosphere. So you removed the tritium from the exhaust gas.

The xenon is more difficult. The Germans developed a charcoal adsorber in which you pass the gas across activated charcoal. It's about the size that could fill almost this living room, maybe six feet tall. And the high-molecular-weight gas has a surface absorption reaction with the charcoal. It sticks with it for a certain time. And they designed that so it would stick with the charcoal for maybe about thirty days, which was long enough to cause the decay of all of the xenon.

Both of those processes, which are now commonly used throughout the world, are things that were done to control environmental releases. And this is just an example. There are so many different kinds of environmental releases--I could talk for hours on this--releases of things like radioactive cesium to the cooling water. And if this is cooled by water from a river, like the Columbia River, or water from the ocean, like Diablo Canyon, the calculated concentration in the cooling water was below the allowed water concentration limits. But many people pointed out that some of those limits did not properly account for the possible uptake of the radioactivity from mollusks--mainly sea urchins, clams, oysters--and that there was a potential danger much greater than that.

Wilmsen: From concentration in the food chain.

Pigford: That's right. I was delighted to see these new concerns get brought up and dealt with. And I can say the industry has a good record. I can fault them by not initiating all of that themselves and simply saying we rely on the government concentration limits, and yet I can understand their point of view. We're not in the business ourself of deciding what's safe. That's the government's operation. They are part right, and part wrong. But at least we've learned that they must be more alert to these needs.

It's hard to get across. A designer wants to have a design

goal, he gets it, he says that's the official goal, he designs for it, puts in a "factors of safety" for uncertainty, and expects that to be accepted. But that's not real life, and it costs the industry a lot of money in trying to adhere too quickly to that design approach. [tape interruption for telephone call]

It's been one of my principal activities throughout the years. I was mainly in the design of reactors for many years, which got me into a lot of physics, mechanical engineering, and environmental control. And I'm still active in that.

III THE UNIVERSITY OF CALIFORNIA: STARTING AND CHAIRING A NEW NUCLEAR
ENGINEERING DEPARTMENT, 1959-1964

An Invitation to Start the Nuclear Engineering Department at the
University of California at Berkeley, 1959

Wilmsen: Yes, I want to come back to that later, I think. I wanted to ask you about coming to the University of California in 1959. How did it come about that you were asked to come to Cal and start the nuclear engineering department here?

Pigford: Oh, that idea was initiated while I was at General Atomic. We had built this wonderful laboratory in La Jolla and it became kind of a watering place, a visiting place, especially for physicists and chemists. It was a constant through-point for some of the most famous people in the world. We had Glenn Seaborg there during the summer--for the whole summer--on workshops that we originated. Edward Teller, Hans Bethe. Seaborg was a former chancellor and a Nobel Prize winner in chemistry and so forth.

Well, Seaborg and I had community of interest because he's a chemist and I was a chemical engineer. And he had developed in the laboratory at Berkeley, Gilman Hall, the process that was used to separate plutonium from discharged nuclear fuel at Hanford. As I mentioned last time I had gotten into that area many, many years later at Oak Ridge, working on a much improved process, the purex. And the first book that Benedict and I wrote for my teaching was on Nuclear Chemical Engineering, and some of the chapters dealt with that. The first edition of that book had already been published about the time I went to General Atomic. So Seaborg and I became good friends.

I also became good friends with Teller. Teller was on a committee to search for a person to come in and head nuclear engineering. And whether Seaborg suggested me to Teller or

Teller thought it was a good idea and consulted Seaborg, I never learned.

There were a few other people on that committee who had been formed for that purpose, among them Kenneth Pitzer who was then dean of chemistry. He might have been involved in some way with the General Atomic work, but it was mainly those two contacts that I can remember.

Well, let's see, I've gotten things backward. Even before that laboratory had gotten started, I had been invited to come out to Berkley to give seminars and lectures on what we were doing at MIT. It may be the first time I came out was as early as 1954 or '55. There was kind of an amorphous group of people who were interested in nuclear engineering but they hadn't formed a department then. And so that's how my contacts first got started, but I never thought seriously about coming to Berkeley until I talked to Seaborg and Teller.

Wilmsen: What did they do to convince you, or that made you think more seriously about coming?

Pigford: Well, it was mainly Seaborg. I'd already learned enough in my MIT days that there's a lot of politics in a university, although I was still to learn that there was more than I ever realized. And having the chancellor personally on your side is bound to make a lot of difference.

Wilmsen: Yes.

Pigford: I had in my mind some new ideas, new ways of starting a department, new groups of people, and I was convinced that Seaborg was going to support me for whatever my ideas were. I hadn't really thought about leaving MIT, and I was very happy there, but it was a novel opportunity.

Wilmsen: Yes. I want to come back to that, but I'm curious why UC had decided at that time to start a nuclear engineering department. That seems kind of late in the game.

Pigford: Yes. I said I came out I think in 1955, and I found a lot of interest then, not specifically with the department, but in the subject and what could be done with it. Well, why not earlier? In 1952, when we started at MIT, we were certainly not the first. It was buzzing throughout the country.

One speculation is that engineering at Berkeley was in a

turmoil in those days. This was not to disparage engineering at Berkeley, but it had been developed, and quite well, under the concept that engineering is basically one field. They still basically have that concept at UCLA. The two leaders in that concept were Michael O'Brien and [Llewellyn] Boelter. Boelter became dean at UCLA and O'Brien dean at Berkeley.

That concept sprang up early at Berkeley, and continued through the 1940s. But the emergence of highly specialized fields and the introduction of a large amount of science in engineering that was a natural offshoot of World War II, there was more introspection: What are we, therefore; and what can we do to bring in these new people who are maybe not engineers? I was considered an engineer, so that wasn't my problem, but the whole atmosphere was buzzing: We need to change our ways. They weren't about to drop engineering. They weren't about to start departments, I should say, because Berkeley and UCLA were noted throughout the country for being this broad field of engineering.

Also, people felt it wasn't working. Then by the time they got around to talking to me, there was some strong dissention between Michael O'Brien and maybe with the chancellor. I never knew enough about it.

I think at least in nuclear engineering, Glenn Seaborg felt he needed to step in and at least do something about nuclear engineering. In fact, he stepped in farther, and they set up departments all throughout the campus in engineering. Now they're department heads rather than heads of various groups--officially departments. There's a lot of autonomy in each department.

So I think that whole melting pot--a very healthy era of soul searching as to how should we go about the future--led to trying to solve those problems rather than getting on into nuclear engineering, which would have been an enormous departure from their previous stand. That's my speculation.

**Establishing the New Department: Opportunities and Challenges, and the
Need for a Research Reactor**

Wilmsen: Then you were department chair from 1959 to 1964. What were your

goals?

Pigford: I was chairman also two other times.

Wilmsen: Right. I'm trying to keep this somewhat chronological.

Pigford: Sure.

Wilmsen: You mentioned you had some new ideas about starting a department? What was it you wanted to do differently in starting the department here at Cal from what you had done at MIT?

Pigford: Well, I had been exposed to the excitement of really good science and materials, and at MIT we didn't have anybody in materials at that time. That was a mistake. And General Atomic, even though their projects were not successful overall, their materials work was excellent. So I wanted to have that as a strong part of the department. And it became a strong part.

At MIT our first approach was for Benedict and I as chemical engineers to learn the physics and teach the physics. I think that was a healthy approach, but what I had seen in my consulting work at General Atomic and other activities was that if you can find them, if you can find a good physicist who is interested in working on that kind of applied problems, and those are rare, they are so much more facile in mathematics, for example, than we were. Nowadays many good engineers can rival the physicists in mathematics. In those days I didn't see that, and so I wanted to strengthen that part of it. I guess those were the two main things.

Then I also wanted a strong central core, which in my own view had been molded by my MIT experience, that you needed a certain kind of system engineering at the center of it to unite it all together. I was supposed to be one of those, and I needed some more people like that. And we made some mistakes, as you always do, but we got some very good people, and the department established itself as being strong along those lines I've described.

Wilmsen: Were there any particular opportunistic moments or opportunities that came along that perhaps you hadn't expected or things that really helped you in getting the department going?

Pigford: Well, sure. When I talked to Seaborg and Teller and Teller's committee about coming there, I pointed out that you had to have a lot of laboratory. Doing these things just on paper is too

sterile. And they said they have just the solution as there was the laboratory out at Livermore. Teller was the head of Livermore at that time and he could arrange declassification and so forth and provide for all the wonderful things we could do. Well, they thought that was a good opportunity.

I told them I didn't think so because it's an hour's drive, and my experience is that in a true university environment if your students don't have a laboratory within a few minutes walk, it doesn't work. It gets separated. Even at the University of Michigan the new laboratory they built for nuclear engineering was far enough from the campus, maybe a half hour drive, that that was a major problem. You couldn't have the intermingling with other students, which is the number-one thing you look for: intermingling with other students.

Wilmsen: Do you mean other engineering students, or just other students?

Pigford: Other students. That was the great opportunity at Berkeley because there's more intermingling there than there is at MIT.

Wilmsen: Why is it that there's more at Cal than MIT?

Pigford: Well, partly, MIT is so dedicated toward good engineering they help you get started towards that, and if you respond, you become more focused, but that's also a trap. Berkeley doesn't do that, which means that its engineering is not as good as MIT's is. And yet I think that the development of the individual, his education, is much better. But that's a value judgment that's arguable. So I can't say which one is better, although I guess I do conclude that you've got to mingle. And at Michigan, just losing mingling with other engineering students is a major problem.

So back to the question: my reaction was puzzlement. But I told Seaborg that I needed to build a laboratory and a central part of it would be a research reactor, not the kind that some universities were putting up like Stanford where you can only demonstrate things. Demonstrations are maybe okay for lecture hall lectures, but not for research.

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Pigford: So in other words, I wanted a reactor that can be used for real research: measuring new things, to validate what we're doing in theory, and so forth. And he says that's a tall order, that

campus space is treasured and limited, and we have just the place out at the Richmond Field Station where they had built a naval ship towing tank for naval architecture. And in fact, during the early part of the cold war, Ernest Lawrence and Louie Alvarez from the Lawrence Berkeley Laboratory had proposed to build a large reactor facility at the Richmond Field Station to produce additional plutonium. Now I think it was a naive idea. They finally tried to build at Livermore.

Richmond is a very congested community, but at least it wasn't out of the question within the university system to do something like that. But I asked them how long it takes to drive to Richmond and they said it's a half hour. I said it won't do. That led to lots of arguments that we had.

Seaborg wanted to support me and he said, "We have just the thing. We have a brand new building project going on and you know it's hard to get on the track for building a new building with the state, but we could give you a large part of that space." And I said, "Can you give me space with a commitment that if I raise the funds, we can put a research reactor in the basement?" He swallowed and said yes.

Before we came to that point we were shopping around, looked at Strawberry Canyon, and that led to a lot of understandable concern on the part of Ed McMillan, who was then director of the Lawrence Berkeley Laboratory, because he had his eye on those spaces up in Strawberry Canyon as places to expand LBL. And they've since expanded.

I wasn't much interested because LBL itself is too far from the campus from my point of view. I had grown up really in my educational work at MIT. Probably you've never been there, but it's a small campus, and in fact most of the departments are all housed in just one enormous building. And it really works. In spite of their focusing, you still profit from that closeness.

So Seaborg then offered me the space in Etcheverry Hall. But that had to go by the Berkeley Campus Development Committee--called the BCD Committee--which really looks over, approves, and authorizes uses of new space--a very powerful and important committee.

I knew I was going to be battling many people. Some members of that committee were from biology and afraid of getting that close to radioactivity, understandably. And every member of that committee had to accept on some faith that we could handle it and

wouldn't let any radioactivity get over the campus beyond the allowable limits, or that we knew enough about them to say they will be far below the allowable limits. Or if they didn't accept that, they could then say it's too big a chance to take.

Where we were going to put the reactor in the basement would have a playing field right above it and a volleyball court, and they were worried about the students getting exposed to radiation. Those are real justified concerns and it took a lot of explaining, discussion, and in the long run, support from the chancellor.

Wilmsen: Ultimately were the emissions lower than the allowable limits?

Pigford: Yes. Yes, we did pretty well on that.

Wilmsen: Who were some of the people who raised these concerns?

Pigford: Hardin Jones who was in the Donner Laboratory--a very fine guy, very knowledgeable about ecological effects of radiation. He'd worked a lot with radioactivity in the Donner Laboratory, but we were going to produce more radioactivity than anybody had ever seen on the campus which is the nature of a reactor.

Sanford Elberg was the chairman and he, I know, was from one of the humanities departments. At least he's in the College of Letters and Science; I may be wrong about humanities. He was an extremely fair person, and he insisted on giving me a fair hearing and he didn't ride over anybody's objections or questions, but we ironed it out.

Wilmsen: Was there any particular argument that ultimately swayed them or was it just a lot of explaining?

Pigford: I don't know what persuaded them. I don't think anybody would simply take the support of the chancellor as enough because, by nature, the faculty would polarize itself against the chancellor on a thing like this.

Wilmsen: Yes.

Pigford: Seaborg was very, very good.

Funding, Designing, and Using the Research Reactor

Wilmsen: Were there any other challenges that you faced in establishing the program?

Pigford: Oh, sure. I had several new faculty positions to fill and we wanted to fill them quickly. We wanted to get off to a good running start.

We had to raise money to build a reactor. And already, as I said before, for many years colleges throughout the country were lining up to get on the list for supporting funds from the Atomic Energy Commission. The funds were doled out by the National Science Foundation and they had a solid calendar of when each proposed project would get funded. And that was a pretty long calendar. So we developed a proposal involving just almost any conceivable group on the campus that could have some possible interest, using radio isotopes--which are plenty of people, including the Lawrence Berkeley Laboratory--and we developed that very quickly and were successful in getting that funded. We got to the top of the list.

Wilmsen: How did you manage that?

Pigford: Well, I guess by contacting and welcoming many fine scientists and engineers to participate. And it was a lot of money for those days. It was about a million dollars, and it was all for the reactor.

But the building had to be greatly modified. The whole bottom floor wraps around a very large laboratory--still there--which is a self-contained room with its own ventilation and ventilation control for safety. Very special, because if we were to have a spill of radioactivity in that room, we would immediately confine the air; it had double door access. If we had a spill, all the air in it would be exhausted through charcoal absorbers to purify it. So a lot of effort went into being able to almost completely isolate that laboratory from the closeness of the surrounding campus. And it was expensive. I think the state probably put in an extra two million dollars.

The laboratory itself was more expensive than the reactor. And of course there was a lot of backbiting in those days from my colleagues in other departments. They didn't know me, and I was pretty young then. I was only thirty-seven years old. But we survived, and actually, in spite of the normal turf battles,

which are the most frequent kind of battles at the university, I think we got along pretty well.

Wilmsen: Did you have to have special staff on the experimental reactor?

Pigford: Oh, yes. We had to have a full-time reactor supervisor, and two reactor operators. Later, we had more than that because the reactor was successful. It became popular for research, and in fact, it also became very popular for research outside the university. Lockheed, for example, on the peninsula was building some electronic devices for satellites in the military program.

There's a lot of radiation in space that can wipe out a semiconductor, and they found one of the best ways of qualifying those devices as being sufficiently reliable was to come put them near a reactor and expose them to nuclear radiation. They used the reactor a lot, so we had to go into some around the clock operation.

And then we had to have a dedicated health physicist, which was unusual for the campus. There's a campus environmental health and safety group which oversees health and safety in laboratories and other things in campus operations, and we had to go beyond the normal tradition and have one such individual devoting full time to that facility. That required funds, operating funds.

Wilmsen: Then how did you use the reactor as a teaching tool?

Pigford: Well, reactor demonstrations were always part of it. It's very exciting to run a reactor, and this reactor was unusually safe. You could blow out a control rod and it would simply heat up a little bit and shut itself down. It incidentally was developed by General Atomic and became the world-wide research reactor. (That was not one of my projects there.)

So with a little bit of instruction, and after the students had learned principles of reactor design, we could put them on the control console. They could start it up and go through power changes and even eject a control rod and watch the burst of energy. And I think it was a good teaching tool, but as I said, in my view, a minor purpose of the reactor was as a demonstration.

Then we had people from all over the campus using it. We had a hollow tube going through the eight foot thick concrete shield and terminating right at the surface of the reactor, and

so nuclear particles like neutrons will escape from the reactor and stream through this tube. It's a very powerful technique of neutron diffraction. Actually, diffraction, which has been used for years using x-rays to bounce off atoms in solid lattices in solid materials, is the way of identifying solid structures. But they are limited in x-ray diffraction because they're not able to look at light elements like hydrogen very easily, or oxygen, because it takes a lot of electrons in an atom to bend--diffract --an x-ray.

With neutrons, those respond especially to light elements. They brought in a whole new field for materials research. And that's the kind of research that you see in solid-state physics and materials science. We had a project funded by NASA which was aimed at developing ion-propelled engines for space, where instead of ejecting hot rocket fuel that's been burned, which is the way they're propelled in space, now you have a device that can ionize cesium--make it a gas, ionize it--and then with an electric field, cause those ions to move rapidly out the rear end. And that's far more efficient for long distance space exploration, but it had several materials problems. To get to the end of that story, one of them involved learning how long one of the electrodes could last before it became destroyed by the equivalent of corrosion--we call it ablation--and so we took one of the electrodes and exposed it to neutrons, made it radioactive and then exposed it to the ion generator. Measuring the amount of radioactivity coming off in even a short time, gave an extremely sensitive measure of corrosion rate, ablation rate--much more so than using traditional chemical techniques.

Also, the people in the Donner Laboratory were interested in biological effects on small animals like mice.

Wilmsen: Donner Laboratory?

Pigford: That's part of the Lawrence Berkeley laboratory [LBL], but it's on campus.

So in designing the reactor, we even designed a fairly sizeable room inside the shield. You could put a cage of animals there, then crank the reactor down to the end of the water tank it's in, and it could expose those animals to radiation. Then they would take them back to the laboratory and study the biological effects. So it serviced a lot of people.

Wilmsen: Now eventually it was shut down, through, wasn't it?

Pigford: Yes.

Wilmsen: Why was that?

Pigford: This was during my third five-year term as department chairman. The reactor was no longer adequately serving the campus programs of teaching and research. It was still very popular with LBL, and of course many people would say there are students involved there, but in the 1980s LBL was mainly professional staff with not many students, so I couldn't make the academic argument.

The work from Lockheed was still growing. The reactor was making lots of money for us, because we would charge Lockheed, for example, and charge LBL, but we could see that its use in the academic program for research--not demonstrations, but for research--was greatly diminished. And I pointed this out to [Mike] Heyman, who was the chancellor then. He was either vice chancellor or later chancellor, but I think it was when Heyman was chancellor that I pointed out to him that he should know this: we were happy with the income, but it was no longer a major part of our academic program. I knew that the campus was always looking for space and I said I had concluded, and most of my colleagues agreed, that the way our academic program had materialized, its use for research within the department and campus was not very large anymore and that I thought that we should either make it a service facility to continue what it was actually doing, but administratively decouple it so it could run a little more efficiently, or get rid of it. But that would be very expensive. And we got an estimate and I gave Heyman the quote--I can't remember if it was two or three million dollars--expecting him to say we shouldn't touch it. Instead he said it's a bargain.

He was very fair. I don't think he was motivated by the frequent anti-reactor demonstrations, because we'd learned to live with those and it didn't hurt us academically, but the cost of high tech laboratory space on the campus is enormous and if he could free up that much space, he could show, I think, that it was worth doing, and so we did it.

Wilmsen: And when was that? What year? Approximately.

Pigford: When we look at the last year I was chairman was when we made the decision. [looking through papers] It's not in this. Whatever the last year was that I was chairman. Probably about 1987 or 1988.

Wilmsen: Well, I can look that up on your CV.14

Pigford: Yes. We made the decision and then after I retired from the chairmanship, the chancellor asked me to take over the project of seeing that the reactor got decommissioned and dismantled. And this was a big project--a lot of safety issues and so forth. I organized the decommissioning project and got it started. It took several years.

Working with Chancellor Seaborg, and Relations with Other Academic Departments

Wilmsen: What was it like working with Glenn Seaborg?

Pigford: Oh, a delight. He's just a wonderful person. He'd go out of his way to do anything for you.

Wilmsen: And you mentioned that you had pretty close ties with other departments like chemistry and physics?

Pigford: Oh, yes, yes. Also, in material science and engineering--mechanical engineering, electrical engineering--and having those close ties was part of the reason I came to Berkeley.

Wilmsen: Now, one thing I'm kind of curious about is sometimes in social science departments, people who do applied research are sometimes kind of treated like second class citizens. I was wondering if that applied as well in basic science fields.

Pigford: Oh, sure. Well, first nuclear engineering is not a basic science in the term we use academically.

Wilmsen: That's what I mean, yes.

Pigford: You would say physics. Physics would be a basic science. But yes, the physicists by very nature look down on engineers. And we look down upon the physicists, although somehow I don't think we put ourselves on as steep a pinnacle as they do. When I look at and try to understand some of the work that some of the Nobel

¹⁴Professor Pigford's last year as chairman of the department was 1988.

Prize winners in physics do, especially in theoretical physics--I can understand experimental physics better than theoretical physics--it's just baffling to me. And I respect it. Within our department there's also that kind of hierarchy. Nuclear engineering was off to a blazing start and was looked upon with great envy as the new boy on the block, getting enormous expectations, but a lot of intercampus rivalry.

Nowadays, with the loss of the economic competitiveness of nuclear power, which is the main thing, in my view, that's not well understood by the public--I'm no longer excited about that kind of nuclear engineering; the excitement has worn off. It's time for change, and they are changing things. There's really essentially a whole new bunch of faculty. They've got some wonderful ideas, and I wish them the best. I'd be in there working, too, except you find you need some young blood.

Wilmsen: How did those kind of hierarchies effect the department when you were chair?

Pigford: Like with other departments?

Wilmsen: Yes.

Pigford: We got more than people thought we deserved because, as I say, we were on a pinnacle from the beginning, and we used that very strongly. Is that what you mean?

Wilmsen: Yes.

Pigford: And somehow--and I've often questioned the wisdom of this--I also got on so many committees, like committees to revamp environmental health and safety for the campus. I guess they assumed this young guy coming in has some sort of secret and we'll let him solve other problems. And they were fine, but they took a lot of time.

I served on the committee on privilege and tenure, which is a terribly sensitive committee. That's where you hear cases of complaint among the faculty or against the faculty. And I was kind of overwhelmed by too much interaction, really.

Wilmsen: Well, we're just about at the end of this tape. Shall we stop there for today?

Pigford: Is this a good stopping point?

Wilmsen: I think so.

Pigford: All right.

Wilmsen: I have a bit more to cover next time. Actually I'm pretty much done with the founding of the department. I wanted to ask you about your research that you initiated on the diffusion of fission gases, and about the Bodega Bay controversy. And we can do that next time.

Pigford: Okay, the research topic was essentially the first one I was involved in that I can remember here. And Bodega Bay I really was not involved in. You'd think I would have been, but I hadn't gotten in the prominent safety analysis and public concerns that I did later on. The Bodega Bay controversy was already well under way before I came. I can give you my views on it, but they do not reflect any direct involvement.

**Major Research Programs in the Department at Cal, and the Founding of
the American Nuclear Society**

[Interview 3: October 14, 1999] ##

Wilmsen: Today is October 14, 1999. This is the third interview with Thomas Pigford. [tape interruption] I wanted to start today actually with a question about how it came about that you co-founded the American Nuclear Society.

Pigford: Well, it was in the early fifties, my guess is sometime maybe in 1954 or '55 when we were in the business of teaching nuclear engineering at MIT and the field was growing. So many of the people in the field decided it was a promising enough field and growing enough that it should have its own professional society.

I wasn't one of the organizers, but I was invited to be present and to participate in developing the charter. That was done at some national meeting, probably in Chicago, and from there a charter was developed. People could join and they paid dues and they started electing officers--the things that a normal professional society does.

Wilmsen: Did you start publications in association with the society?

Pigford: Did I start publications?

Wilmsen: Well, I assume the society started publications.

Pigford: Yes, I see what you mean. The society didn't initiate it right away. Some professional journals had a monthly news edition about activities in the profession. Then it had a technical journal called Nuclear Science and Engineering that came out monthly. I started publishing in that, although I and my colleague had already been publishing in journals of the American Institute of Chemical Engineers, so we weren't without a place to publish.

But the society grew rapidly and became a very viable society. It's not as large as, say, the equivalent professional societies in mechanical engineering or chemical engineering because it's still a more specialized field.

Wilmsen: Last time we talked about how you started the nuclear engineering department at Cal. I wanted to talk a little bit more about those early years at Cal. I guess I'll start with what were the major research programs in the department?

Pigford: There was a research program on what we called the physics principles of designing nuclear reactors, namely, how does the chain reaction get started and how you can calculate when you have enough material to reach criticality. That's a very sophisticated field of applied mathematics, and that was very attractive to the faculty and students because it's a strange thing about nuclear reactions: you can predict their rate of reaction much more precisely than you can with chemical reactions.

Already that had become evident during the Manhattan Project during the war, where some of the world's greatest physicists were engaged in trying to predict what it took to make an atomic bomb. They developed many very sophisticated mathematical tools to do the prediction and that led to the need for high speed computers to do the calculations that these mathematic equations would specify. That led to the development of the modern digital computer.

And so our research in that was a continuation of what we taught in the class to students about these mathematical principles. Every aspect of it had some challenging problems that had not yet been worked on. When you build a nuclear power

reactor, you put a lot of things in the reactor to extract heat and make it last a long time that aren't at all present in the atomic bomb. It's a much more complicated system, and that took a lot of research to advance the field.

We had one specialist in that, Professor Harvey Amster, who came to us from the Westinghouse Bettis Laboratory which was the laboratory set up by the Navy but operated by Westinghouse to develop design techniques for designing nuclear submarines. He brought some of those techniques and was very sophisticated and had many ideas on further improvements. That was one area of research.

Then another area was on what we called a nuclear fuel cycle, which includes designing the reactor. It also includes carrying out the many other operations that are required to make the whole system work. For example, the uranium has to be enriched in the isotope ^{235}U . Natural uranium is really not very useful in nuclear power reactors, or even in naval reactors. So separating the isotopes of heavy elements, of a heavy element like uranium, was engaged by the Manhattan Project because one of their parallel approaches was to make highly enriched ^{235}U and make bombs from it. That was successful and that was the Nagasaki bomb.

We taught the principles of that to our students, and that was a whole new set of mathematical equations that had been developed--some of them--during the war. We had to modify those as new ideas came up, and that led to the research on isotope separation. Similarly, there was research on chemical processing, especially chemical separations of discharged fuel that comes out of reactors.

During the war years, the Manhattan project worked on that, mainly carried out by Professor [Glenn] Seaborg on the Berkeley campus, where they dissolved irradiated uranium--uranium that had been in a reactor--in acid, worked out techniques of purifying the plutonium--separating the uranium and separating the highly radioactive fission products which became high level waste.

Well, those techniques developed during the war did the job but they were very inefficient. They made enormous quantities of waste, and so in the 1950s there was a national effort to develop new techniques which could make cleaner separations, reduce the waste volume, and recover more of the uranium and plutonium, and for the nuclear fuel cycle--the commercial fuel cycle--to eventually recycle those. That was an active research program.

Wilmsen: Where did the impetus for that national effort come from?

Pigford: After the war?

Wilmsen: Yes.

Pigford: The impetus for having a complete fuel cycle for commercial power reactors. In those days it was expected that reprocessing would be a major part of that. In those days they expected the breeder reactor to be the foremost power reactor, and to make a breeder reactor you need plutonium, and to get plutonium you need to recover that from the spent fuel from light-water reactors.

It turned out that the breeder program was not all that successful in terms of its timing and so it's on the back burner.

Wilmsen: You described it as a national effort, so was that something that the government was promoting?

Pigford: Yes, because the government was devoting a lot of the efforts of its laboratories--not Livermore, but Oak Ridge, and Argonne [National] Laboratory in Chicago--to developing the nuclear power fuel cycle.

Wilmsen: Okay.

Pigford: We had research on that, and then there's the more specific area of research on the nuclear materials problems: what makes a good material for nuclear fuel in a reactor? It has to stand up to an extremely intense environment of radiation. Fissions are going on right in the nuclear fuel, the fission fragments cause lattice displacements, they alter the properties of it. To be economical, a nuclear fuel material in a reactor must last for several years, and that's a very daunting requirement. That was one of our major research programs at Berkeley and it still is.

And it wasn't just on nuclear fuelling material. The metal cladding, the metal envelop that surrounds the nuclear fuel rods, also had to be corrosion resistant in the presence of high temperature. It would receive intense radiation from the nuclear fuel which was right adjacent to it. It would embrittle and tend to crack, but it had to preserve the separation between the coolant and the fuel material itself, and that was a tough problem.

Another research program we carried out was related to the

space program. This was well after Sputnik, but only a few years later, and so there was a national effort to develop all sorts of propulsion engines for space application.

One type called the ion propulsion was of interest for the possible long range journeys in space, like going to Mars. It wasn't related to developing missiles that could go from United States to Russia, which required chemical propellants, but this ion engine could use electricity generated by a compact nuclear power reactor. That electricity would ionize an alkaline metal such as cesium, which is easily ionized, and then it would accelerate the cesium ions using electrostatic potentials, so they discharged from the propulsion end of the rocket. They could be discharged so rapidly, so much more so than the products of a chemical reaction, that this could be a very efficient propulsion engine.

But it had many, many, new problems, and we had a research program carried out for NASA--the National Aeronautics and Space Administration--and part of it was related to materials, these ions of cesium. Some of them would strike the solid surface and cause sputtering of the material from the surface, which would contaminate the environment and reduce the efficiency. So we used a nuclear research reactor to study the sputtering process, which is one of the specific materials problems.

Then we had research in nuclear instrumentation to do experiments that would be involved in developing new types of power reactors. You needed instruments to measure the neutrons, their fluxes and intensity, and to measure the gamma rays. It would be desirable that they be very compact and could go into small holes in the whole assembly. Some of our people worked on developing new and better detection techniques.

We had some research related to nuclear fusion. This was initiated by Professor Robert Pyle. It was fairly basic, to study the fusion reaction. They were using some experimental facilities at the Lawrence Berkeley Laboratory on that.

We had research in what we call health physics: What are the biological effects of nuclear radiation? For example, some of the industries and the national laboratories were beginning to work on plutonium fuels with the prospect of future use in breeders or future use as plutonium-recycling power reactors. It emerged that plutonium oxide would be a likely candidate for those fuels, even though it was not used in making weapons. Then it turned out that if you're working with plutonium oxide, that

creates a new health hazard because it's easily fragmented into finely divided particles and if they get into the air, they can easily be transported as an aerosol. When people breathe them, the particles can lodge into the lung tissues. When plutonium decays radioactively, it emits a helium nucleus called an alpha particle, and that damages surrounding tissue. It stops very quickly in the surrounding tissue, but it imparts a lot of energy to the few clusters of molecules of the tissue itself and that can give localized tissue damage and could cause lung cancer. That was a new area of health physics that had come up.

We teamed up with a person--Dr. Patricia Durbin--at the Donner Laboratory on the campus, which is really a part of the Lawrence Berkeley Laboratory. She was doing experiments in her animal facility on tracing the transfer of plutonium into the body from the lungs. Even though it's a tiny solid particle, it can move through the lung tissue and get into the blood stream, and she was studying the effect on animals, finding where the plutonium goes.

Turned out it goes, in that case, finally to the lymph nodes. Usually you think of plutonium going to the bones. A graduate student and I were interested in what we call transport modeling--calculating the rate at which various radioisotope species could transport through the air and water. With Patricia Durbin's help (it was a joint project), we applied it to the transport of plutonium particles in the human body and were able to calculate how much of that plutonium would end up in the lymph nodes and what the biological damage would be. That became the first measure, scientific measure, of how much aerosol--finely divided particles--of plutonium oxide could safely be breathed into the lungs.

Those are a few examples of the research. We also had research on basic nuclear reactions themselves. We had one on the energy states in the nucleus. That had been a research topic back in the days of the 1930s and forties, and in those days it was in the province of physics. But physics had moved on beyond that area into much, much higher energies, and so what was called nuclear physics became the province of the chemists and the engineers and some of our faculty were doing that kind of research on nuclear physics, studying the energy states and transition levels in the nucleus. Those are some examples.

Probably I've done a lot of people a disservice, but those are some examples that come to mind. Oh, yes, another important area is the extraction of heat. Coolant, going through a nuclear

power reactor has to extract heat very efficiently and rapidly. That's why it's popular in many reactors to use liquid sodium or water. We've talked about that before.

When the idea came along to let the water itself boil inside the reactor, then the steam could go directly to a turbine, thereby eliminating the need for an intermediate heat exchanger, where, in the naval reactors, steam is generated. The details of the boiling phenomena became very important because how much of the coolant space is occupied by liquid and how much by vapor? It's all churning around and being pumped through. Knowing the amount that's still liquid is necessary to do the neutron physics calculations because the neutrons are moderated by the liquid water. That introduced a fascinating combination of neutron physics and heat transfer and our research led by Professor Virgil Schrock specialized in that.

Collaboration on Research, Edward Teller, Competing for Grants, and the Cold War

Wilmsen: What kind of collaboration was there with other departments like chemistry and physics? You mentioned Patricia Durbin.

Pigford: Well, I was the faculty member collaborating with Patricia Durbin.

Wilmsen: Oh, you were?

Pigford: Yes, but I had worked in transport analysis as a chemical engineer; I was nothing but a dilettante in the area of health physics, but we worked together. The studies on nuclear physics, energy levels, were in collaboration with the people at the Lawrence Berkeley Laboratory who were then called nuclear chemists.

And some of our students had just the right interest. They weren't worrying about the frontiers of high-energy particle physics, the areas of research by pure physicists. Our students were more interested in the details about nuclear fission, the nucleus that were still emerging, and its applications. We collaborated with the nuclear chemists and metallurgists at the Lawrence Berkeley Laboratory in the study of nuclear fuel

materials which I initiated. And that was the first doctoral research project I supervised.

There we were studying uranium carbide, which is a possibly better nuclear fuel than the new fuel of uranium dioxide. Although, it turns out finally that we realized uranium dioxide is better for most applications than uranium carbide. I was particularly interested in how much of a fission gas is made during exposure in a reactor, and how much would escape as radioactive gas out into the coolant, which could be a major problem because those radioactive gases tend to get into the environment--though not all of them. And in that research I collaborated to some extent with people in material science, who would be in ceramics and would be experts on things like uranium carbide.

Wilmsen: What was your student's name in that?

Pigford: That student was Hagai Shaked, a student from Israel. When he graduated, he went back to Israel and worked at their nuclear research laboratories.

Wilmsen: Now you mentioned last time that you were good friends with Edward Teller.

Pigford: Yes.

Wilmsen: Did you do collaborative research with him as well?

Pigford: No, not really. He would not call it his research. He is a great physicist and his research interest in physics would lie in the much higher energy level region that the physicists were interested in. But he also had a practical interest and he, like many great physicist like Hans Bethe at Cornell, had been involved in the applications of nuclear reactions where they had worked in the Manhattan Project during the war. And they continued to enjoy the involvement with nuclear applications like nuclear power reactors or reactors for space, as that field emerged, and they did so as consultants.

Teller and I would find ourselves consulting together for some industrial laboratory, also with Hans Bethe. There, each of us was applying his own skills and background towards trying to solve the problem that our client was working on. And it was a lot of fun because I found that both Teller and Bethe really had the instincts of great engineers.

In fact, Teller had been trained as a chemical engineer in Hungary before he became a physicist. His father wanted him to be sure that he had a good job, and just like my father, he thought engineering was a great field.

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Pigford: Evidently, Edward Teller's deep interest in physics emerged and took over, and he became one of the world's great physicists. Then as he was director of the Lawrence Livermore Laboratory at the time I came to Berkeley to be on the faculty, his laboratory did have some applied projects they were working on--for example, nuclear reactors to propel space vehicles, rockets--and we frequently interacted on those projects--I, as a consultant.

And through the years he liked to develop his own ideas of how the application should go. He is a brilliant man, a very fast thinker, and he calls me up and asks me what I think. Usually I tell him that I think his ideas show wonderful imagination but are not very practical. And that must be the reason he calls me up, to have that different point of view. We have worked through the years on this, off and on, not very closely.

And I've worked with other physicists like him. Let's see: Hans Bethe, Freeman Dyson at Princeton Institute of Advanced Study. I mentioned before that Teller was a member of the search committee that Seaborg, the chancellor, had set up to find a head of nuclear engineering. So when I ran into problems, when I first came to Berkeley, he was then still on the faculty. I would consult him as well as others.

Then, I hadn't been at Berkeley very long before a new issue came up with Teller. He saw as head of the Livermore Laboratory a need for some invigoration of new ideas that were not just making weapons. He wanted to do it by having students participate at Livermore, so he proposed to set up a special graduate program there called the Institute of Applied Science which would be applied physics, mathematics, chemistry. Its makeup would be very similar to our new department of nuclear engineering, although we had more engineers. He never thought much about the engineers, but the work that our faculty on the campus were doing would be similar to what he imagined this group at Livermore would do.

He ran into a lot of resistance within the faculty at Berkeley when he proposed this be an offshoot or an off-campus

academic unit under the Berkeley campus. It was also viewed as a threat to a new department of nuclear engineering. I wasn't opposed to it because I had a great admiration for what he did, knew that he could make a good program, and I expected there would be room for everybody. He wanted us to participate with him, but I had a strong feeling that an hour's drive away is too far to result in any real cooperation academically. Even going up the hill to the Lawrence Berkeley Laboratory creates a separation which is not healthy.

His biggest resistance came from other parts of the campus where people thought he was simply trying to do everything in one department. I remember one meeting, people from the humanities were saying this campus offers even to your science students and engineering students the university environment: humanities, music, arts, and so forth. And that's why some departments--most departments--want to be on the campus.

But Teller frequently does not anticipate such questions. He should have, but he improvised in this way--and he's a very fine pianist, incidentally. He plays, even though he has one bad foot. He plays Mozart beautifully, and his answer to the committee was, "I will play for them." That's the way they would get their humanities. Well, I think that, among other things, helped to sink his proposal to be part of the Berkeley campus.

He then took his proposal to the Davis campus. They were much more hungry for the kind of affiliation, identification he could provide and so they officially set up what became the Department of Applied Science, under Teller, as a branch of the Davis campus. I don't think the interaction between the two is very much. And they still have that department there.

Wilmsen: Was there much competition for grant monies for all the various research programs you described?

Pigford: Yes, because, for example, I and my colleague Donald Olander, Professor Olander, were working in nuclear materials. Then the people in the department of Mineral Engineering and Material Science--that was the name of that department, which was a well established department--felt that we would be competing with them for grant money. And we would. We did. It turned out that we were both successful, so I think competition doesn't hurt anybody.

I think the research in nuclear physics didn't get much grant money and it became more of an arm of the Lawrence Berkeley

Laboratory. I expected a lot of feeling of competition with mechanical engineering. Here we established a major program in fluid mechanics and heat transfer related to the boiling-water reactors, and, well, they had enough work themselves to go around. They're a very fine department, and so the competition was not at all unhealthy.

Wilmsen: What kind of impact did the cold war have on the research programs? Obviously it had a huge impact on military applications of nuclear power and nuclear weapons, but what about the civilian applications of nuclear research?

Pigford: Well, we weren't drawn into that. We could have modified our program to get more into the cold war applications but we didn't. We were still focussed towards the civilian applications. It tended to close off contact between our program and similar ones at other universities, and between university programs of nuclear engineering and some of the government laboratories. For example, the Oak Ridge National Laboratory was supposed to be a very broad research and development laboratory. A lot of its work was necessarily devoted to solving problems related to the new production reactors at Savannah River which were for the cold war. And some of us had contacts through consulting, but the problems we ran into there were not the sort we could easily bring back home. Some of it we could, but not much, so effectively the cold war tended to develop more isolation between university programs and the government laboratories.

And I think that's true of Livermore, too. Weapons development was their main--has always been their main--forte, and we haven't had any academic programs in connection with that. Some have consulted on a weapons program, but not many.

Wilmsen: Why was Berkeley so focused on the civilian applications? You said you could have gotten more connections with the military if you'd wanted to, so I'm curious why you kept that focus so strongly on the civilian side?

Pigford: Well, we wanted it to be open, unclassified, and not secret. That itself is a major thing. We don't want our students to be locked into a program that is secret. We believe in the civilian applications in the long run, whereas this cold war we expected would have its own shorter term life.

Wilmsen: Now how did Edward Teller feel about Communists?

Pigford: Oh, he hated them, mainly because he grew up in Hungary and there

was, as I've learned, a Communist uprising there long before World War II and apparently they did some terrible things to the people. I don't know the history too well. I don't know if that Communist development during the thirties was very successful. And that's different from them having become Communist later when after the war the Soviets took over. He hated the Communists, and he was paranoid, really --still is, about the Communists. He might say justifiably so, but at least it's an unusual level of fear and hate.

The Nuclear Engineering Curriculum at Cal

Wilmsen: Shall we move on to teaching?

Pigford: Yes.

Wilmsen: Okay. How did the curriculum in nuclear engineering change over the years?

Pigford: Well, the original curriculum was modeled pretty much after the MIT curriculum except we injected also material science into it, and health physics, which MIT didn't have.

How did it change? Well, the fusion program expanded. And in addition to Robert Pyle, who was the first faculty member in it, we added a new assistant professor, Ed Morse. That would have been in the 1970s. (Pyle has died. Morse is still there.)

Then later we added Kenneth Fowler in the mid-1980s. Now Fowler has retired and is no longer there, but the fusion program strengthened through that time. Morse brought in a lot of research funds and new research ideas, as did Fowler.

The work on boiling heat transfer and other kinds of special heat transfer continued at a very steady progress, turned out a lot of good work, and continues as such but under one faculty member, Professor Virgil Shrock, and he's retired but still active.

These various areas that we're talking about tended to have usually one faculty member. It's unusual in a department to have only one faculty member in your main areas. Usually you have two or more faculty members in each main area. It was a small department.

And materials--the materials work under Olander became really one of the best in the country and still is solidified. I worry about what's going to happen to it when Olander retires. The work in nuclear instrumentation seems to have gone down. Are we talking about teaching?

Wilmsen: Yes. How the curriculum changed.

Pigford: How the curriculum changed. More recently, say, in the last five years--more than five years--my observation since I've retired, and I'm not very active in the department, is that the department is recognizing at last that it needs to change its curriculum. We were for decades centered towards peaceful applications and nuclear energy, especially for commercial nuclear power. As I mentioned once before, it became evident to me in the late seventies that that field was, for academic work, not very promising. We like to think that we turn out students who want to do research and development in industry or in other places and work on new ideas towards the ultimate application we're interested in, and if you're not developing any new nuclear power reactors, there's no good place for our graduates to work. So that emphasis has decreased. It's a big change. I had introduced, in the late seventies, research in radioactive waste disposal, which I forgot to mention as one of the department's research programs. That research quickly blossomed to a main, very big research program of the department before I retired and it's still fairly active. At the same time, the teaching moved in that direction too.

Wilmsen: Is that the major emphasis of the department, now?

Pigford: I don't think they would call it a major emphasis, but that emphasis has grown, and teaching about nuclear power reactors themselves has decreased a lot.

Wilmsen: What are they preparing students for now, if the commercial nuclear power side of things is more or less on the way out?

Pigford: Yes, well, there are plenty of jobs in waste disposal. And they are emphasizing more and more the interaction with the bioengineering program, which, as you probably know, is a new push on the campus. There's a new department, and they've even gone into the field of tomography, which is doing scans on the brain and on the rest of the body. These involve nuclear reactions and so the development of instrumentation for that, techniques of sensing the nuclear radiations and interpreting

them, is occupying more and more time.

Wilmsen: It's another connection to the health field.

Pigford: Yes.

Wilmsen: What were the major challenges and opportunities in the area of teaching? I guess, starting back when you first started at Cal, still talking about the department at Cal.

Pigford: Well, the first major challenge was to develop a coherent faculty. In those days you couldn't put together a department of nuclear engineering by hiring graduates in nuclear engineering to teach. There weren't any, or those would be just assistant professors. We wanted a mature faculty quickly, so we hired specialists in the areas I mentioned, and each of those specialists had come from a specialist group doing that work at other places, like the naval laboratories.

Well, we wanted to knit them together so they could feel like nuclear engineers. We wanted the man teaching reactor physics to understand the materials problems and vice versa, so we initiated a plan that we would rotate teaching assignments and theoretically hoped that every person on the staff and on the faculty could teach every course in the department. That became a severe burden on a few people but--and we never went that far--people began to be able to speak the same language. We would use each other's class notes, and some books emerged from it. That was the first major challenge.

We had to carefully bring in people from the outside who were experts and try to translate what we learned from them through seminars and curricula and so forth into things we felt like teaching, so it's how to emerge a new field.

It was exciting. The students got caught up in that and they needed that too because the students we took by and large were not from undergraduate programs in nuclear engineering. We started out as a graduate department, as most places did, so our incoming students were from physics, mechanical engineering, chemical engineering, chemistry, and we had to adapt our courses to them so that they could develop the mathematics that was needed, which is more than most engineering. Chemists don't have much mathematics, usually, and the physicists had to learn some practical thermodynamics and so forth and so on. So we had to construct courses for the purpose of remedying the background deficiencies. We didn't have enough time to teach them, give

them all the remedial courses, and then go on into the graduate level, so that assimilation for the students was a major challenge. It became easier many years later as undergraduate programs originated and then that became a strong source of graduate students.

I would say those are the major challenges. Oh, yes, we had to define the standards. Being a graduate department, especially at Berkeley, we were determined that our graduate students would be at least as scholarly and as well trained as those other departments, and so we introduced the technique that had been used at MIT, through my years there as a student and a teacher, of giving special written examinations to students who wanted to be doctoral students. It's not used in many departments. In many departments you pass your courses, then if you convince your dissertation committee that you're qualified--I'm oversimplifying it--you get to be a doctoral student. Now we don't even call them doctoral students unless they have passed a general comprehensive written examination.

Wilmsen: You mean other nuclear engineering departments in the country don't require those qualifying exams?

Pigford: That's true. And there are many departments at Berkeley--in other fields--that don't require it. Now many others, I must say, may have it, but frequently that's an examination given by the dissertation committee which is four or five people. You don't get as clear a rigorous screening process that way. First, if you do it along the way in your dissertation, it's hard to fail a guy. You keep him on until he finally does it. We insist that this screening be done before people have worked on a dissertation and it be very objective, which means the entire faculty submits questions and the entire faculty is involved in the grading and decision on who passes and fails. That was a major effort and still is, as far as I know, to be sure that our doctoral students are selected of the kind that we think we must have. It's kind of brutal because to the others we'll say you can get a master's degree and that's it.

Wilmsen: Did the faculty stay actively engaged with that process? I mean, did they continue to all submit questions?

Pigford: Well, they did throughout my involvement with the department. I don't know what they do now. It's a lot of work. We gave them twice a year and the questions you write down are very searching questions. Many of the faculty complain, but also it had a lot to do with, again, cross-fertilization among the faculty because

we would see the kind of questions our colleagues were turning out. When we got together on grading--one has to be very polite and discreet, but you can sense it if your questions aren't really up to the caliber of what these other guys are turning out. So that was a major undertaking from the beginning and I think it reaped good results. In terms of whatever measure there is in quality, we had a good reputation for our productivity, and as far as I know, still do.

Wilmsen: Was there a high failure rate of students compared to--well, I guess if other departments around the country don't do that, I guess it's hard to compare.

Pigford: MIT does it. Yes. We don't want a high failure rate. We prefer everybody pass. I think occasionally everybody did, but usually not. We did have failures, and that's higher than most places have.

Wilmsen: How did the student body change over the decades? Did you find you got more students, or decreasing interest or an increase in interest?

Pigford: The interest has gone down. The quality has gone down. And that's expected. When we started, we were the premier applied science center.

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Pigford: We were the premier applied science department at one time, and so we attracted the top students. I think as the field has matured, it has been a disappointment in many ways, that is, mediocre quality. I think many of the nuclear engineering undergraduate programs haven't turned out as good graduate students as we'd like. That's a problem because many of the programs in nuclear engineering throughout the country have gone by the wayside and so they are not turning out many graduates in nuclear engineering, which reflects the kind of job market there is. And that affects the quality.

Reflections on Citizen Activism at Bodega Bay, and Student Activism in the 1960s

Wilmsen: I wanted to ask you about the Bodega Bay controversy, also, which

you said that you actually didn't have much involvement with, but I was wondering if you could just kind of sum up what you thought about that whole thing at the time.

Pigford: At the time?

Wilmsen: At the time, and now if it's different.

Pigford: Well, it's difficult without injecting my own value judgements after the many years, but I was amazed to learn that PG&E wanted to move so fast in building nuclear power plants. They had a planning map that showed so many new nuclear power plants spotted along the Pacific Coast of California. And they were serious. Bodega Bay, I guess, was to be the first one.

They didn't have any clear guidance from government which in those days would have been the Atomic Energy Commission, which was the only regulatory guide. Any good guidance says what the safety requirements would be, so they had to kind of develop it themselves. And as I understand it, there was an earthquake fault running right through the site, and part of their facility would be spanning that fault. Well, I recall that they consulted many structural engineers, and if you put enough reinforcing steel in it and concrete, you can survive a displacement.

Wilmsen: That was their opinion.

Pigford: Yes, and they had some very qualified experts working on it--many people in structural engineering on the faculty at Cal.

You have to be careful where you anchor it so that you don't anchor it on both sides of the fault in a way that it tears itself apart. You have to allow the motion to slide underneath.

And the farther along they got, they saw what a mammoth facility this had to be. They still had the intent to build it.

Of course there are many people who just felt it wasn't the right thing to do, and it was an important issue to argue because out of this, and out of a few other various situations like it, the AEC developed a criteria for safety for siting and so forth.

Wilmsen: So the discovery of that fault did have an impact on later reactor design?

Pigford: Oh, yes. What happened is the person in charge of the rather small staff at the Atomic Energy Commission in charge of regulation safety finally took a stand and said, "We will not

allow it to span a fault." He said, "That will be the rule and is the rule." And that was the proper thing to say. He wasn't going to waste any more time about sliding structures that could survive. No good. And it was the right thing to do and it took a lot of courage and that's what settled it.

In hindsight it's easy to criticize PG&E. On the other hand, they were going to build something that was brand new to everybody. Why not build it to use the best technology for structural integrity? A bridge spans a fault, and we find ways of dealing with that.

It's easy to criticize them, and I do now, for being naive. It was an example of the attitude that was prevalent especially in the utility industry, which was really making the decisions: shall we come up with the money and build a nuclear power plant? The decision is not in the hands of the scientists or the regulators, it's in the hands of the customer. Then you must get the design made and get safety approval.

They said, in those days, and I speak generically, "It's just like another coal plant. It's a lot of steel, a lot of concrete, we feed fuel into it, we look for high temperatures, we make steam, and sell it."

Well, of course, it is not a coal plant. You don't have to build a coal plant to such exacting specifications. A coal plant is more easily repaired. You can shut it down and get at the fault, whatever it is, and fix it. And so, to a large extent it was a fault--mistake--of that attitude. And that attitude persisted for years--for years--on up into the eighties, I guess. Maybe some people still have it. I don't know.

Wilmsen: I wanted to ask about the campus activism that was going on in the sixties--the Free Speech Movement and anti-Vietnam War demonstrations and all that sort of stuff. What did you think about that at the time?

Pigford: Well, you'll have to understand that most of that activism was on the south side of the campus where social sciences are, and whether it should be or not, the students involved in those kind of programs thought more about what are their emerging problems and interactions and opportunities in society. Engineering students, on the north side of the campus, were said by the south-side students to have blinders on. They're more dedicated towards their profession. Their whole curriculum is focused towards becoming an engineer. Whereas a humanities student would

say "becoming educated" is "my purpose". And so the engineering students weren't much involved in the Free Speech Movement.

And I was troubled that they weren't, actually. I frequently asked my students what do you think about these things? Then as some of the merits of the south-side activism became apparent--and there were a lot of merits: certainly updating curriculum, making them more pertinent, and making them more humanistic; those apply to engineering as well--we initiated in our department, and I think some other engineering departments did, some round table discussions with students about this. We didn't stir up any great passions, I think, but we did benefit from it.

We looked in the papers, and it looked like another world on the other side of the campus. I think it's too bad it's that way because I think both sides need more of the other.

Wilmsen: Were any of the faculty in your department actively involved?

Pigford: I don't think so, not in the sciences and engineering. I know there were two or three people in physics who were actively involved. One physics professor actually canceled classes, which got him into a lot of trouble with the administration. I got involved in that only in the sense that in those days I served on what is called the Campus Committee on Privilege and Tenure where if there is, say, a complaint against a faculty member, if it's significant enough, it comes before that committee to adjudicate it, and the administration's plan to discipline this professor came before us.

Wilmsen: Was that over the Free Speech Movement, or the anti-Vietnam War?

Pigford: That was over the Free Speech Movement. He wasn't the only one. A few people canceled classes, and we look upon meeting classes as sacrosanct: you don't slight that.

Wilmsen: Yes, and how was that resolved in the committee?

Pigford: Well, there were several cases, actually, but some kind of discipline was applied and I've forgotten what it was. He wasn't fired, for example. I've forgotten what the reprimand was. Something.

Wilmsen: How did you feel about how the administration handled the Free Speech Movement?

Pigford: Well, they were naive. They just weren't prepared for it.

I admired Clark Kerr a great deal. He was a fine president --unusually good. I don't know him well enough to know really what he was feeling when he felt he must resign. I wished he hadn't at the time because he's a man of great strength and I think the whole situation would have come out better if he had stuck with it. On the other hand, he could sure be frustrated and discouraged that there seemed to be to him, I imagine, no way to really bring resolution to it. And he was irritated with pressures from the state administration.

Wilmsen: Would you say that the way you characterize the engineering students and faculty involvement in the Free Speech Movement applied to the anti-Vietnam War demonstrations as well?

Pigford: I think so. I think so. Of course, I remember the demonstrations about Vietnam, but I don't recall those issues affecting this, causing deep reviews of what we're doing with students. I don't remember any reviews of curricula for example.

Wilmsen: Were you afraid of any sabotage of nuclear facilities like the experimental nuclear reactor during those years?

Pigford: Not really.

Wilmsen: No?

Pigford: No, we had to build effectively an isolated experimental room--enormous room--for that reactor that could have its ventilation closed off from the rest of the building in case we had a spill of radioactivity in the room. To get into it, you had to go through air-locked double doors which were under continued surveillance by the police department electronically. So even though we never designed it intentionally to be like a burglar proof or terrorist-proof facility, it was.

IV EARLY INVOLVEMENT IN NUCLEAR SAFETY, THE ECONOMICS OF NUCLEAR POWER,
AND A CHANGE IN RESEARCH FOCUS

Member of the National Atomic Energy Safety Licensing Boards, 1963-1974

The Origin of the Boards

Wilmsen: Okay. I wanted to also ask you about your service on the National Atomic Safety Licensing Boards. How did you become a member of that board? That was in 1963?

Pigford: 1963. Yes. Well, the idea of having such boards originated from Congress in the early sixties--maybe '61 or '62. They foresaw the rapidly growing interest on the part of utilities in building nuclear power reactors. The government was responsible for regulating safety and protecting the health of the public.

The parallel approach in the federal government would be, say, for example, to license a radio station. The owner of the proposed station would apply for a license to own, operate, and transmit over the air. There weren't many safety concerns in that, but it would be reviewed very carefully for financial competence--that would be one thing affecting the customers--by an administrative law judge, a lawyer.

In this case there's a whole bunch of lawyers who work for the government as administrative lawyers. And that would be the first legal decision on whether that license should be awarded. They do that for television stations.

The Congress didn't want to put such a decision in the hands

of just lawyers, so it initiated a variation. There would be, for each application, a board of three people to review the application and all the defense of it. Safety would be a major issue--the major issue. Those three people would make the first legal decision as to whether that particular facility would be safe if built according to the specifications.

Wilmsen: Was the emphasis on safety because there had, by that point, been some accidents at nuclear reactors? There was one in England in 1957 or something, I think, and a couple of others.

Pigford: Yes. Of course, the one in England was not a nuclear power reactor but it would be common sense to say there's not that much difference. It was a real issue.

Already the public was beginning to be wary, as they should be, of the potential damage from a nuclear power reactor. You can simply calculate the amount of radioactivity in the reactor and assume it gets spread over the surface of Earth--how many people it could affect. It's potentially enormous damage. And so both the government and the industry--mainly the government--knew that this was coming and they had a job to do.

They even, in preparation for it, had a study done by one of their laboratories--the Brookhaven National Laboratory--to calculate what are the potential consequences from a major nuclear power reactor accident. There they made rather extreme assumptions of how much radioactivity could be distributed and they calculated the exposure of people in the surrounding community. The potential cost and loss of life was enormous.

Already the government was proposing, and the industry was strongly supporting, the proposal that there be a federal insurance policy for each reactor that would indemnify the owners against such casualties. And once that became a matter of debate in Congress, then the public began to wake up to the fact that this is potentially a very big issue. So that's why the Congress started this new approach.

Now it doesn't mean that this board was the first safety group to review the safety. The Atomic Energy Commission had already begun to build up its safety review staff for this purpose. That consisted of a lot of people. Now it's under the purview of the Nuclear Regulatory Commission and they have, I guess, around 7,000 people in that whole operation. It might have been less than 100 at that time on the AEC staff. So it wouldn't come before the board until the staff had concluded it

was safe, and usually it would take the staff three to four years to complete its review of an application.

But the board was the first entity that would make a legal decision on safety. Then that could be appealed by any party up to the commissioners themselves which would act as a legal appeal body and then to the federal district court and so forth. Such appeals have been made and have gone all the way to the Supreme Court.

So Congress dictated to the Atomic Energy Commission that the commission should set up a national panel of scientists, engineers, and lawyers to carry this out. Glenn Seaborg was the chairman of the Atomic Energy Commission then. He had left his chancellor's job. This was one of the first things that confronted him and he already had a case.

Fermi-1

Pigford: The Detroit Edison Company--the electric utility in Detroit, Michigan, serving a large community of consumers--was the one that had decided it would go ahead and build a fast-breeder reactor. I mentioned this previously. It became very controversial, and already hearings had been carried out by a lawyer, hearing examiner, and it had gone without even a decision being made by the examiner. The case had been appealed to a higher court for authority on jurisdiction. I think it went to the Supreme Court, which then remanded it back to the Atomic Energy Commission and said effectively, "Take out these other issues and complete it," so Seaborg asked me to be the first technical member on that board. Then he appointed an engineer from Idaho to be the second one and we had an administrative law judge as the chairman.

Wilmsen: What were their names?

Pigford: Samuel Jensch was the administrative law judge. He had been a Harvard graduate, Harvard Law School. He clerked for the famous Supreme Court justice, Felix Frankfurter. The technical member was named Warren Nyer, and he'd worked at the Idaho Nuclear Laboratory. He had been a member of the team that had built the first atomic pile, or first nuclear reactor, in a squash court at the University of Chicago during the Manhattan Project, one of Fermi's projects.

So we three carried out the hearing. The utility company brought in Hans Bethe as a consultant who I've mentioned before. He is one of the country's great physicists.

A labor union, which was well financed, was the intervenor, and they'd already carried it to the Supreme Court, so that was a powerful intervention. We had to step in and resolve that issue.

Wilmsen: And how did you resolve it?

Pigford: With great difficulty. [laughter] We reviewed their design and we got Bethe to testify. He had reviewed part of it, we got their design people, and many outside consultants.

The greatest safety issue hinged on the possibility of some loose part--some piece of metal, trash--that was in the sodium coolant system getting swept up in the coolant by rapidly moving fluid and going up towards the fuel assemblies where the sodium was to cool the fuel assembly. If that foreign piece, whatever it was, were to lodge against the entrance, the sodium to that particular fuel assembly, or fuel rod, would not be moving. And to show how sensitive this is, within a few seconds, the fuel rod would melt if it didn't have the coolant. And so that was one of the principal safety issues.

They said, "Look, we put in strainers, we know all those things could theoretically happen, but we don't have any loose material in a sodium coolant."

Well, we were in the process of conducting the hearing and already they had the sodium coolant in the facility. They were not allowed to start it up and make it critical, but they were testing it out. They were allowed to do this, partly because the rules were changing and they had been under more lenient rules in the beginning. And so already they had molten sodium in the pipes and in the reactor vessel, and we came in one Monday morning--they'd gone home for the weekend--and they had a new sad look on their faces and they said, "You will read about it in the paper, but there's a loose part now in the sodium."

What happened is that they had wanted to put a little neutron monitor down into the sodium in preparation for starting up so it could measure the reproduction of neutrons and approach to criticality, and they had silver-soldered it to a long stainless steel rod, which was to be lowered down into the sodium and held there. And they'd forgotten that silver solder

dissolves in sodium, and so the neutron monitor was somewhere in the molten sodium, maybe an inch long.

And we said, "Well, there's the problem. It could lodge in your fuel assembly and cause a meltdown." They said, "No, we've done calculations and it's too heavy. There are not enough sheer forces on that piece down in the coolant. There's not enough energy to bring it up." And their consultants said that was true.

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Pigford: So we then went through the procedure: "But if it were to lodge, or something like that, and you have a meltdown, what would you do?" It was required that there be a procedure in hand, such that they could shut down their reactor quickly, avoid the meltdown spreading to other fuel assemblies, and any great radioactivity that was created would be contained in the double-wall containment of the facility. Well, we finally concluded that we thought all of that would work. They would lose their reactor, but that's their own dollars and cents and it wasn't an issue of safety, and so that was the main thing to be resolved and we gave them permission to do it.

They ran for a few months, and then they had a meltdown. It was a partial meltdown, and fortunately, things worked, and it did not cause any damage to people inside or outside the facility.

What actually caused the melting was that there was a new thing they had added along the way which was a metallic cone of zirconium metal underneath the reactor, but located in the sodium pool, so that if there were a melting of the fuel, the molten fuel would come down and see this Chinese hat of zirconium metal which would not melt, and it would then divert the molten fuel away so that it would not become critical again.

Well, apparently there was a fabrication problem getting it into the pool. It was kind of an afterthought, and there was a piece of that zirconium that tore off--a little piece of metal a few centimeters across--and it did lodge into the entrance to the fuel assembly and caused a few fuel assemblies to melt.

Well, that didn't bother us because we said, "We told you so," but it wrecked that facility and that's one of the main reasons they never ran that as a plant.

Wilmsen: Now that was the Fermi-1 plant, right?

Pigford: Yes, Fermi-1. They later built a water-cooled reactor called Fermi-2, and it ran quite successfully. Probably they're still running it, to my knowledge. Well, so that's how I got involved.

Making an Independent Finding of Safety at Diablo Canyon, Circa 1966

Pigford: And then I sat on many cases. I think that the responsibilities serving on those boards worried some of the people on the panel. Glenn Seaborg held a meeting of the national panel before any board got started and he showed us the charter to each board which had come out in legal language. We read it over and he said to us--now I say this not to disparage Seaborg; he was a very great man, but he's a scientist and he believed in his staff. He says, "By the time this comes to you members of the board, by the time the case comes to you, it will have been so thoroughly reviewed by our staff that all technical issues will have been resolved. Your main job is to conduct a public hearing so that in an orderly way, you give the public a chance to air their complaints."

Well, I and a friend of mine who was from Livermore pointed out to him that the charge to the board was not quite that. It said we had to make out our own independent finding of safety, and that doesn't mean you accept the staff's recommendation; it could be no more than a recommendation. Both my friend from Livermore and I pointed that out to Seaborg.

He said, "Don't worry about it."

Well, my friend from Livermore immediately resigned. He apparently never served. And I told Seaborg if I served, I would have to do exactly what I was charged to do.

He said, "Do it as you see fit," and I did and I kept doing that, much to the consternation of the utilities and also the AEC staff.

Along the way I asked lots of questions and demanded lots of answers and caused lots of lawyers to work overtime on the part of the utilities and their engineering staff. There were two different cases in which I participated that led to negative decisions. In one case the whole board turned down the

application. In another case, in the late 1960s when the Pacific Gas and Electric company applied for permission to build its two nuclear plants at Diablo Canyon near San Luis Obispo, I was on that board and I brought up some issues that in my opinion were not resolved. I have already discussed that at some length in one of our earlier interviews.

I ended up writing my own dissent from the Diablo Canyon Board on two grounds--one, a procedural ground, which you'd call a legal ground, and another on a technical ground of safety--and that caused a lot of consternation throughout the country.

Wilmsen: Now, in Diablo Canyon were there big demonstrations against that nuclear power plant?

Pigford: There were some, but strangely in those days [circa 1966]--it's very strange to me--they were not well-organized or supported. It wouldn't happen that way again. There were two main objections. The neighbors--it's in farming country--didn't seem to have much objection to it.

Now the cooling water from the plant would go into the Pacific Ocean and along that part of the Pacific Ocean there are a lot of sea otters. They worried a lot about the possibility of radioactivity in the cooling water--more than should be there--getting into the shellfish, like oysters and so forth, and getting into the sea otters and causing damage. But it was only a few commercial fisherman who appeared to object, and they couldn't expect to make much headway against the battery of highly paid professional attorneys that PG&E could mount.

The other objection was that the geological and seismic investigation was not adequate. I discussed that earlier.

Then there were a couple of ranchers, but the nearby ranches are so big there that they didn't have any effective concerns. It was weakly opposed, really.

Why didn't the Sierra Club oppose it? They'd opposed Bodega Bay, and it had gotten in actively into the selection of the Diablo Canyon site. PG&E from Bodega Bay had learned that they really needed to try to work with the Sierra Club--very powerful intervenement [**or intervention?**]--and as a result, PG&E offered two or more sites to the Sierra Club and Sierra Club indicated that of those sites, it had less opposition to Diablo Canyon. It turned out that many people in the Sierra Club felt that the Sierra Club was wavering in a dissent. It later caused an

enormous rift within the Sierra Club, and I think maybe that contributed to the manager of the club leaving the club. But it may be that accidentally, or from some misunderstanding, the Sierra Club did not even participate in the hearing.

So here it was not actively opposed. I was the only opposer--except for Mr. Vrana, an amateur geologist.¹⁵ I will come back to that later.

Meeting the Provisions of the Atomic Energy Act: A General Electric Case

Wilmsen: You said that there was one other license application that you opposed. What one was that?

Pigford: It was a small experimental fast-breeder reactor that General Electric wanted to build in Arkansas. In those days, designing fast-breeder power reactors was a very important activity at General Electric and they did it with government money. They needed to conduct some experiments that had never been conducted before to prove that the reactor would safely shut itself down even if it had something like a control rod malfunction. To save money, they went to a utility company in Arkansas which didn't want to build a nuclear power plant, but wanted to be on the record as supporting nuclear power, as most utilities throughout the country wanted. They decided they will offer the site and even pay up some money to help the industry move ahead, and so that required a safety license, safety approval to build it.

The people who were the partners in building it were General Electric and the Atomic Energy Commission, which was putting up about half the money. They formed kind of a consortium and they got some money from the Federal Republic of Germany, which was also interested in moving ahead with fast breeders. Germany was interested in the experiment and they wanted to be a part of it and have access to results and get the publicity and so forth. I think they maybe were going to put in something like 15 or 20 percent of the money.

Well, one of the criteria for getting a license goes beyond

¹⁵Professor Pigford revised his discussion of Mr. Vrana's objection to the Diablo Canyon nuclear power plant and inserted it as part of a longer insert later in this transcript (see pages 144-150).

safety: it's financial competence and also consistency with the Atomic Energy Act which was enacted by Congress probably in 1952, during the Eisenhower Atoms for Peace program. One part of that act says the government will not support or give a license to any nuclear facility owned by a foreign government.

Well, the chairman of this was a professional administrative law judge. We were all administrative law judges; I was a judge for fourteen years, just part-time, even though I'd known nothing about law.

Wilmsen: You mean as a member of this panel?

Pigford: Yes. And so he said he was worried about whether this consortium would fairly meet the provisions of the Atomic Energy Act. At the first open meeting he asked the lawyers from General Electric to please look into that issue, consider the prohibition from the Atomic Energy Act, and try to convince our board that you're not violating it.

They said, "It's only 15 percent from West Germany. The project is controlled by GE and Atomic Energy Commission and they're the ones who are really going to make the decision."

Well, our administrative law judge--his name was J.D. Bond--had written many, many reviews of normal applications, like licenses for radio and TV stations, and so he knew something about the parts of law involving the government--government litigations--concerning what is meant by ownership.

He dug into the law books and found the case of the Brown Shoe Company in Boston, which must have been back in the 1920s or something like that, in which there was an issue of stock owned by many people. It was a publicly-owned company. Maybe it was anti-trust, I don't know, but there was some litigation as to whether one of the stockholders who owned a lot of stock had undue influence. Maybe 20 percent of the company could be held, as an owner, in the sense of being able to exercise undue influence on financial decisions.

The courts found that, yes, that was undue influence and he would be called an owner. J.D. Bond then generalized and said, "There is Germany"--which is either 15 or 20 percent, I've forgotten my numbers--"and so legally they represent ownership to the extent that it violates the 1952 Atomic Energy Act. Therefore we will not give you a license." And he convinced me and the other technical member that this was a necessary

decision.

It upset the whole industry and nuclear community, but the Atomic Energy Commission quickly fixed it with Germany and established a new decision process in which Germany was not a decision-making party at all. Germany didn't care. All they wanted was the publicity.

Making Technical Improvements: A Condition of Licensing

Wilmsen: It was many years later, when they had the big demonstrations there, that I was thinking about at Diablo.

Pigford: Oh, they've had so many demonstrations, they're fairly routine. It shows that even though there was strangely little public opposition during the construction permit stage in the 1960s, that opposition has mounted. But I don't think even yet it has ever amounted to really well organized and financed opposition.

It may be that the best organized and financed opposition was on Fermi-1. Why the labor union would undertake this--such a challenge--I have never understood, but they did.

Wilmsen: Now in a case like Fermi-1, where the first board was brought in, it sounds like it was well after construction was underway.

Pigford: Oh, in the case of Fermi-1, yes, they were allowed to go ahead and start construction because the criteria were just evolving. But they got caught up on that, and so by the time the new licensing board system came into place, the plant was almost completely constructed.

Wilmsen: So basically the role of the board then was to make recommendations for how to--

Pigford: To decide if they could operate. They still officially had to complete a few things. Well, what the company wanted was permission to go into operating license, but we told them they weren't ready. There were too many questions we had--concerns--so we said it's still a construction permit. Even though the remaining construction would not be very much, they could not go into criticality testing without our permission. So, it was a major decision, but we were perfectly willing to not be influenced at all by the fact that the plant was already

constructed.

Wilmsen: Now did you have a hand in developing the criteria and standards as well?

Pigford: Well, along the way, each board did.

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Pigford: They never would let the Republic of Germany come in again. I also never again heard of that way of calculating iodine doses to adults and neglecting infants.¹⁶ And oh, a major thing: I refused to participate in the way that Seaborg had initially said. He said, "If technical issues are not in controversy[, **you**] don't need to worry about that." Well, I said I can't do that, and so I went into every technical issue that I thought was germane. Now, I cannot claim that this set the pattern for the rest of the boards, but I know it had some influence.

Determining an Acceptably Low Hazard

Wilmsen: How did you go about determining if that hazard was acceptably low?

Pigford: Well, to be frank, in the long run it's a gut feeling. But it's not as ad hoc as it may sound. First, the Atomic Energy Commission had set up criteria: What are the allowable outside doses from the operation of this plant? They have two sets of criteria: one for normal operation when you still emit radioactivity to the environment and the other for accidents.

So we did not engage in evaluating the adequacy of those criteria. I know those criteria were challenged, but they were not challenged before the Atomic Safety and Licensing boards; they were challenged directly to the commissioners of the Atomic Energy Commission, who had to handle that. What we did is we reviewed the design for the proposed operation--considered all the what-ifs, like what if you lose emergency cooling? What if you have this piece of metal lodged into the entrance to the fuel assembly? It was a long list, and we could keep adding to that

¹⁶See insert pages 144-150.

list as long as we wanted to.

They had to provide answers both in oral testimony and written back-up. The reason I say it's a gut feeling is because most of our questions were on things that had never happened before, and still haven't happened. That's what safety analysis is, if it's done properly. And so when you get answers and you say these are calculations of what would happen, you ask have they used sufficient conservatism in selecting the parameters, for example. It's refined enough nowadays, but we went through this exercise I think mentally.

Nowadays we would first come up with what ways could some of the parameters, like the strength of a piece of metal, be different from what you would use in a calculation. That's always possible: manufacturing errors and so forth. We ask them nowadays to give us a distribution of probability of it being this much weaker and so forth. It requires an enormous amount of testing different samples from different manufacturers, and putting all of this into a calculation to tell us the probability, of a certain calculated response measured in terms of an off-site person getting this much dose, or the probability he'll get that much.

Then, nowadays, I'll choose as a criterion--which is normally done in assessing safety of structures like buildings and bridges--I'll accept everything up to the last 5 percent [i.e. 95 percent confidence]. And if the last 5 percent, according to the calculations, look like they are not too far away from the standard, I'll accept it. And that's a gut feeling. Even deciding 5 percent is a gut feeling; some people want 90 percent. Generally the industry, influenced by regulators, uses 95 percent. We weren't so refined in those days, because we didn't have the probabilistic data.

The Need for Better Public Involvement

[Interview 4: October 28, 1999] ##

Wilmsen: Today is October 28, 1999, and this is the fourth interview with Thomas Pigford. Last time we finished talking about your serving

on the Atomic Energy Commission Safety Licensing boards and I had a couple more questions on that. The first one is how did the opposition to nuclear power at public meetings affect you personally?

Pigford: How did they affect me? Well, they took a lot of my time because the boards were supposed to be the forum at which the public could voice their concerns, and when the boards were doing their job right, they had to hold lots of hearings. It affected me also in that I grew to have a better understanding of what these intervenors were after and sympathize with them because it was very hard for them to get answers from the staff of the Atomic Energy Commission and from the applicants which would be the utility companies and the consultants.

I don't intend to imply anything negative about any of the parties, but I don't believe the parties--namely, the Atomic Energy Commission and the utility companies, the applicants--really took the time to understand what the intervenors were really after. I developed, I guess, more of a concern that somehow it wasn't giving the intervenors quite the forum they apparently needed.

Now that might have affected me more indirectly. In the seventies I got more and more into doing research on environmental issues and policy and I'm doing that still. And some of this interest was triggered by concerns over the points that intervenors were bringing up.

Wilmsen: So there were two things, then: the forum for them raising the issues and then the issues themselves?

Pigford: Yes. It also became apparent that the utility companies by and large were trying to supply answers, but it's just like two ships passing in the dark; they weren't communicating very well. You can say that also about the staff of the Atomic Energy Commission.

Wilmsen: They weren't communicating with the public? Or with the industry? Or both?

Pigford: Both.

Wilmsen: Both. Okay. And how did you think that the forum was not meeting the needs of the public?

Pigford: Well, the whole process was not meeting the needs. The process

starts with the first announcements of intent to apply for a construction permit. I think the utility companies learned, finally, that they had to start interfacing with the public at that level even before then because the public was interested and they wanted to feel that they were being consulted from the beginning, whereas the forum that was provided officially through the licensing boards was way down the pike.

The utility company would acquire the land, with which they were allowed to do whatever they wanted; work with contractors, equipment suppliers to develop a design of the plant, which they're allowed to do; and then apply for a construction permit.

Already they've invested hundreds of millions of dollars in the project, and I think the public would like to have been consulted during that phase.

Finally, usually after three or four years of review of the application itself by the staff of the Atomic Energy Commission, it comes to the board. The staff of the Atomic Energy Commission reviewed it strictly, by and large, as a technical project. There was no official forum for intervening at that level, and yet they should have been working with the public--they and the utility companies. And that wasn't happening. So by the time it got to the board itself, positions tended to have become polarized. That's not a good atmosphere for exchange of views and trying to understand the other people's views.

Wilmsen: Now, while you were serving on those licensing boards, it might have been towards the end, I'm not exactly sure of the date, actually, but the Atomic Energy Commission was then split into the Nuclear Regulatory Commission and the Energy Research and Development Administration [ERDA]. What kind of effect did that have on your work with safety licensing and also any other impact it might have had on you?

Pigford: That splitting itself, as I recalled it, didn't seem to make much difference, but officially our boards then became an arm of the Nuclear Regulatory division, which is what they should be. I suppose it got more structured. They set up an Atomic Safety and Licensing appeal board which was a board that continued with four or five people that were on it, and so if any of the parties--the intervenors, or the utility companies (the applicants)--wanted to appeal the decision of a given board, they could go to the appeal board.

Sometimes the appeal board would remand the issue back to the board itself that wrote the decision. The appeal board tried

to streamline the work by suggesting that some boards took longer than others, were more thorough than others. That might have helped a little bit. By and large, though, I don't think the transition to being under or part of the Nuclear Regulatory Commission's proceedings really affected me very much.

Wilmsen: Then how did the appointment in 1971 of James Schlesinger as head of the Atomic Energy Commission affect your work on the safety licensing board?

Pigford: I don't think Schlesinger's appointment affected any of the work I can remember as a member of the board. He seemed to stay removed from that process, and I don't think he ever met with us. Seaborg had met with the entire panel from which boards are drawn, but I don't think Schlesinger did. I can't remember any of that. He did affect the Atomic Energy Commission, and its effect on a subsequent organization, ERDA, was well known, but that had little to do, as far as I can remember, with my own experience in the licensing.

Wilmsen: Because he announced that the AEC would be a neutral defender of the public interest. What kind of impact did that have on the kind of controversy you were referring to earlier?

Pigford: Well, I had forgotten about that announcement. I guess I didn't even remember it. My recollection is I didn't see any effect. That may be unfair to him, but it may not have affected any of the boards I was on.

Wilmsen: What kind of effect, if any, did Earth Day in 1970 have?

Pigford: What was Earth Day?

Wilmsen: It was a national event planned by Senator Gaylord Nelson to raise public awareness of environmental issues.

Pigford: I cannot remember any of that.

The Economics of Nuclear Power, and Shifting Research Focus to Nuclear Waste Management

Wilmsen: Okay. Then moving on into later in the 1970s, you started

getting interested in analyzing the cost of electrical energy from nuclear power plants. How did it come about that you got interested in that?

Pigford: Well, I had been interested in that almost from the beginning of my career as a nuclear engineer, and [Mason] Benedict and I, in writing and teaching at MIT, felt that we must teach engineers how to analyze the economics because the product, if it's useful, must meet economic criteria as one of the foremost measures of success. Even in our first edition of our textbook we presented equations and techniques for economic analysis. That was highly refined in our second edition which wasn't published until the end of the seventies.

Wilmsen: What conclusions did you reach from those analyses?

Pigford: Well, we concluded that nuclear power could be economical. It was a close call comparing it with coal-fired plants to generate electricity which was, at that time, the real contender--coal and nuclear. We were more interested in identifying the effect of economics on the design of reactors and on the importance of various operations in the fuel cycle. We learned that the overwhelming cost of nuclear power lies in the capital costs, the costs of constructing the plant in the first place, whereas the operating costs are a much smaller component of the total than it is, say, in coal-fired plants. So that gave considerable credence to the belief that a nuclear power plant would operate with lower fuel costs. I think that turned out to be the case, especially when compared with coal. It's very marginal in that comparison when you compare it with gas, which was a new feature, not present in the 1970s.

Wilmsen: When did gas come in?

Pigford: Well, gas was beginning to come in in the 1970s. It came in in the late seventies in terms of being used to generate baseload electricity--large plants to make electrical energy. It came in very heavily towards the end of the seventies, and that is the main reason why there were no more orders for nuclear plants. The last order for a new plant occurred in the late seventies, I think.

The reason it came in so strongly was because it had a reasonable fuel cost, but a very low capital cost--very low--so low that neither coal nor nuclear could compete with gas because of that. And that's still the case.

Wilmsen: Now what kind of impact did these conclusions that you reached on your economic analyses have on your research and your future career?

Pigford: Well, as I say, we got into economic analyses from the beginning. That showed us a clear pathway towards a very saleable and important product: nuclear energy. And that reinforced our enthusiasm for teaching and contributing to the development of nuclear power. However, towards the end of the seventies I saw the turnaround of the economics caused by the natural gas. I was taking it on faith, but with some surprise, that there were enough resources of natural gas to clearly fuel a gas plant for its entire life--thirty or forty years of the plant--which had been a problem in earlier years. Those resources mainly came from the expanding fields of natural gas in Canada. Taking that into account, then, it seemed to me quite reasonable that utilities would place no more orders for nuclear plants.

I wasn't interested in just teaching the principles of a nuclear plant; I wanted to educate students, young engineers in doing innovations and designing them--designing better ones. If there are no new orders, there's no need really to design new nuclear plants.

With of course some reservation--we didn't know how long that situation would last, but I thought it was going to last a long time--I turned my own interest to do research and teaching on the disposal of radioactive waste, and that blossomed out very quickly. We started that probably about 1977, although I had written a few papers about it as early as 1974, I think. And it blossomed into a major research program funded by various countries and the Department of Energy.

Wilmsen: How did your colleagues in the department respond to your shifting your research focus?

Pigford: Well, one of them, Professor [Paul L.] Chambré, who is an applied mathematician, saw the importance and opportunities and he joined me in the research. That became the major part of his research, too, and we formed a research team. And that's a fairly large team for a department. Most of the faculty have individually carved out areas of their own: one man in nuclear materials, one man in thermal hydraulics and heat removal, and so forth. I think the department as a whole was very happy to see this blossoming in a new area. I don't recall others really materially changing their own research as much as Chambré and I did.

Wilmsen: What was the impact on teaching of your change in research focus?

Pigford: Well, I still taught the same basic courses I'd been teaching before. I added a new course on principles of waste disposal and added a major new seminar course because this new effort was very popular with graduate students. In fact, many graduate students were sent to our department from foreign countries--Taiwan, China, Japan, France, and so forth--specifically to work with that new research group. Our seminar course blossomed and it became a very exciting field.

Two More Terms as Department Chair, 1974-1979, and 1984-1988

Restimulating the Department

Wilmsen: And then in the mid-seventies, you became chair of the department again, and then served through the late seventies.

Pigford: I think I said 1974, didn't I?

Wilmsen: Yes.

Pigford: Yes, that's when I became chairman again.

Wilmsen: What new issues had emerged in the department since your previous tenure as chair?

Pigford: [sigh] Well--

Wilmsen: How was it different?

Pigford: It was somewhat unusual in administrative practices within the university to ask a chairman to repeat his chairmanship so soon--and they asked me to repeat two times--but the administration asked me to do it. In both cases for the repeat appointments, they felt the department needed to be restimulated. I was somewhat surprised at that. But they felt there were problems and so they asked me to come back in as chairman to restimulate the programs, and that was the main reason.

It's not the best job in the world. The first tenure as chairman was maybe one of the best jobs in the world because I was creating a new department and had the resources given to me by Chancellor Seaborg to hire a lot of new faculty at tenure level. We put together a great team. And that was very exciting. It's never as exciting to come in again and try to rectify or restimulate. It's a harder challenge, really.

Wilmsen: Why did the administration feel that it needed restimulation?

Pigford: There were reports from our periodic review committee. We had an outside review committee--outside the department, outside the university--which we set up when we first formed the department. It had academics on it and professionals in the field, and they felt that our department had lost much of its connection with the real world. With the new technical challenges of nuclear energy, that connection had sort of drifted or become more diffuse, and they wanted the department to develop more focus for that purpose. That was the second time I was chairman.

The third time that question came up again, and also we had this very expensive facility which I've described before--the reactor laboratory--and its use in teaching and research within the department had gone downhill. It was actively used, but it was used actively by people from Lawrence Berkeley Laboratory who wanted to use it to prepare radioactive isotopes, and by outside contractors such as Lockheed which was developing missile systems and satellite systems for the military. They involved very sophisticated electronic systems and there's always a worry and uncertainty about whether those electronic systems can survive in space where they can be exposed to cosmic rays and other radiation from the sun. They could simulate that by putting them in our reactor and testing them out.

The reactor was making money for the department but it was not being used much for teaching and research, and that was an example of one of the problems the campus had with the department. And so again, it's a similar thing: question the focus, refocus where appropriate, and restimulate the department.

I thought those concerns were quite well taken. At that time it was not the result of an external review committee, but I set up a special external review committee to review the department when I took over for the third time. They gave us a lot of strength in their strong recommendations, and so that helped us carry them out.

During that third tenure as chairman, we got the faculty of our department to itself question the use of the reactor and its importance. We concluded that making money from outside users was not the most important thing academically, and that campus space, which is very dear, could be better used for other purposes. So we managed to get a fairly sizeable allocation from the state with the support of Chancellor Heyman to decommission the reactor, and that was underway when I completed my third term as chairman.

Wilmsen: Now you mentioned that you were surprised the second time they asked you to be chair?

Pigford: Yes, and I was surprised the third time, too.

Wilmsen: Why were you surprised?

Pigford: Well, I was hoping I wouldn't have to do this. My research was blossoming more, in both cases, than it ever had in the past, and you can't undertake administrative jobs like that without cutting into research and teaching. But I saw the need and they convinced me, so I took the job.

Wilmsen: Was your research blossoming because in the past you'd had responsibilities as chairman?

Pigford: No, I don't think that had much to do with the research.

Wilmsen: Why was it blossoming at that time more than at any other time?

Pigford: The last time I was chairman I was in this new field of waste disposal, and that one had taken off like a rocket. I'd been in nuclear engineering for thirty-four years and I'd become well-known internationally, and we had lots of people who came to participate. They sent a lot of post doctoral scholars to work with us.

Wilmsen: This was in the late seventies?

Pigford: Yes.

Wilmsen: Okay, and what did you do to restimulate the department in the 1970s?

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Pigford: In some cases it appeared that some faculty members were drifting along in their research. The focus towards the department's mission had become diluted. We tried to encourage more faculty members to get research grants, which in engineering and in the physical sciences is necessary, really, to do much in the way of on-campus research. That helped a lot. Just, it's an ordeal to write proposals for grants. They don't come to you. You have to write proposals, follow-up, keep on top of it. And that led to restimulation.

Then, getting the department itself to take a position on the future of our research reactor was not an easy thing because the reactor was a great asset to the department; it had been, and they felt it could be in the future. But we realized that the research we were doing had less and less to do with the reactor, so the department finally, in my third term as chairman, came to a very uniform view that we should take the bold step and get rid of it. We weren't told by anyone to get rid of it. We took the initiative.

New Research Directions

Wilmsen: So the two things, then, were stimulating the faculty to get more grants and to get rid of the reactor.

Pigford: And to initiate new courses and new seminars in areas that could be more productive for research.

Wilmsen: What were those areas?

Pigford: Again, the waste disposal was one. Already fusion had blossomed, and during my third tenure as chairman, we added more faculty in the area of nuclear fusion. But the largest growth of research during the eighties was this program in waste disposal. And that came to me as a welcome surprise. It utilized my background as a chemical engineer, which is what I was educated in at Georgia Tech and MIT.

In chemical engineering one of the disciplines is what we call transport analysis--studying how materials transport through liquids or through gas. It's very important in designing separation systems like, for example, making gasoline out of petroleum, or purifying nuclear fuel, and so forth.

But in waste disposal, the greatest emphasis is on burying waste--whether it's radioactive, or chemical, and so forth--deep underground. The goal is to do it in a way that it will never seriously contaminate ground water that future people could get to, and so the key is not how to build it, but how to prove that it's safe.

The proof lies heavily in mathematical analysis, starting with solid waste. Because over the long term--thousands of years, tens of thousands of years, hundreds of thousands of years--you cannot really ensure that something buried will not be contacted by ground water, you have to ask when it gets contacted, what are the consequences?

The waste containers corrode. It releases radioactivity in some small amount that gets in the ground water, and that contaminated water must travel through rock or soil--rock, if it's a good, well-selected site. Eventually it may get out someplace beyond the repository, and what you worry about is future humans may unknowingly drill a well, extract water, and use that water for drinking water and for growing food which they eat.

The laudable objectives are to design it in a way that such people don't have to take any action to protect themselves, but instead the repository design is good enough that what does get out into the water is at such a low concentration that what radiation dose the future people get as a result of using that water is far below the allowable dose, which is the criterion for safety.

So I've described a transport process: corrosion, dissolution into water, the water moving through rock, which can interact with the rock--the radioactivity can adsorb or absorb on the rock, but that only holds it up temporarily, maybe for a few thousand years--and then it finally gets to the environment. That may take thousands or tens of thousands years, but it finally gets to the environment. When I say get, I mean some small fraction.

And that's the key to proving safety, to develop a credible and defensible mathematical analysis of that process. That's what I studied--not in terms of radioactive waste--but that's one of the principles I've studied in chemical engineering. It was great fun to find that that's what the field of waste disposal needed. They hadn't had chemical engineers working in it from this point of view before, and that was the major thrust of our

research program.

Wilmsen: And what were the other areas of research?

Pigford: In the department?

Wilmsen: Yes.

Pigford: Nuclear fusion.

Wilmsen: Yes, you mentioned fusion.

Pigford: My colleague Professor [Virgil E.] Schrock was working on heat removal processes. Again, it was related mainly to reactor design, and especially reactor safety, because it's maintaining reliable heat extraction that is the key to getting safe nuclear reactors.

You can shut them down, but you still have an enormous amount of radioactive decay heat that has to be removed. If that's not removed, the reactor will overheat even though it's shut down and can melt; it can release radioactivity that can get mainly into the air, which is the worry. He has been doing research on that for years and is still doing so.

Then another colleague, who is about the same age (we were all about the same age when the department started), Professor [Laurence M.] Grossman, has been quite dedicated towards the theoretical physics approach to designing reactors, predicting the transport of nuclear particles throughout the reactor structure. And these are the chain carriers that can cause nuclear criticality.

Well, he advanced the work in many new fields generated by the blossoming interest in using nuclear power for space applications, which meant it had to be a very compact reactor, very small reactor; they only need a few hundred kilowatts for a space vehicle. That introduced a whole new environment in which nuclear power reactor had to perform.

Then there was biomedical research. Professor [Selig N.] Kaplan had an affiliation with Lawrence Berkeley Laboratory and also an affiliation with the UC San Francisco Medical School, both of those places. (The Lawrence Berkeley Laboratory has on the campus the Donner Laboratory, which is its research arm in bio-research.) And that was still emerging. There were many issues: What are the biological effects of radiation?

More data came from survivors of the two nuclear blasts in Japan during World War II, the sole survivors who were on the fringe, who weren't killed because they got lower doses of radiation. It's based upon what is learned from those survivors, who are still being followed up, that has established the standards for what is a safe amount of radiation. In fact, that standard through the years has become tightened up. What was considered safe back in the 1940s and even in the fifties would not be allowed today. My colleagues were continuously involved in following up that work.

Professor [Donald R.] Olander's work in materials science had established one of the unique laboratories in the country on materials science for nuclear applications. He has always been on the forefront of the need for new information. He got involved even in studying some of the unusual properties of rock that is used by some countries for disposing of radioactive waste.

One of the attractive approaches is to find a natural bed of salt that is left over from the time 200 million years ago when the oceans evaporated. We have enormous quantities of that in the United States in the south and in the west. Digging out caverns and tunnels in that rock and putting the waste solids in those tunnels is one of the most attractive ways of disposing of waste of any kind. The rock itself--these layers of rock--are something around 100 feet thick, and they're deep, maybe several thousand feet underground.

Natural salt, which is a rock, is plastic and under the influence of the overburden of earth, it will creep slowly. Like a plastic, it will deform and move in and enclose the waste solids with a continuous layer of rock as if it's a cocoon surrounding the waste solid. And no water can get there; it would have to dissolve out the salt. These salt beds are so thick that there is a slow dissolution of salt by ground water at the boundaries, but those boundaries are far enough away that the hydrologists have established that would take millions of years.

So it's a water-free environment, except there are tiny little inclusions of water that were left over in the evaporation process which are tiny bubbles in the salt itself, and when the waste heats up the salt--radioactive waste--it causes a temperature gradient, and those brine inclusions migrate up the temperature gradient towards the waste.

Professor Olander did some outstanding research in his laboratory to measure the rate of those brine inclusions' transport through the salt. He had been a graduate student in chemical engineering at MIT several years after I graduated and he also had learned a lot about transport processes.

Wilmsen: Was he one of your students?

Pigford: He was a student of Professor Benedict's. We were both officially in the chemical engineering department at that time even though we had started nuclear engineering. He did do research on a different topic for his doctoral dissertation related to the chemical separation to extract plutonium and uranium from discharged nuclear fuel. He did research on that for his doctorate, but after joining our faculty he had turned more to materials research.

So those are some examples. Undoubtedly I'm neglecting some, but those are the ones that come to mind immediately. That's one of the great things about a small department: it's small enough that you can all sit around a conference table and argue and discuss and each person himself has his own ideas and they overlap.

Quantifying Safety Standards, Economic Issues, Fuel Reprocessing, and Concerns about Weapons Proliferation in the late 1970s

Wilmsen: Now backing up to when you first started getting interested in environmental issues, we talked about the safety licensing board not being the appropriate forum--or you felt it wasn't. Then there were environmental issues that you were getting interested in, also. What were the environmental issues?

Pigford: Well, this goes back to the late sixties and early seventies. There were two areas of environmental issues. The biggest one dealt with ultimate safety. Suppose there is some accident, the reactor gets overheated, and it emits radioactivity into the air. That's inside the big containment, but the containments have some tiny perforations in them. It's hard to make them completely tight. The issue is how much could get out and where it would go.

Wilmsen: Yes, we talked about that in one of the early interviews.

Pigford: Did we? We also talked about the other environmental issue, which is not concerned with accidents, but with what we call routine emissions. I think we did talk about that, where the early nuclear power plants had big stacks, like smoke stacks, and the main purpose was to make it possible to discharge environmental contaminants at a fairly elevated height so they'd mix well with air before getting down to people.

Wilmsen: Yes, we did talk about that one, too. So those were the major ones?

Pigford: Those are the major areas. Now my contribution on the first one --on the safety--was I felt that there had not been enough serious analysis of what could happen if some of the safety systems failed and more radioactivity got out into the environment. So I developed some calculations showing the potential consequences, which is a hypothetical number: how many people could get high doses if this safety system failed completely?

I calculated the potential radiation doses as the contaminated cloud progresses out over the vicinity away from the reactor. I intended this to then set more quantitatively the standards of protection that the designers of the containment system would be required to measure up to. I know my calculations frightened a lot of people in the nuclear industry, but many members of the public were beginning to realize that this is what the facts are, and I was endeavoring to get them out into the open. Those results are still being used in setting the design requirements for safety.

Wilmsen: So they frightened the industry because of additional costs that they would incur?

Pigford: That's part of it. Also, the industry realized that this information in the hands of intervenors could be used against them. Of course it was not my purpose to affect the intervening process, but I think the public should know all of these things.

Wilmsen: Now in the early seventies, the Arab oil embargo kind of rekindled some interest, or perhaps stimulated a new drive to pursue nuclear power as an alternative to oil.

Pigford: Oh, yes. In fact, that drive had not diminished. It was already well stimulated in the mid-sixties, but it just added to the

incentive for nuclear plants. But oil is used mainly for transportation fuel, very little for generating electrical energy, which is nuclear's big application. The oil crisis did stimulate more work on electric-powered vehicles, which, if they had emerged economically, could have increased the demand for nuclear energy.

Wilmsen: And so in the context of that drive to pursue nuclear power, just a few years later is when you started to realize that the capital costs of nuclear power plants were great enough to present a pretty significant economic problem.

Pigford: Yes. I realized that natural gas was a new challenger, and because of nuclear's high capital costs the problem was worse than industry had realized.

Wilmsen: Okay, what came of those concerns about threats that were raised by the Arab oil embargo as the years progressed?

Pigford: That situation, the oil embargo, was legitimately used seriously in good conscience by the nuclear power industry in promoting more construction of nuclear power plants. It also led in the seventies to more and more funds for research on other alternatives, like clean power from coal, which is a very important effort still going on, and also research for other alternatives like wind power and solar power; so the oil embargo helped stimulate nuclear power.

As the economic challenges came up a few years later, those were so strong that as soon as the utilities found enough assurance of continued supply of natural gas at a reasonable price for the plant life, they opted for natural gas, regardless of the oil embargo.

Wilmsen: Then in the seventies you were still doing research on nuclear fuel cycles.

Pigford: Yes.

Wilmsen: How did those changes in the economic situation and your interests in nuclear waste effect your research on the fuel cycles?

Pigford: In the early seventies, our fuel cycle research was focussed on developing proper cost analysis techniques for the nuclear fuel cycle. That's somewhat complicated because in the nuclear fuel cycle, the fuel stays in the engine in the plant, running for

about three or four years. You have to lay out money for the fabrication two years before it goes in, so there's an appreciable time lag of several years between purchase of fuel and getting the result. That adds an important cost, namely, the cost of financing that early purchase. It's much more important in the nuclear fuel cycle than it is in the case of the fuel cycle where you buy fuel and burn it rather quickly, so that led to the need for better analytical techniques. The technique we developed has been used and is still being used by some of the utility companies.

Then there was the issue, do we need to go into fuel reprocessing? And in my view, from the point of view of a utility, that depends on the cost. Which is cheaper? So with my graduate student, Jor-Shan Choi, we published analyses of the relative costs of not reprocessing fuel versus reprocessing it and recycling uranium and plutonium.

That was timely because there were already two large-scale commercial reprocessing plants under order. One was under construction, and it was the Barnwell Plant built in South Carolina. The utilities thought reprocessing was a good idea; well, the owners of the Barnwell Plant thought they could do their reprocessing at low cost. It turned out to be uneconomical when finally the real costs were known--but to be fair, the ground rules had changed on them.

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Pigford: The Barnwell Plant was the first commercial-scale reprocessing plant in this country. It was under construction in the mid-seventies. In our analyses we pointed out that if those costs from reprocessing went up much higher it would not be economical to reprocess. Instead, just treating the spent fuel as a waste would be the most economical because of the changes instituted by the Nuclear Regulatory Commission who now wanted the plutonium product converted to a dry solid.

Another change was the Nuclear Regulatory Commission, I think, reasonably wanted to have more onsite storage for the radioactive waste that was produced. That added so much to the cost that the industrial owner of that plant decided he would not make those changes and he would not complete the plant.

That was a shock to the industry. Another plant to be built by Exxon for the same purpose was cancelled. At the same time, President Ford and later President Carter themselves said that

plant should not be completed because they wanted to send a signal to the rest of the world that it would be too vulnerable to the proliferation of nuclear weapons, namely, that the recovered plutonium from reprocessing could be used to make nuclear explosives, and they wanted to be sure that no other country would go into fuel reprocessing.

Back to the U.S.: so Carter adopted a national policy that that plant would not operate. When Reagan became president he lifted that ban on reprocessing, but the industry then saw what the costs would be. Even though another chemical company considered buying the plant from the first owner at a real bargain price, because of the add-ons that had to be made, it concluded that it would not be economical to complete the plant and do the reprocessing. And that was predicted from the analyses that Choi and I made, which showed how much you can afford to pay for reprocessing.

If it got greater than that break-even cost, then you should not do it. We'd never expected it to turn out that way, that our analysis would kill that industrial project, although when it was finally killed, other people were also doing similar analyses.

But it was right to turn down that project, because it would be too expensive. So there is a case where what we had gone into back in the early seventies, simply to refine our technique, gave an answer to a major national decision that was forthcoming in the early eighties.

Wilmsen: How did you feel about President Reagan's approval of making weapons from plutonium from commercial reactors?

Pigford: Well, Reagan didn't approve of that. He simply cancelled Carter's order that that reprocessing plant should not be completed. I am not aware of Reagan's endorsement of using reactor-grade plutonium for weapons.

Many of us in the profession felt that Carter's order of cancellation, which of course worked in the United States, would not itself have the effect on foreign countries that he hoped it would. And the foreign countries--Japan went ahead on reprocessing; their plant is not yet completed. France went ahead; it has built two plants since then. Britain has gone ahead. And they, I think, effectively thumbed their nose at the United States and said, "That's a policy of the United States it wants to undertake for its own selfish reasons. We believe that our reprocessing is safe and we will safeguard this product and

put it back in reactors and use it as a fuel." So already, even when Reagan came into office, it was obvious that Carter's policy was not having the effect on other countries. And that was his only purpose, just to dissuade other countries from reprocessing, and so I felt that Reagan was right that Carter's policy did not have the desired effect.

Now, I'm about to publish an article shortly that concludes that in those countries that have continued reprocessing--France, Britain, and soon Japan and also Russia--they have accumulated a large stockpile of separated civilian plutonium initially that was planned to go back into reactors and be consumed as fuel. But very little of that has happened, and I think the stockpile is just plain dangerous.

I and my co-author, who's a professional journalist who has followed this field for years, are recommending that civilian reprocessing be stopped because obviously it's doing the wrong thing, and that we also take steps to deplete that stockpile and get rid of it. So the issue of whether to do fuel reprocessing or not is a very complex one. It's caused me to go through a complete 180 degree change on that. This article will be published in the National Academy of Sciences Journal called Issues in Science and Technology. It will be coming out in the spring of 2000.

Wilmsen: Who's the co-author?

Pigford: My co-author is Luther Carter, who is a professional journalist. He once was on the staff of Science magazine, but he's been a freelance journalist for many, many years.

The Politicization of Public Concern Over Nuclear Power

Wilmsen: Now, when you decided to go into research on nuclear waste management, were there people in the industry or in the department at Cal who felt that that was not the way to go?

Pigford: No, I don't think anyone thought that was not the way to go. But

that's too broad a question, I think.

Our research was focussed on developing analytical techniques, mathematical techniques, to predict the transfer of radioactivity through geologic media. This implied that the disposal technique would be to create underground caverns and put the waste down there.

Now there were some people who felt that there were alternatives that were better. And one which technically I think is better is to go in for deep sea-bed disposal. Go way out into the Pacific where there are some unusually deep trenches, and embed the waste in a long canister that embeds itself in about 100 feet of sediments at the bottom. From the studies by oceanographers it does appear that the circulation of water down there is so low that there's very little chance--and this is from a transport analysis also--that the small amount of radioactivity that gets to the ocean water at the bottom will get to places where people use ocean water.

I think technically it's a better technique than geologic disposal. The problem is a political one: no one owns that land where the deep trenches are and so the problems of getting international agreements were just thought to be overwhelming.

Another more attractive approach is to shoot the radioactive waste into the sun, which would require concentrating it to reduce the weight. And that's where it belongs, because the sun is so radioactive. But there the technical challenge or problem is the abort rate of missiles, of space vessels, and so when consulting the people in NASA, we concluded that that was just untenable.

- Wilmsen: Now also in the 1970s there was a ballot initiative in California, Proposition 15, which did not pass, but there was some debate about it, which would have ended the construction of nuclear power plants in California.
- Pigford: Yes, that was when Jerry Brown was governor, which must have been in the mid-seventies. And your question is?
- Wilmsen: What kind of impact, if any, did that have on your activities and activities in the department?
- Pigford: Well, I guess ostensibly none, because we expected it to fail and it did. On the other hand, it began to waken us up more that these concerns about nuclear power were not going to go away, and

they shouldn't go away, and were becoming more and more politicized, and that's when they can have some stronger effect than they had in the past. That, I guess, strengthened our resolve that things have to be done right and openly so that they can resist challenges like that.

Wilmsen: There was some legislation passed by the California State Assembly that came out of that debate that restricted construction of a nuclear power plant. Did that have any impact on your research or teaching?

Pigford: I think that came out later--still in the seventies. It said, "No new construction until there has been a demonstrated technology, suitable technology, for disposing of radioactive waste," and that certainly stimulated me to do more research on waste disposal.

Wilmsen: Yes.

Pigford: Well, I think it came out about the time when the natural gas competition was becoming apparent. And that same resolution has been adopted by many other states. Certainly the Department of Energy which was in being at that time--it still is--argued that that demonstrated technology would be available pretty soon (and they were dead wrong). So it was not treated at the time as being a formidable barrier, but if there were an economic climate that would promote additional construction, then I would think that would be the question: what consists of demonstrated technology?

And that would be in the courts, I know. People would argue a pilot plant can be a suitable demonstration--or even calculations, because we're going to rely on calculations in the long run, anyway.

But that never got as far as the courts. I think the utility companies were beginning to prepare cases where they felt they could prevail, but they never got that far.

Wilmsen: Why didn't they get that far?

Pigford: The natural gas. It's not worth it.

Wilmsen: Now during all the debate around that legislation and Proposition 15 and so on, at one point Hans Bethe remarked that he and other experts did not know whether the emergency core cooling system was adequate. I was wondering what you thought about that

remark.

Pigford: Do you have a date on that by any chance?

Wilmsen: I don't have it with me, but it would be probably 1976.

Pigford: It's an important one because Hans Bethe is to be listened to. The trouble is I don't remember it well enough. My sense is I agreed with him. And I've forgotten what was done about it. I'm sure it was not dropped. Unfortunately my memory is too fuzzy on that. I guess if I'd been involved in it, I would remember it, but I wasn't serving on licensing boards anymore and I already was getting into radioactive waste management.

Wilmsen: Another question I had was the nuclear power debate became so polarized that it was almost akin to the abortion debate with the protagonists and antagonists being so deeply entrenched in their own points of view that you could hardly talk about it.

Pigford: You don't listen to each other after that.

Wilmsen: Yes, and each side probably could never convince the other side of their point of view. Of course in debates like over the bills in the Californian State Legislature and other places, both sides would bring out their own experts to testify in support of their own point of view. So my question is, I'm curious what you think can, or should, be done when the experts can't agree.

Pigford: Well, ideally, if you create the right environment for a discussion, I think some of the polarizations go away--maybe temporarily, but I've seen it happen. Create an environment where you get these people together for a weekend, without any legal decision to be made, and a very good moderator, and you go away feeling you have engaged. I don't think that was tried very much.

Another thing is--because I believe that that does have effect--people want to be listened to, and if you show you're listening, then I think it cuts through a lot of barriers.

But both sides have to realize that it's not experts who make any decisions. In the best sense, the political process does that. And the experts must realize that what they're expert on is not the only input to decision. That's why so many projects simply get canceled like the superaccelerator in Texas. The political process just saw the costs.

That was the great project to build the world's biggest accelerator--high energy physics; government-sponsored. The physicists were the experts. And those physicists that work in that area are really a closely knit group and they do feel that their work in terms of need is terribly important. They're good, but there are real limitations, and faltering on progress and cost can properly shake up the sponsors. They would have gotten along better if they (the physicists) had been close enough to the sponsors to realize that cancellation was a possible outcome.

And I would say this about all of the polarization in nuclear power. We degrade politics, but politics are still our way of representing the public good.

Wilmsen: Why do you think that people didn't try to create the environment that would be conducive to the different sides really engaging with one another?

Pigford: Oh, there are many reasons. The polarization is indeed strong and it reflects such professional egos. Each side had points that were valid. Each side could point to those points and say, "Therefore, we are right."

Well, that's not necessarily the right conclusion. And the utility companies have a culture that the criteria for that success has been reliable electrical energy at lower and lower prices. They've succeeded, and have been insulated from the public by the Public Utilities Commission which is supposed to represent the public. But the Public Utilities Commission doesn't really represent the public. They can easily be co-opted by the producers of electricity. The Public Utilities Commission also has that motivation to ensure reliable and cheap power. Well, that's a system in which it feeds on itself. And it's incestuous and it's very hard to break through that and realize that there is a new era wherein decisions are made external to that.

Of course that sounds like I'm mainly criticizing the utilities. But I think that the intervenors overreacted to what they considered to be the selfish impenetrable utilities. I think the utility companies are dedicated towards doing the right thing. So for each side, that can explain the polarizations.

They're so polarized that--for example, I told you about Pacific Gas and Electric at Diablo Canyon. It would have cost Pacific Gas and Electric a few thousand dollars of lawyer fees and so forth to hold an afternoon of hearings to let this poor physics teacher have his say. They found it easier to go along

with the board chairman. They should have been arguing for the hearing, and they would have if they'd really believed in the process. That approach, and not judging the physics teacher, I think would have done so much.¹⁷ Why didn't they do it? I think they were dumb. But so much of it is dumb.

I mentioned earlier after PG&E's Diablo plant finally got built and went into operation, then there were a lot of legal suits because it cost so much to rebuild the plant twice. The Public Utilities Commission of California did not allow them to recover those extra costs by charging more for electricity.

I got involved in that in consulting for the committee of board of directors which was empowered by the full board to investigate and act for the company to recover malpractice insurance because of failures of offices of the company and the board of directors to properly manage the project. And that was my last encounter with PG&E. Chancellor Mike Heyman got me involved in that.

Wilmsen: As long as we're talking about Diablo Canyon, I was wondering what effect all the demonstrations--this was in the late seventies. You've talked about the demonstrations with the physics teacher that were in the sixties, but then in the late seventies there were some big demonstrations there with, at one point, 40,000 people. It was a few months after the Three Mile Island accident that 40,000 people demonstrated against the Diablo Canyon reactor, and I was wondering what effect that had on your research and teaching, if any.

Pigford: None.

Wilmsen: None? And what did you think of Governor Jerry Brown asking the Atomic Energy Commission to deny the license to Diablo Canyon?

Pigford: Well, I've got two reactions. First, I think it's very important for the state to get involved in that issue. I do recall that I thought the reasons he gave were pretty shallow. At that time I was no longer involved in the licensing because this was around 1979 and I was no longer on the board, but I do remember him doing that and I remember I thought his reasons were pretty weak. I was impressed that he would take the initiative to do it; I was underwhelmed by his reasons. I think he could have done a better job than that.

¹⁷See insert pages 144-150.

Wilmsen: What did you find weak about his reasons?

Pigford: I just don't remember them. I just remember that was my reaction.

V ACCIDENTS, SAFETY, CONFLICT, AND ARMS CONTROL

The Accident at Three Mile Island, 1979: Implications for Safety, and
Industry's Response ##

Wilmsen: How did it come about that you were selected to serve on the Presidential Commission on the accident at Three Mile Island?

Pigford: I don't really know, but I was telephoned by someone on the staff of President Carter. I asked him why and he said, "Well, the president thinks you're the only guy who can do the job," which of course I didn't believe. I expect they always say that. I think--and this is now conjecture--I had by that time been identified as both a member of the nuclear engineering profession as well as a challenger of some of the things the profession did. You can see that by my research papers on environmental effects and so forth, and that evidently made me acceptable to both sides, I guess. Someone has told me that. I can't remember whom. So I was the only technical member of the commission, at least from the nuclear engineering field.

[The commission was obviously carefully selected for political balance.¹⁸ The chairman was John Kemeny, president of Dartmouth College, a mathematician, and co-creator of the BASIC computer code. Probably the most influential member was Pat Hagerty, an engineer and CEO of Texas Instruments. Others, in addition to me, were Paul Marks, then vice president for health sciences and now director of the Sloan Kettering Institute; Russell Peterson, president of the National Audubon Society and once an organic chemist at du Pont; Ted Taylor, a one-time physicist who had worked at Los Alamos for a few years and was now a freelance philosopher-scientist with independent income and

¹⁸Professor Pigford added the bracketed material during his review of the draft transcript.

filled with wild ideas about new energy sources; Carolyn Lewis, a journalism professor from Columbia University; Bruce Babbitt, then governor of Arizona and once a geophysicist; Cora Barrett, professor of sociology at University of Wisconsin; Lloyd McBride, president of the United Steelworkers of America; Harry McPherson, partner in a Washington law firm and advisor to Jimmy Carter; and Anne Trunk, resident of Middletown, Pennsylvania, a town near the Three Mile Island (TMI) reactor.

It was ostensibly a six-month assignment. A sizable full-time staff of engineers, physicists, medical experts, lawyers, and a large public relations staff was assembled; Headquarters were in downtown Washington. The commission conducted many highly visible hearings. Congress endowed us with full power to subpoena witnesses and documents. Our charges were broad and allowed a wide-ranging investigation. There was much public interest. Some people thought that we might recommend the immediate shut-down of all commercial nuclear plants in the U.S.

Others expected us to whitewash all of the tough issues. We did neither. Hearings were conducted in a strict legal framework, with questions framed by members of our commission and responses guided by batteries of lawyers representing various parties, including the TMI plant personnel and management, the Department of Energy, the Nuclear Regulatory Commission, the State of Pennsylvania, et cetera. Court reporters produced a written record of testimony within four hours after each hearing session.

During a dull and boring hearing in Washington, I got up to stretch, strolled around the hallway, and met Diane Sawyer, who was then a young TV correspondent for CBS. She was eating her lunch. She shared her sandwich with me.

My role was a delicate one. Being the only member really versed in nuclear power technology and safety, I am sure that most members expected me to put up a strong defense of the safety of nuclear power plants. I did nothing of the sort. In fact, I was so offended by what I considered to be professionally irresponsible training and management of the TMI facility that I instituted on my own a tougher investigation than even the commission's professional staff carried out. I got Kemeny, the chairman, to hire a professional engineer to assist me, something no other commissioner had. I hired Bruce Mann, who had been one of my graduate students a few years previously. He was in residence at Berkeley during the six months. He reviewed the piles of documents that were accumulating and traveled with me to commission hearings. We even arranged informal sessions to run down various leads. He kept the TMI staff in Washington on their

toes.

As I had done in licensing board hearings for the AEC and NRC, I insisted that all parties to the investigation, whether nuclear protagonists or opposers, be given the right of fair hearing. This surprised my colleagues. During our first public hearing in Hershey, Pennsylvania, not far from the TMI site, a group of public citizens from a nearby community wanted to express their concern about the sloppy way the TMI plant was run.

They were not yet organized with their own legal counsel, but they thought they had some important things to say. Our chairman ruled that they were out of order and would not be heard without counsel. I objected, but was overruled. I walked out of the rest of the hearing that day, in protest. It made a delicious story in the news the next day.

But let me first tell you what caused the TMI accident. The TMI nuclear power plant was one of two reactors located at Middletown, Pennsylvania. Early, on March 29, 1979, a pressure-relief valve opened to relieve a slight over pressure in the water-coolant--a normal operation. But the valve failed to close, sending the plant into a shut-down condition, automatically inserting control rods to stop the nuclear chain reaction. Even when shut down, the nuclear fuel rods need to be cooled to remove the heat generated by radioactive decay of the fission products. The special emergency cooling system turned on automatically and injected cooling water into the reactor core. So far, everything was operating safely as designed. However, the operators misinterpreted the reading of water level in the reactor. They thought, incorrectly, that the reactors' coolant system was completely filled with water, something to be avoided because a small amount of steam must be maintained at the highest point in the coolant system to absorb small volume changes caused by temperature fluctuations in the system.

Without thinking properly, the operators turned off the emergency cooling pumps. The accident began at this point. For over two hours the reactor fuel elements were without cooling water. About half of the fuel elements melted, at about 5000^o F, and released large quantities of radioactive fission products to the water coolant system. Enough radioactivity was released to the containment building and to the outside to cause a general plant emergency. Finally, after two hours of overheating and melting, someone else came in and turned the emergency cooling system back on. But the damage had already been done. If the operators had simply kept their hands off the control equipment no accident would have occurred.

Clearly, the operating staff had not been properly trained. Their first diagnosis was wrong, and it should not have taken them two hours to realize that rapidly rising radiation levels and temperatures indicated that the fuel needed to be cooled. But the overall system of safety awareness and supervision of safety had also failed. Nuclear plants are complicated. They are not like coal plants. Many things can go wrong; safety awareness demands learning from mistakes at other nuclear plants throughout the country. The Nuclear Regulatory Commission (NRC) claims that it oversees a national communication system to ensure nuclear safety. But NRC's safety system failed. Just two years before the TMI accident a similar nuclear plant in Ohio suffered the same stuck-open relief valve. Operators there were confused by the seeming rise in water level. For twenty minutes confusion prevailed, then emergency cooling was restored. The Ohio incident was reported to the reactor manufacturer and to the NRC, as required by procedures. But no one communicated it to the TMI plant staff. Both the NRC and the reactor supplier, Babcock and Wilcox, were at fault. The incident taught the Ohio plant that better training was needed, and they implemented better training. The incident at TMI told us that an inherently safe and well designed plant can be dangerous in the hands of poorly trained operators and inadequate managers who do not recognize the need for well-trained operators. It told us that the reactor supplier and the NRC itself were dangerously weak links in the national system of nuclear safety.

We found many complaints that the TMI control room was poorly designed. Certainly a better instrument to indicate reactor water level was needed. But, in my view, these complaints came from operators who were covering their mistakes. For example, the operator responsible for status of valve line-ups had not noticed that a crucial valve in the emergency water system had been closed by someone. But the position indicator was working; a warning red light was flashing in front of him. The real problem became evident when we asked him to recreate the scene. He had an enormous paunch which removed the flashing light from his field of vision. Such problems were supposed to have been picked up during emergency drills.

Further weakness in the management of nuclear safety was revealed by near-tragic mistakes made by the NRC staff and commissioners three days after the accident. Within a few hours after the accident the world-wide news media descended on the TMI site, seeking information. The TMI management was sadly unprepared, but the NRC stepped in and took charge. Their

on-site spokesman, Harold Denton, was able to keep information flowing, and that began to quiet public fears. But Denton and the NRC specialists in Washington began to hear about hydrogen formed by reaction of Zircaloy fuel cladding with water at the high temperatures during core melting. We later learned that this hydrogen had already escaped into the containment building and burned, without serious results. But then the NRC experts remembered that fission-product radiation, as existed for days after the accident itself, could decompose water into hydrogen and oxygen. They calculated, incorrectly, that this explosive mixture could continue to form and build up to a high enough pressure that its chemical explosion could disrupt the reactor vessel itself and its surrounding containment and spew radioactivity over the countryside. The five NRC commissioners themselves were doing their own calculations, aided by their staff experts. Harold Denton agreed with the commissioners and he advised Pennsylvania's Governor Richard Thornburg to order evacuation of a rather large surrounding area. But Harold Denton, the NRC commissioners, and NRC safety experts were making a fundamental technical error, which was obvious to me when I heard of their predictions. More on that later.

Fortunately, Governor Thornburg and his staff foresaw the chaos and frantic efforts to escape the impending disaster, with much loss of life, if he lent credibility to the claimed danger by acting on NRC's recommendation. During the few days of the accident and recovery he had observed that things actually moved pretty slowly in connection with this accident. He took a wait and see attitude. By the end of that day he was rewarded. Enough telephone calls had reached the NRC from the nuclear engineering community to awaken them to the technical facts. They accepted their mistakes and called off their recommendation for evacuation. The explosive high-pressure hydrogen bubble that they had postulated did not exist. But all of this had been followed with intense interest by the international community and particularly by those residents who would have been evacuated. Many had evacuated at their own initiative once they heard about NRC's claims. This explains a conclusion by our commission that the most serious health effects from the accident was the trauma resulting three days after the accident, from NRC's foolish errors.

And what was the mistake that NRC made? Indeed, they were correct that hydrogen would continue to be generated by fission-product radiolysis. But will it continue to evolve as a gas, along with the oxygen also generated? The answer is yes if one thinks only, and incorrectly, in terms of radiolysis at

atmospheric pressure--typical of the usual experimental conditions when radiolysis is studied by chemists. It was such chemists who were advising the NRC commissioners, including the chemist who was then director of NRC's Division of Safety Research. But they were all wrong. The many nuclear and chemical engineers (including me) who telephoned in the correct information pointed out that at high pressure hydrogen dissolves and recombines. This was known even to the less sophisticated reactor operators. They knew that they added hydrogen to the pressurized water coolant to suppress any net evolution of hydrogen from radiolysis. Thus, the hydrogen at TMI could only build up to that partial pressure sufficient for its recombination into water.

Within a few weeks Congress reacted to the sloppiness and errors of the NRC--as evidenced by the TMI accident--and demoted the NRC chairman.

The closed sessions of the commission were the most interesting, especially after we had become acquainted with each other and had learned enough of facts and opinions from testimony to frame the issues for our decision. Russell Petersen made many passionate pleas for a nationwide abandonment of nuclear power because it is unsafe. When asked for evidence, he cited the human errors at TMI that caused the accident to become serious. There, training and supervision were deplorable, which did logically raise the question of how other nuclear plants are managed, and it questioned the adequacy of the NRC oversight for safety. He was correct, but we had already had a look at how other nuclear plants were run, where the preponderance of evidence showed careful operation. NRC oversight did need strengthening. We ultimately recommended a new and tougher system for oversight and for management awareness, rather than abandoning what had proved so far, with the exception of TMI, to be a system of safe operation.

Ted Taylor turned out to be the gadfly of the commission. He claimed that it would be a favor to abandon all nuclear power. He said that in his backyard on Long Island he was conducting some experiments involving heating buckets of water with sunlight. He promised that within a few weeks, well before our report was due, his new invention would obsolete and replace all commercial power plants, whether operating on nuclear fuel, coal, or natural gas. Pat Hagerty and I had never heard of Taylor's invention, but we knew something about the problems of producing central-station electric power from solar energy. Yes, one can imagine enormous mirrors and lenses that focus the sun on a dark

target which becomes very hot and can heat water to the high temperatures and pressures needed to drive turbines to generate electricity. But a system to generate the billions of kilowatts typical of just one central station plant would be so far from anything reasonable and economical as to be preposterous. Taylor said that he had a secret substance that, when dissolved in water, would collect sunlight and do the job cheaper than has ever been accomplished in power-producing technology. We gently suggested that he bring us the results of his experiments, which he had promised would be within a few weeks. We never heard from him again on that subject. He spawned a few other claims of this sort.

Ann Trunk, the housewife from Middletown, Pennsylvania, proved to be one of the stars of the commission. She was quiet, aware of her lack of technical knowledge, but listened carefully and politely asked simple questions when she didn't understand. Her one-page personal supplementary statement was concisely stated, to the point, nonpolitical, and contributed much to the authority of our report.

In October our commission met with President Jimmy Carter in his cabinet meeting room to deliver our report. He listened politely but did not ask a single question. He showed little of the interest that I had expected--expected because he had also gone to Georgia Tech and then served in the nuclear navy under Admiral Rickover, where he became, as he called it, a "NEW-KEW-LAR" engineer. I have seen no evidence that he himself acted on any of our recommendations. He seemed more focused on having his photograph taken shaking hands with each of us as we lined up and lock-stepped out of the cabinet room.

But Congress did act. When we appeared before them they asked hard and pointed questions and were especially incensed about the many inadequacies of the NRC, which had contributed so much to the accident occurring and which had mistakenly escalated the fear of the accident into a near disaster. They called for a major revamping of NRC.

An interesting incident happened with my old friend Hans Mark during the TMI investigation. Hans was then Secretary of the Air Force. He called one evening and asked to meet me at a quiet restaurant nearby. Instead of being chauffeured in his elegant limousine, he arrived in an old beat-up Chevrolet, which he said he used when he was attending to personal investigations on his own. He had read news accounts of proposals being made by some commissioners that TMI should lead to a moratorium on

nuclear power in the U.S. He said that he was concerned because without nuclear power much more oil would be required to generate electricity and there would be less petroleum products available to propel his airplanes. It sounded far-fetched, because little oil is used to generate electricity; coal would be the alternative to nuclear, not oil. I think that simply his political antenna was alerted, and he wanted to find some way to become involved in the politics of the commission study. But he persisted. He said that for national security interests, I should talk immediately to Carter's national security advisor, Zbigniew Brezinsky. It had to be done that evening. I was curious, so I agreed to go along with it.

Hans explained that, by protocol, he was not free to contact Brezinsky directly. It had to be done by one of the several air force generals who were his aides. We drove to the Pentagon. Hans' office was an enormous cavern, guarded by a battery of MP's. His desk was flanked by flags of the nation and air force, and countless models of military aircraft were suspended from the ceiling. Hans directed his sergeant to telephone one of his generals, who was supposed to be at Hans' beck and call in case of national emergency. He planned to direct the general to contact Brezinsky and arrange a late-evening meeting. The general's wife answered and said that the general had been playing late-afternoon golf and was uncertain when he would return. This was before the days of cellular phones. Hans seemed a little embarrassed, but he then directed a call to the other general on call. That general was out for an evening at the officer's club. Hans was a little flushed. He ruefully admitted that an air force secretary doesn't have the necessary clout in Washington. Hans said that he would arrange the meeting for the next day. I never heard from him again during the TMI investigation.

The report of the TMI Commission was well received by government and industry. It had handled the politics carefully. It recommended [a] revamping of the Nuclear Regulatory Commission. It pointed out how a well-intentioned utility company, the General Public Utilities that owned TMI, had vastly underestimated or completely abrogated its responsibilities for safety management at all levels, and it pointed out that the entire nuclear industry in the U.S. was extremely vulnerable to mistakes of one of its member companies and to inadequacies of the national system of assuring and regulating nuclear safety. This led the industries to create their own Institute of Nuclear Power Operations--of which I was its first advisory council

chairman--discussed elsewhere in this oral history.]19

Wilmsen: What effects did the accident have on your research interests and on teaching?

Pigford: Well, I had to give up teaching. The chancellor allowed me to give up all of my administrative duties, and teaching as well, for six months, I think it was. The commission was appointed for a six-month term.

On the research, I couldn't drop the research. It was blossoming so much, but I turned over the preparation of our reports to a team consisting of Professor Chambré and two or three of the postdoctoral research scholars who were on the team, and they did a fine job. I think I did continue actively in the research seminars when I was in town. I had to do a lot of traveling.

Wilmsen: What were the implications of the accident for industry?

Pigford: Well, quite profound. First, the industry quickly realized that this would give an enormous impetus--I say deservedly, they would say inevitably, maybe not deservedly--to question and challenge the safety of nuclear power.

A more powerful reaction in industry was the demonstration that the industry might not be able to survive another accident. And they don't mean just the industry that owns the reactor, but the whole industry, because the whole industry is deeply affected by poor performance in any one of the plants.

It was especially important because the utility that owned the plant had to purchase electric energy from other suppliers outside its network to supply its customers. It is obligated to maintain a supply. That's very expensive. It nearly went bankrupt in doing that, and I think that was the most shocking thing to all the other utilities.

So their [the other utilities'] reaction is first they contributed their own money to help the utility survive--the owner of the one that had the accident. Then they stepped in with a very powerful response.

They had learned that the accident occurred because of human

error, not of any technical deficiency in the safety systems. That reflected upon the management, the training of operators, the training of engineers, training of technicians. When that training is faulty, it reflects on the plant manager and finally all the way up through to the chief executive officer of the company. So the industry--those utilities that own and operate nuclear power plants--formed a new safety agency of their own, separate from the government.

They call it an institute. First they formed a new insurance system such that each utility with a nuclear power plant could purchase insurance against a situation in which it had to purchase electrical energy from an outside source to deliver to its customers. That insurance company was set up, and then this institute that was set up--whose home was in Atlanta, Georgia--had the function of setting its own standards for management and training and operation. It would inspect periodically each utility and each reactor station to ensure that those standards were met. And if they were not met, the owner of that reactor could not qualify for the insurance against loss of power. So it had teeth in it.

I know it very well because I was asked to be chairman of the advisory council for this new group--which is called INPO, Institute of Nuclear Power Operations--and I was very much impressed by the intent and seriousness of all the utility companies with nuclear power plants throughout the country. They would send their chief--CEO--to meetings of INPO to show their intent at the highest level.

And INPO was tough. It was headed by a recently retired navy admiral who had been in Admiral Rickover's fleet of nuclear ships and submarines. This was Admiral Eugene Wilkinson. He was the skipper of the first nuclear submarine, the *Nautilus*. And our advisory council would review their work quarterly and see how INPO was doing.

Then I was consulting separately for a couple of utility companies and I could see how they turned to when the INPO inspection team came there and inspected their operations. Turned out to be a very potent group.

It caused a lot of expense to the utilities. They had to beef up their training staff and so forth. Even their responses to questions from the Nuclear Regulatory Commission were reviewed by the INPO investigating teams, and it developed a good record. There were very few instances of similar breakdowns in operative

performance. Not just operators, I know INPO did arrange to get one CEO dismissed because they felt that his company was not taking seriously their commitment. I think INPO was communicating with the stockholders and the board of directors and got the CEO canned. That operation turned out to be very effective. It has an excellent record. At least there haven't been anymore accidents of that [Three Mile Island] magnitude in the U.S.

INPO was copied by other countries, who first could subscribe as observers. The utilities in other countries could subscribe as observers to the INPO operations and then later they set up a world organization modeled after INPO with headquarters in London and Tokyo. So that illustrates the reaction of the utility industry.

Wilmsen: INPO dealt primarily with management issues?

Pigford: How to administer the plant. They would even review the CEO himself. They would ask if he knew about this incident and how his staff was responding to it. They expected him to be involved.

I guess that's the navy approach towards management, which has a good record in the fleet. Of course they lost two submarines, but except for those, it's a good record.

Wilmsen: Were there any results of the Three Mile Island accident with regard to changes in reactor design or safety features?

Pigford: Oh, sure. The Nuclear Regulatory Commission had its own team. First they had to correct their own operations. See, they set up INPO shortly after our commission report went in, and the commission I was on was disbanded. So I went from the commission to being head of the advisory council of INPO, but that was just a consulting job. Immediately, training was challenged. And now, after that, every plant has its own simulator which is a full scale control room with the exact replica of all switches and knobs and displays. It runs a computer instead of running the reactor, and the computer can be programmed to simulate all sorts of design conditions including some pretty bad accidents.

One of the inadequacies our commission found at Three Mile Island was that those people had been trained not on an exact replica. They had even been trained on systems that were quite different from their own reactor. Some of the operators apparently got confused on which knob does what and so forth. So

that's one of the examples: the training simulators are required.

Management Blunders in the Construction of the Diablo Canyon Nuclear Power Plant

Wilmsen: Now in 1981 Nunzio Palladino, who was Reagan's appointee to chair the Nuclear Regulatory Commission, told the House subcommittee that the problems at Diablo Canyon had lessened his confidence in the licensing process. What kind of effect, if any, did the problems at Diablo have on your assessment of the licensing process?

Pigford: [Let me begin by describing my first interaction with the Diablo Canyon project.²⁰ In 1967 I was appointed by the Atomic Energy Commission [AEC] to serve on a three-member Atomic Safety and Licensing Board to review the license application to construct Diablo Canyon. Congress set up these boards in 1963. I was the first person appointed to the national panel from which these boards are drawn. I had served on several such boards before being appointed to the Diablo Canyon board. Nowadays the members of the national panel are officially designated as Administrative Law Judges.

By the time the application reached the licensing board, it had already been reviewed and judged adequate by the staff of the AEC. Our board reviewed the piles of safety analysis reports compiled by Pacific Gas and Electric [PG&E]. There was an enormous number of reports from PG&E consultants on geology and seismicity, testifying to the adequacy of the site and adequacy of the seismic design. There were several complaints from nearby landowners, fishermen, and other people potentially affected by the plant. The board conducted public hearings, ostensibly to resolve these disputes.

The hearing was held at nearby San Luis Obispo. PG&E

²⁰Professor Pigford wrote this description of his involvement with issues concerning the Diablo Canyon Nuclear Power Plant during his review of the draft transcript. This insert replaces the portions of the interview transcript in which these issues, including Mr. Vrana's objection to the power plant (originally on page 106), were discussed.

brought a large battery of legal counsel. There were several lawyers from the AEC. The intervenors had no lawyers. One intervenor was a physics teacher from Santa Barbara, a Mr. Vrana.

He was also an amateur geologist who had devoted much of his spare time to tramping over the coastal lands near the proposed Diablo Canyon site. He claimed that the trenching ordered by the PG&E geologists actually revealed possibilities of previously undiscovered earthquake faults that were not taken into account in the seismic analysis. He requested the board to extend the hearing for a few hours so that he could present his case. Lawyers from PG&E vigorously objected.

I requested the board chairman, a Washington lawyer named Gleason, to extend the hearing. The board was charged with giving the public a chance to express their case, and Mr. Vrana's concern was relevant to one of the most important issues of reactor siting. Chairman Gleason overruled me on the grounds that Mr. Vrana did not have the credentials to make his case. I later learned that Gleason's overrule was motivated by his hurry to return to Washington, where he was in a hot political campaign for election as County Manager of Montgomery County in Maryland, a powerful county just north of the District of Columbia. Gleason was one of those lawyers in private practice who served as consultants to the AEC and were appointed to chair the Atomic Safety and Licensing Boards.

Also, during the hearing, I learned from the AEC staff that some radioactive iodine would be released to the environment in plant cooling water. To assess safety, they assumed that people might drink some of that water that would flow off the site. They calculated the potential iodine dose assuming that only adults would drink the water. The potential effect is for iodine to metabolize to the thyroid, where it resides and decays, possibly causing thyroid carcinoma. The AEC staff claimed that the iodine concentration would be low enough to be safe.

But years previously, national and international bodies on radiation protection had established that in the case of iodine assimilation the dose should be calculated assuming infants drink the contaminated water or drink milk from cows that have grazed on grass contaminated with radioactive iodine. The important difference is that the thyroid of the infant is about twenty times smaller than that of an adult, so radioactive iodine reaching the infant's thyroid becomes more concentrated and has a far greater chance of inducing cancer. Also, thyroid carcinoma in an infant is more life threatening than it is for adults. I requested PG&E and the AEC staff to explain why they had ignored

this in their calculations. No explanation was given. I concluded that the safety analysis did not, in this respect, meet the government standards for safety. Gleason urged that we look into it later, but no explanation was heard from PG&E and AEC.

Gleason hurried back to his political campaign, after scheduling a series of telephone conference calls with me and Hugh Paxton, the other technical member of the board, to formulate the board's decision. Meanwhile, I was much concerned about the failure in our hearing to address these issues. I consulted a member of the law faculty at Boalt Hall, Berkeley's law school. Clearly, part of my concern was failure to give due process to Mr. Vrana to make his case. This was not a technical issue, but I was determined to pursue it. The law professor, who later became a member of the California Supreme Court, pulled a thick law book from his shelf, opened it to an appropriate page, and suggested that I read it. It was all about responsibilities for due process. He said that he could not advise me, but that the book might be helpful. From this I learned to couch my concern in legal language, which became part of my ultimate dissent.

My two colleagues on the board seemed surprised that I would tie up PG&E's project with such mundane issues. When the chairman finally sent me a draft of the board decision to sign, I simply returned it unsigned, accompanied with my written dissent.

It caused considerable uproar throughout the country. A stalwart member of the nuclear engineering community had let them down!

PG&E appealed my decision to the next higher legal authority, which was the Commissioners of the AEC sitting together as an appellate court. In their great wisdom, the Commissioners found that ignoring Mr. Vrana's plea was "harmless error". Also, by that time the AEC staff had been stimulated to redo their calculations of iodine dose. They requested some design modification and concluded that my concern about radioactive iodine was now satisfied.

Thus, PG&E received the AEC license to construct Diablo. In retrospect, as will be revealed here in subsequent discussion, a previously unknown earthquake fault near Diablo Canyon was later discovered, although it may not have been connected with the fault that Mr. Vrana had suspected. It led to expensive reconstruction. It showed that the original seismic investigation was inadequate.

Board Chairman James Gleason was not elected as County Manager.

Question: Who discovered the new earthquake fault?

Pigford: After about five years of construction, when Diablo was nearing completion and several billion dollars had been invested, a scientist at the U.S. Geological Survey had learned of some seismic data gathered by the Shell Development Company in connection with prospecting for undersea petroleum reserves offshore of southern California. The data had been gathered many years before the Diablo project was conceived. The scientist informed the AEC. The AEC concluded that Diablo, as now constructed, could not withstand the greater shaking that could result from the newly discovered offshore fault. Much stronger supports for the reactor equipment would be required. Much of the plant would have to be rebuilt.

Then PG&E and its contractors made a bad design mistake that resulted in improper installation of the new earthquake supports. This was discovered several years after the first reconstruction began.

Question: What happened?

Pigford: Diablo Canyon is an installation of two nuclear plants. As the new earthquake supports were being installed, workmen discovered that in one of the plants the heavy equipment components did not fit the earthquake supports. The reactor equipment could not be installed within the reactor building. The infamous "mirror-image problem" had been discovered! Again, construction was halted.

It happened this way. Like many electrical utilities, PG&E liked to have mirror-image installation of otherwise identical equipment in adjacent generating facilities. Typically, a control room would contain the control panels of two adjacent facilities. It looked real neat to have one control panel the mirror image of the other. In a cylindrical reactor building the reactor vessel is mounted in the center, and peripheral equipment such as pumps, valves, and heat exchangers, is arranged in the annular space between the reactor vessel and the reactor building. It presented another opportunity for a mirror-image layout, although there was no fundamental technical reason for doing so.

It was easy to design the mirror-image layout. PG&E did its

own plant engineering and design, but a seismic-design consulting firm named John Blume had the job of designing the layout for seismic supports in the reactor building. It was so simple. A design would be made for one of the units on transparent paper. The paper would be flipped over to create the mirror-image design of the second unit. Unfortunately, PG&E used two identical designs, not mirror-images, to install the equipment supports. However, the reactor equipment was fabricated with nozzles, pipes, and controls according to a mirror-image layout. Thus, in one of the Diablo plants the equipment did not fit the new seismic supports. In that plant the seismic supports and associated equipment had to be reinstalled before the plant could fit together. This was very expensive. For the second time, much of Diablo Canyon had to be reconstructed.

By that time, in the late seventies, federal responsibility for safety oversight of commercial nuclear facilities was in the hands of the Nuclear Regulatory Commission [NRC], which had been split off of the old AEC. The NRC staff had become disgusted with PG&E's poor management of Diablo Canyon engineering, its inadequate quality control, and its seeming carelessness in monitoring and documenting the design and construction. NRC ordered an unprecedented complete design and construction review of Diablo Canyon. PG&E was required to document and review essentially every design decision and construction action of the entire plant, to ensure that no other errors had occurred. It was a massive undertaking that required several years of a staff much larger than that required to design and construct the plant.

Meanwhile, PG&E was paying interest on its construction loans, adding billions to the ultimate cost of Diablo Canyon.

Diablo Canyon is an extreme example of why some nuclear plants have cost so much.

Question: What was your later connection with the Diablo Canyon project?

Pigford: In the mid-1980s I became a consultant to a special committee of the PG&E board of directors on a major litigation. It came about in this way. After the dual-reactor plant at Diablo Canyon was finally completed, PG&E applied to the California Public Utilities Commission [PUC] for permission to recover the cost of overruns at Diablo Canyon. It was a major issue, amounting to potential recovery of many billions of dollars. The issue was hotly contested, and that litigation took several years. PUC concluded that PG&E was not entitled to cost recovery because the overruns were due mainly to poor management. PG&E's board of directors was then advised by its own legal consultant that they,

as well as the PG&E management, were likely subject to substantial claims by stockholders on behalf of the company. The Board then appointed a special board committee, consisting of those directors who had not been directors during the period of questionable management and large cost overruns. The committee was empowered to investigate, on behalf of "the company," and take whatever action it deemed necessary. I suspect that the board never expected the committee to do anything.

The committee hired its own legal consultant, a large law firm named Heller Ehrman. Heller Ehrman then hired me as its technical consultant. My job was to review the entire record of Diablo Canyon over its some twenty-year history, beginning with site selection and continuing through construction and beginning of operation. My assigned task was to determine the truth of the matter, whether or not the PG&E management and its board had acted responsibly with regard to project management and control. From this, along with legal considerations, the committee could decide whether to seek damages on behalf of the company and its stockholders. This was a far different assignment than I have had in other cases as an expert witness for one of the parties in a litigation. There I was expected to testify in behalf of only one side of the litigation. That always troubled me. There is never only one side in terms of fault and responsibility. Here I was not so constrained.

Heller Ehrman collected all of the relevant documents, which filled a room in their offices on Post Street in San Francisco. I poured through the documents, sorting out the issues based on my experience with Atomic Safety and Licensing Boards. For help on management issues, I engaged my old friend, Ken Davis, who had recently retired as a senior vice president at Bechtel Corporation, a well-known engineering firm in San Francisco. It took several months. During the process I had to learn more about engineering design of equipment to resist seismic shaking.

I discovered a fundamental error made by one of PG&E's engineering consultants, a professor in civil engineering at Cal Berkeley. He misinterpreted one of the seismic design requirements established by the NRC. The mistake should have been recognized by PG&E's engineering staff.

I could not fault PG&E's reliance on its geological consultant, who failed to seek data from Shell Development that would have revealed the offshore earthquake fault. PG&E had hired one of the top notch consultants in the business, a professor of geology at Stanford, a man of international reputation. In hindsight, he should have been more thorough.

The mirror-image problem revealed numerous blunders by PG&E managers at so many levels. There was lack of timely review and follow through. PG&E had in place a management review process that would involve top management and the board in reviewing major decisions. The board minutes revealed little attention to what was happening. NRC requests for information and explanation were not carefully responded to.

I provided extensive documentation of the events for Heller Ehrman's review. Heller Ehrman passed it on to the board committee, which decided to proceed against management and the rest of the board on behalf of the company. It must have been a shock to some, who evidently expected little to come of the committee's investigation. Led by PG&E's new CEO, who had not been involved as manager during the Diablo mistakes, the current management and the board accepted full responsibility. Damages, to the extent of a few hundred million dollars, were paid by the PG&E's insurer. The funds were deposited in PG&E's treasury. Some of PG&E's top management retired early.]²¹

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Wilmsen: How did the problems at Diablo affect your assessment of the overall safety of nuclear power?

Pigford: Well, again, it was a shock in my confidence in the ability of the engineering companies to perform according to specification. And some of it spins off on the Nuclear Regulatory Commission. They were shaken.

Wilmsen: Then did INPO deal with those kinds of issues as well?

Pigford: Oh, yes, indeed.

Wilmsen: So things improved after that?

Pigford: Yes. The Nuclear Regulatory Commission influenced improvement and they had to also improve their own inspection. They had to improve their earthquake fault evaluation, which went back to the U.S. Geological Survey. And INPO certainly emphasized that in the plant inspections. In the follow up, how do you know it's built according to what you see on paper? It takes a lot of effort to find that out.

²¹End of inserted material.

[Interview 5: November 18, 1999] ##

Pigford: I also think that settlement [between the PG&E company and the management and board] was large enough that it resulted in being able to cover all of the stockholder lawsuits that were still coming in. I'm not positive about that because they came in later, after my part of the work was finished.

And that took a lot of my time. But, like so many things I get into, it was a worthwhile diversion and very interesting--interesting to see how big corporation executives act and respond.

Wilmsen: Did you mention whether they could recover the costs from the rate-payers?

Pigford: No, they could not. The PUC did not allow that.

Wilmsen: Yes, that's what I thought you had said.

Pigford: Now, what was recovered as damages from the investigation that my team was involved in was strictly malpractice insurance.

Wilmsen: Yes.

Pigford: How did PG&E finally recover the cost overruns to the plant? Well, they were given a special permission by the Public Utilities Commission. I don't know the proper terminology for this, but effectively they could treat the electricity production from Diablo Canyon as being outside of the normal control of PUC; it gave them the opportunity of wholesaling. They did a wonderful job--excellent record--of managing the plant, and it turns out they've made a lot of money off of it. They've more than recovered the costs.

They're now said to be considering selling the plant, but for an entirely different reason: because of the recent--within the past few years--court decision to deregulate the utilities that produce electricity. I don't understand the details of that completely, but it has become less attractive to utility companies to own high-capital-costs plants that are producing electricity.

So that's the story about the big case with the PG&E directors.

Reflections on Resolving Conflicts between the Nuclear Industry and Its Opponents

Wilmsen: Interesting. I guess this is somewhat related--not really, but it has to do with controversy, too. Another follow-up question from last time. I asked you about what you think could be done when the two sides disagree and they just can't get together, and you said that you thought that if people could really create an environment where they can actually sit down and really talk to each other with a good moderator, that would be one thing.

Pigford: Yes.

Wilmsen: And you said you've seen that happen. I was wondering if you could just describe an example of a case where you've seen that happen.

Pigford: Well, INPO [Institute of Nuclear Power Operations] and its function has many such cases. I've described INPO before.

Wilmsen: Yes.

Pigford: INPO is something the industry itself set up outside of the regulations and rules of the Nuclear Regulatory Commission. Unfortunately the Nuclear Regulatory Commission cannot do that because it has set up rules which are typical of federal regulations, and it must be done in a legal framework with exparte communication laws and all of that. INPO doesn't have that problem, and I think the weight of the INPO conclusions, which are published, as to whether a company is doing things properly or not is enormous--almost maybe more so than the weight of the conclusions by the Nuclear Regulatory Commission.

INPO can at any time ask the operating people or the management people that they leave their lawyers at home and get both sides of an issue in a room. And it takes a good moderator. They have skilled people, who are respected, to do that, and I think it has worked beautifully.

Now, to be sure, these are people who are not so polarized against INPO. Most people associate deep polarizations with people who are against big business or big corporations who are polluting the environment. I think most people I know are

environmentalists, actually, in big corporations and otherwise, but I won't dwell on that distinction since I've defined what I mean. Take, for example, the strong conflict between environmentalists like the Sierra Club and the utilities that build nuclear power plants. They become far more polarized than they have to be. Once it gets to the arena of a formalized hearing for decision processes under the purview of the Nuclear Regulatory Commission, they adopt those rules of formal legal communication. But they can always say just between themselves, "Let's get together for a weekend without the lawyers present talk to each other." I'm sorry to say it is seldom done.

It may be because of so many reasons. First, there's usually so much money at stake, I guess a construction permit, and I think also it's due to blindness on both sides. I think many environmental organizations I've worked with, and tried to work with, sometimes don't want the issues settled. For example, when we decided to decommission and remove our research reactor on the Berkeley campus, there were some who were very unhappy that it went away because it became a good whipping boy to show what they were against and to get their name in the newspapers. And likewise, you can see, surely, that the management of the utility companies don't enjoy face-to-face attacks from environmentalists. But real and sincere communication can be done. After all, that's the principle by which our former senator, George Mitchell, negotiated for the peace in Ireland. He was a well-known political figure, no longer in government, but quite experienced in trying to see both sides. He went there as a mediator without any authority and apparently that worked, or at least it's working better than it was before.

Wilmsen: Yes.

Pigford: And we see examples of that in Israel versus the Palestinians, and I think those techniques should be employed.

Wilmsen: What kind of work have you done with environmentalists?

Pigford: Oh, I've given a lot of them free consulting--the Sierra Club--and given many talks--invited talks--to their meetings. I guess that's one reason I was selected to serve on the commission on Three Mile Island.

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Pigford: The people in the White House were looking for somebody who wouldn't be strongly objected to, and--I understand they had

smoked out both sides on this issue. Many of the papers I have written have challenged the safety work of the utilities--the engineers and the regulators--on the safety of nuclear facilities--and that has appealed to environmentalists.

I guess I'm identified now as both a supporter of their Yucca Mountain project in Nevada to build high level waste repositories, because they had sponsored some of my research and they're using some of the theoretical methods that I and my colleagues and students developed to predict safety. Also I've found myself identified as a friend of one of the most active environmental organizations that's opposing the plant.

Wilmsen: Which one is that?

Pigford: The name is something like Citizens for Safe Energy. And also the staff of the State of Nevada--they have a technical staff and they're much opposed to the project. Each of these organizations--the project itself plus its opponents--have good points.

Wilmsen: I actually have questions on Yucca Mountain for later on.

Pigford: Of course there could be a conflict of interest if I'm helping both sides. And if it came into a legal situation, I would recuse myself, which I would be very happy to do, but I feel free to help each side on points that I think are worth helping them on. As long as they're trying to design a good repository for high level waste, I want to help them. As long as there is argument on what is the proper safety standard, I am very much against proposals for more lenient standards for Yucca Mountain.

Wilmsen: What does the Department of Energy propose?

Pigford: I just turned in over the weekend, at the request of the National Academies of Science and Engineering, my evaluation of the EPA-proposed safety standard. It has some terrible inadequacies. If adopted, it would in some respects allow more leniency with regard to protecting public health and safety than anything that we would allow today for a licensed nuclear facility. And in a couple of places that's true of the Nuclear Regulatory Commission, and in also the Department of Energy.

Wilmsen: Is this environmental group, though, in Nevada looking to build the best possible repository, or do they want to not have one at all in Nevada?

Pigford: I think they would say yes to both of those, but they are most vocal on not wanting that one in Nevada.

Wilmsen: Okay. Well, we can come back to Yucca Mountain.

Pigford: Okay.

Wilmsen: I wanted to cover some of your kind of advisory-committee-type activities and then get into your work on the geologic repositories: Yucca Mountain and WIPP, the Waste Isolation Pilot Project.

**Safety Reviews of the River Bend, Louisiana, and Rancho Seco, California
Nuclear Power Plants**

Wilmsen: You were on some advisory committees on nuclear reactor safety and operations. One was for the River Bend Nuclear Power Plant in Louisiana, and also the Sacramento Municipal Utilities District [SMUD].

Pigford: Yes.

Wilmsen: Can you describe what working on those advisory committees entailed?

Pigford: Well, the one at River Bend had a clear charter. We were a committee to report to the board of directors of the Gulf States Utility Company, which is--or was at that time--the owner of the River Bend Nuclear Plant. We were to meet periodically, like three or four times a year--first, before the plant was built, to review the plans, to review the progress towards construction, and mainly to review those issues that were related to safety of the public. Of course, practically all of the work is somehow related to public safety--the adequacy of the design itself for safety.

General Electric Company was the designer and supplier of the steam generating system, and we would meet with GE personnel to review their designs and see how they were getting along, and evaluating the many, many safety issues that come up.

We would report periodically to the board. The board, interestingly enough, did not want any written reports from us.

They wanted entirely verbal reports, and we were happy to comply.

I've seen this approach many times. I think it's because in the modern realm of discovery and the sunshine law, any member of the public can get most of the documents in any recurrent meetings like that, and they just didn't want to have these on record.

Then as the plant got built, our committee itself did ask for a few changes. Most of what we did was to see if what had been developed as the criteria for safety would be adequately met by the design. In some cases we didn't see they were adequately met, and this applies both to management as well as to physical systems.

In some cases we found that the company personnel in places were weak and needed better training. I do remember in one case we found that one of the managers of one of their departments was so weak they should transfer him to another plant that was nonnuclear. And they did. Maybe they just let him go, I'm not sure. So anything that had a connection with safety we got into.

Then as the plant ran, we would periodically review how training was done to new people coming into the work force: how their training was reassessed, or rejuvenated periodically.

And in any complex system like this, there are always new discoveries. Something goes wrong. Something wears out and has to be replaced. That's a safety issue.

Then the Nuclear Regulatory Commission itself has new questions that come up from other plants that they want to address for this plant, and so it was an active business just following that.

I eventually stopped consulting for them when--I guess it was a few years after I retired--the plant was bought out by another company which decided it wanted to have the business of operating nuclear plants throughout the country. Gulf State Utilities had only this one nuclear plant and didn't want another one because it was such a pain in the neck to manage with all of these reviews going on. So they sold it very happily and have stuck to producing electricity from natural gas.

Then with regard to Rancho Seco [owned and operated by SMUD], this was somewhat different. The plant had been running several years. The reactor was built by Babcock and Wilcox, an engineering company like Bechtel. In fact, Babcock and Wilcox also was the company that built Three Mile Island.

The plant ran fairly well, but it kept getting into problems which I think were mainly management problems. I did not yet have any connection with it, and it got on the list of badly run plants published by the Nuclear Regulatory Commission. That is a list that people don't want to be on because it affects their credit rating, their ability to raise money by floating bonds.

Rancho Seco is the kind of organization that never should have built a nuclear power plant. It's a small utility, like a co-op, not privately owned, not owned by the state, just officially a publicly-owned utility. I don't think they made much electricity in any other way even; they may have made a little. They weren't really able to cope with the complexity of management--dealing with regulations and so forth--and after some bitter findings, the Nuclear Regulatory Commission required them to be shut down for a year, which cost them a terribly large loss of money to buy replacement power.

Then the board of directors of this utility had been entirely different than the board of PG&E. These directors run for office, get elected by the people in the vicinity: by housewives, dairy farmers, so forth and so on. That's not to disparage those people. It's important to have people from the public, but they were without any professional experience in management. So they thought they could make up for that by hiring a well-paid employee to manage the plant.

Well, plants aren't that easy to manage. It has to have the board behind it. The board officially was responsible, tried to do its job, and it nit-picked on things that were the wrong things to worry about. It got into a terrible management problem; so then there was a referendum initiated by the people in the vicinity that they shut down the plant permanently. It got modified to say that they should have a special study by an independent engineering organization be completed in one year and then have a special committee review the results of that study to advise the board of directors whether they should abandon the plant completely. And I was one of the people selected to serve on that special committee, which was a review committee.

We reviewed it for one year, and the special engineering firm which was evaluating what had to be done to get the plant started up didn't come out with a glowing report; it didn't come out with a terribly negative report, either. I think it wanted to get a contract to continue its evaluation. As I recall, I think our committee concluded that we knew enough. But could the committee reach a conclusion?

It turned out it couldn't. So we each wrote our own reports, and I wrote my report concluding that the operating utility, which is SMUD, had no business running the nuclear plant and they should try and sell it. It was already shut down. Well, that completed my involvement.

It's interesting. They couldn't sell it. There were some offers but they had so many contingencies in them, they weren't really attractive. So the plant is still officially owned by that utility district.

And it's not decommissioned. They don't have enough money to decommission it, so there it is. It's not running, anyway.

Wilmsen: And so it just sits there?

Pigford: Yes. And all of its radioactive fuel is still there--spent fuel.

Wilmsen: They must have some costs just in keeping it cool.

Pigford: Oh, sure. They have to even have a guard force to prevent people from stealing the spent fuel and extracting plutonium. They are legally obligated, and it's an expensive legacy.

Wilmsen: Yes.

Pigford: Now, they can not decommission it until they have a place to send their spent fuel. In that article Carter and I wrote about getting Yucca Mountain right, we strongly urged that there be a national interim storage facility for purposes just like that. And many utilities--some of them running out of storage capacity on site for spent fuel--need that.

The SP-100 Project: Nuclear Power for Space Applications, 1986-1990

Wilmsen: Then in the late 1980s, you were a member of General Electric's advisory committee on nuclear safety for the government-sponsored SP-100 project to develop nuclear power for space applications.

Pigford: Oh, yes. This was not mainly a safety committee, although safety is always part of any design development in the nuclear field.

GE had this contract with the Department of Energy--and some of the finances came from NASA--to develop a functioning nuclear reactor in space. The view from the sponsoring agency was that devices in space were getting so popular that at some power level--each unit has its own power source in space--they were going to need something better than batteries or photocells; they needed something in the hundreds of kilowatt range, which is a rather small nuclear power plant.

It couldn't begin to be economical, but economics were not the main criterion. They wanted something that could fly into space and be reliable for decades, possibly even to power manned trips to Mars, or outlying planets. This required much heavier vehicles than those that had gone to those places already. And so it was, from a technical point of view, a fascinating project.

I remember one of the problems I learned--one that I had never encountered before--arises from the fact that there's a lot of debris in space circulating around. It's partly from abandoned vehicles--satellites--some of which had partly burned up. Some of them are from normal space debris, like meteorites and other debris in space. Some of them are tiny specks of dust, some of them like grains of sand, some of them even like marbles or a hammer. Even the grains of sand, if they impinge upon this nuclear power system, can punch a hole in the metal structure and cause the coolant to leak out.

This was designed with molten lithium metal which is, from a heat-transfer point of view, the most effective coolant we know about. If it leaks out, it would be lost to space and the reactor might not be cooled anymore. It would have to shut itself down. It's not a safety issue until the reactor finally reaches the atmosphere. Even if you don't have such a projectile causing loss of coolant, the system has to be designed so that ultimately when the reactor reenters the atmosphere, it's protected from overheating and can be collected and stored so it does not contaminate the environment. That was mainly where safety came in.

Everything about that reactor was new and different. And it was a lot of fun to work on.

Wilmsen: Now, we talked a little bit before about the problems with designing a nuclear-propelled aircraft and those kinds of things, so what made it seem feasible to design some kind of nuclear-powered spacecraft at this point in time?

Pigford: Because it had a different objective. The objective of the nuclear-propelled airplane was to make it possible for a full-scale bomber airplane to cruise in air for long distances without refueling. It's nemesis was the development of the chemically-fuelled rocket which could deliver a payload to Russia and didn't have to cruise with people, because the nuclear airplane was to be a bomber and it was there to be able to bomb somebody. The Intercontinental Ballistic Missile [ICBM] could deliver a bomb any place on Earth and it worked.

Now the SP-100's mission was to supply an amount of power that could not be satisfied any other way. It's really a small amount, but that amount was larger than any that could be feasibly developed for a given device by other means--other means being solar cells, photovoltaics, or radioactive decay where you put some plutonium into a device and its heat from decaying generates electricity, not a reactor. Oh, you could build larger photocells, but finally the size of the array becomes so great, and the maintenance and even deploying it just becomes out of sight.

So we had a mission, and one can always question whether it was valid, but assuming that you need that amount of electrical energy on a device in space, I would still say that a nuclear reactor is about the only way I know of doing it.

Wilmsen: But you still have to overcome the problem of aborting rockets taking off with a nuclear payload.

Pigford: Yes, but the new reactor is not radioactive until it's run. So you get it into space in a stable orbit, then you turn it on.

Wilmsen: Then you turn it on. I see.

Pigford: So the rocket-abort, I think, could be satisfied, that is, environmentally. Then, the real issue is once it becomes highly radioactive by having run, at the end of its life--and every device has an end of life--either you shoot it into the sun, which could be done by sending up a chemical rocket to do that, or else let it reenter the atmosphere. And the techniques for preventing space vehicles from burning up when they reenter are valid, and they would have done that for this device.

The project moved along. In fact, they were about to construct a land-based test of it up in Hanford, Washington, but this was in the era when Congress was taking very tough stands on projects, and NASA didn't have a good defense as to when it

needed this plant. So Congress cut it out. And it was a very expensive project.

It was fun to work on, but on the other hand, a true scientist and engineer doesn't like to work on those things unless he believes in them. And I must say at the start of the project I know the GE team believed in it, and it was exciting. My only trepidation was that NASA had not identified a clear mission. It was something on their long-term future books, and those are the kind of things that tend to fail when you get the red-ink process in the government. Maybe rightly so, I don't know.

Investigating the Chernobyl Reactor Accident, 1986: Implications for the N Reactor at Hanford, Washington, and Reactors in the Former Soviet Union

Wilmsen: Okay. Tell me about serving on the expert consulting group to evaluate the Chernobyl accident in 1986.

Pigford: The secretary of the Department of Energy saw the very implications, that even Congress was immediately asking: do we have any reactors like Chernobyl? If we do, we should get into that and investigate very clearly whether they are susceptible to that kind of accident.

The quick response from the nuclear power industry was, "No, we don't, because ours are water-cooled reactors. They differ in important details from that Chernobyl reactor." And they were right. They could sustain that argument, but the trouble is, we had a reactor at Hanford, Washington, that was built in the 1960s when Glenn Seaborg was chairman of the Atomic Energy Commission.

It was in the days of the cold war when our military wanted more plutonium production.

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Pigford: The N Reactor at Hanford is the one that was similar in many ways to this Chernobyl reactor. Both of them had graphite to moderate the energies of neutrons, to make them critical. Then both of them used a lot of cooling of uranium fuel rods to generate power.

Now how did this N Reactor come about? In that era--still the cold war--the military in the United States said it needed much more production of plutonium to make more bombs. And that was at a time when producing power from nuclear power reactors was considered to be one of the best new things around, and so the Department of Energy [DOE]--it was AEC at that time--decided that it would combine the two and make a plutonium-producing power reactor at Hanford. It would build and own the reactor, and it would officially sell the steam to a utility company, which would put a turbine and condenser right near the reactor to generate electricity which it would distribute to its customers. And it ran successfully. And it was running even in the era of Chernobyl, which was around 1986, I think.

So our committee, or council, or whatever it was called, was asked by the secretary of energy to report directly to him. He had already set up large technical task groups at DOE laboratories--Oak Ridge, and Oregon, Idaho--to do their own evaluation of what really happened at Chernobyl. We were to look at those results, and we had to learn ourselves what happened at Chernobyl and then report on any other problems of a similar nature with the N Reactor.

I'll tell you the end result of our report. Again, it was not a report by the committee itself, but a report by individuals of the committee. The secretary claimed he could get the true essence of what each person felt that way. I think also it's a way of diluting the results in whatever way you want, because you can quote the ones you like and play down the ones you don't like.

And the final result: one member of the committee recommended shutting down the N Reactor immediately, but frankly for reasons other than the Chernobyl accident. I recommended that they change some of their practices. It seemed unlikely that the N Reactor could get into the same operational mode that caused the Chernobyl accident--which in the long run was operator error--but I found so many safety problems anyway with the N Reactor, that I recommended that those be solved before they start it up again.

Now that is skipping the details of what we did. It was a fascinating study, especially the part to learn about what happened at Chernobyl.

We went to Los Alamos, which has the privilege of controlling or directing the use of some of the spy satellites

because Los Alamos is partly concerned with arms control. We had a spy satellite, not hovering, but going across Russia several times a day. And when it crossed over Chernobyl, it would measure the temperature of the cooling water and temperature of the structure in various places of the damaged reactor, so we could even look at its temperature profile. We could see even people standing out on the road nearby. In those days we weren't allowed to say what the sensitivity is of the spy satellites, but it is quite remarkable.

I'm not sure we learned anything that helped us, but at least it helped us get a feeling for the Chernobyl accident. We learned more about what really happened by talking to the physicists and engineers at the various government laboratories who were deeply involved in studying that and had learned a lot. So we knew, we thought, what caused the Chernobyl accident to happen.

It was a design itself that was flawed. It's a property of graphite-moderated reactors that are cooled with water to make power that if you get an overheating problem, some fuel gets too hot. And it boils the water coolant. That water (just regular water) has been mainly a neutron absorber, a poison--and so, if you boil some of it away, you tend to go supercritical, and that makes the power of the reactor shoot up rapidly. Well, it caused a steam explosion which was of enormous magnitude. It lifted the top cover of the reactor container, which was about forty feet in diameter and five or six feet thick, and blew it off the building.

Wilmsen: Wow.

Pigford: Air got in and burned the fuel, burned the graphite, so it was not really a nuclear explosion, although it was initiated by nuclear overheating. But the results of all of that were catastrophic.

Well, in principle, that could happen to the N Reactor. Why did the Chernobyl reactor do that? It does that when you're operating under very low-power conditions. When you're generating a lot of power where there's coolant circulating, it can take away the extra heat that is being generated pretty easily. It was early in the morning at Chernobyl, after midnight, and two operators were out on the turbine building to calibrate some equipment. They asked the reactor control building to put the reactor in that low-power mode because they needed to do a careful calculation, and the operators of the

reactor should have known better. They should try to stay away from that particular mode. But they didn't, and so the people out on the turbine building ran their experiment, which triggered off the reactivity transient, making more neutrons, more fission, and the reactor took off.

Well, the errors were in the training of the people out on the turbine building who were doing the experiment, and on the part of the people running the reactor. Both of them made terrible errors. We concluded it was possible for that to happen with the N Reactor, but they had a much better training program, and the N Reactor was not quite as sensitive to that particular mode of operation. You could still get into trouble.

So on the whole, we found the training seemed adequate and we concluded that their experience over the twenty some odd years of running showed that they knew how to avoid that region of criticality very well. But we went further. I think walking through the plant, evaluating it, learning its design, you have an opportunity to see other things, and I wrote a very long report designating certain places where the operation and the design needed to be improved on other safety reasons, not the Chernobyl one.

The secretary of energy's first response was the N Reactor's needed; he's gotten support from the consultants to keep it running, except for one consultant named Lou Roddis.

But a few months later, three or four months later, DOE decided to shut it down anyway, shut it down forever.

Wilmsen: Why was that?

Pigford: They were approaching a major overhaul. They had to replace a lot of the graphite which over the years tends to change physical dimensions by the interaction of neutrons with it. Neutrons displace carbon atoms from the lattice of graphite, causes it to grow in one direction. That's caused Wigner disease. Eugene Wigner, a Nobel prize winner, identified that mechanism. In fact, he identified that when they were designing the first graphite reactors they had during the war. So after a couple of decades of operation, you have to take the graphite blocks out and put in new ones. And it can be done. It costs a lot of money.

Then there are coolant tubes which are made out of a Zircaloy metal that would also have to be replaced, and that's

also expected after twenty years. Those two replacements are so expensive that they decided to forego any further operation, and they didn't have to do any of the other changes that I recommended.

Wilmsen: Now, did the Chernobyl accident have implications for safety at any other reactors in the United States, or just the N Reactor?

Pigford: Well, only broadly. It's again an illustration that human operators can overcome safety systems and get the reactor in trouble. We illustrated that first for Three Mile Island. Three Mile Island occurred earlier and engendered a great focus on better training, which has continued. And the Chernobyl accident further strengthened that.

It caused enormous increase in the protest against nuclear safety in this country by the protestors, and with good reason, because Russia had been claiming their reactors were safe. Well, they weren't.

Already Russia was a party to the WANO [World Association of Nuclear Operators] which was a worldwide enlargement from INPO that was so successful in this country. And INPO had been formed after Three Mile Island with a great emphasis on careful management. Russia claimed it was deeply involved in careful management, and yet Chernobyl happened. So it shows rightly that careful management has to be given more attention than people sometimes give it.

Well, there are further implications. Chernobyl was not in Russia, it was in the Ukraine. There were four such reactors at one station in the Ukraine. One is still running. There was one in Lithuania, and I forget the other non-Russian Soviet countries. There were several in Russia. Well, now, those that were in the satellite countries, like the Ukraine, and Lithuania, the Soviets assured these countries that those reactors were under the overall management centralized in Russia under famous institutes like the Kurchatov Institute [Institute of Atomic Energy] in Moscow, which is very professional in reactor design.

I think there were a total of maybe forty institutes in Moscow that in one way or another had some responsibility on Chernobyl, in designing it or operating it, something like that.

Well, now that the Soviet Union is no more, Russia does not supply these services to these other countries, and here, Lithuania, which doesn't have a nuclear safety commission of its own or didn't at that time, has to take full responsibility for

the operation. I know that they wish they didn't have it, but they have it, and it's turning out a lot of electricity. The nearby countries--even Sweden across the Baltic from them and Latvia and so forth--are very worried because Chernobyl showed that the environmental effects can cover enormous distances.

I did, many years later, visit the Ukraine in connection with an arms control visit, but I was pleased to find that they had set up their own nuclear regulatory commission which looks like it's doing a pretty good job.

But all of the reactors like Chernobyl, wherever they are, were supposed to have big technological fixes, better instrumentation, and so forth, as well as better training. It took lots of money and the World Bank got involved. Now Ukraine is under lots of pressure to shut down its other Chernobyl reactors and it says it doesn't have the money to do so. It intends to continue running them, even though people throughout Europe really want them shut down, until somebody provides the money to decommission them. And that's very expensive.

And the Chernobyl reactor that had the accident is sitting there. They hastily enclosed it in a sarcophagus of concrete. But it was done so hastily, and the foundation is rather weak, so that's cracking and sinking. And all of that radioactivity--most of it--is still in the reactor. So this is a long-term legacy.

Wilmsen: They'll have to go back and redo the casing?

Pigford: Yes.

Member of the Delegation to the Soviet Union for Cooperation on Nuclear Reactor Safety, 1988

Wilmsen: Tell me about establishing a program of reactor safety in cooperation with the Soviet Union, which came a couple of years after Chernobyl, I guess. You were a member of the delegation from the U.S. National Academies of Science and Engineering.

Pigford: Officially the Soviets--and they were still the Soviet Union at that time--wanted to do more and more to develop a better program of reactor safety. This was partly because of all of the RBMK--the Chernobyl-type reactors--that were in the Soviet Union, and

partly because people were saying, "If that's the way they operate reactors, their light-water reactors"--which is the type we and Western Europe have--"should also be examined because you can expect problems with those too." That was a very legitimate argument. It was also partly, talking about the Soviet motivation, to try to erase the enormous damage to their reputation in terms of nuclear energy.

So their National Academy of Science, which is a very prestigious organization in Russia, invited our academies of science and engineering to send a delegation there to meet with them and for a delegation also to meet with the various institutes mainly in Moscow to exchange technical views on reactor design and reactor safety.

I was one of those who went along; I think there were five or six people. It was a wonderful trip because being guests of the Russian Academy of Science is the very best of all possible ways to enter Russia. We didn't have to go through the long passport control lines at the airport. They picked out our luggage immediately, they put us up in a fine hotel, and gave us a car with a driver. At the airport, the representative of their academy gave each of us a large paper grocery bag full of rubles.

He pointed out that the ruble was not allowed to be on international exchange. That's the Russian Soviet policy and he knew that we wanted to buy things, eat in restaurants, and so forth. They said you cannot use American money, it's against the law. We didn't know how much rubles were in each bag; he said, "Spend whatever you like. You can buy souvenirs, but when it's time to leave, just give us the rest back because you're not allowed to take it out of the country;" so we were having fun.

It was obviously a lot of money. We didn't even know how to count the amount in terms of real value. We found that we could eat at the best restaurants, and we had the best seats in the opera and the ballet and the circus and so forth. We still weren't spending very much money, and I felt we weren't really taking advantage of the opportunity. So I asked the driver, who was our guide and chauffeur--I'd already bought presents for my children and grandchildren and I didn't have room to take home any more--I asked him what could I buy that is small and expensive.

There is a famous gem in Russia which may be Alexandrite, which for some reason they've never allowed to be exported, but he told us not even the Academy of Science could get permission for you to take that gem home, so it became a challenge: what's

expensive and small. Finally we hit upon caviar.

Now Gorbachev had just become the premier, and he was trying to discourage the Russians from drinking alcohol, which was a failure, and discouraging them from spending so much money, because they were strapped for money, on things like vodka and caviar. So there was not much caviar available. Finally we found at a very exclusive restaurant, the chef had a partly open tin that he was guarding and that was very expensive. I think it would have cost several thousand dollars. And my guide almost convinced him to sell it to us, but then he learned that the guide had been employed by the prestigious National Academy, which he didn't trust, and so I came home without the caviar and had to give all of my rubles back.

Wilmsen: Oh. [laughs]

Pigford: In fact, I almost became even richer because in 1958 when Benedict and I published the first copy of our book, a textbook on nuclear chemical engineering, it was translated into Russian without our permission. The Soviet Union did not obey the international copyright laws, so they copied whatever they liked. From a Russian graduate student at MIT, Benedict saw the copy, and we each wanted to get one for our own personal collection. I tried. I applied to the Soviet consulate in San Francisco, and they told me that it was not available. We learned about this shortly before I went to the Soviet Union, so that was about twenty years after the translation. By that time it was not being sold in the bookstores, but the consulate told me, he said, "There's money for you there, probably, because the practice has been when they translate a book, they'll set up a royalty fee account for the authors, and it's in their name at a bank in Moscow. They can go there and get their money and spend it."

So when I went there in 1988, I asked my friend at the Russian academy if he could tell me how to get hold of my bank account, and he had an assistant look into it and he said, "Those bank accounts were just abolished two years ago." And he said, "If you'd gotten it, you would have been required to spend it completely in Russia." And I told him I'd already had more money in that bag or rubles than I could spend. [laughter] That was the end of that project. And really I would have paid a lot of money for a copy of that book. It was on flimsy tissue paper and just a rare thing. I never got one.

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Pigford: Back to the mission: I have not yet talked much about what we did substantively. Mainly we were entertained beautifully, elegantly, expensively, but we did meet with the various Moscow institutes--Kurchatov is the main one.

I found in those institutes reactor designers and physicists who wanted to take this opportunity to design entirely new types of reactors. They were not much concerned with the Chernobyl reactor, or even the newer light-water reactors. They were excited about some reactors of the type that we had put on the shelf years ago that I talked about earlier because they just had problems. I was amazed at their lack of understanding and knowledge about the whole history of reactor development in other countries.

They wanted to set up cooperative programs. Mainly they wanted to find a way of getting American PCs, which were not allowed to be sold in Russia at that time, and they would give us whatever they thought we would like.

They had some interesting new instruments that we thought could be imported into the United States, but that never got off the ground. The State Department was not involved, nor was the Department of Commerce. We passed along the information, but nothing happened.

Wilmsen: What kind of instruments?

Pigford: Instruments to measure noise, the vibration. Each reactor has a noise meter because if you find a vibrating part, it means something's torn and vibrating, or it might even be a loose piece like the thing that shut down and melted the Fermi-1 reactor. There's a lot of technology that's been developed for noise analysis, and even by measuring the spectra of the noise, one can frequently tell what's happening. And the Russians are very good at that. That was the one I remember mainly.

So I was not impressed with the Russian institutes and reactor design. They didn't want to talk much about Chernobyl; they wanted to talk about reactors that they should not be interested in really, if they knew the facts about them.

Wilmsen: Did you convince them that they shouldn't be interested?

Pigford: No. Their attitude reflected to me what I've heard so much about the Soviet system. The Soviets don't use economic analysis to determine what to build. These guys were lost when they got into

the question of what it costs. Instead, they make a policy decision and go build it. Well, that's led them to some bad decisions, like fuel reprocessing, like fast reactors; they have built many fast breeder reactors which they don't need. So it was an eye-opener to me, and I developed a different view of the Soviets and of the Russians. They obviously are extremely skillful in science and mathematics, but as for the kind of engineering to the same criteria that we would do in this country, or any western country, they don't do it.

Wilmsen: Has that changed since the collapse of the Soviet Union, do you know?

Pigford: Well, I haven't had much interaction, but many of my colleagues have because we're plowing a lot of money into Russia to keep their bomb designers happy so they won't sell plutonium to the other countries. So there's a lot of interaction there. It appears that it's still very difficult for science and engineering in Russia to be involved in a free market economy where you have these different criteria. They've got a lot to learn. We've had to send our own engineering firms over there, like Bechtel and so forth, to do some of the jobs that we had hoped they would do.

Wilmsen: Did they actually establish any of the kind of learning cooperatives, or cooperatives for exchange of information, about--

Pigford: Oh, yes. A lot of that has been established. The Lugar plans that came out of the Senate, are putting about a billion dollars a year into Russia. That's just one of them.

Wilmsen: But I meant the ones you mentioned on nuclear power. I thought you said there was some discussion of that when you were there.

Pigford: In Russia?

Wilmsen: Yes.

Pigford: Are we financing some of that? Well, one American company--a company I used to work for, General Atomic--which has been unsuccessful in selling its power reactor concept to this country after blunders on building two prototype plants, has teamed up with Russia to promote that reactor, and the Russians are applying for Lugar Senate money to finance that. General Atomic has finally found a way of getting U.S. Government money: through the Russians. [laughter]

Wilmsen: That's ironic.

Pigford: That's a foolish project.

Wilmsen: When you went, then, that was just a one-time trip. Did you have any further involvement after that trip?

Pigford: Well, through the years I've had Russian visitors come to Berkeley sometimes to see me. We've had one or two various postdoctoral visiting faculty. But that's not much involvement in terms of what is being done in Russia.

Wilmsen: Yes, but in terms of actually setting up a program of cooperation on nuclear safety--

Member of a Delegation on Arms Control to the Ukraine, 1994

Pigford: No, I have not been involved. I was on a delegation in 1994 to the Ukraine concerned with arms control. One of our purposes was to get them to realize some of the dangers that we thought they might not be aware of and to encourage them to work through the State Department and set up a bilateral treaty to help avoid those dangers.

Wilmsen: What were the dangers?

Pigford: It boils down to the misuse of the plutonium that comes out of dismantled weapons. I think that all the people I've talked with in Russia or former Soviet countries have the idea that plutonium is so valuable as a fuel for nuclear power reactors--and it can be used as a fuel--that it should be used for that. Well, before our trip there, I served on a committee of the National Academy of Sciences to consider what to do with the surplus weapons plutonium, and we concluded that it's so expensive to use it as a fuel in a power reactor, just the cost of the fabrication, that somebody would have to pay you to use it. Yet we're proceeding as if a lot of our surplus military plutonium is going to be used as a fuel. Secondly, if you do, it doesn't consume very much of the plutonium; you still have most of that plutonium left in the spent fuel. And the former Soviet countries, particularly Ukraine and Russia, don't see that economic argument because I don't think they understand it.

There's associated danger, because the more you transport this plutonium around--you have to send it to one place to be chemically refined, another place to be fabricated, another place to be transported, and you're talking about using fifty tons of it from each country, and that's enough to build 20,000 bombs--it's sort of inconceivable that you could have a good enough system with good enough security that none of it could be diverted away surreptitiously.

I don't think we impressed them very much, but Congress is putting enough money into Russia for this purpose for them to actually do something more than just storing the plutonium. All they're willing to do is use it as fuel for reactors. But at least, hopefully, it's making the system alert to the dangers and doing a good job on protection. I'm very uncomfortable about it.

Wilmsen: So the Soviets don't have a program for developing a waste repository, or do they? I guess they're not Soviets anymore. I should say Russia.

Pigford: Yes. Mainly it would be Russia. They say it's one of their long-range programs. They don't send people even to the international meetings that I go to on this, and the people I know in this country who are specializing in that area don't know what the Russians are doing. I think we believe they're not doing much of anything.

They have such an environmentally contaminated environment throughout the whole country, that really first having that and not doing much with it to clean it up engenders a culture that says, "That's way down the road to worry about."

This year there has been a proposal that originated in Japan and from a person in this country for Russia to create a interim storage facility for spent fuel--a place for Japan and Taiwan and South Korea to send theirs. And although it's against the Russian law to take in spent fuel from other countries, there are some people in the Duma who are enthusiastic about it. Frankly, I don't think there's enough will in it to make it work. I think the Russians are interested because the owners of the spent fuel --the utilities in these various countries--would be happy to pay Russia to store their fuel, just as our utilities would be, too, if we had a place to store it. And anytime more money is offered, the Russians are very interested. And in the possible prospectus which has been floated, they do say that money would also be used to help get them started on developing a geologic

repository. But that's kind of a footnote more than anything else.

Wilmsen: Well, shall we stop there for today?

Pigford: Okay.

Wilmsen: Because my next topic is actually to start moving into your work on geologic repositories, and that will take a long time, and be probably a good thing to start with in the next interview.

Pigford: Yes.

VI GEOLOGIC REPOSITORIES FOR NUCLEAR WASTE: PREDICTING PERFORMANCE AND
DEVELOPING SAFETY STANDARDS

[Interview 6: December 9, 1999] ##

Developing a Research Program on the Transport of Nuclear Waste Through
Geologic Media

Wilmsen: Today is December 9, 1999, and this is the sixth interview with Thomas Pigford. As I was just saying, I'd like to jump right in with your research on nuclear waste management and geologic repositories. I guess maybe we could start out with you just describing how you got interested in waste management, particularly geologic repositories, because you've talked a little bit about in the seventies when you changed your research focus more towards the waste management end of things.

Pigford: Yes. Well, I think my deepening interest started in the mid-1970s. In 1976 I served on a committee for the American Physical Society to evaluate the future of the nuclear fuel cycle, which means the future of all of the operations that go on to supply fuel for nuclear power reactors and to handle the waste and discharged fuel that comes up. And it was a very interesting study. It took about a year.

We got briefed by many people throughout the country and throughout the world. For one of the briefings which occurred, our committee was meeting at the Los Alamos Laboratory. There was a fascinating presentation by Dr. Harry Burkholder who worked at DOE's Battelle Research Center at Hanford, Washington. He had been doing some studies on evaluating the possibility of reducing the amount of radioactive waste by transmuting it into a more benign form. He approached it as a systems study to evaluate how much gain there would be in helping geologic disposal if such a

system were carried out, and so he got into the area of trying to predict the ultimate fate of radioactive materials when they're buried deep underground; it actually applies on any kind of burial.

Eventually some of them dissolve in ground water and they slowly, over the years, migrate out and some of it gets into the environment. That's exactly the kind of process that has to be analyzed to determine whether auxiliary processes like changing the nature of the waste before it goes into the ground are worth while.

Too little had been done in this country and other countries on developing a systematic quantitative evaluation approach to use as a guide to how much is achieved, in other words, to evaluate the ultimate safety of any disposal scheme. And incidentally, that issue applies to disposing of any kind of waste, whether radioactive or not, and so he was one of the forerunners.

Well, it's a process, chemical transport, which is what I had studied in chemical engineering as a student: How fast does a solid in ground water move along through cracks or pores in rocks and what happens to it? How much does it get diluted? And when does it appear at some distance in the biosphere where human beings might get at it?

The equations for that had already been developed for a general kind of chemical transport, but they were limited to each chemical species being treated separately. And that's fine if you're dealing with something like arsenic, an agricultural waste that gets into ground water, because it doesn't decay away through radioactive decay. You deal strictly with mixing, slow mixing in the underground system, and retention by the solid material--the rock or the soil. It was very easy to translate those equations and then add a term for radioactive decay, which is what we deal with in radioactive materials.

But a new term came up because when a radioactive material decays, what it decays to can also be radioactive, and so you have to treat the daughters of the radioactive decay, and that complicated the mathematical analysis. It turned out that some of the most important species to be considered had at least one or two radioactive daughters, and to the mathematicians, what was originally a second-order differential equation became three second-order differential equations, which is equivalent to a single sixth-order differential equation, a very challenging

thing to solve.

Dr. Burkholder had made an analytical solution, which I was impressed at. And I was more fascinated by his results where he predicted a kind of shock wave would develop and accelerate the motion of one of the daughters or both daughters through the geologic medium. I couldn't believe the results, and yet it sounded like his analysis was reasonable and correct.

But his explanation in physical grounds of the sudden large almost abrupt wave front just didn't seem to make sense, so I went home and started thinking about it and studying it. And I did find the equations to be quite formidable. It seemed as if he had done his work properly, but I and a colleague who was at that time a visiting professor from Japan, Professor Kunio Higashi from Kyoto University, worked it out with me--and I'll give Professor Higashi most of the credit--and found a fundamental mathematical error in Burkholder's mathematical analysis. We also, more importantly, I think, discovered the reason for the abruptness of the wave front. By wave I mean change in concentration in the moving ground water as it moves along so that there was an abrupt increase in concentration and it made it go more rapidly than we had ever thought would be possible from the simple single-species equations.

Well, we learned that, yes, that was a valid result for some cases, although it did not follow the properties of the kind of a shock wave you get from an airplane when it crashes through the sonic barrier. We thought it was worth studying further. Since we had found some errors in his equations, we decided to undertake more research and correct the errors and study this whole process more thoroughly than he had had time to do.

Dr. Burkholder meanwhile had left the Battelle Laboratory in Hanford and had gone to work for the laboratory Battelle had in Columbus, Ohio and did not have time to continue the research himself.

At that time the Battelle Columbus laboratory was the national coordinator for studies of high-level waste disposal, and so we generated a proposal for research funding that officially went through the Lawrence Berkeley Laboratory because this would be a proposal for money from DOE, and DOE likes to funnel its funds to the university through one of its major laboratories. So I became officially a senior research scientist for the Lawrence Berkeley Laboratory even though all of my work was done on campus with students. We had very little interaction

with the laboratory scientifically.

But I and others were invited to come out to Battelle, Columbus, to give a verbal presentation of our proposal, and one of the reviewers of our proposal was there, and he was Dr. Burkholder. I felt a little embarrassed because we were pointing out he'd made an error that flawed the mathematics, but we thought it was worthwhile to do it right and continue. And I expected him to say okay, he will do it, but he said he was too busy and he decided he would sponsor us. So that's how our research group got started officially with the sponsoring agency.

Wilmsen: Was this proposal for work on geologic repositories, or just--?

Pigford: Yes, because Dr. Burkholder was now a member of the management group at Battelle Columbus which managed for DOE the overall work on geologic disposal. And so that was our entry into the field. I don't think they had had anybody else at universities doing research for them. We were probably the first. And the program blossomed.

We corrected the equations, made many demonstrations of the results in terms of how it affected the design of geologic repositories, and those equations are very efficient. It's not often done in engineering nowadays that people simply use classical mathematics and come up with equations. They tend to put the problem on the computer, which breaks the problems up into little finite steps and makes approximations to solve the problem step-wise through the entire medium and time-space. Computers can afford to do that because they can do these calculations quickly. The trouble is, you lose the generality of the results, whereas an equation can tell you that something changes as, say, the square root of time. The computer doesn't tell you that, it just tells you, at a given time, how much is there for a given set of parameters. Knowing the general relationship allows you to understand the results better and also follow trends and predict trends which is what you need to do in developing a new design concept.

And so we were very proud of these equations, but the trouble is that people in the field who were the contractors for DOE who were actually trying to design repositories didn't do that kind of mathematics anymore. Much to my chagrin, the computer has displaced mathematics too much. So they asked us then to do the calculations for them. We put our equations themselves in a computer, which is much more efficient than the normal way a computer works. Our computer equations, which can

solve functional equations, could give us results far more quickly and more accurately than the normal way a computer works--the normal finite difference techniques.

We thought they would then be happy to use that computer code. Well, they were. And for a while we became the custodian of that code. It meant the user didn't have to remember unusual functions, like hyperbolic functions and so forth; he simply had to run the computer code and it would tell him what he wanted, even though it was the new streamlined computer code that used those functions in its own calculations. So they liked that. They started using it.

But then we wanted to add more cases to the code, so we changed some of the boundary conditions of the mathematical analysis. And as any successful computer code experiences, there are variations that come out with improvements every year. Not improving the basic mathematics, but improving the number of cases it can handle as to whether the dissolution rate of the radioactive solid is constant with time, or varies exponentially, or with any kind of prescribed variation. Extended equations could handle those situations.

So every time we came out with a new modification we had to send it out to the many users and educate them on how to use it, and I realized that that was not the thing a university should be involved in unless your whole interest is serving computers, which ours was not. The computer was a minor part of our interest. We wanted to develop new equations and new generalities, so we got DOE to designate one of its laboratories --the Pacific Northwest Laboratory where Burkholder used to work --to be the translator of our new equations that came out every year or so into a new computer code. And we got off the hook on that.

And those equations are still there, buried in extremely complex computer codes that are now used to evaluate waste disposal, and I'm sorry to say, the engineers and scientists who work on designing waste disposal systems now don't seem to have much idea of what's inside their code. I found that they only would read the first report and read the generalities and the physical interpretation of processes. It would save them an awful lot of time and money if they understood it better, but that's not the way complex engineering necessarily works now. It's too bad.

Well, that got us off to a start, and as we found we could

embrace more and more problems that arose in the designing of waste management systems, our work got more popular with the Department of Energy. They had solved the code-maintenance problem by having the Pacific Northwest Laboratory hire some mathematicians who could translate our equations into each new version and work with the people who would use computers. We got more deeply into the analysis, and our program grew and grew. Soon it became the largest extramurally funded research project in the department--maybe one of the largest in the college of engineering, I'm not sure.

And it was exciting. We enlarged the staff, I had to have somebody come in as a business officer to work with Lawrence Berkeley Laboratory on accounting, which is a pain in the neck. We had to make presentations frequently to the DOE contractors who were designing repositories, and that was interesting, but still took a lot of time, so I hired a business manager who was also a scientific person and he did a lot of that.

We had six postdoctoral research fellows who were sent by Japan. They had heard of the work and they thought it was a good place to train their own people into this emerging field of predicting performance of waste management systems. They would come and work with us for one year or two years, and all of them were graduate engineers or scientists, and some of them were university professors. And all very, very competent people.

Then we had many, many graduate students who came to us from our nuclear engineering department as well as other departments because this work is general for any kind of waste disposal. It is more complicated, as I described earlier, when you get into radioactive waste disposal because of the simultaneous migration of the radioactive material and its daughters.

Origins of the Recommendation for a Geologic Repository Program

Wilmsen: Now when you say waste management systems, what other kinds of systems are you talking about other than the geologic repositories?

Pigford: Well, suppose we worry about disposal of chlorinated hydrocarbons? That's an example of a frequent problem where there has been a surface operation like an airplane maintenance

facility where they work on engines and use chlorinated hydrocarbons as cleaning solutions to clean off their engines. These are terribly toxic materials--and PCBs from transformers--and if they are not extremely carefully controlled, those contaminants go into the ground. They infiltrate from rain, from surface water, penetrate, and then they move through the soil, which is porous soil, to get into rock and move through that. They can chemically adsorb on the soil or rock. But that's a weak chemical reaction and it only retards their movement through the soil. These we would call near-surface disposal problems.

Wilmsen: So your research then was on waste management of toxic materials in general, not just--

Pigford: In general, but it was funded entirely by the Department of Energy for the purpose of waste disposal and geologic disposal of radioactive material.

Wilmsen: Now where did that idea for geologic disposal come from to begin with?

Pigford: Well, back in the 1950s and early sixties, it was certainly apparent to many in the field that something had to be done eventually about the rather intense radioactivity from the spent material that is discharged from a nuclear reactor. That's mainly the fission products that had been separated even during the war for the first chemical separations at Hanford. They recovered the plutonium but left the waste sitting there in tanks. These are near-surface tanks, and they knew that someday they had to go in and clean those out and find a more permanent solution because the amounts in terms of the amounts of radioactivity were simply enormous. We knew that some of them had half-lives long enough that they would still be around after decades, and thousands, and hundreds of thousands of years, so the ideas of the long-range, long-term disposal began to be studied in the early sixties.

And they considered many of the alternatives. I think we've talked about the alternatives here before, haven't we? Or have we? The obvious alternatives are like disposal in space.

Wilmsen: Yes, we did talk about that.

Pigford: And then that was a clear problem of abort rate of rockets. Then another one was disposal in the sea.

Wilmsen: Yes, we did talk about that, too.

Pigford: Which, I think in terms of being a predictable disposal technique and being quite safe, is much better than deep geologic disposal, but it has an obvious problem: who owns the sediments at the bottom of the ocean? These are the deep parts of the ocean and that's an international political problem. In fact, since we last talked, I've learned that there was a London conference, I think, of the nations who are confronting disposal of radioactive waste a few years ago, maybe two years ago, at which they all agreed that disposal in the deep ocean would not be considered. I think it was a mistake to agree on that because surely the political problems are probably extremely difficult, but it's still a very good technical solution.

And so by sifting through those options, a committee of the National Academies of Science and Engineering came out with a recommendation that geologic disposal should be pursued most actively. That recommendation at that time was--

Wilmsen: What's the date on that, approximately?

Pigford: Roughly in 1960--early sixties, 1963, or something like that. And I think at the same time they said there's an option possibly available that looks most attractive: to dispose of the radioactive material in natural bodies of salt left over from the evaporation of the oceans a few hundred million years ago. And they gave a lot of impetus to working on that, but it really wasn't worked on very actively until the 1970s. And I think that's in response to your question how did the concept of geologic disposal arrive.

**A Theoretical Breakthrough in Predicting the Rate of Appearance of
Radioactive Materials in the Environment, 1988**

Pigford: Now, shall I continue on this discourse?

Wilmsen: Sure, on the--

Pigford: This is on some of the evolution of our research program.

Wilmsen: Yes.

Pigford: So we developed a very exciting and healthy research program.

The funding level got up to a level of around half a million dollars a year, which is a very large level for work that's strictly analytical. It's cheap work. It pays only the salaries of people. We didn't even use computers very much because our analytical solutions were so efficient.

The reason it is so heavily analytical is attributed to my colleague, Professor Chambré, who was a member of the faculty of nuclear engineering. He still is. He's retired. And he has had a joint appointment in the department of mathematics as well as nuclear engineering, and he was always sought after when there was a new and difficult problem to analyze. Being a classical mathematician, he wanted to do it by classical mathematics, although he was willing to use the computer when the computer was necessary. For example, classical mathematics will frequently turn out a beautifully general result, but it says that in some instances you must perform a certain integral, which is in principle a summation over some wide limits of certain mathematical functions. Sometimes those integrals are so hard to evaluate that you give up unless you have a computer.

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Pigford: With a computer you can solve an integral like that, which is part of the classical solution, and turn out numerical values. That's where a computer really should be used--on things where it's unique. And that has helped classical mathematics a lot to have computers used in that way. Well, that was Professor Chambré's whole professional interest, professional skill, and personality. And it was a privilege to work with him on this; I like mathematics, and he taught me a great deal of mathematics in the process. And our students really seemed to thrive on it.

Along the way, we developed some new concepts on how to treat parts of the problem. For example, when we think about the process of burying solid waste material and eventually it can get exposed to ground water and dissolve, well, the first starting point in analyzing the process is how rapidly does it dissolve? If it doesn't dissolve, then it's no problem. If it dissolved very rapidly, it's a worse problem. How rapidly? We call that the source term.

And so throughout the country, people working on solid materials, especially for high-level waste disposal, had the concept of--at that time--taking the separated fission products and dissolving them in glass. Glass looks like a very refractory material that shouldn't dissolve much in water, but how do they

find out how rapidly it dissolves? Well, they would put some little samples in a container of water held at constant temperature, and every few weeks measure the concentration of the dissolved material in water. Well, that's fine if it dissolves like in a day or a month or year or a century; that's all quick--too quick for waste disposal. If it takes longer than a century, it means your experiment is going to take a long time to yield a result.

As a matter of fact, they were beginning to realize--well, we pointed out, I think--that anything that dissolves as fast as a few centuries is going to be a hard problem to solve in terms of waste disposal. It will release finally a contaminant concentration in ground water that will transport out to the environment that is just intolerable. It became evident that it must take many thousands of years--maybe ten or a hundred thousand years--for it to dissolve for it to be a suitable waste form.

And they said, "Well, maybe it will take that long. Our experiments have only gone so far." But they were just extrapolating, with no scientific basis. Without any theory of how the dissolution process works, all you can do is guess at pushing--putting some sort of line through the data points and see what it's like in the future.

Well, that led to many arguments, all of which were fruitless, as to how to extrapolate. There were many conferences. And at that time, we didn't have any theory to offer, but we then had the idea--oh, yes, the idea came this way: I was listening to a presentation from another person from the Battelle Pacific Laboratory at Hanford, a very competent chemist, and he pointed out that in some of his experiments the concentration of some species in the solution in the laboratory would continually increase with time, like boron, or like sodium, cesium; but on the other hand, the concentration of some of the species would rise to a maximum and then level off. And that was true of plutonium which was one of the contaminants you worry about. So it suggested that plutonium was reaching some solubility limit.

They were on the right track because plutonium compounds in the environment form very insoluble compounds: oxides and hydrated oxides. That's one of the fortunate things about plutonium, and that's true of some of the other contaminants that are a problem both in radioactive materials and nonradioactive materials. So that could set an upper limit on the greater

dissolution: by assuming that all the ground water coming by the buried waste would reach solubility concentration with respect to these little-solubility species, the rate of dissolution then would be the product of the ground-water flow rate and the solubility.

Well, I agreed with them. That seemed like an upper limit, but that upper limit was a pretty high dissolution rate. I asked them how much ground water do we associate with a buried waste solid. They said, "Well, we'll just take the amount of ground water that is heading towards it and that would intersect it if it were transparent."

But it was evident in the discussion that, No, you don't have to have moving ground water touching the solid, or even that the dissolving in the ground water occurring very near the solid can diffuse out into other ground water farther away. And anytime it diffuses away, it reduces the amount nearby the solid, and more will dissolve. So you had to have the possibility of contaminating more ground water. Then I found in one international conference a few months later in Berlin a whole session devoted to the question of how much water can dissolve a soluble radioactive solid, and here this is a flowing system rather than the confined experiment in the laboratory which uses just a pot of water.

So it depends on how fast the water is flowing past it, and the discussion again got into the area of, Well, it's more than that that is headed towards the waste because of diffusion. They knew I had worked on diffusion processes a lot and they said, "How much more water?" And I said, "It depends on the diffusion coefficient." And I said, "If the diffusion coefficient is high enough, it will contaminate all the water for miles around."

But they said that requires a very high diffusion coefficient. I said, "Of course." So I came back home and Chambré and I talked about it. And rather than trying to predict the actual rate of dissolution, we said, "Let us assume, conservatively, that the thin film of water actually on the surface of the solid gets to the solubility concentration which is the limit. How rapidly can that material--dissolved material--diffuse out into the other water?" That depends on the flow rate of the water as well as the diffusion coefficient, and it was a complicated problem.

I studied a problem like that once when I was a chemical engineering graduate student because dissolving chemicals as

solids is a frequently encountered problem in chemical engineering. In those days, to solve that problem we had to consider the diversion of water around the solid, which changes, streamlines, and creates turbulence, which is extremely difficult to handle mathematically. We could only work on simplified asymptotic solutions to that problem.

Well, one important difference occurs in waste disposal. If you have anywhere near a good site, the water flow is quite slow.

Rather than flowing at a few tens of centimeters per second, or feet per second, as it does in a normal industrial process, it flows maybe a few inches per year, as the migration rate of water through a reasonably compacted low-porosity solid, or even through earth. Through porous earth it might be as much as a few meters per year, but in the kind of rock we were considering for geologic disposal it was a few centimeters a year, even as low as a few millimeters per year. At that low flow rate, turbulence does not occur. It's creeping flow. Mathematically we call it potential flow, and the mathematics of nonturbulent flow are so simple, or the processes are so well defined, that we found we could solve that problem exactly analytically.

What emerged was a new tool of predicting not the actual dissolution rate--that might be still very, very low because of a low rate of chemical reaction between the solid and water--but conservatively assuming the chemical reaction rate is very large, it's limited then by the rate of diffusive-convective transport from the maximum concentration at the surface into the surrounding medium. But it depends upon the porosity, upon the water velocity, and the diffusion coefficient.

So we had a well-defined, bounding calculation that was a useful one because it gave new limits on the rate of dissolution, and they were much better than was ever expected. So we came out with a theory that showed that at least if you were worrying about plutonium, the rate of dissolution of plutonium from the waste solid would be so low that it would almost disappear as a worrisome contaminant in the environment. And that still holds today.

We extended it farther and applied it to the rate of dissolution of borosilica glass itself, the structure of glass. That can be limited by the solubility of silica in water. Then immediately we found that that was going to be very, very low, and that had a possibility of restricting the rate of dissolution of even the soluble fission products because they can't dissolve until the glass is restructured. Well, that tool was a very

important one and is still used today as the fundamental basis for predicting the source term--the rate of appearance of radioactive materials. It predicted the bounding value, the upper limit value. It required experimental measurements of the solubility, the diffusion coefficient, the porosity of the rock, and the rate of movement of water through the rock. Now that was a breakthrough.

Along the way, I had, in this process, served on the Three Mile Island committee which effectively took me away from most of the work for a year, and I had professor Chambré and one of the Japanese colleagues write the project reports during that year. Otherwise, I wrote most of the papers and reports.

The Waste Isolation System Study, and Brother's Role in Developing the Theory

Pigford: Then after that came the waste isolation system study, which I chaired for the National Research Council, sponsored by the Department of Energy. It was to evaluate the national work on geologic disposal of radioactive waste from a systems point of view. What they meant by system was looking at every part of it and seeing what it took to predict the safety--because that's the big problem, predicting the safety--and what design it took to make the predictions show it was a safe system.

Fortunately for me, my brother, who was, as I've said before, five years older than I and a very well known person in chemical engineering, was on the committee. I was chairman of it, which delighted me because very seldom was I in a position to be in a more authoritative position style administratively than my brother who is older and better known. So as I began to develop these ideas, which I'd heard about through my own research and hearing about those experiments, I asked him if he didn't think maybe the rate of diffusive convection from the surface or mass transfer as we called it would possibly limit the net rate of dissolution. He was an expert on mass-transport analysis and had written several books on the subject.

And as older brothers frequently do, he pooh-poohed the idea because from his experience in such applications that was a pretty high rate. It would be intolerable. But his experience had been with normal, chemical engineering systems where there is

turbulence and rapid flow and so forth, so his rejection caused me to be even more interested in the idea, and that really spurred me on to work with Professor Chambré.

We came out with this result, and I knew that it was going to be hard to get him to accept it because it showed that he was wrong, and he's very seldom wrong, although he's a much more scientific person--objective--than it sounds from his first remark. So I assigned him to write up the chapter in our committee report which was a three-year-long study on how to predict the suitability of a waste solid. Then I sent him a free-hand draft of our theory, and of course he was the logical person to review it because diffusive transport is his field. I worked in it a little as a graduate student and occasionally in designing reactors, but he was the international expert. He saw the draft and he called me up and said it was correct and he wrote it up in the report. And he gave some very good tips on how to present it.

At the same time he was a consultant to the du Pont company, mainly in Wilmington, Delaware--the headquarters--consulting on regular chemical engineering operations. The du Pont company was also operating the Department of Energy facility--they call it Savannah River, but it's actually in Aiken, South Carolina--which was built during the early stages of the Cold War to make more plutonium. Also there was a group at that laboratory in Savannah River which was responsible for developing glass solids for radioactive waste, and so we were adding an entirely new way of predicting the performance of their solids.

In fact, along the way, our predictions showed that these experiments they were doing in the laboratory with a closed container were worthless because they did not simulate at all the controlling process in a real system where water is moving past a solid. So they invited us down--I guess they invited my brother and me to go there--and we made some presentations. And officially they said they understood and were delighted and would change their work. The trouble is they didn't have people on their staff who were very much versed in this kind of chemical engineering. Even to this day their work has been not very productive, I think. But the people working on geologic repositories have taken over this theory. It's a fundamental one and is widely used.

Outside Reviewer for the Swedish Program, 1986-1991

Pigford: And I introduced it to the Swedish program. About that same time Sweden had moved more rapidly on establishing a program in geologic disposal than any other country, including the United States. It developed a nice conceptual design, and--

Wilmsen: That was for low- and medium-level waste, wasn't it?

Pigford: Well, they had three parts: those two, plus high level. They haven't yet built a high-level facility, but I'm talking so far about their conceptual design.

They had been extremely careful and perceptive about the need for outside review of all of their work. And I think it has resulted in a program that is by all means the most outstanding in the world, even though officially their schedule is not as tight as the one in the United States. So they formed an international committee of selected scientists from different countries: two from England, I from the United States, and one from France. Yes, I think that's it. And our committee met in Sweden to review their work; we did it more from the systems point of view--overall protection of performance. I think that was a program that lasted about six years.

I got to know Sweden very well, especially in the winter because they would only invite us there for the winter months. They pointed out that in the summer months, when Sweden is so nice, they all are on holiday and it wouldn't be worth having us come. [laughter] We managed to change that because as a group of so-called prestigious consultants (we met as a group), we could suggest certain studies they do--experiments, analyses--and give them a schedule, and we were able to get them to agree, reluctantly, that the schedule called occasionally for meetings in the summertime. So I got to see Sweden in the summer, but I know it better in the winter months. And I still love the country and I still go there and meet with them occasionally. In fact, I was there in September of '98, and gave a sort of overall summary paper on the state of the art of prediction. Then in May of this year I went there, so our contacts are still strong.

Wilmsen: What do you think accounts for the Swedish success?

Pigford: Well, perhaps more care about interfacing with the public. They are a small country, and the public can have more effect upon a given project than I think it has in a country like this. Of

course our public is larger, but communications are more complicated. In Sweden, Stockholm is just like a small town, and they could have a meeting involving practically all of the interested technical people in the whole scientific community to discuss a particular problem, and you can see a consensus arising. We don't do that in this country. Our scientific community is enormous, and I'm afraid, as a result, we kind of give up and don't even try to do it. We have gestures in which we say, "Here is an international symposium exchanging the ideas," but the people who come to those symposia are by and large only the workers in the field, and that's not the way of communicating with the scientific community. If the scientific community doesn't know or doesn't understand, then eventually the public is going to be in worse shape because they rely upon their own icons in the scientific community.

Sweden is able to do this. It did run into a very difficult problem in getting approval for the siting of some of the latest nuclear power reactors that were built there. Their nuclear power reactors furnish a very healthy percentage of their total electrical power, and they ran into difficulties in getting permission to start up those reactors. There was an agreement, which really became an agreement in the Swedish Parliament, that said they would be given permission to build them, complete them, and run them, providing they agree to shut them all down in I think the year 2004. And of course that's not a very long time for a power reactor to run. And they're running it very successfully.

Well, from that experience, it was right for them to foresee that they were going to have a tough problem on any kind of waste disposal. And so they approached it very carefully. They sought outsider reviews, foreign reviews, almost at every opportunity. Published it at many meetings with concerned people. Those were very effective, and they continue to do that. That's why I'm confident they will be the first country to really have geologic disposal. They're also very good technically, but I don't think their technical skill is that much better than, say, the United States, or Canada, or France. It may not be as good.

So, the research program blossomed and--

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Pigford: I did a lot of traveling, giving talks about our results, and also working with the repository designers at their working meetings to try to help the design come along.

I was approaching retirement and I was looking for someone--some young person--to come in, to take the reins over. Professor Chambré, incidentally, is much older than I, and he was working into his late seventies during those mid-1980s.

Let's see, I became seventy in 1992, I guess it was, and I was hoping to retire. Earlier, I had to cut down lecturing because my hearing was so bad that I could not have the kind of exchange--spontaneous exchange--with students in the didactic process that I always like to use in teaching. I felt I was undercutting the students. And then they'd given me the administrative job again of chairing the department and I did that; tried to rescue some of the faltering programs, and initiated the decommissioning of the nuclear research reactor. And I just needed to get out of so much administration. I intended to continue the research--which I still do--and I finally retired. You don't really retire until your graduate students have completed their work, and my last graduate student completed his work last year, I think. I've been officially retired for eight years.

So I don't have the same kind of association for the group creative process that was so valuable in the late seventies and throughout the 1980s, but I can do work on my own. I review work, and through that I publish reviews, and contribute a little to the better understanding of the results that I think are there, techniques that are there that people don't really understand or I haven't learned yet. And it is said that the longer you do this, the more you get into policy, which doesn't require much new creative work, but requires some initiative to challenge the socio-technological issues and barriers. I'm deeply into that right now with respect to arms control, which maybe we'll get into later.

Wilmsen: Okay.

Pigford: And I'm publishing more papers every year by myself than I published when I was not retired.

**Waste Transport, and the Government's Program to Select a High-level
Nuclear Waste Repository**

Wilmsen: Now how did you get involved with Yucca Mountain? On your CV you've got a couple of things where you did service for the Nuclear Regulatory Commission. There was a national peer review panel for geologic criteria, and then you were a member of the board of radioactive waste management.

Pigford: Yes, those are specific actions: like a committee was set up to do something. And meanwhile I was having this continued activity of broad research for the Department of Energy.

Now, how did I get involved in Yucca Mountain? Well, when I first started this research under the sponsorship of the Department of Energy, they had, essentially, four different geologic disposal projects going. One was the WIPP project, the Waste Isolation Pilot Plant in New Mexico which was officially for what we call transuranic wastes, which means not fission products, but scrap material and trash that had been produced mainly in the process of making weapons, making plutonium, like contaminated gloves, turnings from lathes and so forth. It doesn't have the intense radioactivity of fission products, but it's very long-lived and very dangerous if assimilated into the human body. And that project is strictly for that kind of waste. It's a geologic disposal project in natural bedded salt.

The other three projects were alternatives of what kind of geologic disposal medium to use. One was on using the basaltic layers that are deep underground underneath the Hanford Reservation. They are there from very ancient volcanic eruptions which laid down layers of basalt, and then the volcanoes erupted again, and another layer, and so forth. They could access those for waste disposal by digging a very deep vertical shaft from the surface. The one in Hanford for basalt was for commercial high-level waste.

An alternative was a project to develop a repository for commercial high-level waste, again using natural salt, but not the WIPP facility, and that salt could have been in some places like New Mexico, Texas, or even salt domes like in Louisiana or Mississippi. In that case, they knew a lot about the location and extent of the salt and had learned a lot about that from the WIPP project. The only trouble was, there was no federal land that had salt beneath it, so it would have to be newly acquired real estate.

Now the third project was to use the volcanic ash, which had consolidated to make a rock called tuff, tuffaceous rock. One of the features of this is it is from an ancient volcano that didn't

make lava, but it spewed out fly ash into the atmosphere, and the ash settled and consolidated to leave a very firm but very porous rock called tuff. And I think obviously a main feature is it occurs in land already owned by the government. In fact, it's in Nevada very near what is called the National Testing Station, which is where they have done underground testing of atomic bombs.

Wilmsen: So this tuff layer is underground?

Pigford: Yes. And actually they decided that it would be easier to dig into the side instead of going in from the top and drilling down to access the tuff: to go into the side of a small mountain so you could have a horizontal entry with railroad cars and trucks carrying the waste in. And so that little mountain is Yucca Mountain--it's not a very steep mountain--just looks like a big bump on the horizon. And the waste would be emplaced in a tuff layer about 400 feet deep below the surface. Then another roughly 400 feet below that is an underground aquifer, unfortunately. Another feature of the site is that there's very little rainfall--just a few inches a year--and because of that, the rate at which water infiltrates through this rock, they thought was only about a millimeter per year, now they think it's more like fifteen or so millimeters per year. Still very low, but the rock is very porous, there are fractures in it, and so it does allow water to move on down.

It also has a problem that it's not saturated with water, so there's a lot of air in the pores. In fact, air probably breathes in the lower part of the mountain and, due to natural circulation from buoyancy, moves up through the mountain and escapes from the top, so it flows countercurrent to the water.

I should point out that the WIPP facility didn't feel that it needed to use mass-transfer analysis to predict long term performance. And I think it's right, because there you put the waste in a hold in the salt--it's a cavern-like tunnel--and leave it there. The salt is actually quite plastic as opposed to other rock, and from the force of the overburden, it slowly collapses, and in about a hundred years it will surround the waste tightly like a cocoon, and so it will not allow water to penetrate. So our type of analysis was not useful.

The system analysis there to predict long-term performance related entirely to the hypothesis of future human intrusion. Humans may be prospecting for future minerals or oil, as they do now in that area. So without knowing better, some future

petroleum company may drill an exploration well down that intersects some of the waste and brings it to the surface, and also creates a shaft in which water can come down and dissolve some waste, dissolve some salt, and by that mechanism contaminated water could flow back up to the surface and get out into the moving underground water. Quite a different situation.

Then in 1987, the Department of Energy, or really the Congress, which was getting very annoyed at the large expenditure and slow pace of development, ruled that there would be only one main project for the commercial waste, and they selected the Nevada tuff site for that purpose.

I think technically they made a mistake. I think it should have been one of the salt sites. But I think they were swayed by the looming politics of public opposition and state opposition for any of those sites, especially if land had to be acquired from somebody, and they already owned the land at Yucca Mountain.

Before they narrowed it down to one site for the commercial waste, they were even supposed to be prospecting for a fourth site for a second repository. The first three sites were to select the first repository, and they should be prospecting for a fourth site for a second repository.

They thought, for demographic reasons, it ought to be in the East someplace, and so they found the best place for that was in New Hampshire, which has these enormous masses of granitic rock, and that would have been very much like the Swedish and Canadian projects which rely on granite. But once the people in New Hampshire and other states heard that it was being considered, they developed so much opposition that the Congress cancelled the search for a fourth site. There was such an outcry that the people who worked for the Department of Energy were not permitted to even come into the state. The State of Texas, which was concerned they may select Texas for the salt site, issued a warrant for the arrest of the head of the geologic repository program if he ever came to the state.

Wilmsen: That's amazing.

Pigford: For a while, they were even afraid to fly over it. So that shows the intensity of concern, and it also indicates that maybe our country had not been sufficiently sensitive and receptive to what it had to do.

Wilmsen: They didn't encounter that kind of opposition at Nevada, or not that intense of opposition?

Pigford: No, I think not at that time. There was intensive opposition in the state of Washington and from the Indian tribes. In Nevada the state was immediately opposed, but not intensively, and the Department of Energy argued, "Well, this is where we've been exploding atomic bombs for years. The place is already contaminated and it hasn't done any harm." And I'm afraid the public didn't perceive that that was an invalid argument.

Wilmsen: Actually that was a question I had for you--about the underground testing of nuclear weapons. Because you're releasing radionuclides into geologic media, so you've got the same kind of problem with eventually the contaminants working their way into the surface environments.

Pigford: That's right. So how do they get away with it?

Wilmsen: How do they get away with it? [laughs]

Pigford: Well, technically, yes, some of those contaminants can make it to the surface, but you see, it's a rather short time to expect them to show up. The predicted time for a contaminant to go from the waste package in Yucca Mountain to the nearest environment where people are, which may be a few miles away, is thousands of years. And that would be the predicted time for the test site as well. I don't think the test site has been exposed to the same concerns. On the other hand, the amount of radioactivity on a test site is very small compared with that at Yucca Mountain. It sounds awful that an atomic bomb makes so much radioactivity, but it's not nearly as much as what is made by nuclear power reactor. I'm speaking of the long-life radioactivity. There's a burst of radiation from an exploding bomb that is intense, but that goes away really in an extremely short time. Then it's the fission products, and you worry about the ones with half lives, at least for moving through the environment underground, of tens of years and hundreds of thousands of years. Now the amount of that from the bomb is not negligible, but it's small compared with that from the accumulated operation of roughly one hundred nuclear power plants.

So they have discovered some tritium moving away from the bomb locations. Tritium moves as rapidly as the water itself. It does not interact with the solids. But I don't think it will ever loom to be as much of a public opposition problem as Yucca Mountain for the reasons I've given.

Wilmsen: Because the half life of tritium is, what, eleven years?

Pigford: Thirteen years. So tritium moving doesn't bother me. If it moved actually into places where people dig wells, I'd worry, but it hasn't gone that far. And I haven't studied that. I don't remember the predicted transport times, but undoubtedly it's hundreds of years.

Wilmsen: Okay, so what happened next? We were talking about how Congress wanted to consider a site in the east and then they encountered all this opposition.

Pigford: Well, they dropped the program.

Wilmsen: They dropped that program.

Pigford: Yes. Congress is a political body, and I think probably they did right. Better we concentrate on developing the first repository. That's going to take long enough. The scheduled completion date for Yucca Mountain is to start running in 2010, but I've said in many publications that there's no chance they can make that, and it'll be at least ten years delayed, I think.

Setting Safety Criteria for the Nuclear Regulatory Commission, 1977

Wilmsen: Okay, so what were the specific tasks that these peer review panels had?

Pigford: Which peer reviews?

Wilmsen: Well, on your CV it says that in 1977 you served on a peer review panel for geologic criteria for the Nuclear Regulatory Commission and then from 1978 to 1985 you were a member of the board on radioactive waste management of the Nuclear Regulatory Commission.

Pigford: Okay. On the regulatory commission's peer review panel, our job was directly focussed on the technical issues related to the responsibilities of the Nuclear Regulatory Commission, namely, what are the criteria for safety and what seemed to be the looming problems? Okay. This was a panel whose existence was limited to a fraction of a year--a few months, maybe--and we issued a report. We first addressed the criteria and we found that the Nuclear Regulatory Commission's criteria--did it give

the date of that?

Wilmsen: Of the peer review panel? Yes, 1977.

Pigford: Okay. They had no criteria and they said they were simply using the generic criteria they use for licensing any facility for handling radioactive waste. Well, we thought that was okay because those criteria had been developed during the 1960s and early seventies and revised and revised and tightened up. And I thought they were pretty good. In hindsight we were all wrong. I think they would still be pretty good, but there's a fundamental difference.

When we license a facility, or when the Nuclear Regulatory Commission licenses a facility like a nuclear power reactor, they must see that it obeys criteria for protection of the public--for example, from normal releases of radioactivity to the air or water. They all release some, and the criteria are simple. They've established what are the allowable concentrations in air or water, such that a person hypothetically living right there by the fence post all his life will not get a dose beyond a certain allowable level, which is extremely small compared with background radiation. But it's only for the thirty or forty years of the plant's life. And you can monitor that as the plant is operating to be sure that what they do is right and correct it. And so, an important part of the Nuclear Regulatory Commission's job is to continuously monitor how things are going at their licensed facilities. Well, those two things--the life and the monitoring--are not the same for a geologic repository.

The timespan in which there's a lot of potential danger from buried radioactive waste--this is true for high-level waste as well as even low-level waste--is hundreds of thousands of years.

That means that you have to deal with that issue in setting the criteria. The other part of the problem is that you set the criteria such that no person in the future should have a radiation dose greater than what we normally allow in licensed facilities, which was the first statement of criteria, which I thought was wonderful. There's no way of monitoring it. Of course you can monitor it during the time you're willing to assume our institutional capacity hangs together, but that's a pretty uncertain future--uncertain certainly in the thousand year realm. So how to deal with that.

Both the Environmental Protection Agency, which is supposed to set the fundamental criteria, and the Nuclear Regulatory Commission, which is supposed to set means of implementing them,

knew they had a problem. But we didn't realize that at the meeting of this committee, and so we effectively glossed over that part of the problem, and I've been spending so much of my time in the last six years addressing that problem and trying to get people away from a strong tendency to write much more permissive criteria than was originally stated--but without much success, I'm afraid.

The other part of the study was simply to review what had turned out from the various system studies. And there weren't very many at that date. Burkholder's was one of the few.

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Pigford: A goal of that review was to try to alert the Nuclear Regulatory Commission as to the kinds of technical issues they must be prepared to evaluate when a design comes to them for review. There wasn't much to review then, and so we simply used the few projections that had been made at that time and pointed out what is obvious, I think, that they need to be able to predict from a safety point of view how the system works. We pointed out that the cost of developing a defensible safety prediction was probably in orders of magnitude greater than the cost of actually designing and building the repository. It's more than just orders of magnitude greater: we tried to alert them that even though the parameters weren't very clearly defined on safety predictions yet, they should anticipate a very big problem. That led them to set up their own research laboratory in San Antonio, Texas, to do similar predictions themselves, simply for the purpose of developing the capability to evaluate the predictions from the Department of Energy.

Technical Difficulties, Delays, and Shrinking Rooms at WIPP, 1978-1985

Pigford: That's what I did on that panel. And then you asked about the board of radioactive waste management?

Wilmsen: Yes.

Pigford: What date was that?

Wilmsen: That was 1978 to 1985.

Pigford: Okay. That's a standing board of the National Research Council which is the research arm of the National Academies of Science and Engineering. It's that board that originally recommended geologic disposal back in the early sixties, so it viewed its function as following the general problems of radioactive waste management and sensing out new issues as they arose. And that board creates panels and committees that make in-depth studies.

The committee that I chaired in the early eighties on the waste isolation system study had been set up by that board, which then defined the problem and encouraged the National Research Council staff to seek contract funding for the study from the Department of Energy. When I joined that board, the WIPP project was going and so it established a separate committee to give scientific oversight--not programmatic direction, but oversight--to the WIPP project. And I was assigned to that committee also.

It reported I think once or twice a year to the Department of Energy which paid for the study.

That was fascinating. I got to go down into the tunnels that were being dug at the WIPP facility. It was exciting to be 3,000 feet down in salt--very dry rock. We'd already learned about its plasticity, and we had been told that once they did a room or a tunnel, the salt is slowly moving in on you to close the tunnel. We were happy that its rate of inward movement was predicted to be so slow.

Well, this turned out to be a problem. They originally thought--and I think I started on that in 1977--they would have completed that facility and would be loading it with waste by around 1980 and so they were getting some of the first waste rooms ready. They would load those and then excavate others as the time went on. Well, the project ran into many, many problems.

It did not have local opposition. It was encouraged by the local farmers because there wasn't much business in the area of Carlsbad, New Mexico, but they had opposition from the state. Not strong, but they kept running into new problems that they had not anticipated--technical problems. And they were slow or inept in dealing with them.

Wilmsen: What were some of the technical problems?

Pigford: Well, first, how to make the safety analysis. The Environmental Protection Agency had at that time issued its own safety criteria which are extremely complicated, and it wanted a probabilistic

analysis of doses to future human beings for the next 10,000 years. So the project had to think of what are the possible ways that radioactivity could get out.

I've illustrated one way, which is human intrusion. There are other ways like slow dissolution of the exterior boundaries of the whole salt medium, which are miles and miles away, by flowing ground water washing all the salt away, for example. That was supposed to take billions of years. However, that required probabilistic analysis, which means you consider what are the fundamental mechanisms and what is the rate of movement of that boundary. Well, to measure these things you have to have parameters. You had to develop a probabilistic distribution of each parameter, like the flow of water around each part of the boundary.

There were so many variables and parameters that could affect the overall performance that they put it all in an enormous computer code and it took them about a year with the best computer to run a complete probabilistic analysis. That means that it was really complicated. The analysis was so complicated that you couldn't tell what was really controlling the result. Our committee was very unhappy with such an approach because we wanted analyses that would give us understandable results quickly. And I think just the whole business of setting in place a calculational technique took years and years.

Then there were technical features that hadn't been thought of. The waste comes in there in steel containers. They look like big donuts, about eight feet in diameter and two or three feet high, containing waste inside. They are then stacked in one of these salt rooms. The question is, if some of the extraneous water comes in, as from an inadvertent mining exploration, how rapidly does it dissolve radioactive material? Well, first it reacts with the iron container.

When it reacts with water, the iron container makes hydrogen. We don't normally worry about that hydrogen from corrosion, but if it's all confined by a tight cocoon of salt, it could build up hydrogen pressure. Some analyses showed that hydrogen pressure might be enough to exceed the static pressure from the over burden, which would fracture all of the salt above it. Well, it turned out that was wrong. That was not the correct result, but it took a long time to deal with that. And that has set them back a few years.

As the delays increased, the salt was creeping in on those

rooms that had already been excavated. These were big rooms filled with experiments, for measuring corrosion rates and so forth. So to preserve the rooms, they had to go back in them and put in rock bolts to the ceiling, which means you drill a hole in the ceiling, which is into the salt above the ceiling, and it penetrates ten or fifteen feet up and expands. Then you tighten up the bolt, and it supports the rock that would otherwise be fracturing and filling up the room. They had to rock bolt a lot of rooms to keep the ceiling from falling in because there were more experiments as a result of those delays and new questions. And it just mounted, and it's one after another.

All of the questions were on specific details. For example, the salt is not uniform sodium chloride--table salt. Most of it is pure, solid crystals, but there are little layers of anhydrite, which is a form of gypsum, calcium sulfate, only about a quarter of an inch thick that runs along through the salt. We could see some of those layers exposed when they dug the room. There was the worry that the radioactive gases formed, released from corrosion, could permeate through those layers--which are permeable; they're not cohesive like salt is--and find their way someplace out of the salt deposit. And it took lots of experiments and lots of calculations and analyses to analyze that problem. It didn't turn out to be a major problem from my recollection.

I think the delays caused major problems like having to stabilize rooms that had already been excavated. And they'd already excavated a main horizontal shaft, miles long, from which would radiate disposal rooms as they go through it, and that had to be preserved.

Problems with the EPA Standards for Dose Limits

Pigford: Then, along the way, EPA's criteria for safety for geologic disposal were challenged. The reason they were challenged is not the best of all reasons. The original EPA criteria would cover any geologic disposal facility including the tuff project. By that time Congress had narrowed things down to tuff for the commercial waste. When water gets to the waste, it will release a small amount of radioactivity and radioactive carbon which is present in the metal cladding around the waste. That will form carbon dioxide, which then gets into the air; and they found that

because tuff is above the water table and has air flowing through it, flowing upwards, the carbon dioxide gets drawn up to the surface of the repository. It's a very small--using the most conservative calculations--small amount. The calculated doses to future people using the usual approach--you're assuming there's a person who lives there and breathes that air continuously--were far below the acceptable limits.

Wilmsen: Is this carbon 14?

Pigford: Yes. It has a half life of 5,700 years, so it will stay around for quite a while, and it gets to the surface rather quickly. The air flow is fairly rapid. It may get there in a year or so--a few years--but that's quick for a repository scale.

But the EPA standard, which I said I thought was faulted in the first place, didn't use that kind of criteria for safety for geologic disposal. Instead, EPA originated a standard based upon limiting the accumulated dose to large groups of people--to large populations--whereas the criteria I'd been talking about, and we used in our analyses, were the kind normally used to protect the maximally exposed individual, with the reasoning that if you protect him, everybody else gets less than that and it's okay. But I must admit, it's always reasonable to say if you find the whole world getting exposed to the same dose as that maximally exposed individual, gee, it says you ought to think about doing something better. And there is a principle that is followed and is used occasionally.

Well, EPA addressed only that issue, not the maximally exposed individual. It assumed in its calculations that whatever radioactive material is released from the repository gets immediately distributed over all the surface waters of the northern hemisphere. It ignored the fact that carbon-14, as carbon dioxide mixes with the air, and it used worldwide northern hemisphere calculations of what fraction of surface water is used by humans for growing crops. It assumed a northern hemisphere population of around seven and a half billion people--which may be the right one, I can't quarrel with that--and it found then that over 10,000 years, the cumulative population dose would be above what they set as the allowable dose--this is allowable for the whole northern hemisphere.

Well, in spite of the fact that the dose to the maximally exposed individual breathing the air, which is a mechanistic analysis, is just trivial, their assumptions, without considering mechanisms of how it gets spread and dissolved in all of the

water, led to rather unreasonable limit on the worldwide dose. It said the allowable dose shall be small enough that, over 10,000 years, no more than 1,000 incipient cancers should result, which is an extremely low cancer rate for seven and a half billion people; it is so far below what is attributable to the spontaneous natural cancers, it is out of sight.

Well, that EPA proposal for its new standard came to the board of radioactive waste management in 1980. We analyzed it and we said it was so far away from the normal approaches to health protection and it was just based upon imaginary processes, that it should not be used. Instead we urged the use of regular individual dose criteria. But the EPA insisted and that was embodied in the standard that came out and governed the WIPP project.

The WIPP project didn't have any problem because they found that their gaseous radioactivity really didn't go any place. But Congress learned from DOE that it couldn't meet the EPA standard on the basis of this carbon 14, so what Congress did is it said the EPA standard would not apply to Yucca Mountain.

Now that's the wrong approach. It should have said the EPA standard is not the right standard for health protection, period, for certain reasons. But they didn't even ask the National Research Council to evaluate that. And it dictated that there should be a new standard written for Yucca Mountain, which they obviously wanted to not impede at all, and they said it should be on the individual dose limit. Then they asked the National Research Council to do a study as to what that should be. And I'll get to that later; I served on that one.

Wilmsen: But that's back to the maximally exposed individual, or is that different, again?

Pigford: Congress didn't say how to calculate. Then our committee got into that argument with rather disastrous results, which is another story, and I've been spending a lot of time on that.

Officially Yucca Mountain still does not have a standard. But in our analyses of its performance and also in DOE contract analyses, we have been assuming that it's calculating doses at whatever time they occur to the maximally exposed individual. I guess I should shortcut it to the end on that, and we can come back: Yucca Mountain is not even close to meeting that standard.

But I should say, meanwhile, EPA had not come out with a new standard. Nor had NRC, the Nuclear Regulatory Commission.

But in the summer of this year--July and then in September--both EPA and NRC came out with their new standards, which are based mainly on individual dose, which is the right way to do it.

But how to calculate the individual dose is the problem. And it's because of these problems of elapsed time and so forth, where the difficulties really are.

But what I have presented in several papers is that EPA and NRC are greatly departing from what they promised the public back in the 1960s--that future people will be as well protected as present people are from licensed facilities. Congress has itself tried to intrude every year for the past four years in its proposed legislation--not enacted, but proposed--and the sense of it is to forbid EPA from writing its own standard for Yucca Mountain. Congress seems afraid of EPA messing up the whole system.

And Congress has tried to write its own standard, which is a farce. As an example, it says, "Use the individual dose standard, but don't apply it to the maximally exposed individual.

Calculate for some prescribed area, like all activities within a thirty or forty mile radius around Yucca Mountain, what the doses would be to people at various locations. Then calculate the average dose, averaged over all that population." Well, when I first saw that proposal, which initially came from the Electric Power Research Institute in Palo Alto, I saw the danger, because the average typically is so far below the maximum. So one of my students and I calculated that for Yucca Mountain--calculated the plume of contaminated water that would go down in the lower aquifer. It's not a very wide plume. Then we calculated that dose and diluted it by the people who never see that plume who live in the surrounding area, and there's a thousandfold change in the dose.

I have spent a lot of time writing papers that can be used to counter the proposed legislation. Their proposed legislation has never passed Congress (I think maybe largely because of a threat of veto from the president), and I don't know to what extent my efforts have influenced that. I think the political objections from the State of Nevada have been very strong. So I'm getting off the subject, but we are talking about the problems in Yucca Mountain. In fact, what was the question that led to this?

Dissenting Opinion on Yucca Mountain Standards, 1995

Wilmsen: Let's see. A long time ago, I had asked about the board on radioactive waste management.

Pigford: Oh, yes. Well, the board on radioactive waste management in this instance, at the request of Congress, formed a committee called the TYMS committee--Technical Bases for Yucca Mountain Standards --and our assignment was to analyze the technical aspects of how to set an individual standard and implement it. I was on that committee. We served for, I think, three years, and the report came out in 1995. It was a tough job.

The committee made a clear recommendation, which I supported, based on the calculations that had been reported. By this time many calculations--some from my work at Berkeley, but more importantly those from the DOE project itself--showed that future people will receive the greatest doses far beyond 10,000 years; high doses in the 100,000 to 200,000 year era will be much greater than the doses in 10,000 years.

We knew that the EPA was going to favor the first 10,000 years because they had in their original standard, and they knew that the project was going to favor cutting off at 10,000 years because it's not difficult to meet a reasonable standard on that basis. It's sort of like underground testing, except shifting the time scale; it's after 10,000 years that things get worse. And so our board made an unequivocal recommendation that there was an adequate scientific basis for predicting these things in the era of 100,000 years and beyond, and I contributed to that. But DOE, EPA in its recent proposal, and NRC (the Nuclear Regulatory Commission), all three, have come out in favor of the 10,000 year cutoff. They're out for public comments, so these aren't official standards yet, but I think they will be adopted that way.

I disagreed with the rest of the committee on some of the points.

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Pigford: I disagreed with them on how to identify the representative individual whose dose should be calculated for determination of safety. I stuck to the classical definition that we'd been using through the years for licensed facilities, such that you look at a person near the boundaries of the facility. In this case, he

gets his exposure by drilling a well that goes down into the aquifer not knowing it's contaminated. It's a farming family extracting water for potable water and for growing crops. And a large part of his diet is from food grown that way. And that's typical of the approach that's taken in current licensing.

The rest of the committee thought that was too conservative, and so they proposed a demographic survey of the entire vicinity to see how people lived. Then they had a vague idea of making a probabilistic analysis, assuming that's the way future people will live. And it could be, but what do you do with that result?

They tried to make an illustrative example of a probabilistic analysis, but they weren't very clear. It was so fraught with mathematical errors that I concluded that nothing could be calculated with that technique. I later discovered from a paper given by one of the members, after our report was in--and he's the one who wrote the technique--that it amounted to a vicinity average dose in the long run. It boils down to the same unjustified leniency as that recommended by the Electric Power Research Institute and appearing in proposed congressional legislation.

The EPA supports my approach in its recent proposal. They support calculations of the dose to the maximally exposed individual. I'm glad they do. But they make the problem trivial by their 10,000-year cutoff (I'm against that), and I think that will undermine public confidence, as well as confidence by the scientific community. It will be one of the things that will keep this project from succeeding. In fact, I'll continue to write papers opposing it because of that. So you see, I'm getting identified as an activist, [laughter] which I'm very proud of.

Wilmsen: How did other members of the committee react to your dissenting opinion?

Pigford: Well, some of them were embarrassed. One of them told me he agreed with me completely. But it never got resolved. The chairman is more of an administrator than a scientist. He had a career position in the Department of Energy and he wanted to see the report get out, so they used increasingly vague wording on the contentious points, which is the wrong way to report on a problem. If the contentious points have merit, they should be highlighted, really. So I simply submitted my own supplementary statement, which is part of the report, agreeing with some of it and disagreeing with some of it.

Wilmsen: How did Congress react?

Pigford: Well, you see, even though Congress requested the study be done, that study was officially funded by the EPA. Congress told EPA to fund it, and so our report was submitted to EPA. When EPA came out with its new proposed standard in September of this year, it, according to its own rules--and this proposal is out for public comment--had to give reasons for adopting and not adopting various positions. It had to deal with our report, and it therefore adopted my approach on the maximally exposed individual, except they said calculate for closest individual now living in that vicinity. That happens to be at a little crossroads which is seventeen miles away at a house of prostitution--and they get water from a well--in fact, there is some farming done not far from that. But I had recommended that they follow the practice we have always been following of putting a hypothetical individual closer, where the public could be in the future. And EPA ignored that.

The public does not live closer now because the land is publicly owned just on the other side of this township [at the crossroads] and is not available for farming or what have you. But you can't assume that that restriction will last for a thousand years. That would be ridiculous. But the main thing EPA did was to adopt the 10,000-year cutoff, and it did so in spite of the fact that I have written a lot about that. Through the years, since that report came out from that standards committee of the National Research Council, I've seen how much the EPA and the Nuclear Regulatory Commission and the contractors want that 10,000-year cutoff. Because they don't succeed at all if you let it go for 100,000 years, due to the high doses. So EPA's official argument for the 10,000-year cutoff is dim.

Well, there's a lot of uncertainty on all of these calculations, which is correct. And you'd expect it to be greater the farther in the future you predict which is how they defended it back in the 1980s. They said, "And the uncertainty is so great after 10,000 years, that the results don't mean anything." Well, the strong united decision of our committee was that's not true, that the predictions beyond 10,000 years are remarkably well-based scientifically. EPA still uses that old argument.

I have then done uncertainty calculations and pointed out that the uncertainty actually decreases after 10,000 years, which many people think is counter-intuitive. But it's a rather interesting result that could be obtained easily from our

analytic theories, and I've written two publications on that. What we're interested in is not the uncertainty of any particular dose at any time but of the maximum dose from the maximum concentration in ground water. That maximum concentration is so protected from uncertainties by physical forces and parameters--and sensitive to them--if you ask for the concentration at the edge of the contamination band going through the rock, that's a hard thing to predict because it's being torn apart by dispersive processes. But these concentration bands are tens of thousands of years long. It takes that long for the waste to dissolve. And the maximum concentration occurs far later than the front of the concentration band and it's not subject to dispersion. And the results are really dramatic.

I also took some of the calculations DOE has published on uncertainty. The results are a little more difficult to interpret, but I pointed out that if you look at them carefully and reinterpret them, they show the same thing my calculation does. EPA has not addressed that issue. It simply repeated its claim from back in the mid-1980s that the uncertainties become greater with time. I can't claim too much, but always what I have calculated is the calculable uncertainty. We have the best available predictions of parameter distributions and probabilistic distributions that affect the calculations. Those are what I deal with, and I can calculate uncertainty of any result from that.

But there's always the uncertainty of our models themselves. And this is a rather bold assertion to say that we know the models, but at least they are based upon belief in the science. I believe solubility, which is one of the parameters, is a fundamental measure of limit of concentration in water, and I don't know anything that's going to change that with time. But that's, again, a hypothesis.

So I say within the realm of the science that we know and are able to calculate, the uncertainty of this particular result is such that it decreases with time, and that's the result I presented first in Stockholm in 1998. I presented it again in Stockholm at a different international conference this year [1999], and the result has been noted by many people. I think people who have looked at it scientifically--and I think with some belief at first that it's counter-intuitive--do seem to accept it.

But I think Congress clearly, by its attempts to come up with an even more lenient legislation than EPA has come up with

by going to this vicinity average dose, is interested in doing what it can do to help the project--even though I think it will kill the project by fatally damaging the credibility of the whole system of regulation of Yucca Mountain to protect public health and safety.

VII LOOKING FOWARD, LOOKING BACK: PLANNING FOR FUTURE PUBLIC SAFETY,
AND REFLECTING ON CAREER, FAMILY, AND THE NUCLEAR INDUSTRY AS A
WHOLE

[Interview 7: December 16, 1999] ##

Dealing with Possible Conflicts of Interest in Committee Work

Wilmsen: Okay, I had a couple of follow-up questions from last time. You mentioned that you had served on a committee of the Department of Energy's to review the site characterization for the Basalt waste isolation plant in Hanford, Washington, and you were also on a committee for Rockwell and Westinghouse on looking at that same facility.

Pigford: Yes.

Wilmsen: I was wondering if you could just talk about how serving on the industry committee was different from serving on the Department of Energy's committee.

Pigford: My recollection is that those two committees combined rather than having two independent reviews.

Wilmsen: Oh, I see.

Pigford: They were officially two committees, but we all sat as one body and turned out one report.

Wilmsen: I see. Was there any concern that there might be a conflict of interest having the industry and the government acting as one committee?

Pigford: Well, it's always a legitimate question. Actually, Rockwell and

Westinghouse were both contractors for the government there. Oh, yes, here's the way it happened: the Department of Energy officially wanted its own committee, but to set up a procedure for defraying the expenses of the members of the committee--the Department of Energy is a huge bureaucratic organization and so it asked the operating contractors involved, Westinghouse and Rockwell, to then put me on officially as a member of a committee for them. They're smaller, and they undertake such things at the bidding of the Department of Energy, so they put me on officially as a consultant. So really even from the beginning it was intended to be one committee. It officially was, too.

Wilmsen: So acting as one committee, then, the concerns that were of possible conflict of interest were just not raised?

Pigford: Well, they weren't raised in that context because it was simply paperwork to provide a means of paying our traveling expenses. You see the contractor was there to carry out DOE's bidding, so it was all a DOE-sponsored evaluation.

Wilmsen: I see.

Pigford: Now always conflict of interest would come up--oh, I see. Officially I could be looked upon as being on the payroll of Rockwell and I would be sitting there reviewing their work. Well, actually, this situation occurs in almost every committee you serve on, whether it's industry or government, because somebody has to pay your expenses to go up there. Real conflict of interest issues arise really in a different capacity. Are you also on the payroll or otherwise obligated from some other interested party who might have different motivations than the Department of Energy and its contractors? And we always go through that ritual of examination of conflict every time we are nominated to serve on a committee.

But as I say, in this case it's only an accounting issue, although it might appear to someone from the outside that it could be a conflict of interest.

**Scientific Master to the Court on a Lawsuit Concerning Wartime Releases
of Radioactivity at Hanford, Washington, 1994**

Wilmsen: Then you also served as the scientific master of the court

concerning a lawsuit around radioactive releases from Hanford?

Pigford: Yes.

Wilmsen: How did that experience affect your assessment of the nuclear power industry as a whole?

Pigford: Well, my service to the federal district court occurred at a much, much later time. I think that assessment probably occurred in 1994-95, probably in that era.

The issues there were safety issues involved really in the operation of the Hanford facility for the government and its contractors some fifty years earlier, mainly because at that time the main issues regarding releases of radioactivity during World War II were at the Hanford facility. I guess officially there were still some releases from Hanford on the books in the 1960s and maybe early seventies. I can't remember too well. But the part of the problem that was under my purview as scientific master were the releases during the war years, and some of those releases were large releases.

At that time, the operating contractor, which was then the du Pont Company, mainly did not have--nor did anyone have--enough information as to what radioactive materials would be released when you dissolve irradiated uranium fuel. There were copious quantities of radioactive iodine that came out; it was volatile and got into the air.

Now, I first got involved in that kind of issue--namely, releases of radioactive material to the air--when serving on reviews and doing research in the late sixties and seventies for the nuclear power industry. By that time, we, and the whole technical field, knew enough about that problem. We knew it was an important problem. I think I've related in previous discussions here that the whole technology was evolving on how to control radioactive releases from nuclear power plants, and we learned along the way that those releases had to be much smaller than had been thought allowable even at the beginning of the nuclear power industry in the 1960s.

So really that experience with the nuclear power industry, and also in my teaching and research and writing, in the sixties, seventies, and eighties--and it continues to be an issue--gave me some technical background to evaluate the importance of those problems that occurred at Hanford during World War II. So the two things were not overlapping in time, but there was that

sequence of getting into the Hanford issue later, much after the fact.

It was a fascinating issue. The records they kept in those days were not very good. The class action suits being brought against the government and operating contractors in the federal court were arguing that those releases--and they were mainly during the war years, some overlapping into the Cold War years of the 1950s--had damaged people. Clearly the government and operating contractors back in the 1940s, in the war years, realized that it was much worse than they had expected. They purchased a lot of land across the Columbia River as a buffer zone so that the nearest residence and farming families would be farther removed from the point of release, but even that did not completely satisfy the problem. So looking at the likely magnitude of those releases, the court decided that the plaintiffs--Indian tribes, citizens, whole townships throughout several counties--had a prima facie case, meaning they presented enough evidence to show it was worth a trial.

Then my role became assistant to the judge of the federal district court, Yakima, Washington, who had the thankless job of presiding over the case. He learned it was highly technical and he needed a consultant. There were arguments on both sides by the plaintiffs and defendants as to who this consultant might be. He asked them to come up with a list of names. And the potential damages were enormous, so each side dug in and wanted its own consultant. They spun around for over a year looking for somebody. Finally I guess they must have gotten to the bottom of the barrel, and my name turned up. Both sides agreed that I was okay, sort of like when I got selected to serve on the presidential commission for the accident at Three Mile Island: I was suitable to the environmentalists and I was suitable to the industry.

So then I got to be the scientific master. I'm wandering a little bit in my exposition on this, but I'll tell you at least what the nature of the job was: how to develop enough evidence to make decisions--because the records were faulty, and there were no records even of exactly how much release there was. Then, to know if the citizens--the plaintiffs--received some harm to their health, one needed to know what concentrations, what doses in the air they might have been exposed to. There were no measurements of that. Those measurements would have had to have been taken at a large number of locations throughout several counties, and the doses depended upon the weather conditions. There were no records of the weather conditions, or very bare records.

So the government, the Department of Energy, decided that it would undertake to set up a project to be carried out by the operating contractor--which I think was Westinghouse at that time--to develop a detailed map of what actually did happen. All of this is now happening during the era of 1994 and 1995. And the operating contractor had to go through millions of pages of poorly kept records--just snatches and notes here and there--to piece together what these exposures might have been.

I usually don't like doing consulting jobs for litigation because if you are asked to be an expert witness on one side, the managers, the lawyers, from that side want you to say things that benefit them and not the other side, and to a person accustomed to scientific inquiry, that's a constraint that's very unappetizing. In this case the judge had no preference and he simply asked me to come up with issues and possible areas of inquiry that would give him enough information that he could then reach a decision. So they sent me tons of reports to review, and I wrote up briefs for the judge.

The case to my knowledge is still going on. It had already been in discovery either five or ten years before I got on the job.

Wilmsen: Wow.

Pigford: It has all the earmarks of an enormously complicated case--very difficult to resolve. If it has been resolved, I have not heard.

Wilmsen: Did getting involved in that affect how you felt about nuclear power or the nuclear war effort during World War II?

Pigford: Well, it was a crunched program in World War II, and clearly they did some things in a hurry and were surprised by some of the bad results--but not enough intervention. I have said before that the process that was developed by Glenn Seaborg, his work in Gilman Hall on the campus to develop a means of extracting plutonium from irradiated uranium fuel--which is what they were doing at Hanford--got the plutonium out but it left copious quantities of radioactive waste. It was that extraction process that also released these unexpectedly high releases of radioactive gases. And in hindsight it would have been better if they had done a more thorough analysis of the experiments to outline the dimensions of that problem. They should have done that, and I wish they had. That's all I can say about that.

It gets into the public domain as to whether it was worth it in the war effort. I think that's the wrong question.

Was it a reasonable approach? I can't even evaluate that, because whether that's reasonable depends upon, again, balancing the benefits of getting that plutonium out to make bombs versus the risks of an accelerated effort. All I can say as a scientist and engineer is I wish they had been more careful.

Site Characteristics of Yucca Mountain, and Designing for Future Public Safety

Seismicity and Volcanism

Wilmsen: I also had a couple more questions on Yucca Mountain. In some of the reading I've done, they talked about earthquakes in the area. For example, in 1992 there was a 5.6 magnitude earthquake twelve miles from the repository site, and then I guess there's also a fault cutting right through the site.

Pigford: Yes, there is a fault.

Wilmsen: So I was wondering what your thoughts were on the seismicity of the area.

Pigford: Yes, that fault cutting through the site is called the Ghost Dance fault. There's no doubt about it, it's an area that one would expect to be seismically active. In fact, the rock that they are excavating, in which they will make tunnels and rooms to deposit the radioactive waste, is the product of a very ancient set of volcanoes. Those particular volcanic eruptions, instead of making molten lava, spewed into the air enormous quantities of volcanic ash--the dust similar to the eruption at Mount Saint Helens so many years ago. That fell out and over the years consolidated into a rock similar somewhat to sandstone, but much more porous. That's called tuffaceous rock, or shortened into the word tuff.

Now, does that make it a bad site? I think a first reaction would be reasonably to raise alarm and a red flag on that. It's gotten a lot of attention, and I think deservedly so. It's

certainly still seismically active, as almost any place in this part of the United States. How important is that? Well, it's not nearly the same as worrying about siting a nuclear power reactor on seismically active sites, like Bodega Bay or Diablo Canyon, because there you have the possibility of disrupting engineered systems, such as emergency cooling, that are absolutely necessary to operate in case the earthquake shuts down the nuclear power reactor, as it will because there are earthquake sensors that will do that. But the main thing is to keep that coolant going in so the fuel doesn't melt. It can melt, even after the reactor is shut down, because of the intense radioactive decay heat from fission products in the fuel. That's where the issue is. And if the fuel melts, then it releases its own radioactivity to the surrounding room. In some scenarios it can also cause breaching of the containment vessel, and that radioactivity can get out into the environment. It's an extremely important issue.

In the case of radioactive waste disposal, geologic disposal, the radioactive waste that's put there--and I'll take the worst case: discharged spent fuel from nuclear power reactors treated as a waste, which is going to be the main waste in Yucca Mountain--still has enormous radioactivity in it, but it's been aged for at least ten years before it goes there. The average age will be twenty to thirty years, so the radioactivity that's remaining is a very important issue in terms of potential damage to human health if it gets assimilated into the body. It does generate some heat, but it's not very much. And one of the functions of a geologic repository is that the rock in between adjacent rooms, or adjacent tunnels where the waste is put, is sufficient to absorb that heat. So it causes only a modest temperature rise, and there seems not to be much chance of causing overheating or melting. That's an enormous difference between that and a nuclear power plant.

There's also a logical worry: can the shaking motion disrupt the containers? That's been studied a lot. These are not containers that are part of the overall system, that is, piping and instruments going to it and so forth. You can shake a container and have it roll on the floor, if it ever could get disrupted that much; but these containers are designed to withstand much greater force than that. Well, it's quite possible you can develop cracks in some of the wells, but in fact, because repositories have to be designed to protect the public for tens and hundreds of thousands of years, in the long run you don't depend upon that container very much anyway. It's the basic insolubility, for example, of radioactive materials

like plutonium or neptunium that prevents all except a tiny fraction of them from dissolving in the ground water. That insolubility is going to be there, whether that container is disrupted or not. So I'm persuaded that it's not an issue to forget about and it must always be brought up and reexamined, but I have come to the conclusion that it does not necessarily negate the site for waste disposal.

There's another possible issue. Suppose volcanic activity would produce a hot magma that rises up through the rock. I don't know if that's ever happened in that vicinity because, as I said, that vicinity was created by a different kind of volcano. There have been a lot of studies of the what-ifs on that, and the geologists and seismologists seem to conclude that the probability of that is extremely small. Just bringing the magma up to the level of the waste would not necessarily itself negate things, because that doesn't necessarily create a pathway to the environment. But if you made a lot more magma and it finally came to the surface of the earth and poured out, well, then that would be a worry.

But I have to rest upon what seems to be the consensus from very competent geologists of the probability of that for that region. They can evidently assess that by looking at the traces from earlier earthquakes going back literally billions of years, and they find that the probability of a magma-type excursion is extremely low.

Ground Water, Climate Change, and Long-term Warning Systems

Wilmsen: What about upwelling of ground water? I think people have raised that concern also.

Pigford: Yes. That, I think, is a more worrisome concern. In my opinion it's still a valid issue. There's one person who has taken some of the chemical data and said that it indicates an upwelling of ground water ages ago and it could happen again. We've had several special committees of the National Academies of Science and Engineering evaluate that particular issue and their conclusions in that case are that he has misinterpreted the data. Those are the kinds of questions and studies that are important, and it's one reason why they need a lot of time to bring them up and research them.

Wilmsen: Now on that, one thing that just occurred to me--you mentioned last time a computer model that took a year to work out--I was just curious if climate change data, like from dendrochronological records, had been used in that model. You mentioned that right now the site is a very dry site, not much rainfall, but over time--I don't know the dendrochronology of that area, but--

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Pigford: Climate changes all over the world to be sure. But what I mentioned about the long time to do the probabilistic calculations was in the connection with the WIPP site.

Wilmsen: Oh, maybe that was it. Yes.

Pigford: But the calculations are unnecessarily complicated for the Yucca Mountain site. And there is, of course, good evidence that we go through cyclical climate changes every 30,000 or 40,000 years. Some people say maybe the next one within the next 20,000 years--it's hard to predict. And in my view, because I've said before that the time period of concern, of possible release of radioactivity to the environment, is in the hundreds of thousands of years. A proper safety analysis must include these effects. And even though the newly forthcoming safety criteria from the Environmental Protection Agency for Yucca Mountain specify that emphasis will be given to calculations for the next 10,000 years, which I strongly disagree with (and I was on the committee of the National Academies of Science and Engineering which was asked to make recommendations for the scientific basis for new criteria, and we recommended strongly that the calculations should extend really almost up to a million years to look at the era when the maximum possible concentrations are occurring), at least that warning has stuck to the extent that the proposed new regulations say the calculations will extend as that committee recommended. But EPA still says that for safety determinations it will use the calculations for the first 10,000 years. So I'm just not sure that EPA will give them as much weight as I would have them do. That remains to be seen.

But at least those calculations are being made and are being extended. And how? Well, for example, the last pluvial age, the age in which there was a lot of rainfall, did come to the Yucca Mountain area; it was all over Nevada, all over California. The glacier, the big glacier which obviously created Yosemite Valley, didn't come down that far, but great glaciers like that came pretty close. And the geologists and the hydrologists, even the

paleontologists, are creating scenarios of what those periods were like--mainly what was the water flow like.

I think that can be done. So it would be wrong to assume that those areas, those periods, are being neglected. I've had the most experience in serving on review teams, scientific advisory teams, for the government of Sweden, where there they know a lot about this because clearly they had an enormous glacier there, and they expect to have it again. They'll have it every 40,000 years. It's a fascinating study which I've been involved in a little with them.

Where will future people be in the next glacial period? Well, the view in Sweden is that they will probably tunnel underground because Swedes love to dig tunnels in their wonderful granite rock; a lot of their industries are underground, a lot of their stores. That will be a fairly warm place because the surface of the earth will be protected from the extremely cold, cold winters that come from exposed land. The bottom of the thick glacier is receiving some heat from geothermal energy, and that's what keeps the earth warm, in fact, hot down below. So the Swedes tell me that might be a pleasant place to live.

Well, you've got to protect those people, and can you predict it? Well, I was amazed. The glaciologists and hydrologists in Sweden can re-create what the moisture conditions were under the glacier. And actually they are much dryer than if the glacier were not up there, because the glacier, as long as it's solid water, ice, it doesn't recharge the ground water. And they're not necessarily bad periods for protecting the environment; you just want to be sure the people don't try to dig their homes near the waste. That's what I worry about. They encourage the kind of activities that result in human intrusion, and that's a problem which is always going to be there, and it's legitimate.

Wilmsen: What about working on some kind of long-lasting warning system?

Pigford: Yes. Well, that's what you go to when you want to try to minimize that problem. This country has probably spent more money--mainly on the WIPP project where it cut its teeth on this issue--on how to discourage future people from intruding on the repository. And as long as we have institutional control, like a government and records, it seems reasonable, unless there's a terribly decrepit society, that control would be fairly easy. But it's fanciful to let it go at that. So what kind of warning signs can you build now that would discourage future people?

Mainly, what kind of signs will survive? Well, there've been studies of building a stonehenge-type structure above ground, but archeological records tell us that the ones we see were built maybe 5,000 years ago, and that's not very long. It seems reasonable that you could build something that would last longer, but where are they? Well, I don't think that should discourage us, and I think probably such things will be built. But I must caution that there are no definite plans.

Making the Repository Watertight, and Reducing the Solubility of Uranium

Wilmsen: Now, in reading your article "Getting Yucca Mountain Right"²² where you talked about a couple of alternatives for building a barrier to keep moisture from getting into the rooms where the radioactive waste is stored--one was filling the spaces between the containers with some kind of chemically reducing agent and another was using a two-layered backfill, sand and gravel layers --it struck me that that would add incredible cost onto the building of the repository. Is that a fair assumption?

Pigford: Well, I think the people on the building part of the repository would complain, because a contractor doesn't like to find things that increases cost, say, by a factor of two or something like that. And I'm just using that number hypothetically, but the cost of disposing of radioactive wastes has little to do with the cost of the repository. The main cost is to develop enough information to convince people of its safety.

There are two kinds of cost: one, developing the data and presenting the analyses that are persuasive and conclusive, and secondly, the cost of a delayed and protracted program. That's always true of a complex engineering system. The engineering contractor wants to get the job done as soon as possible because it otherwise continues, and there's interest on investment to be paid, and so forth and so on.

So the Yucca Mountain project resists such fixes as that. I

²²Carter, Luther J., and Thomas H. Pigford, "Getting Yucca Mountain Right," The Bulletin of Atomic Scientists, March/April 1998, pp. 56-61.

think their main risk is that they feel they have a commitment to a certain schedule: 2010 for first loading. That is the official date, and they believe that incorporating a significant design change like that would add several years to the program and that's just not the way of doing business. But they do not claim that there's an urgency to put radioactive waste in the ground by 2010.

At least I believe the government and its contractors believe that monitored surface storage, especially if they built a national interim storage facility like some other countries like Sweden, is really very reliable--except for only the very long run if your institutional controls are lost. That's the main reason why we have geologic disposal.

So the resistance to this is the debits from delays in schedule. There can be many, and the more you delay, the more reasons the public--and not only the public, but the whole engineering scientific community--has to say, "See, it's a much tougher job than you ever realized." And that's true, but at least in the view of Luther Carter and me, and, rightly many other people, the most important thing is a persuasive, reliable design for safety. And I don't think they have it.

The Yucca Mountain project even in 1995 issued a report showing the effect of various alternatives, and there was only one alternative in their report that to me showed it would meet the safety requirements with a suitable margin of safety. That was the one of putting--in our paper we called it a capillary barrier--in a backfill of layers of gravel and finely divided sand surrounding each waste package. Water coming down from above would tend to follow by surface tension the fine holes in the capillaries, and will not go through the larger holes in a bed of gravel where the surface tension effects are much, much smaller. So there you can expect that the incoming water will be diverted around the rooms and not get to the waste containers.

It's not a new idea. The Chinese incorporated that technique 4,000 years ago to protect their burial sites--some of them, anyway. You've seen the write-ups and pictures of this amazing discovery of the large underground rooms where the clay warriors, thousands of them are marching along, not moving, to guard the emperor.

Wilmsen: Yes.

Pigford: That's been preserved by an overhead mound of capillary barriers.

There are similar capillary barriers in Japan, dating back to the historic and prehistoric times, especially up in northern Honshu and Hokkaido.

Wilmsen: Ainu?

Pigford: No, it's before the Ainu, or it may be the Ainu and I'm trying to think of the right word.

Wilmsen: Jomon?

Pigford: Maybe it's Jomon. Are those associated with the Ainu?

Wilmsen: Some people think that possibly they're the ancestors of the Ainu.

Pigford: Okay. And they are the pit-dwellers.

Wilmsen: Yes.

Pigford: Some of the excavations have shown that they also used this technique which they might have imported from China. China's history and archeological evidence goes back much farther.

That's the technique which I think should be incorporated, and many of my colleagues who are watching the project carefully and are involved in it one way or another agree.

I should point out that that technique works only when there is an interface between air and water in the rock, because it's that interface where the surface tension is created. That surface tension is more powerful, the smaller the radius of the pore--the greater the curvature of the material where the interface is. And that's one of the great features of Yucca Mountain, putting waste above the water table in rock that is unsaturated with water--not saturated. Air moves upward through the mountain--it comes in from the lower sides of the mountain--and water moves downward, counter-currently. It's the only repository project currently in the world that I know of that is using unsaturated rock, and it's uniquely equipped then to use the capillary barrier.

There is a proposed project in Australia for geologic disposal of radioactive wastes. It's not proposed by the country of Australia--they don't have any high-level radioactive waste--but proposed by entrepreneurs who want to get the Australian government's permission for them to build such a repository and

let the government then reap the supposed financial rewards of burying other people's waste. Because Australia has large areas of land in the outback and other places where there's not much rainfall, they could, I'm sure, utilize the capillary barrier approach. Can't do it in a granite repository because you go down 1,000 feet in granite or 2,000 feet or 3,000 feet, and it's below the water table and so there are no surface tension effects, no air.

The other approach is to solve one of the problems unique to Yucca Mountain. It arises because air is in the mountain and creates an oxidizing atmosphere when the waste containers finally develop some perforations by corrosion or cracking or whatever, which is going to happen some day, then air-saturated water can weep or permeate into the inside of the container and start dissolving the solids there, like the uranium dioxide in the spent fuel. If it were a granite repository, there would be no air in the water.

In fact, the water in granite is in a chemically-reducing environment because of dissolved ferrous iron, which is a reducing agent, and the solubility of uranium dioxide, which is the waste material from spent fuel, in reducing water is extremely low. Extremely low; it works much better than it would work at Yucca Mountain.

That's a major technical problem in Yucca Mountain--the fact that it's an oxidizing environment, and the uranium solubility unfortunately becomes much higher because of that. It will dissolve more rapidly and would release radioactive fission products along the way, and you have to work harder to keep them from giving enough radioactivity that eventually gets to the ground water. Fortunately some radioactive materials like plutonium preserve their solubility even in likely oxidizing environment like that and they're no problem. But others are, so that's the reason for the two proposals.

I think in both cases, we know how to design a system to do this; it's not something Carter and I imagined. We studied many, many reports, and experiments in other places. But it would require a redesign of the waste emplacement system, and it would be more complicated to load the repository with waste. The current concept is you take these big containers which are made out of a very expensive low corrosion metal, put them on flatbed railroad cars and wheel them in on tracks, all remotely controlled because they're so radioactive, and wheel them into the room. And as soon as the track in that room is filled with

cars, you then plug up the room and do another one.

There is empty space between the container and rock walls of the room, and water can drip from the ceiling onto the waste containers, and it's diverting that drip that needs to be done. You'd have to have a mechanical system that moves or blows in--it can be done pneumatically--gravel, and then on top of the gravel, sand, so that you really fill the rest of the room with this diversion barrier.

Then you could have chemically-reducing material. When you make the waste container, you simply take about thirty spent fuel assemblies from pressurized-water reactors, each one about nine inches across and sixteen feet long, so it's a big container, and you put them in the container. About half of the space in the container is simply void space filled with air. If you simply filled that with a chemically-reducing material, it's easy to show that there's enough chemically-reducing material that it would remove the oxygen as fast as it comes in for hundreds of thousands of years.

It would solve another major waste disposal problem because one of the best chemically-reducing materials is additional uranium dioxide. We have about 500,000 tons of that being stored on the surface of the earth which are the byproducts of making enriched uranium for weapons and also reactor fuel--isotope separation. It's purified uranium; of course it's radioactive. Its half-life is mainly billions of years, but as it sits there, radium grows from decay of uranium and it's an extremely toxic material. In a few thousand years it will grow to enough concentration that it becomes dangerous. It peaks concentration in a few hundred thousand years, and that's a lot of waste.

The government refuses to call it waste because they claim it is beneficial material with a market value. If we ever have breeder reactors, it could fuel breeder reactors for millennia--many tens of thousands of years. But we don't have breeder reactors and are not going to for a long while. It looked for a while like there was a market to make tips for artillery shells because the high density of uranium--enormous density, much greater than lead--makes it the best material for penetrating the armor of tanks. And we're using it, but to the people who want to sell it, there are not enough wars to give a good market for that, fortunately there aren't, and so there sit the 500,000 tons of uranium. Officially it's not called waste, even though they're beginning to realize there's no market for it.

Because something in the government regulations say if it's officially a waste it must be treated quite differently than the way it's treated now, and that would be very expensive. Well, I think that's a national disgrace. It turns out that you couldn't dispose of all the 500,000 tons this way in Yucca Mountain, but it would be a good fraction of it. I think my calculation is maybe 300,000 tons would be needed.

But then if you got that idea going, if we ever build a second repository, then you've got it. It doesn't take any redesign of the rooms or anything. I think the one that has the greatest potential is the capillary barrier, but both of them should be used.

Interim Above-ground Storage of Nuclear Waste to Deter Proliferation of Nuclear Weapons

Wilmsen: Now you've also argued that there should be a temporary above-ground storage facility until the repository is completed--one location where it can all be assembled. And it sounded like you were saying a few minutes ago that that could be safe in that the main issue is having the institutional capacity to keep it going. That also raises the question of non-proliferation of plutonium, which you've also written about, or are starting to write about.

Pigford: Yes. Yes, I believe that our country and really all other countries in the nuclear business are not giving enough attention to the dangers of weapons proliferation from the plutonium made from nuclear power reactors. Although, our country, I find, is most sensitive to that issue.

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Pigford: I do believe that of all of the disposal techniques for spent fuel--at least in this country where all of that plutonium is from nuclear power--the disposal techniques in geologic disposal are the best way of reducing the proliferation risks. I think we sorely need the above-the-ground storage facility for two reasons. One is to give enough time to do Yucca Mountain right, because they will never make that schedule, and they should be examining alternatives and doing a better job in terms of what they're doing now. Having that national storage facility will take the pressure off meeting an earlier deadline. The other

reason is it would also give us a safer, more secure place than where the spent fuel is being stored now, which is at reactor sites, some of which are decommissioned sites--no, shut down. Some sites have shut-down reactors; they can't decommission until they get rid of that spent fuel.

And essentially there's no place to put the spent fuel. Indeed, some utility companies are negotiating with the Indian tribes to put in a centralized storage facility which they will pay for on Indian land. And that might help, but in the absence of that, I--we have scores of utility companies that own nuclear power sites. And I think they are all well motivated and they have good guard forces to, among other things, avoid theft of their material. But in the long run, when a nuclear plant is permanently shut down the self-interest of an owner to continue maintaining storage facilities tends to diminish and gives way to doing it just because the government requires it. That will happen to those sites where the reactors are shut down. Then surely that's much more vulnerable to terrorist action to steal spent fuel, for the purpose of obtaining plutonium, than if it were at a federally owned, carefully guarded central facility. You're replacing scores of dispersed facilities, each operated by a different entity, with one central facility.

I don't think that central facility will guard that spent fuel of plutonium as well as geologic disposal, although that's arguable. Some people feel like there's going to be too much temptation to intrude on a geologic disposal facility to steal the spent fuel and get the plutonium. It's going to be hard to do, but that's still arguable.

[Luther] Carter and I feel that geologic disposal is in the long run much safer, but the main thing is to get that interim facility in being now. The fuel all officially belongs to the government, to the Department of Energy. The Atomic Energy Act, or the Waste Disposal Act of the 1980s committed the government to take title to it in 1998. The Department of Energy didn't have any place to put it, so they tried to not take title to it. The courts found them obligated to take title to it, but said that they have to simply make arrangements for its storage, and it appears that what they're doing is simply paying the utility companies to store it. There we are back in the same box, so that's why we recommended strongly the interim storage facility.

Wilmsen: Now how have your experiences evaluating nuclear waste repositories and your overall research on nuclear waste management affected your view of the nuclear power industry as a

whole?

Pigford: Yes, before I answer that, we should return to the question of proliferation, because I'm actively involved in that from another point of view, as well.

Wilmsen: Okay.

Pigford: Shall we do that or take your question?

Wilmsen: Well, as long as we're talking about nuclear proliferation, let's do that.

Pigford: All right. As I have said before, even though I'm retired, I like to keep my hand in on technical issues, and every year I get more into public policy issues related to nuclear energy. I've been writing a lot about the need for better safety standards for geologic disposal, giving lots on papers on those, going to international meetings, even trying to influence Congress on its proposed legislation. I don't remember whether we got into that or not. Did we?

Wilmsen: Working with Congress on proposed legislation?

Pigford: Not working with them, I'm working against the people who proposed it.

Wilmsen: No, I don't think we did get into that.

Pigford: Well, I'll come back to that in just a minute then.

Wilmsen: Okay.

Pigford: I've achieved what results I think I can achieve on influencing the standards for geologic disposal, and I have not been completely successful, but along the way, I got involved in the question of proliferation of nuclear weapons, which every year I become more and more alarmed at the actions and the possibilities. In 1994 and '95, maybe '93, I served on a committee of the National Academy of Science which was to study means of disposing of surplus military plutonium. We haven't talked about that.

Wilmsen: We mentioned it. We talked a little bit about it.

Pigford: All right. Then to review it briefly, there the study was motivated because in a bilateral agreement with Russia, a certain

number of the former nuclear missiles are being demolished in each country. And the nuclear warhead material then is a concern--what to do with it, mainly how to keep it from going into other nuclear warheads. Each country will surplus fifty tons of plutonium, and think of what that could do if it got into the wrong hands. Fifty tons of plutonium. If each warhead requires, say, just for the argument, five kilograms of plutonium in it, that could make 10,000 nuclear weapons or 20,000 for the total between the countries. And five kilograms of plutonium is not very big. It's very dense. It could fit into a small suitcase and it could be adequately shielded with paper or plastic so that the potential for diversion of this to other countries is great. And you can bet your life some other countries are actively seeking to get material like that: Iran, Iraq, North Korea, and probably others.

It has an enormously threatening potential, so the committee was asked to study what techniques could be used to reduce the threat of diversion. We considered several techniques. One means which the Russians were greatly enthusiastic about was to use the plutonium as fuel in nuclear power reactors. They haven't done it. The French are doing a little of that now with their civil plutonium.

Plutonium acts very well from a physics point of view--it fissions and makes things critical in chain reaction--but fabricating it into fuel is so toxic that it has to be done in small work units like glove boxes, instead of a larger production scale, as when fabricating uranium. So it's very expensive to fabricate it, and we've found that from the cost of facilities for fabricating plutonium fuel for reactors, that it would not be economical for any nuclear utility company in the United States to do that. We also found that the Russians still are not onto a capitalistic accounting technique and they make decisions on the basis of what a project is to do, or on other senses of national need or national gain. It's hard for them to believe that it's not economical even though they haven't even built a fabrication facility. We point to facilities built in Germany that never started up; it showed the cost. But Russia is still dead set on doing that.

The other part of that proposal that's wrong is the fuel stays in the reactor about three years, but for this kind of plutonium, only about half of it is consumed to make heat, and the half that's left, that's spent fuel. Well, the people that have nuclear fuel in processing plants thought we can reprocess that and recycle and eventually burn it all up. But it turns out

it's so expensive to reprocess that kind of fuel, they're saying just irradiate it in the reactor one time. So you still have cut down the amount of plutonium by only a factor of two. Instead of making 10,000 weapons, it's 5,000. You haven't done enough. And that illustrates one problem. We haven't convinced the Russians of that.

The other technique is to put it directly into a geologic repository. To do so you dissolve it in borosilicate glass which is being used to incorporate highly radioactive fission products in a form from reprocessing we did in the days of the Cold War. That forms a log of glass that can go into a repository, and it's pretty stable.

Our plans are to do both of those. We're trying to convince the Russians to do both as well. We hope that the Start II Treaty, which is still not negotiated, will then release a lot more nuclear material to be disposed of. And there's at least another fifty tons of plutonium--at least that much--from each country. So that's a big project that's going to take ten to twenty years to complete.

In this country, the government has contracted for some of the nuclear power reactors in the East Coast to be fueled with surplus weapons plutonium, and the government will pay the cost of that. So half the plutonium will be done that way, and then that spent fuel will go to geologic disposal when it's ready. The other half of plutonium will be made into glass.

But in doing that study, then, Luther Carter and I recognized that there's a growing inventory of spent fuel from nuclear power reactors. The nuclear power industry had earlier thought that that inventory would not be very great because that spent fuel would be reprocessed.

France and Britain are doing that now, commercially, and they're doing it for other countries. And Japan plans to start up its own industrial scale reprocessing plant. Their plans call for it in the first decade of the next century. They probably won't make it. But the total capacity of those three plants--Russia doesn't have a commercial plant--are not enough to handle all the spent fuel that's coming out. Spent fuel inventory will grow and grow. And the inventory of separated civil plutonium will grow and grow. It can be made into bombs.

And it appears that those three reprocessing plants are not economical now--they thought they were during the first few years

of operation--and Japan probably will not complete its plant. So the spent fuel inventory is growing, and each ton of spent fuel is about ten kilograms of plutonium: enough to make two bombs.

Of course it's in a well-protected form because the spent fuel is so radioactive that it would take a lot of skillful technique to steal it. The containers are sixteen to eighteen feet long, each weighing a half a ton, but it can be done. The amount of plutonium incorporated in the accumulating supply of spent fuel which has not gone to geologic disposal will be in the neighborhood of 1,000 tons in the next decade. The world inventory of separated civil plutonium will soon reach about 250 tons.

Even worse, when France and Britain have reprocessed spent fuel for Germany, Belgium, Switzerland, Japan, and some other countries, those countries aren't ready to use the plutonium. And that was the reason they had that fuel reprocessed: they thought they would have a use for that plutonium. Some of them thought they would have breeders by now, which they won't. France is moving to use some of its own plutonium as fuel, but there are now about 100 tons of separated plutonium on stockpile in France and Britain at those reprocessing plants.

They're not run by the military. These are commercial ventures, although both are owned by the government. But somehow we cannot believe that there is the same kind of incentive to protect that plutonium from theft as there would be to protect the surplus weapons plutonium which is still under the military. I can't say for sure, because I don't know the details.

And the amount of plutonium that's growing--separated plutonium--continues to grow until they decide to shut the plants down. Even then there's that spent fuel. So our latest missive, which is coming out in February 2000 in the journal from the National Academy of Science called Issues of Science and Technology, is to call world attention to this and ask that it be given the same priority handling as protecting, safeguarding, and disposing of surplus military plutonium. It's going to cause a lot of flak, which makes it that much more fun.

We're recommending that the reprocessing plants in France and Britain be shut down. It doesn't bother us much because we're pretty sure they are not economical. Their remaining argument, which is a very persuasive one, is that in France that reprocessing plant gives jobs to 7,000 people. It's in Normandy and they don't have a lot of industry there anyway, but we're

hoping to stimulate enough world opinion to maybe help that happen.

Now let's see.

Efforts to Stop Overly Lenient Standards in Proposed Legislation

Wilmsen: You were going to talk about Congress and people introducing bills that you're opposed to.

Pigford: Yes. Incidentally, I can give you a copy of this paper if you--

Wilmsen: I think you might have given me that one.

Pigford: I probably did.

Wilmsen: Yes, I think you did.

Pigford: Okay, so you know that story. Now, as for my efforts on influencing the safety standards for Yucca Mountain:

In the 1980s when I served on the National Research Council's board on radioactive waste management, EPA had sent its proposed protection criteria to us back for review and comment. And I found that the criteria were foolish, unprotective, and an enormous departure from any reasonable standards that we use nowadays, without any justification for the great leniency.

Congress got into it for Yucca Mountain because they heard that Yucca Mountain would have a hard time fulfilling those criteria. So Congress then asked the National Research Council to undertake a study of what the technical basis should be for proper and adequate criteria for health protection for future people from Yucca Mountain, and we wrote our report.

It's a very complicated issue. I dissented strongly from all of the members of the committee in terms of how to make the calculations. They wanted to effectively calculate radiation exposures to the average people. I'm sure I've talked about this.

Wilmsen: Yes, you did talk about this. But I don't think you made a connection, or not a strong connection, with Congress.

Pigford: All right. So in that background, I got into this. EPA was supposed to come up with a new standard for Yucca Mountain. Congress told it to. For years EPA did nothing. Finally in August of this year, 1999, four years after the National Research Council report was issued, EPA came up with a new standard. Congress reasonably got very annoyed with the lack of action by EPA. It's foolish to have a project like Yucca Mountain designing towards a safety goal that hasn't been set! It's like designing a car when you haven't decided how many wheels it will have. So industry, the nuclear industry, was pressuring Congress to do something--mainly to create the interim storage facility, but get Yucca Mountain moving better. So Congress proposed new legislation; it proposed about two different bills--one in the House and one in the Senate--every year beginning in 1996 or 1997.

I was asked by some of the environmental groups to look over the proposals. I was also asked, I guess informally, by some of the state agencies, and I found that Congress was proposing to bypass the EPA completely. I can understand they're fed up with the delays, but by-pass EPA completely and they, Congress, will write the standard? Well, Congress, in my view, should never get into this. It's highly technical, detailed public policy. You can do so much mischief unknowingly.

For example, every piece of such legislation that has come out has had leniency in it--sometimes hidden, but built in--that would create allowable release of radioactivity that would be thousands of times greater than what we now allow for, say, licensed nuclear power plants. These things weren't so obvious, but I've done enough work in the field and know how to calculate the result of various proposals. The vicinity average dose that came from Congress--where did they get it? They got it from a lobbying institute of the nuclear industry, and I fault the nuclear industry for giving them that. That itself would then mean that some people would get very high doses. If the vicinity average dose met the Congressional proposed limit, which was pretty high itself, then the poor farming family who might live over the stream of contaminated water--use that for well water, and for watering crops and drinking--could get thousands of times greater.

Pretty soon I got into the routine of writing to senators and to congressmen every time these came up. I've never done that before. I've always felt that you should be very cautious, because I think the political process is so important, and I

don't know enough about it. But I knew this was wrong, so I've been doing that. I don't know if this had any effect. In fact, every year, the proposed bills never got through because Congress was warned that the president would veto them. And I suspect the president's main reasons for saying that is because of political pressure from the State of Nevada, but recently he's also said, "and because it would be an unnecessarily lenient standard." Whether that's a result of my efforts, I don't know, but at least the Congressional legislation has never gotten through.

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Pigford: A more recent bill proposed by Congress has dropped the proposal of regulating safety on the basis of the vicinity-average calculated dose. It doesn't have these leniency factors of thousand in it, but they're still not adequate. It endorses the 10,000-year cutoff. In fact, in my view, it's a sham. But I've gone about as far as I can go.

Wilmsen: So writing letters was the extent of your involvement with Congress other than serving on various committees and advising?

Pigford: In recent years I haven't testified, although I guess I was asked to, but I haven't testified on this issue. During the earlier years I testified to Congress on reactor issues and I mentioned that before, I think.

Wilmsen: Yes, you've mentioned that. Did you have any hand in shaping legislation such as the 1982 Nuclear Waste Policy Act, or the amendments that were passed in 1987, or the 1992 Energy Policy Act?

Pigford: I know I did review some of the proposed legislation. In both of them I was concerned that they had a provision that no interim storage facility would even be initiated by the Yucca Mountain project until they had safety approval for the geologic disposal facility. I knew that was just wrong. They need their interim storage facility now for the reasons I've given. And I think it will be decades before they get safety approval for that design. I was told that that was put in at pressure from the executive office, which would have been Reagan or Bush, or both, to appease the State of Nevada who thought that an interim storage facility, if ever constructed, would then by default commit to putting the geologic disposal facility at Yucca Mountain, in Nevada. It would effectively commit the project to Nevada, even though at that time the official policy was to see if the site would be suitable for a repository.

And that's a pretty good argument, but I think the balance is they need it so badly and have needed it, that they should have gone ahead. I probably wrote something about that through the National Research Council. I can't remember. I didn't get into the business of writing to senators and congressmen until later.

Wilmsen: But that was a provision of the 1987 act, wasn't it?

Pigford: Yes.

More Caution Needed: Reflections on the Rapid Development of Nuclear Power

Wilmsen: Yes. Okay, back to the question I asked a while ago: How have your experiences with evaluating nuclear waste repositories and your research in nuclear waste management in general affected your view of the nuclear power industry as a whole?

Pigford: Well, as a narrow answer, the industry itself officially is not responsible for high-level waste management--spent-fuel waste management. The act gave the Department of Energy that responsibility. Yet the industry had an oversight right because they were paying for the project through taxes on electricity sold, and I think they should have exercised that oversight right. They tried to influence DOE's supervision of the project, but it never amounted to much. And because I think industry generally is better than government at carrying out complicated engineering projects--like the Alaska pipeline and things like that--their oversight would have helped. They are used to knowing that you have to be more conservative and allow for uncertainties. Their influence was sorely needed.

In other countries like in Sweden, the responsibility for the high-level waste disposal--in fact, with all of the waste disposal--lies with their industry, which is privatized. And I think it's a much healthier project, as a result. The government does have responsibility in oversight and approval and so forth.

So that was lacking. In a broader point of view, it became more and more evident to me as I saw the delays and the likelihood of much more delays, because I was deeply in the

technical analysis of these projects, that waste disposal was going to have an enormous effect. The problems of waste disposal, which are technical as well as political, are going to have an enormous effect upon the continuation of nuclear power. And it should. The industry frequently made claims that it's only a political problem; it's a trivial technical problem. Well, they were wrong, and our research at the university showed that. DOE's own safety analyses now show that! You need to read between the lines of all the project delays and so forth. I think that the reality of that has caused a lot of disenchantment within the industry itself. It's one of the reasons they are not building any more nuclear power plants, although the main reason is the economics as well as greater disenchantment on the part of the broad scientific community and the public.

Wilmsen: Knowing what you know now, do you think it was wise for the government, and industry also, to pursue nuclear power so aggressively as they have beginning with Eisenhower's Atoms for Peace program?

Pigford: Like if I knew then what I know now about the availability of natural gas? Well, of course the answer is no. But that kind of hindsight is not very useful. I think the better way to frame the question is could the industry, and the government in sponsoring the industry, have done things to avoid some of the pitfalls and near disasters? And they could.

It was right to develop the industry without the appearance of natural gas, which was a complete surprise. Nuclear power was economical; coal was going to have to become much more uneconomical because of the environmental controls; oil you can't afford to burn in power plants; and they were right to proceed. I think they proceeded too rapidly. There were extravagant claims made that were unjustified, and they should have been more cautious technically.

More Reflections on the Department at Cal

**Hans Mark: Department Chair, the Free Speech Movement, and
National Defence**

Wilmsen: I had a few more questions about the department at Berkeley. Hans Mark succeeded you as the department chair in 1964. I was wondering if you could talk a little bit about how he was selected as the new chairman.

Pigford: Well, it was an unusual choice, and of course the choice was made--the procedure is the dean nominates a man, and the chancellor approves it. But you mean what really happened. It was an unusual choice because he was not a full professor and he was junior to several full professors in the department. But the dean canvassed every member of the department like he was supposed to, and he also interviewed many other people who have had a lot of dealings with the department during those years and he selected Mark. And I think it was mainly because already Hans Mark was showing signs of brilliance as a leader--especially in scientific projects, but also as a good administrator.

Like many such people, he was somewhat controversial. He's very active and works very hard, and sometimes you create a little baggage along the way. I've forgotten his position at the Livermore Laboratory. Well, before he came to us, he actually was an assistant professor of physics at MIT, or an associate professor; I've forgotten which.

Wilmsen: Was he one of your students in that naval program?

Pigford: No, no, no. He was in the physics department.

Wilmsen: Oh, in the physics department.

Pigford: I didn't know him then. He worked in the summers at the Livermore Laboratory and he was a protégé, really, of Edward Teller, much more so than I was. I guess he had some administrative duty at Livermore in the physics division. I don't think he at that time was much involved in weapons work. Not to my knowledge.

Wilmsen: At MIT or when he became department chair?

Pigford: Well, I was thinking of when he worked at Livermore.

Wilmsen: Oh, at Livermore.

Pigford: MIT, I'm sure not, and not as the department chairman at Berkeley. The reactor project had gotten started. It was not yet under construction, but that was an active project--very focussed--and he took that over, did a very effective job. Very charming, energetic, friendly person, and very smart.

Wilmsen: Then he took over the chairmanship, and then just about immediately the Free Speech Movement broke out.

Pigford: Yes, well, that stirred up another side of Hans Mark. His family had been, I think, in Austria. And his father (he was a very famous organic chemist). He escaped and brought the family from that country when the Germans took over. The tradition in that family was that the oldest son would be a professor like his father, and that was Hans.

I've heard the story that Hans Mark had other relatives, maybe an uncle or grandfather, who were active politically in that country. And there's a political side to Hans Mark which he told me several times was part of his heritage. So when the Free Speech Movement broke out, well, he headed into it, and we had some wonderful initiatives in our department to talk with the students, have meetings with them in our department.

In fact, we found the students were kind of lackadaisical and not that much interested in it. We tried to get them interested, not in the rioting part of it, but in the academic issues that went with it. There were some very legitimate ones, and Hans saw the need for that, and he organized things like that. He spent a lot of time on it.

Wilmsen: Did any of the students respond and get interested?

Pigford: Oh, yes, yes. He warmed up a lot of students, and we undertook to revise our curriculum, which we did, and it was for the good.

Wilmsen: How did you revise the curriculum?

Pigford: Well, we had been initially mainly a graduate department. It was a graduate program that I started, seemingly as it was at MIT. But we saw the need for involving ourselves with undergraduates, which we needed to do to become more of a part of the university.

We felt undergraduates needed to have the chance of being exposed to what we teach, which more and more of them are doing.

And so we, I think at that time, initiated the undergraduate program. We made sure that we carried out student appraisals of teachers and posted the results; we were one of the first departments to do that. We had effectively a departmental ombudsman, as the university has one. Some of those things have died by the wayside in the department as well as on the campus.

In our department we were perplexed that actually so few of the students on the north side of the campus, which is mainly engineering and the geosciences, seemed much interested in the Free Speech Movement.

Wilmsen: That's interesting.

Pigford: I guess some people viewed us as stirring up needless problems.

Wilmsen: You mean by trying to stimulate student--

Pigford: Yes.

Wilmsen: How did you feel about Hans Mark's involvement in nuclear weapons research?

Pigford: Actually, I don't know that he was involved in nuclear weapons research. I'd say he was with the physics department at Livermore during the summers from MIT. Now it's reasonable to suspect that maybe he was, but I don't know what he did. Well, I do know he was working on instrumentation, and some of that might have been motivated by nuclear weapons. He was not, to my knowledge, involved in design of nuclear weapons.

Now, many, many years later, after he left the university and became a fairly influential person in the Defense Department, he finally became secretary of the air force. Before secretary of air force, I think he had an appointment with the Pentagon relating to, among other thing, nuclear weapons.

And he's back there now. After leaving Washington, he became chancellor of the University of Texas--this is the whole University of Texas system. Then I think a year ago, he retired from that and went back to Washington to be the senior technical man in the Defense Department, and I'm sure that's loaded with nuclear weapon issues.

Wilmsen: Was anybody doing nuclear weapons in the nuclear engineering

department at Cal?

Pigford: Not to my knowledge.

Few Women and Minorities in the Department

Wilmsen: Now when you were chair, the three times that you were chair of the department, how did you handle questions--or did such questions come up--about women and minorities in nuclear engineering?

Pigford: Very little. It was apparent that there were precious few minorities. We did have some women, but they were a small percentage. And some of them were pretty good. I frankly don't know how we compared with other departments. I suspect civil engineering might have had a better record of more women. I don't know about minorities. I don't think the statistics of our department or any of the engineering departments bear well for that.

We had a lot of Muslim country students. Iran tried to very actively get into nuclear energy and sent students to many universities. That was for a short period. So that's just the record and not much was done about it.

Wilmsen: Was there any particular reason why not much was done?

Pigford: I guess we just sort of found that we were like, as I say, every other department. I don't know any department that really had much of a record of seeking out and doing something about it. Well, I guess we did a little. For graduate student applications we would consider, among other things, how to balance our minorities, but we didn't find much opportunity. It just was an unattractive field for them.

Committee Work on Campus

Wilmsen: What have been some of your more important committee assignments on campus? What you feel are more important.

Pigford: Well, I was--and I think I must have mentioned this--a member of the committee on privilege and tenure.

Wilmsen: You did mention that.

Pigford: Which is an innocuous sounding committee, but it's loaded. Complaints about faculty, mainly, against faculty, or even faculty against the administration, are immediately referred to that committee. It's very sensitive because faculties can have such hard feelings, petty feelings, sometimes for good reason.

When I first came to the campus, I was just loaded on committees. I guess it's because the chancellor and the dean thought since I was a pretty young person to be coming in as a department chairman--I was thirty-seven years old, which was a very senior appointment at that age--I was supposed to hold my place, whatever that means. And among the power struggles, and there were a lot of those--especially on getting space--I got to learn the ropes.

I immediately found myself on a committee on environmental health and safety. There's a campus organization--nonacademic--which administers environmental health and safety programs throughout the campus, especially in places like chem labs, where there are noxious chemicals, and anyplace dealing with radioactive materials. Since I was about to greatly expand that kind of activity--including a nuclear reactor--I guess they wanted me to at least be exposed to the kinds of concerns. The committee, I found, had lots of power--maybe not clearly delegated authority, but it didn't bother us--and we shuffled personnel in that administrative unit. If we didn't think they were doing the job, we hired a new head of it. I mean, officially we recommended to the chancellor that he hire a new head, and found the person and so forth.

And being a department chairman, then you're immediately on several committees with the deans because he then has to administrate and get consensus and has to consult--at least, he's learned it's a good idea to consult the department chairmen on budgets, on competition for space, and things like that. It is the competition for space that is the most intense one. Budget is next.

Wilmsen: What are some of the major battles for space that you remember?

Pigford: Well, I was immediately thrust into that because our department didn't have any real laboratory, and we wanted it to be heavily

experimental. We had rather crummy offices. In those days there were wooden buildings called the T buildings, and we were in building T-4. Those had been constructed during World War II. They were expected to be torn down right after the war, but they survived for about two decades. Actually I liked that because we had essentially a building to ourselves and we didn't worry too much about the amenities.

We were deeply involved in participating in the design of our wonderful facilities in a new building project. I came at just the right time, because it's hard to get involved in a new building project on campus. It's not easy. People fight for space in new buildings.

They wanted to put our department out at the Richmond Field Station where civil engineering had some facilities (naval architecture has a ship-towing tank out there), but I insisted we be as near the middle of the campus as possible because we wanted to have the library and seminars from other departments at our elbow. We were going to be as interdisciplinary as possible. And that was a real battle. Properly so, because a lot of people who knew about nuclear fields and radioactivity feared that we might really do things that would be a great detriment to the campus. As it turned out, we turned out really to be one of the more innocuous uses. It was with people like chemistry and health sciences and Donner Laboratory, who are just on the fringes of nuclear, who don't know enough about it--it's not their main thing--where the carelessness has occurred through the years.

And there are so many committees for--I was on a statewide committee on privilege and tenure--for all the campuses. I was on the dean's committee to plan for a new future for the college of engineering. What's the future? What are the new directions, and so forth. And they do this about once every three or four years. In fact, I found myself spending it seemed like all my time on committees. It was hard to get any work done.

But I insisted on teaching. During my last tenure as chairman when my hearing was getting bad, I didn't teach so much, but during my first tenure, I taught as much as I could. I started several new courses. I taught, I think, every course in the department, and I tried to get the other faculty to do that, too.

Sibling Rivalry, Music, and Family Support

Wilmsen: Now where has your family fit into all of this? You mentioned your brother.

Pigford: Yes, well, I have already mentioned him. He was both an inspiration as well as a challenge. It turned out to be hard on me to have selected the field of chemical engineering that he was already so famous in, however, there are a couple of stories about that that are worth repeating.

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Pigford: As I said, I always felt I was dominated by his reputation, even through high school, especially when I got to college and graduate school. He was five years senior, but I came to the Berkeley campus first. He then decided to leave his university, University of Delaware, in about 1967 and took a post in the Department of Chemical Engineering. Since Berkeley thinks it's run by the faculty, seniority has much to play, and I was senior to him because I'd been there first. It was fun to see my name coming up more frequently than his.

Then I got another way of getting even with him. We both love to play music. He had been a very accomplished clarinet player in high school, and I had played the french horn five years later. I was not very accomplished, although I played it in college, too.

By the time he had come to the campus, I had finally taken up the oboe, which I love to play. I had loved the sound of it, and finally simply neglected some of my work in the mid-sixties to learn to play the oboe. He was still playing the clarinet, so we organized chamber music groups--usually a woodwind quintet of oboe, bassoon, clarinet, flute, and french horn.

And in that group the oboe is the lead instrument. It is the soprano instrument. Of course the flute is, too, but the oboe has a stronger voice. We would get together about once a week and play chamber music either at his house or mine. We had a tool and die maker who was a good bassoonist, a surgeon who was a french horn player, and occasionally my daughter, Julie, played the flute with us until she got so good that she wouldn't play with us any more. But the nice thing about chamber music is that it's very compact, as you know, and you have different voices. The way you play it, you can throw a certain connotation of the

music line to the next player and make him respond in a different way. You can be sarcastic, you can be aggressive, you can be tawdry, negligent, asking for a lazy approach, whether he wants to play aggressively, and there's a lot of personal interaction without words going back and forth. And being the senior player of the quintet because of my instrument, I was able to tease my brother a great deal.

Wilmsen: Oh. [laughs]

Pigford: I have mentioned how our research finally brought us together. That happened actually after he had departed from Berkeley and gone back to the University of Delaware many years later. But the music also brought us together in a way we never experienced before. When I was a kid, he was so much older than I that we just had nothing to do with each other, although I learned even then the fun of teasing him as a young kid brother can do.

I was able to make him so angry. One night he was working on his amateur radio and he picked up a B battery, which is pretty heavy and threw it right at me and cut enough over the eye that I was able to convince my mother that he was just a demon. And that was a lot of fun.

Now, let's see. Now the question involved more than my brother.

Wilmsen: Yes. Other family members?

Pigford: Well, of course they were all very supportive in different ways. The fun of teaching my two daughters music--enough so that they found it fine; then you have to learn music, plus instilling self-discipline so that practicing music became a way of life. That was fun. And my two wives--my first wife, Katy, now deceased, and now Betty: they've both given me great, great support, putting up with an awful lot of crap.

[Let me tell you something about them.²³ Chronologically, there was my first wife, Katy. Her maiden name was Catherine Kennedy Cathey. She grew up in Florence, Alabama, a small town about two hours drive from my hometown, Meridian. She was educated to be a school teacher, but she didn't like that. So during World War II she took a job at the Reynolds Aluminum

²³Professor Pigford added the bracketed material during his review of the draft transcript.

Company, just across the Tennessee River from her home town. Reynolds was participating in the war effort, and they sent her to Cambridge, Massachusetts to work in a metallurgical laboratory they had there. After the war she became a research assistant in the Department of Metallurgy at MIT, and was there when I returned to MIT as a graduate student in 1945. I met her one day when she was visiting a friend, Phyllis Dakin, who was a secretary for Professor McAdams in chemical engineering. We found that we had mutual friends, some guys from Florence who occasionally showed up at beer parties at the graduate-student dormitory where I lived. One was called Whiskey Joe McClosky.

I occasionally ran into Katy in the MIT hallways. I started hanging around Professor McAdams' office. Through the years I had developed a kind of phobia against Southern girls, probably arising from what seemed to be their aloof behavior because I did not play football. I never saw any of them play tennis or any other kind of sports. But Katy was different. She had overcome her Southern drawl and could speak with proper diction. We both loved Boston. She liked to play tennis and swim. And she was a lovely young woman. We were married on December 31, 1948. That entitled her to list me as an income-tax deduction for the entire year of 1948.

Katy kindly and patiently put up with my long and late hours in my research laboratory, where I was doing experiments for my doctoral dissertation. Then a year later came the new assignment to be the head of MIT's new School of Engineering Practice at Oak Ridge, Tennessee. We trundled down to Tennessee in our old Chevrolet named Persephone. We really didn't want to live in the south again, but it was only a two-year assignment. But Katy put up with a lot. Although I was now a member of the regular faculty in MIT's chemical engineering department, I was still a graduate student writing my doctoral dissertation. That occupied lots of evening hours and weekends. Never had I been so busy. Because of the general secrecy of all the work at Oak Ridge, I could not share with Katy the details of our technical work at the Practice School. But she was a fine house mother for the many MIT graduate students, who were housed in a nearby apartment. And we were buoyed along in our anticipation of returning in two years to MIT, in Cambridge.

Then back to MIT in 1952 for a new and exciting career to start nuclear engineering with Manson Benedict. It was a relief to be out of the secrecy environment. We bought our first house in Lexington, Mass. Our first daughter, Cindy--her maiden name is Cynthia Thomas Pigford--was born that fall. Two and a half

years later our second daughter, Julie Catherine Pigford, arrived. This was the closely knit family that supported me in every possible way throughout so much of my career. Sadly, Katy developed a whole series of serious medical problems in the 1980s which led to her untimely death in 1992.

Our life was filled with hard work and joy--music, sailing, hiking, skiing, and devotion to learning. Cindy and Julie both graduated from UC Davis in biological sciences. Cindy was then trained as a laboratory technologist for work in a hospital. Julie enrolled in graduate study at UC Santa Cruz, where she received her doctorate in biochemistry. Both are happily married. They have presented me with five lively grandchildren.

The oldest, Cady, is in her second year of college. This fall Daniel will enter UC Berkeley as a freshman and member of the Cal Marching Band. Matthew, Cindy's youngest will enter college in another two years. Julie's Carolyn is a budding young artist. Young David is still in kindergarten.

After Katy's death I was immersed in work, work, work--largely through serving on university committees, safety-assessment committees, consulting, and travel. My pastime, what little there was, turned to the Berkeley Tennis Club, which I had joined a couple of years previously. Except for a few pick-up games with family, I had played little tennis since forty years ago, when I played at Georgia Tech. My mind remembered the kind of tennis I played in college, but my aging body demanded something else. I slowly learned the much slower movements of senior tennis. But I was lonely and depressed, with little feeling of purpose in life.

But then life became wonderful again. I met and married Betty Hood Weekes, a beautiful, intelligent woman also from Boston, educated at Vassar. The tennis club sponsored mixed-doubles tennis socials during the summer. Each participant is given a card with a pre-arranged schedule of partners and opponents for the evening, similar to a senior prom. In the first set this lovely blond woman, whom I had never met, was serving. She aced me on the first serve, shattering my ego as a macho tennis player! In the last set, we played together. We wiped out the opponents! Then I took her sailing and learned that she had grown up racing a twelve-foot Herreshoff sloop in Marion, Massachusetts, where her family had summered. She has a sunny, happy, outgoing personality and is most considerate of others. I was smitten! Developing a deep and lasting relation with Betty became the principal purpose in my life. It still is.

We were married in November 1994, in an outdoor service by a lake, at a ranch in the wilderness of northern California, near Boonville. Our home is in Oakland, built on a lot burned out by the devastating Berkeley-Oakland fire of 1991.

Betty has two daughters from a previous marriage, both lovely and talented girls. Janvrin teaches at a private school in New York city; Laura is a budding actress in Hollywood.

As wear and tear from sports like tennis finally take their toll, Betty and I turn more to activities like gardening, walking, travel, and skiing. Downhill skiing doesn't have the competitiveness of tennis and sailing, but it is much easier on the body, as long as one is careful to avoid falling. Skiing has become our favorite sport together.

Betty has no training in science, which is a real asset in our relationship. Her own career has led her through service on various nonprofit organizations. She is interested in my work and encourages my continued professional activities, now consisting mostly of presenting papers at meetings on safety, disposal of radioactive waste, and arms control and writing on socio-technological issues. She has a fine sense of right and wrong and effective presentation, which I value enormously. Her input benefits my efforts to write articles that communicate more to the general public. She comes from a fine New England upbringing. Her insight into the visual arts of painting, sculpture, and architecture have opened new vistas for me.

Betty is now the joy in my life that makes living worthwhile! The honors, kudos, and successes in my long professional career pale in comparison to the love and companionship that I get from her.

I dedicate this oral history to Betty.]²⁴

We have to go, Carl.

Wilmsen: Okay. That was just about it, anyway.

Pigford: If any subjects come up that you want me to add by note, give me a call.

Wilmsen: Okay.

²⁴End of inserted material.

Pigford: And I will add these written questions Betty had asked in response to the first interview transcript when we get into the editing period.

Wilmsen: Okay.

APPENDIX A**TRIVIAL PURSUITS:****A COLLECTION OF CHRONICLES**

These chronicles are for the pleasure and education of Catherine, Daniel, Matthew, Carolyn, and David*, little kids who are curious about their grandfather's pursuits, however trivial they may be. The chronicles are not filtered for reading by children. That is left to the mothers, Cindy and Julie, who grew to be fine adults with little filtering by their parents.

These chronicles are also means to keep in touch with classmates, friends, and colleagues, who are pressed into service for research, review, and writing about real events. The author takes no responsibility for errors and mistakes from those sources.

In view of poor memory and unreliable data, any resemblance of the events and characters of these chronicles to actual events and people may be purely coincidental.

Thomas H. Pigford

Berkeley, California

September, 1990

* David arrived six years after the original chronicles. By this update we warmly greet him and welcome him into the family.

Chapter 2. FOUR LESSONS FROM TOM SAWYER

This chapter is dedicated to Mr. Thomas Sawyer, teacher, tennis coach, basketball coach, and teacher of special lessons of life.

Tussling at Marion Park School

Tennis found me when I was a little blonde-haired kid in the fifth grade at Marion Park Grammar School. Even though it was unbecoming for any member of my family to waste time playing games, tennis became a major force in my life. For me, tennis was entirely a happening of unlikely chance, a consequence of unusual environment and circumstances. Tennis became my only available outlet for tussling and competing, fighting and football having been ruled out by the misfortunes of nose bleed and injury.

Little boys, like puppies and little bears, need to tussle with each other. At Marion Park School tussling occurred during the short morning recess, called the "little recess" and during the longer noon recess, or "big recess". The best tussling was usually in the form of fights. I was neither a pack leader nor one of the weak runts. I was as tall as any of the boys, though a little skinny. Joe Blanks, a good friend, well coordinated, confident and aggressive, usually emerged as the pack leader in our Marion Park group.

Unfortunately, my tussling was severely limited because my nose bled at the slightest bump, especially during the intense moments of a fight when adrenaline was up and blood pressure high. I had become so accustomed to the nose bleed in early years that it was no bother to me, but it frightened my teachers and aggravated my mother. She spent much time repairing and laundering the shirts handed down from my brother so that I could be properly dressed at school. All of my brother's shirts were white, so I was a startling bloody mess when my nose bled. It must have embarrassed my teachers to send me home with bloody shirts, and they always telephoned my mother to apologize for what had happened at their school. This embarrassed my mother, who implored me not to fight. She did not appreciate my simple solution of buying some red shirts or dyeing red those handed down from my brother.

My brother, Robert, five years older than I, had been a model child. My mother said that he had never fought, but it was an unfair comparison. My brother did not have the nose bleed problem. He was a normal healthy kid, and he surely must have fought during the tussling of his early years at Marion Park School. Probably, without a nosebleed limitation, he was

able to carry a fight far enough to serve the purpose, and serious fights of anger were actually rare. I did see him once in a fight, off the school grounds. East of my father's enormous garden on Poplar Springs Drive was, in those days, undeveloped land with deep eroded gullies. Years later John Watts' family built a nice house there, adjacent to the grounds of the new Meridian High School. My brother was still at Marion Park school, so I must have been less than six years old when I heard much yelling and laughing from the gullies. I crept nearby and saw my brother being held down by the boys in his gang. He had obviously been fighting. The other boys were forcing him to smoke a cigarette of a local gray-leaved plant that we called "rabbit tobacco". I was too little to interfere. For years that memory of my brother being held down appeared in my dreams and nightmares. I tried rabbit tobacco years later, when I was old enough to build a tree house and have a place of my own to try forbidden things. It wasn't worth fighting over.

A virtue of any gang of little boys is their ability to make the most of any situation. There were several different boy clans in my class at Marion Park School. When things became dull, it was good sport for one of them to organize a fight to see my nose bleed all over my white shirt and to witness the silly anxiety of the teachers. After a few years of this at Marion Park School, it was understandable that I would become cautious about being an active competitor, yet like all little animals I had a great desire to compete and find my place.

Football At The Green House

Organized sport was a safer way for me to compete, but any contact sport had its problems. I was a member of two weekend gangs. One, a couple of miles north of my house on Poplar Springs Drive, included some really strong kids, Harris Walker, Ed Henson, and Nate Williams. We had battles with other gangs, using wooden guns to shoot heavy rubber bands made from automobile-tire inner tubes. I was a leader of that tussle because I had learned to make special wooden guns with clothespin triggers that could fire a whole salvo, like a machine gun. Only occasionally did one land in my face, but no one cared.

But, there was no physical body-body contact in rubber-gun battles. It was too much a test of ingenuity and trickery. Even to us kids, it was obvious that the real test of manhood in Meridian was to play football. Weidmann's great restaurant was plastered with photographs of football heroes. The entire town was focused towards the high-school football season. Football athletes and former football stars were much revered. Football was attractive to kids for tussling. We saw the high-school football players doing rough things like tackling and blocking each other, with many injuries, all officially sanctioned by the school and praised by

the community. Our gangs would congregate on weekends for unsupervised "sand-lot" football. The school playing fields were either too far away or not available, but there was a fine green house on Poplar Springs Drive that had an enormous front lawn. I soon learned to think of it as The Green House. It had been our football field as long as I could remember.

We wore no protective equipment for football, no helmets of any sort. Face masks were unheard of in those days. Some lucky fellows did have cleated shoes given to them at Christmas. They had a real advantage, because we so tore up the lawn of The Green House that better traction was needed in the loose dirt. Of course, there were many nose bleeds. With no teachers around to be concerned, the game continued, although I sometimes had to drop out because of heavy bleeding.

We felt that we owned The Green House. I do not recall who lived there during the third and fourth grades, but during the first part of the fifth grade, in the height of the football season, The Green House was vacant. Probably the former owners had moved out in desperation, tired of being caretakers for the week-end football gangs. We were much concerned about the new family that might come. They might even want to have a nice green lawn like their neighbors. Someone, probably the real-estate agent, had already begun shooing us off the big lawn at The Green House, to restore it so that the house could be sold or rented.

In the midst of our fifth-grade year the new family arrived, some people named Gainey, from Jackson, Mississippi. We knew that it must be a large family, for a lot of people seemed to live in that house. We stayed off the lawn for a few weeks. Then, without asking permission, we again began our weekend football games. To our surprise and delight, we were not asked to leave. In fact, a boy in that family named Andrew, who was already in Junior High School, occasionally joined us. He was strong and well coordinated, an outstanding athlete. He was too good and too big to play with us much, but he would occasionally run a play, act as referee, or show us how to pass. He later became a football star in high school. He was a role model to us all.

Andrew's younger sister, a little dark-haired girl named Emma Gene, joined our fifth-grade class at Marion Park. On her first day at school she was confused about the "little" and "big" recesses and spent all of her lunch money on snacks at the morning recess. We must have thought that she was pretty dumb, and we must have teased her about being a backwoods girl from Jackson. Little boys always look for some nonconformity to pick at, to disguise their interest and curiosity. Otherwise, we never noticed her.

In our view, girls in our class were nothing more than unattractive nuisances, and we were already on the defensive because the girls usually made better grades than we.

The Mother of the Green House

The new family in The Green House was wonderfully tolerant about our football. It was a nice arrangement. Our gang felt that we really owned The Green House, and we were happy to have such good tenants. Some times the mother, Mrs. Gainey, would even bring or send out lemonade and cookies.

I was specially grateful when she noticed that I left the game because of excessive nose bleed. She was wonderful! She didn't become excited at the sight of blood like the teachers at Marion Park School, she didn't lecture me on my foolishness in playing a rough contact sport, and she made no silly complaints about my bloody shirt. She led me into the shade and brought crushed ice and cold towels, meanwhile chatting about the nice game that was still going on. It was such a nice experience! I wanted the bleeding to continue forever.

Mrs. Gainey's motherly attention was repeated many times, and I played harder and had even more bloody noses. To me, she was a dream of a mother, the sweetest and most beautiful imaginable! She had a heavenly, warm, inner glow. I soon had to leave football and bloody noses, but I grew to love Mrs. Gainey. She and her family were in our church, the Central Methodist, and they sat on a pew four rows in front of ours. I didn't like having to go to church, and in my erratic attendance I noticed that Mrs. Gainey's children did not always attend. But, Mrs. Gainey was always there, always remembering me with a kind word after the ceremony. She even made me feel better about going to church. My mother admired her as much as I did.

The Last Play in Football

Football came to a quick and drastic end for me. In one scrimmage at The Green House, when at defensive tackle, I was blocked and bowled over by Nate Williams, a monster of a guy. I landed on my back. Ed Henson, the ball carrier, drove through the hole in the line, directly over me, stepping on my upturned face. He was one of the lucky fellows who had shoes with hard rubber cleats. His shoe loosened some of my teeth, but with the good traction he drove on to a touchdown. He was showing the style that made him a football star in high school. His cleated shoe made a mess of my upper lip. Fortunately, the long laceration was on the inside and did not show, but it should have been stitched. For a few weeks I could hardly talk. For years I thought I had a horribly distended lip. I expect that the lip really healed within a few months, but I worried about it long after the Meridian era. I was clearly through with football! No reasonable coach would have put up with a chronically nose-bleeding player.

My parents would certainly not let me participate again in such a mutilating game.

If only helmets had been required and if face masks had existed, it might have been a different story. It would not have been a good story, though. Football is a team sport, and a football player must finally become a spectator. I soon found that I much preferred individual activities, activities that I could continue throughout life. There was one exception, a fulfilling year on a special basketball team organized by Mr. Tom Sawyer, the subject of a later chronicle.

The tussling of football was over, and nose bleeds continued to prohibit useful fighting. I needed an activity without physical contact to find my place and to compete.

Tennis and Nell Sanders

Tennis became a favorite pastime for me, during the fifth grade at Marion Park grammar school. There was a tennis court on the lower level, at the south end of the school, just outside the classroom where Miss Brownlee taught the first grade. It was a dirt court, always in poor condition. I have no idea why a grammar school would have a tennis court.

Tennis was not one of the subjects or organized sports. The fifth-grade teacher, who was either Miss Alison or Miss Majure, must have liked tennis, for she organized a tournament. I had never played, but tennis looked like a simple game, although it seemed a little dull, stupid, and sissy. It was nothing like the gang action of softball or football, which I played with my friends on the big lawn of The Green House. I expressed my disdain to my friends, A. L. Cahn, William Blum, Winston Cameron, George Bounds, Joe Blanks, and Clyde Brooks, some of whom were already playing tennis. It probably led to another fight, of which there were many among the boys during the big recesses at Marion Park. This time, instead of getting another bloody nose, I accepted the dare to enter the tournament. It was disastrous! That I was matched in the first round against some girl was bad enough. Worse, she thrashed me soundly. William Blum also succumbed, so he and I proceeded to learn the game.

There were few courts around. The city courts at Highland Park were too far away. We were not allowed that far from home. The Marion Park court was in terrible condition, and lots of little kids wanted to use it for hop scotch or to play tennis with four or five on a side. If only we had been patient, my brother actually built a full-fledged dirt court on the vacant lot across the street from our house, where the miniature golf course had once been. He never played much and did not play well, but he was a whiz at building things. Taking care of the dirt court was too much trouble, so I later converted it to basketball.

Just as in later years when we learned to play golf by sneaking onto the back holes at the Northwood Country Club, William and I learned of a

fine court at the Sanders residence, two blocks south on Poplar Springs. The Sanders lived in a large Tudor mansion on a hill, with an imposing winding driveway from the street. They had their own nice hard-surface tennis court on the southwest corner, surrounded by trees and hedges. The court was never locked, and we believed that the Sanders family would not know if we slipped onto the court in the late morning or early afternoon.

None of us knew much about the Sanders, other than that they were fabulously wealthy, from cotton gins and hosiery mills. They had lots of cars. There were five Sanders girls, Claire, Stella, Julia, Helen, and Nell, the youngest and most bewitching. There was whispering amongst us ten-year-olds that the girls were beautiful, rich, wild, and had been married many times. Probably it was simply that they were the usual well-bred girls of Meridian but were of college age and beyond, too ancient for us to comprehend. There was a book about the Sanders family, in the style of Peyton Place, written by Ed Kimbrough in the 1930's. There were also three sons, Benny, Jimmy, and Bob. Mr. Sanders worked hard and long, in town and out, but he must have gotten home frequently!

All of that made tennis at the Sanders quite exciting. The elderly mother, whom we referred to as Old Lady Sanders, was a tough old bird but friendly and kind to little boys. She waved to us when she saw us playing, and requested only that we disappear when the family wanted to use the court.

Next door to the Sanders property, on the north side and on Poplar Springs Drive, was another mansion, known to my friends as the haunted house. It was occupied by an uncle of my mother's, Uncle Cass Cochran and his wife Ethel. They must have been wealthy, but I can remember being in that house only once or twice. It was fabulous, with a grand piano and oak-panelled walls. The most striking possession was in the bathroom. It was a low porcelain basin with faucets. My sister, Mary, and I were told that it was a "bidet", but its function was never explained. We were too shy to ask. The bidet seemed just the right size for bathing a baby, so I assumed that it had been installed ages ago when their children were born.

But, it should have been as high as a sink. I concluded that it was installed so low so that a little kid could climb in for a bath. It seemed like a clever idea in bathroom training of infants and toddlers. Of course, I was the youngest of my family and knew nothing about infants. There was not another bidet in Meridian, nor did I see another until years later, when I and other Navy officers were invited to the estate of the Crane family, the owners of Crane Porcelain, in Ipswich, Massachusetts. Mary's second encounter with bidets was in a trip to Paris.

The Cochrans had a crotchety gray-haired old retainer named Henry, who was also their chauffeur. It was he who would shout at the grammar-school students on their way to Marion Park School. Probably that was why

the house was thought to be haunted. It was good that the Sanders' tennis court was on the other side of their property, away from interference by Henry.

One day, William and I were battling on the Sanders court, with tennis balls so worn and fuzzless that it was more like a game of ping pong. A beautiful lady stepped out onto the court, wearing a strange short white dress, which I later learned was a dress made especially for tennis.

We started to run, but she kindly told us that she was Nell Sanders and asked if she could join our game. She opened a new can of white balls, a new experience for William and me. She was evidently an experienced player. She knew how to keep score, and she returned balls consistently and easily. She played the singles court, against William and me in the opposite doubles court. She won, in the most polite and graceful way. We wanted her to go on winning forever! This improved our tennis considerably, both because we learned what it was like to play with real tennis balls with fuzz and because we loved to watch Nell Sanders and her beautiful tennis strokes.

Occasionally, while Nell was playing the troisome with William and me, an elegant long convertible would ascend the winding driveway, carrying a suave looking fellow who looked like William Powell. Invariably he wore white flannel tennis clothes and had a new can of balls. The first time that he came, Nell shooed us away so that she could play tennis alone with this idol. On later occasions we were allowed to stay for doubles with Nell and her boy friend. More frequently we were simply asked to stay around and have lemonade. I could never imagine why Nell wanted us to stay, because her boyfriend was even better than Nell, and both were out of our league. It may be because a ball would occasionally be hit over the tall fence surrounding the court. Unless chased instantly, it would scurry down the hill of the Sanders lot and into a rain gutter, or down another hill towards Marion Park school. We were good at chasing balls. We even recovered balls for Nell and her boy friend between points, like ball boys in championship tennis matches.

I still wonder how Nell became such a good player. Tennis was not considered a suitable serious activity for a girl in Meridian. There was no girls' tennis team in High School, and I never heard of girls taking lessons. The one exception was the daughter of Mr. Guy, whom I describe later, who played only with her father. Maybe in one of her many marriages Nell had lived in the North, where tennis for girls was accepted, perhaps she had gone to college in the North, or perhaps it was another way for her to express her infamous nonconformance with Southern tradition.

Nell played a good game without seeming to work very hard, a result of some good coaching somewhere. It seemed to me that she never even perspired, which is hard to believe in the stifling humid summer heat of

Meridian. In fact, it is my perception that all of the girls of my age, through grammar school, junior high, and high school, never allowed themselves to be put into situations that required much physical exertion and perspiration. Well bred Southern girls just didn't perspire! Probably that is why Nell kept William and me around to chase balls, even those that weren't hit off the court. Maybe that is why so few Meridian girls actually played hard tennis.

The First Diversion

William and I were getting pretty good, playing on the Sanders' court and with Nell and her boyfriend whenever possible. I practiced every day, with whomever was available. Getting rackets and balls was a continuing problem. In spite of this dedication, my concentration on tennis was suddenly interrupted on the first day of school, in September 1934. That was the year that I was to enter the seventh grade, at the Kate Griffin Junior High School, two miles away.

I was much depressed when instructed to ride to Junior High with my aunt, Alma Harrington. She lived next door to us, with my Aunt Cora Harrington, two lovable old maids. My Aunt Alma taught arithmetic in the seventh grade, and Aunt Cora worked in the Junior High lunchroom. Aunt Alma drove to school every day in her old black Chevrolet. I objected strongly to having to ride with anyone, because I did not need to be escorted. I could easily ride the two miles on my bicycle, which I rode everywhere.

My objections disappeared the first morning of school when I saw waiting on our porch swing the most beautiful young lady with coal-black hair! She was Snow White without the Seven Dwarfs, although I may have the Disney chronology out of place. She was Emma Gene Gainey, the little dark-haired girl who then lived in The Green House, about a half mile north on Poplar Springs Drive. Her mother had arranged for Emma Gene to walk to our house, to ride with my aunt to school. How could this little dark-haired girl, whom I had never even noticed during one and a half years of grammar school, suddenly have changed so much and become so desirable? She had a smile that would warm the faintest heart. I was smitten!

There was, in fact, an earlier affair, though only a short-lived infatuation. In the fourth grade at Marion Park School there was a lovely young teacher named Miss Helen Williams, probably in her twenties. She had just come to teach, and I fell for her on the first day of class in September 1931. My mother soon noticed my strange behavior and gently advised that Miss Williams was known to be already spoken for. My despair was great, but common sense, undoubtedly expressed by my mother, prevailed. I wasn't about to become involved in that love triangle. Indeed, she had

married and departed by the time we went to Junior High. It may have taken longer than a few days for me to get over my disappointment with Miss Williams.

As far as I know, Emma Gene did not play tennis. I never thought of asking her. The idea of doing such things with girls never occurred to me until I met the Harding girls, at the Longwood Cricket Club, ten years later. The puzzle of the tennis-playing Nell Sanders had already been filed away as an inexplicable anomaly. Thus, tennis disappeared from my agenda. Instead, there was much more bicycle riding. I frequently cruised by The Green House, hoping to get a glimpse of the little dark-haired girl.. One day when her older sister, Marguerite, came out and photographed me on my bicycle, I thought that I might be making progress.

Emma Gene was a close friend of another friend of mine, Dorothy Brewster, the daughter of a friend of my father's. Dorothy was the first girl in our Marion Park class whom I knew. She was smart in all subjects but art and was terrific at hop scotch, but I have no memory of her playing tennis. I don't remember if Dorothy rode to Junior High school with my Aunt Alma, but she and Emma Gene and I were good friends in the seventh grade.

As much as I enjoyed the company, riding to school with my aunt subjected me to more pressure on arithmetic than I wanted. She expected me to do well because I was a boy. During these depression years boys had to learn things to eventually get jobs. My sister never received such attention. Once in my aunt's seventh-grade class Jeanette Freeman, a smart little girl who had gone to Stevenson Grammar School, raised her hand to ask a question. Many other hands were up. Miss Harrington cut her off with, "Jeanette, we don't have time for your question. These boys have questions, and they have to learn and get jobs." Wanda Johnson was also in that class, a little girl then almost as meek and timid as I. She was well prepared in mathematics, having studied under Miss Edna Lockard at Stevenson, who would rap students with her ruler if they didn't remember the "Tables and Combinations". Wanda soon got over her mortally petrified fear of going to school.

But the recesses and rides to and from school had become both exhilarating and difficult. I soon found that I was more ready for tennis than for girls. I had always found it difficult to talk with any girl, but until now talking with girls never seemed worth doing anyway. Here was a new ball game for which I was unprepared, and I was getting worse. My mouth became so dry and my throat so tight that I was frantic. I never thought of writing notes. This one-way exchange was headed nowhere! After a few months of pure bliss and deepest frustration on my part, never

expressed by me to anyone, Emma Gene logically turned elsewhere. I returned to tennis and, very happily, I was allowed to ride my bicycle to school. Emma Gene and I had become good friends, and we still are.

My knowing the little dark-haired girl from The Green House, a young replica of her mother, caring, friendly, and intelligent, made it difficult for all girls who were to follow, until thirteen years later, in Cambridge, Massachusetts, where I met Katy, my lifetime partner in tennis and in other pursuits.

The First Lesson From Tom Sawyer

When in the ninth grade, at the Kate Griffin Junior High School, I learned that Mr. Tom Sawyer, the history teacher and tennis coach, wanted to start some new kids in tennis. Mr. Sawyer was a tall, lanky person with many athletic interests. He must have played on the tennis and basketball teams in college. I suspect that he had been teaching for only a few years when my class entered junior high.

Through whatever selection process there was, the new tennis kids were George Bounds, Winston Cameron, and I, all from the Marion Park Grammar School clan. No prior knowledge or ability in tennis was required. Winston was motivated to be a great player. His father had been a tennis champ some place, and there was a private court at his home, a great place that I visited often in later school years and during occasional visits after leaving Meridian. George's motivation was to make the tennis team. He was a natural athlete. I simply wanted to be able to beat my friend and competitor, William Blum, who, although smaller than I, was getting too good. Also, I was already playing a lot of tennis, though not well, and I needed a source of tennis balls. A can of balls cost all of 50 cents, and we needed new balls at least every week. At that time I was convinced that my parents were poor as church mice, although in fact they were only very frugal, with priorities that did not include money for playing games. My meager summer earnings could not carry me through the school year, and there was little left for tennis balls and rackets.

During the first two semesters of Mr. Tom Sawyer's coaching we played not a game of tennis. We never rallied by hitting balls to each other. We never even got on a tennis court. He met us every afternoon in the Junior High gymnasium. He had worked out the basic fundamentals of the tennis stroke. Facing perpendicular to the net, represented by the gym wall, the racket arm goes back 35 degrees, with the racket an extension of the straight arm and wrist. When hitting the ball, the arm itself swings only a little, at the shoulder. At the same time the upper body rotates, the weight shifting from the rear foot to the front foot. The arm and racket move through an exactly horizontal plane, meeting the ball as it comes

abreast, and continuing as follow through with the arm and racket pointing toward the net and the upper body facing the net. The momentum carries the body toward the net, ready to put away any return from the opponent. That is all there is to it! If we added any further motion, we would receive scathing criticism from Coach Sawyer.

To demonstrate, Coach Sawyer would bounce a ball on the floor from his left hand and stroke it towards the gym wall with the open palm of his racket hand. After a week we were allowed to do the same stroke with a racket. Each of us did that hundreds of times each day, several tens of thousand times during the year. At no time were we allowed to hit the ball as it bounced back from the wall. It soon became like a formal ballet movement, with Coach Sawyer clapping his hands for rhythm.

After a couple of months we advanced to learning running footwork, bouncing in anticipation and leading with the foot that would be nearest the net, once the direction of the next ball became apparent. Coach Sawyer stressed the importance of anticipating where in the court the ball would land, running as fast as possible to the proper location to set up the stroke, coming to a dead stop, and then performing the ballet motion of hitting the ball. He would stand against the wall, protected by a chair, and toss balls to each of us, trying to keep us guessing where the next ball would land. Of course, he was exaggerating by having us run to position and plant our feet. He was trying to get us to overcome the lazy habit of waiting too long to get in position.

I didn't realize it at the time, but Coach Sawyer really wanted to teach us to move to the ball and hit it in a continuous fluid motion. When we finally reached High School the football and basketball coach, Mr. Jim Baxter, became our tennis coach. He obviously knew nothing about tennis, but evidently the powerful High School athletic magnates did not tolerate outside coaches from Junior High like Mr. Sawyer. Coach Baxter did try. He would sit and watch us play, bewildered but patient. He finally suggested one day that I really did not have to run so hard in between shots. I was overdoing it. Even after the point was over, I would run like mad to retrieve the balls for the next point, probably a hold over from chasing balls for Nell Sanders. He was a big help.

We were impatient kids, especially during the first semester under Coach Sawyer. This kind of ballet exercise with no play was not what we had in mind. Tennis was supposed to be fun! However, Coach Sawyer was ready for our frequent complaints. He would kindly assure us that this was the hard but sure path to competitive tennis, and he reviewed what each of us had told him about our goals. By that time I had learned to refine my goals to something more noble than merely beating William and getting free balls. After reminding us of our goals, Coach Sawyer asked if we still believed in them. We were not like modern kids, who would tell him to go shove it! We were supposed to be achievers. After our soulful confirmation that we still wanted to become great players, he would then settle the argument by saying,

"WELL, FELLOWS, IF THE END DOESN'T JUSTIFY THE MEANS, WHAT IN THE HELL DOES?"

This is the first lesson of life from Coach Sawyer.

I swallowed it all, hook, line, and sinker. I took Coach Sawyer's lesson literally and seriously as a noble goal and followed it astutely for some time. I later learned that Coach Sawyer had adopted a favorite W. C. Fields expression. Probably Coach Sawyer did not say "hell", but something more suitable. Unfortunately, I was too young, too easily awed by authority, to perceive the humor and irony of W. C. Field's expression. That was the first of two important lessons from Coach Sawyer's tennis clinic, lessons that went beyond the mechanics of tennis itself. It took time to unlearn it. This is not a criticism of Coach Sawyer. Teachers and coaches have to say things to relieve tension and lighten the atmosphere. Undoubtedly he did not realize what a literal and believing person I was.

I have never forgotten the expression. It sometimes has led to trouble when I have used it to relieve tension in our intense seminar arguments at M.I.T. and Berkeley.

Physics From Tom Sawyer: The Second Lesson

Coach Sawyer did not restrict us from playing on weekends. Naturally, with all that practice, we became pretty good. During the next few years Coach Sawyer would take us to the clay courts at Highland Park, that were reserved for the tennis team every weekday afternoon during the school year. He would toss balls to each of us, which we would stroke across the net, still not rallying or playing. During rainy weather, or when we became careless and sloppy, it was back to the gym.

We were all surprised at the results. Being an extension of the arm and the upper body, the racket was a resilient backboard. A ball hit

harder by the opponent would automatically be returned with more pace. It was easier to play against well-trained opponents who hit hard, low ground strokes. The rhythm was steady and perfect. However, whenever I faced a player who was crafty and hit spinning balls, soft balls and bloopers, I had much trouble. The timing was all upset. Thus, I was still having trouble keeping ahead of my friend and competitor, William Blum, who still played like we used to play, with more guile than skill. He was such a good friend, though, that he let me convince him that he should learn Coach Sawyer's technique, which I set out to teach him. I was not the teacher that Coach Sawyer was. I really did not understand the mechanics of the stroke that well. After a few attempts at teaching William, I had suitably ruined his game, so one of my goals was achieved after all.

As the effectiveness of Coach Sawyer's stroke of minimum motion became apparent, I became most intrigued with the mechanics of arm articulation that he had worked out. How could something so simple and effective emerge from what otherwise seemed to be a horribly complicated human skeleton, with so little rhyme or reason? Mr. Sawyer asked us to think of ourselves as simple stick skeletons, with the racket arm behaving as a single stick. He demonstrated with a toy wooden skeleton that he made.

What had originally been a rather boring daily routine of stroking, stroking, stroking became more interesting as I secretly tried variations from Coach Sawyer's stroke. They were interesting to hit, but none gave the resilient pop of the ball off the sweet spot of the racket that could be achieved with Coach Sawyer's stroke. While stroking there was plenty of time to day dream and imagine. I couldn't believe that it could be so simple as a bunch of bones working together like levers. Obviously, there had to be some cables operating on those levers, which, of course, were the muscles and tendons. Coach Sawyer's stroke was really controlled and powered by the long diagonal back muscle anchored to the shoulder.

For a while I became interested in structural anatomy, the way that careers in medicine sometimes begin. However, the grammar-school fights had convinced me that medicine was not my cup of tea. I later learned that the fundamental mechanics that Coach Sawyer was using are the foundation of physics. It may have been our high school physics teacher, Miss Victoria Wiss, who consolidated my interest in physics, but it was actually Coach Sawyer who got it started.

Even though Coach Sawyer may not have consciously taught it, the beauty of mechanics and physics was the second of Coach Sawyer's unique lessons that I remember. Two others derived from a special basketball team that Coach Sawyer organized, the subject of a later chronicle.

Russell Grossnickle

At about that time we began to follow tennis players of our age in other towns, expecting that we might some day meet them in tournaments. One boy, from Columbus, Mississippi, named Russell Grossnickle, was already famous. I saw him play once in a tournament in Jackson, Mississippi. He was an excellent player, with flat, ground strokes just like those taught by Coach Sawyer. Russell's father was a professor at the Mississippi State College for Women in Columbus. He coached two daughters as well as Russell, requiring them to have tennis workouts twice each day. The older daughter was already ranked within the top ten women players nationally. The other daughter, Russell's twin sister, was also said to be a good player. We never saw either of the girls play, because there were no tennis teams for girls and no tournaments in the high-school circuit.

I was specially intimidated by seeing Russell bring three rackets, all new looking, together with several unopened cans of balls. An ace of spades was stencilled on the strings of each racket. He was one of the first to wear the new white tennis shorts. When warming up he wore a soft white tennis sweater knotted around his neck, even in 90 degree heat.

Grossnickle's antics during a match would shame even John McEnroe. When his opponent seemed to have his game under control, Grossnickle would interrupt the play and sit on the court to remove his shoes and adjust his socks, anything to interrupt the rhythm and concentration. He liked to wave his arms just as his opponent was serving. In one match, against Jackson's best player, a boy named Burnham, Grossnickle departed from his normal game and returned one moon ball after another. This was our first introduction to the insidious gamesmanship of tennis, something that Coach Sawyer never bothered with, to his credit. Russell had obviously been coached well in the tactics of gamesmanship. He was even more skilled at psychology than in the mechanics of hitting tennis balls. I expect that his father must have been a professor of psychology, specializing in the psychology of abnormal behavior.

As Winston, George, and I emerged and finally became the MHS team in regional and state tournaments, I was always anxious about having to play Grossnickle. Strangely, we never met nor played, nor did he play Winston or George. Our one contact with Grossnickle was at that boys' tournament in Jackson, for young players who were on the verge of emerging into high-school tennis. Jim Stuckenschneider, Emma Gene's husband, knew Russell Grossnickle at Columbus High School. He reports that Grossnickle's seeming invincibility in tennis began to crumble in high school. It is incredible that Grossnickle could have sunk so quickly, because he was a really outstanding player in the state boys' tournament.

Some readers of earlier versions of these tennis chronicles have urged that the principal players of our high-school team should meet again for a fiftieth-anniversary challenge match. It would be good to see Winston Cameron and George Bounds play together again; they were the finest young doubles team that I have seen. I had planned to invite Russell Grossnickle as my partner. He would be ideal. All of us will have lost most of our athletic skill and timing, so gamesmanship and psychology are likely to decide the match. Russell's superb mastery of tennis would again be recognized!

Sadly, I have recently learned that Russell was shot down in World War II.

The Western Grip

An entirely different approach to the backhand was drilled into me when I was invited to join the doubles game of Mr. Ben Cameron, Winston's father. The others were Dr. H. M. Ivy and Mr. Waldo McQuaig. We played on the Cameron's court. Mr. Cameron had once played top competitive tennis. He knew his game. Although many know of him as Judge Cameron, because he later became a judge in the Federal District Court, I knew him as Mr. Cameron and think of him fondly in that way. I liked to play on the team opposing Mr. Cameron's doubles team, because he hit the ball low and fast, ideal for returning by Coach Sawyer's flat ground strokes.

However, there were dire consequences! The first time that a ball came to Mr. Cameron's backhand side, he zipped it back harder than any forehand I had ever seen. The next time it happened I observed that he simply rotated his wrist and racket through 180 degrees for what would normally be a backhand shot. The palm of his racket hand was then facing the net, in the same way as in a normal forehand shot. He used the same strong arm muscles to support the stroke as in the normal forehand stroke. As a result, he really didn't have a backhand. His stroke was a little contorted when the ball bounced low to him, but he could demolish a high-bouncing ball to his backhand. Some good contemporary players, such as Boris Becker, use a partly rotated grip to power a high-backhand return, but I have not seen another player use the completely rotated racket as did Mr. Cameron.

It soon became a matter of survival. Mr. McQuaig was always my doubles partner. He had only the wimpiest of serves. When McQuaig's serve came to Mr. Cameron's backhand side, landing in the middle of the service court only a few feet from my position at the net, it was easy for Mr. Cameron to drill his powerful shot right at my belly button. To keep from getting plastered, I learned to keep my racket in front of me at all times, making it easy for Mr. Cameron to pass me down the alley.

I had never heard of such a shot. I thought for a moment of questioning its legality, until I remembered that Mr. Cameron was a successful trial lawyer and would undoubtedly overwhelm me with legal precedent as he had with tennis balls. I learned later from Winston that Mr. Cameron's technique is called the Western grip, popular with some players during the 1920's and early 30's. Mr. Cameron himself came to my rescue, gently suggesting that nothing in the rules required me to play at the net while my partner was serving. I did try playing from the backcourt while Mr. McQuaig served, and we won a few more games. I still preferred playing at net, however, because that is the way I had been taught and that is the way we saw the professionals do it. Playing at net was fun when the service went to Dr. Ivy, because his returns were easy to put away, sometimes at Mr. Cameron's feet. It would have been impolite to Dr. Ivy to play net when serving to him and to retreat to the back court when serving to Mr. Cameron.

Doubles at the Cameron's, with those same players, became a way of life, renewed during the war years and M.I.T. years when I would come home for a short visit with my parents. Always at the end of the match would be Mrs. Cameron with a pitcher of lemonade and cookies. She was called Polly by Mr. Cameron and Winston. She was a great lady, and still is. Some of my best and warmest memories of Meridian are concerned with the mothers of my friends.

The Highland Park Courts

Coach Sawyer achieved well-deserved fame and popularity in Meridian as he became more identified with the tennis team. I never knew how or why it happened, but Mayor Clint Vinson consulted Coach Sawyer about the use of the city tennis facilities at Highland Park. Coach Sawyer had arranged with Mayor Vinson for the tennis team to have free use of the two Highland Park courts, which were the only good courts in the city.

V. C. Rhodes had been in charge of the two clay courts. His job was to wet, roll, and line them, and he could charge people for playing on them and keep the fees. There was a tennis caretaker's house there, where V. C. strung tennis rackets. V. C. was a good tennis player, but for some reason he was not on our tennis team. He was a year older and may have been on earlier teams. V. C.'s mentor was a nice elderly gentleman named Mr. S. N. Guy, who had an attractive blonde daughter named Frances, who played tennis with her father. She had fine tennis strokes, but she never played hard or perspired.

At the end of the ninth grade Coach Sawyer became concerned about what I told him of the dire poverty of my family; at least it was my view at the time, because I never had enough money for essentials like tennis.

Even with a source of free balls from Coach Sawyer during the school year, I had much trouble buying tennis rackets and tennis shoes. My father pointed out that a tennis racket had little to do with a good education and getting a job. In fact, it was bound to lead to useless diversions and trouble.

Coach Sawyer arranged with Mayor Vinson for me to take over as manager of the Highland Park courts. It may have been unfair to V. C. Rhodes. Actually, V. C.'s interests had already turned elsewhere, to Jeanette Freeman, who had become a lovely, lissome brunette. He sold me his racket-stringing equipment at a bargain price, which I soon paid from the new high court fees that I charged.

I had suddenly become wealthy! During the first summer I kept most of my earnings and ended the season with \$57.00. With that I bought two new nets for the next season, repainted the tennis shack, and began the next season with a flourish. I saw V. C. little during that first summer.

I think that he and Mr. Guy were a little miffed when I charged them to play. I never saw V. C. after that.

I later realized that Coach Sawyer, in arranging for me to manage the courts, had expected that I would practice and play a lot and thereby strengthen the team that would some day represent the High School. That is exactly what I wanted, but my good intentions to play and practice were diverted by my growing interest in tending and manicuring the courts. The courts were of red clay, the kind that are still used in the French Open. Every day I leveled the clay with a long wooden scraper, like a giant rake.

Then I watered the clay carefully, enough for the clay particles to soften and coalesce. When the clay was just the right softness, I compressed and smoothed the clay with a large metal roller weighted with water. The result was a beautiful dark red surface, better than even the new composition courts that I had seen at expensive clubs in other cities. A smooth-soled tennis shoe can slide a little on clay. Sliding smoothes the transition from running to stroking.

In some clay courts the lines that define the court are made either with white tape fastened to the clay or with a liquid mixture of water and slaked lime, laid down from a wheeled lining machine. Tape is hard to see on clay. I did have a wet-line machine, but I preferred to lay a line with dry white lime powder, using a wheeled machine. It made the whitest of all lines, in striking contrast to the dark red clay. Of course, the dry-powder line was quickly ruined by trampling feet, so it had to be renewed frequently. When I learned to do the courts this way, with perfectly straight lines guided by strings strung tightly across the courts, I felt that I had created a work of art. It was as fulfilling as that great course in Palmer Penmanship taught by Miss Brewster in the seventh grade.

The courts were so pretty that I could hardly bear to play on them. In fact, I preferred that others not play on them and sometimes improvised excuses to avoid accepting reservations to use the courts. After every few sets, I would intercede to again wet, roll, and line the courts. Thus, the would-be artist overcame the budding young tennis star, a blow to my progress as a tennis player and to my summer income.

In those days serious male tennis players always wore long pants. The really good players wore white, and the champions and professionals wore white flannel trousers. William Blum's father, who was co-owner of the Liberty Shop, a high-class boutique, was the buyer and spent much time going to exotic places like New York city. I was particularly proud when he played on my courts. His white flannels added an elegant touch that was not unlike the tennis stadium at Wimbledon, except for clay courts instead of grass.

Except for those few players who really knew tennis and complained about my eccentric requirements to beautify the courts, most players began to identify me as the "Highland Park Pro". I was even paid by some well-meaning parents to give lessons to their children. I received tips for providing such nice courts. I had never even heard of tipping. The first tip was included in the handful of money given to me as the customer left the courts. I discovered it later and ran after him to return his overpayment. I couldn't believe that people actually paid more than something really cost, but at his kind insistence I kept the extra payment and soon became accustomed to this great practice of tipping, great to the one on the receiving end

Eighty-Pound Gut

Even some of the seasoned players foolishly assumed that with such a professional approach to playing and court management, I must be an old hand at stringing rackets. I advertised stringing at \$6 for rayon strings and up to \$15 for the best sheep gut. My first stringing job was on a brand new T. A. Davis frame, made in Australia, that some wealthy person had purchased in New Orleans. I had never before seen such an expensive frame. My knowledge of racket stringing was limited to the questions asked at the sporting goods store in Meridian, where I had once had a racket strung. There they asked if we wanted it strung to 60, 65, or 70 pounds, meaning the tensile force on each string as it is anchored in the racket frame. Coach Sawyer had instructed me to have my racket strung at 60 pounds.

My first customer, with his new T. A. Davis frame, obviously had more money than sense. When I asked him how tightly he wanted his racket

strung, he asked how tight could I make it. I didn't have the foggiest notion of what would limit the string tension, and I knew that I could pull 70 pounds if I had to. I then examined the tensioning device that I had bought from V. C. Rhodes. It was calibrated at various forces, from 55 through 80 pounds, so I told the customer that I could go to 80 pounds. I bought the very best gut strings available, which turned out to be rated to 80 pounds. Without any practice on a cheap frame, and having never strong a racket, I proceeded to load that beautiful T. A. Davis frame with the tightest strings ever seen in Meridian.

As I later learned, there is a string-tension limit set by the frame, as well as by the strings. The T. A. Davis was a light frame, designed for touch players. Fortunately, it was one of the new laminated frames, so instead of simply buckling under the enormous compressive force from the strings, it merely distorted. Unfortunately, a tennis racket is expected to be symmetrical, with an oval-shaped head. To my horror, the T. A. Davis had taken on a skewed ovate shape, not unlike an egg that is sitting on its pointed end but tilted away from the vertical. Otherwise, the 80-pound gut strings were as tight and hard as a board!

Faced with the inevitable exposure of my inexperience in racket stringing, to be likely followed by my downfall as a tennis pro, I developed a remedy probably never intended for a well-made tennis racket. All tennis rackets in those days were of wood, so each racket was kept in a wooden press when not in use, to keep it from warping. I put the now ovate T. A. Davis in one of those presses and inserted wedges between the press corners and the racket frame to force it back into a symmetrical oval shape. Then I filled my coffee pot with water, turned up the heat, and steamed the frame, carefully avoiding moistening the gut strings. I was delighted to find the next day that the racket retained the proper elliptical shape when removed from the press. I proudly turned it over to the customer, who exclaimed about the beautiful tight stringing and the flashy symbol stenciled on the sweet spot, the center portion of the strings. Since the time that I had seen Harold Grossnickle's terrific game and gamesmanship, I knew that badges like tennis clothes, rackets, and racket stencils could be important to the compleat player. It was simple to make a cardboard template that would cover all of the strings except for a cut-out shape in the center. I made many exotic cut outs! For this customer, it was the ace of spades, stenciled with black lacquer.

The customer gave me a big tip. I advised him to keep his racket in a press whenever possible. A week later he returned, much chagrined. His racket had warped into a skewed ovate shape, though not as pronounced as when it was first strung. He apologized for such shabby treatment of my handiwork and begged me to either restring it or to do another new T. A. Davis frame for him. Meanwhile, I had consulted Coach Sawyer about string tension. He explained that 80 pounds is ridiculous for all but a few

professionals. At 80 pounds the strings are so tight that the ball immediately rebounds from the strings, although it does indeed rebound rather rapidly. What is needed is a compromise, usually at about 60 pounds, so that the ball stays in contact with the strings long enough for the player to exercise more control or to impart top spin or underspin. I gently suggested to my customer that he might prefer a more conventional string tension, and I offered to restring his T. A. Davis, to see if it would stabilize the frame. By then, he felt so guilty that he was willing to trust my judgment on anything. I then proceeded to remove the new gut strings from the T. A. Davis frame, dissolved off the ace-of-spades stencil, forced the frame back to its original oval shape, let it sit for a week, and then restrung it with the same gut at 60 pounds, followed by a new stencil. It was a great success!

Playing the Short Court

Another of my regular customers for reserved courts was a man who always played with the same opponent, always nearly won, but never quite succeeded. It was the lazy habit of most of these players never to change courts during a match, or, at most, to change only after each set. This man, whose name I conveniently forget, noticed my love for dry white lines and the need to rejuvenate them frequently. He asked me to put down a special back line for him on the end of the court that he always took, about a foot closer to the net than the regulation line. His idea was that his opponent would not notice it, and with this advantage he would finally win. He promised me that it was only a joke, and that after the match he would inform his opponent of the joke and that I could then restore the line to regulation. I had once used such a technique in ping pong with Billy Ackerman, who usually beat me, moving the net away from center so that one of us would be handicapped, but only by agreement of both parties.

It was easy to prepare the court as requested. My client did indeed win the set. This time the opponent asked to change courts for the second set, because the sun was in his eyes. I noticed that nothing was said about the short back line, and the opponent did not notice it. This time, my client, now on the side with the regulation lines, with his opponent on the short court, won the second set. He was overjoyed. As they left, I asked the losing opponent how he liked playing the short court. He was incredulous that it had happened. Apparently this had been a money match and had already been paid off. My client argued that his second-set victory in the disadvantaged court more than made up for his rigged victory in the first set. I left them to argue, and I was happy not to have that client again.

USLTA and the Tennis Pro

I was still interested in actually playing tennis. With all of this tennis pro activity, I began to worry about my eligibility to play in the high-school tournaments. Also, it was such a wonderful job that I felt guilty about making money out of it. I felt that I should be paying Mayor Vinson for the privilege of caring for those beautiful courts. This unhealthy attitude towards working for a living prevailed in almost every job I have ever had. Fortunately, I have learned not to express it so clearly to my employers, lest they take me up on my offer to pay tuition for working for them. However, this was my first experience with loving ones work, and I suggested to Mr. Sawyer that I become officially an amateur and let everyone play free.

Coach Sawyer assured me that it was not as if I were making a living out of tennis, and he was certainly right. However, during those days tennis was the most professionally restricted of amateur sports. The U. S. Lawn Tennis Association governed every player's habits and life. To qualify for a USLTA-sanctioned tournament a player could not represent any equipment manufacturer, nor could he receive income from the sport. Even receiving donations to pay for travel expenses to tournaments was officially forbidden. Although the USLTA did not officially sanction the multitude of tournaments between high schools, the statewide tennis association had the policy that all high-school tournaments must meet the USLTA qualifications. Fortunately, no one raised the question of my eligibility. Also, I easily managed not to get rich from tennis in Meridian.

Asphalt Courts

During the next two summers, until leaving for Georgia Tech, I managed the new asphalt courts at the new High School. They were near my house and were much easier than clay to keep in condition. However, I never identified with those courts and with that environment as I had in previous years at Highland Park. Perhaps by then I had begun to realize that making about \$50 for the summer was not the pathway to fortune that I had earlier thought. The compelling problem was that the asphalt courts were dull, dreary, black, with permanently painted lines, and I could do nothing about it. Even the nets were cheap affairs made of steel Cyclone fencing. Net-cord shots, resulting from a ball hitting the top of the net and going over, occurred more frequently with the wire nets. Those nets must have been designed by a mechanical engineer from Harvard or Stanford! They were useless for good tennis. The steel is so resilient that it deforms and rebounds from the ball impact and propels the ball up and over into the next court. Substituting the cotton nets that I had bought for the Highland-Park courts solved the problem and restored fair tennis.

The Southern Regional Tournament

Tennis is an example of the many "doing" activities that I so much enjoyed. I became intensely absorbed in each, although not in all at the same time. Once I had achieved, along with George and Winston, status as one of the central three for the High School team, I began to dream of eventually becoming good enough to be a playing professional. A traveling team of professionals played an exhibition match in the High School gymnasium, including Fred Perry and Ellsworth Vines, the great British champions. I envied them greatly; it looked like a fine life.

Not only was my idea of becoming a professional unrealistic, it shows how poorly coordinated and conflicting were my various emerging goals. Having become convinced, however incorrectly, that my family was living in poverty, I was determined to follow a career that would lead to steady income, enough to buy tennis rackets, golf clubs, and tennis balls. In those days, long before the era of lucrative prize money for winning tournaments, tennis professionals barely existed financially, only by giving lessons or being supported by some frustrated socialite. I knew I would never qualify for the latter. But, the real awakening came from emergence into the tennis world outside of the Mississippi high-school tournaments.

In the summer of 1939 Coach Sawyer suggested that Winston, George, and I enter the annual Southern Junior Tennis Tournament, that was to be held in Memphis, Tennessee. Coach Sawyer helped scrape up funds for the trip, with promises that we would bring glory to Meridian and to our benefactors. Official sponsors were forbidden in those days by the U.S. Lawn Tennis Association. I may even have hitchhiked to the Southwestern University at Memphis, where the tournament was held. That it should be called "Southwestern" reflects the then provincial attitude that nothing of importance exists west of the Mississippi River. In our more enlightened age, after Presidents Nixon and Reagan, its name has been properly changed to Rhodes University.

After registering and certifying that I had brought at least one unopened can of new balls, I was overwhelmed by the dozens of tennis courts, each with the new green composition surface that I had only read about but never played on.

The next morning the bulletin board carried a clipping from the Memphis paper, The Commercial Appeal, of news about the tournament, including the first-round draws. There were hundreds of entries, from states all over the country, even though it was billed as a "Southern" tournament. With so many players, it was not possible to mention many by

name. To my horror, the headline of that clipping announced, "McGehee Meets Pigford in First Round". It was headline news because Dick McGehee, from Miami Beach, Florida, had won the tournament the previous year. Usually the defending champion and top-seeded player is given a bye in the first round, but in this tournament there were no byes. That first match was scheduled for 10:00 am. that day.

To reduce expenses the tournament required that each player bring to his match an unopened can of new balls. One is opened for the match, and the winner of that match was to keep both the used balls and the unopened can. I was already in awe of McGehee's reputation as a devastating single player. After settling down, being only a little rattled by the spectators in the stands on the center court, where we were the featured match of the day, I applied everything I had learned from Coach Sawyer, but to no avail. I may have won two or three points during the match, but never a game. He won at love, 6-0, 6-0. The match was so short-lived that McGehee ended up with not only the unopened can but an opened can of three hardly used balls. Maybe that is why he played so well, to finish the match quickly and be ahead one can of balls.

I was also intimidated at the Memphis tournament when I found that most of the players were wearing shorts, which were just coming into vogue. I stood out like a sore thumb, in my white cotton pants. Today a kid would simply take a pair of scissors to them, relishing the distinction of fringed cutoffs. In those days no one would consider such a slovenly practice.

On the next morning the Commercial Appeal evidently felt that it must report the results of the match headlined the previous day. It was most kind. It observed that Dick McGehee was off to a good start, and that he was likely to be well primed for later matches, having been so vigorously pursued by Pigford in his first match. What they meant was that I ran like mad after every ball hit by McGehee, and even managed to chase down a few.

That was before the days of Coach Baxter, who frowned on wasting such energy when there was little chance of winning points. The Commercial Appeal also kindly commented on my classic flat stroke, obviously representing much tutelage and practice, and suggested that I might consider learning a proper serve. The reporter also commented on my gentlemanly behavior.

I did indeed enjoy seeing Dick McGehee make such beautiful shots. I congratulated him on every winner. In fact, almost every ball he hit was a winner. He might have hit a rare double fault, but I remember none. In our senior year, Coach Baxter, the High School football and basketball coach, became the High School tennis coach. Mr. Sawyer had not joined the faculty at the new High School but had elected to remain at Junior High. Coach Baxter instructed me to drop that gentlemanly approach of forever

congratulating my opponent and to become viciously focused towards winning.

He was the psychologist that I needed! It was a new idea to me, and it greatly increased my pleasure in tennis. I still congratulated the many successful opponents at the end of matches, but only then, and I even won a few matches. Coach Baxter knew nothing about tennis, but he knew about competition.

Having tested my tennis prowess against the outside world, it became evident that the pathway to become a successful professional, one who wins matches, would be more tortuous than I had expected. Also, I had begun to realize that in those days even the real live tennis professionals had to scratch for income. There were enough other fun activities to use in shaping a career.

Coach Sawyer's Basketball Team

Two other important lessons in life came from the basketball team that Coach Sawyer organized in 1938. That was the first year that the new High School was open. Mr. Sawyer remained on the Junior High teaching staff, so he organized a new basketball team that took over the gymnasium that had previously been used by the High School. Because Coach Sawyer was no longer the High School tennis coach, there was no indoor tennis instruction during that winter.

During the late summer of that year I was hanging out at the YMCA in Meridian. I liked to go there to swim and shoot basketball. Also, my brother had just gone north to the University of Illinois and wanted me to keep an eye on his girl friend, Marian Pinkston, who had graduated from The Mississippi State College for Women and had taken a job at the YMCA. The YMCA director was heard to be pursuing her. The YMCA basketball court was a little runt of a court, shorter than regulation size. The baskets were mounted directly on the end walls of the court, so there was no room for a player to run under the basket after making a driving lay-up shot. I had been practicing there for years and had learned that one can drive towards the basket by running slightly up the wall, depositing the ball in the basket when he reaches the top of his run up the wall. It wasn't too difficult with good sneakers.

Coach Sawyer frequently came to the YMCA. He saw me practicing the wall-climbing lay-up, and he saw how effective it was against the other kids who were playing there. I never had been a really good basketball player, but Coach Sawyer invited me to join his new team that would play in the Junior High gymnasium. That gym was also shorter than regulation length, and the baskets and backboards were mounted directly on the opposing walls. It was a new team of boys who had never played together. I was the only one who had practiced the wall-climbing lay-up, so Coach

Sawyer built his offense around that play. Evidently there was no rule against running up a wall to reach the basket, because basketball courts were not supposed to have baskets mounted on walls.

I was excited at the opportunity to play on a real team. Our next-door neighbor, Mr. Carl Myer, William Blum's uncle, was always interested in helping my pursuits, and he helped on this one. He subscribed to outdoor magazines that had stories about fishing, boating, and sailing, magazines that I avidly read in my tree house while dreaming of future activities. The magazines frequently advertised boating shoes called Topsiders, sneakers with little suction cups on the soles to prevent slipping on a wet boat deck. Mr. Myer ordered a pair for me. They were perfect for basketball at the Junior High gym. With those shoes I was able to run up the wall even closer to the basket, although never high enough for a slam dunk. Some of the other boys on the team also ordered some deck shoes.

After weeks of intensive practice, for which Coach Sawyer was famous in both tennis and basketball, our team began a season of scheduled matches against teams from small schools in the surrounding countryside. Some of those schools had only outside courts, others had courts in regulation gyms or in barns, but all of them had the baskets and backboards mounted on posts or suspended from the ceiling, so that a player could make the conventional driving lay-up, running under the basket to stop after sinking the basket. None of the other schools was prepared for our gymnasium. The forwards of our opposing team quickly learned, from getting plastered against the wall under the basket, that they had to abandon their short-court game. They were overwhelmed by our technique of running up the wall, but were unable to copy it effectively because it took lots of practice.

We never lost a game on our home court, in our non-regulation short gym. This was the greatest team success that I had yet experienced. The lesson, the third one of strategy and principle that I learned from Coach Sawyer, was:

PRESS THE HOME COURT ADVANTAGE!

Unfortunately, when we played these schools on their own courts, with properly hung baskets, we were not prepared for the normal running lay-ups. We had no place to practice them. We never won a game. Reflecting on this experience, I realized that I had learned the fourth lesson of life from Mr. Sawyer:

BEWARE OF THE OTHER FELLOW'S HOME COURT ADVANTAGE!

In fact, one of the schools had already learned the first of these two lessons. Their basketball court was in a converted barn, with overhead beams that had once supported a hay loft. If a player attempted a shot anywhere behind the line where penalty shots are made, the ball could bounce off a beam and into the hands of the opposing team. Here the play was entirely short-court basketball for us visitors, but some boys on the host team had marked on the floor positions from which they could thread a back-court shot high over the beams to the basket.

Because the small country schools were overjoyed at the chance of playing basketball in the big city of Meridian, most of the games were at our short gym. We had a season of smashing wins. The most satisfying was against another new team in Meridian. The new Junior College had just opened. It had a new basketball team that practiced in the gym at the new high school. Several of its players had been on the High School team the previous year. We beat them on our home court.

Also, I kept my eye on the young lady at the YMCA. She became my sister-in-law the next summer. Andrew Gainey, the boy from The Green House, had developed a golden voice. At the wedding he sang "Oh, Promise Me." He was also great on the baritone horn and once played a solo transcribed for baritone, "The Carnival of Venice", in a Sunday afternoon band concert at the Band Pavilion in Highland Park. There was no one in Meridian to give lessons on playing the baritone horn. How Andrew became so accomplished on the baritone is a mystery.

The next year, flushed with success in basketball under Coach Sawyer, I went out for the High School basketball team, coached by Mr. Jim Baxter. It was an outstanding team, starring my old pal, Ed Henson, who, in earlier years at The Green House, had used my lip as a stepping stone to athletic success. Unfortunately for me, the new gymnasium had a regulation court, with no end walls to climb for lay-ups. I enjoyed watching Ed from the bench, but that is all that I did. I soon realized that playing in the band at the basketball games was more exciting.

Coach Sawyer at Mississippi State College

Tom Sawyer would continue developing young tennis players in Meridian through the years of World War II and thereafter. His well-deserved reputation as a superlative coach was soon recognized, and he became Professor of Political Science and tennis coach at the Mississippi State University (MSU) in 1949. Almost immediately MSU became a dominant force in tennis in the Southeastern Conference. For the next twenty years Coach

Sawyer never had a losing season. His dual meet record with other schools was 216-36. His MSU teams won seventeen Mississippi Intercollegiate titles, either five or six Southeastern Conference Championships, and they were Conference runner-ups in three other years. In recognition of this superlative record, Tom Sawyer was inducted into the MSU Sports Hall of Fame in 1975.

It is well-deserved recognition of a distinguished career in coaching tennis. I add my own testimony to Coach Sawyer's patience and skill in teaching the wonderful games of tennis and basketball to an awkward little kid, and especially to inspiration that I received from Coach Sawyer in the field of mechanics and physics.

The tepid heat and humidity of Mississippi was good for our tennis. Playing actively in those conditions required more than usual concentration, and mental concentration is the key to competitive tennis. However, winters were sometime different. When frigid winds from the North dipped into Mississippi, we could experience bitter cold. Snow was rare, but Winston Cameron has researched the historic cold weather experienced in January of our last year in high school. Based on his wartime studies in meteorology at UCLA and his usual legal thoroughness in research, Winston reports that a freeze arrived on January 22, 1940 and remained until the afternoon of January 28. A low of 2° Fahrenheit was reached on January 27. Five and a half inches of snow fell on January 23. Of course, there was no equipment for snow removal. The absence of traffic let the snow stay on the streets for several days, and on the ground until February 1.

We had occasionally seen dustings of snow in previous years, but never enough for snowballing and real snow men. For this rare event, school was discontinued, so we concentrated on winter sports. With Winston's brother, Ben, we constructed a sled with wooden runners, capable of holding as many as five people. There were no hills for sledding, but the big sled was to be towed by a big black Buick Roadmaster, supplied by Edgar Morrison's family. The towline was half-inch Manilla rope, about sixteen feet long. By letting air from the tires, the big Buick was able to traverse along every road in Lauderdale County. Going straight was boring. Edgar swerved the big Buick, slinging the sled from side to side, sometimes from ditch to ditch. The most thrilling moments were when the Buick had to slow down or stop. Our sled was large and heavy, and we had not thought of adding brakes. The person in the front of the sled was responsible for keeping us from plowing into and under the Buick, but he was seldom successful.

No one required us to have crash helmets, seat belts, and crash equipment, or to file an Environmental Impact Statement, all now necessities of modern life. Probably our parents did not really know what we were doing. That was the best winter that I remember in Meridian.

Tennis at Georgia Tech

Going to Georgia Tech from Meridian High School was a lucky accident, similar to most of the important events in my life. It was the result of a misperception by my father. My brother had studied chemical engineering at Mississippi State College and then went to University of Illinois for graduate study. But, these were the depression years. My father concluded, incorrectly, that education at Mississippi State had not been good enough for my brother to get a job, so that he had to study further to make up deficiencies. That was to be avoided by sending me to Georgia Tech. It was not an easy decision, because it cost a lot to travel to Atlanta, Georgia, and out-of-state tuition was expensive. I didn't complain.

There have many good players who were taught the same strokes that we learned from Coach Sawyer, including the number one player on our team at Georgia Tech, a boy named Nelson Abel from Monroe, Louisiana. I was proud to see him demolish his counterpart during a match with Princeton. The Princeton team was exotic. They had beautiful clothes and a coach, Frank Parker, who was a famous professional, a winner of the U.S. Open in singles or doubles, and playboy, a plaything of a wealthy New York socialite. In contrast, our Georgia Tech coach was a dumpy mathematics professor. I never saw him with a tennis racket. He must have volunteered as coach to earn credits toward tenure.

After the freshman year and a summer lifeguarding at Meridian's Highland Park pool, tennis came to a halt for me, except for occasional visits to Meridian, to serve as a target for Mr. Cameron's blistering shots from his backhand court. Tennis was no longer a number one priority. For the first time, I found learning things from textbooks and laboratories to be the most fun of all. For me, concentrated study was impossible without exercise, so I turned to competitive swimming, a rigorous year-round activity that required only one hour each day. The few years of conditioning from swimming greatly improved my tennis, when it resumed years later.

To M.I.T.

I was so enthralled with the academic program at Georgia Tech that I wanted never to graduate. Colleges protect themselves against such dilettantes by blocking registration when academic requirements are fulfilled. I was amazed to find that some of the famous northern schools would actually pay my tuition and living expenses to enroll there for graduate study. The best deal was at the Massachusetts Institute of

Technology, in Cambridge, Massachusetts, where I timidly entered in October 1943.

I was adopted by Rowland Bevans, a classmate in chemical engineering. He had already been a graduate student for a year or two before I arrived in 1943. He taught me to ski, in New Hampshire and Vermont, and we had many fine weekend trips on the snow trains, staying overnight in ski lodges or farmhouses and eating apple pie for breakfast. The Boston winter was terrible for tennis, so Bevans began teaching me to play squash, an indoor game similar to handball but played with a long, slender racket. Learning squash took a few years and is discussed later. We had many fine games of chess together.

Learning to Speak

M.I.T. was even more thrilling and fun than Georgia Tech, something I never thought possible. I was deferred from military service because I taught chemical engineering to Army students at M.I.T. In the mornings and evenings I served these same students chow in return for free meals. Teaching an M.I.T. course was more than I had ever hoped for. Unfortunately, it nearly ground to a halt after the second class. The course that I taught was a laboratory course on chemical engineering operations, including distillation, as in separating alcohol from water, and ion exchange, as in water softeners used to remove dissolved salts from household water. My job was to give an introductory lecture for each class, to prepare the students for the experiment. The lecture on ion exchange was carefully prepared, but after a few words about "ions and ion exchange", several student hands went up. The question was, "Sir, what is this iron that you are speaking of?" Of course, the lecture had nothing to do with "iron". After continued questioning, I wrote the words "ion exchange" on the board, producing a sigh of relief from the students. Now they were with me.

Of course, the problem was mine, not theirs. The word "ion" is pronounced with a hard "i", like "eye". That requires speaking from the front of the mouth, with tightened vocal cords. I had never learned to speak that way. My "ion" came out as "ahn", sounding to them something like "iron", from the back of the mouth. None of my students was from the South.

Writing on the board salvaged that lecture, but similar problems continued. It took so much time for translation through writing that the students fell behind in their experiments. I was getting a little frantic. It reminded me of a communication problem in the seventh grade, although that one had nothing to do with Southern accent. Soon there was a student delegation to the Department Chairman, Professor Warren K. Lewis, the world

giant of chemical engineering. The delegation politely told Professor Lewis that they liked this new man, Pigford, but they could not understand him and were falling behind in the course. Completing this course was an Army requirement.

Professor Lewis was from southern Delaware, which waffled its allegiance during that misunderstanding between the states and almost joined the Confederacy. He called me in and explained that he was partial to me as a fellow Southerner. He warned me that my deferment depended on my teaching that course, and that I was obligated to provide the instruction that M.I.T. and the Army expected. After listening to one of my lectures, he concluded that he could not understand me either. He was a talented and innovative man. The solution was to give Pigford special instruction in speaking English. Boston has many such courses, for Italian and Irish immigrants.

Professor Lewis arranged for an intensive course that occupied much of my time for two months. The instructor worked on fundamentals, much like Coach Sawyer's approach to tennis. After weeks of exercising the throat, mouth, lips, and tongue, I was able to say "ion" with a strong, hard "i". My course was salvaged. I was even taught to call an automobile a "cah", and other words in New England brogue, speaking like Jack Kennedy. Fortunately, the New England brogue did not help in teaching the Army class, so it did not persist.

Speaking this new kind of English, with hard vowels and consonants, required conscious positioning of the mouth and throat before every such word, and it still does. Speaking that way in lectures finally became a habit, so there went much of my Southern accent, something I never expected or wanted to happen. One course in pronunciation would have no lasting effect, but after eleven years of subsequent teaching at M.I.T., with legitimate complaints by students when I became sloppy, speaking with hard consonants and vowels became imprinted.

The aftereffects were dismal! In the next trip to Meridian to visit my parents, I was properly accused of "talking like a Yankee". I could then easily break the habit, but eventually it became too difficult.

Sailing

A major feature of M.I.T. is its sailing. M.I.T. is located on the east side of the Charles River Basin, a large body of water that separates Cambridge from Boston. I first lived in Boston, on Marlboro Street, in the fine old Back Bay section. It was bracing to walk across the long Harvard Bridge every morning and evening, in the miserable freezing winter weather.

M.I.T. maintained a fine sailing club on the shore of the basin, just a few minutes walk from the classrooms. There was a fleet of forty sailing dinghies, all lightweight, lap-strake wooden boats, fourteen feet long, designed by Nathaniel Herreshoff, the famous Cape Cod naval architect. The boats were all identical, for intercollegiate racing. Also, they could be checked out by students and staff for pleasure sailing.

Sailing had long been one of my favorite hobbies, even though I had never sailed before. As early as I can remember I would pour through the outdoor magazines handed down by our neighbor, Mr. Carl Myer, William Blum's uncle. Reading the stories about sailing occupied many wonderful hours while hidden away in my tree house. From those magazines I learned all of the strange terminology of sailing. For example, the "boom" is that stick that projects out from the mast to hold the sail down and that bumps heads when turning across the wind. "Port side" is the left side unless you are facing backwards. A "line" is a rope used to pull something, but it must never be called a rope, unless it is no longer useful as a line or unless you work for a chandlery. Lines that control the angles of sails are "sheets", even for little sailboats with no place for sleeping. I kept accumulating this knowledge of sailing words and was ready to use it in real sailing in Boston.

When spring came I went to the M.I.T. Sailing Pavilion and asked to take out a boat. I expected that a test would be required, so I had thought out every sailing maneuver in my mind. I learned, unfortunately, that M.I.T. requires everyone to complete a four-month sailing course. With the war and military service nipping at my heels, I decided to try the Boston Sailing Club on the far side of the Charles River Basin. Their boats were about the same size as those at M.I.T., though not as good. They required no formal instruction, so I asked for a test to qualify as skipper, a person who can take a boat out by himself. I had no time to waste on beginning as a crew. I had never actually been on a sailboat, but it seemed pretty simple.

Fortunately, it was one of those hot, windless days in Boston. Otherwise, I probably would have repeated the disaster of my first tennis tournament, at Marion Park School in the fifth grade, when I boldly entered the tournament without ever having played tennis. There was no breeze, except for a weak puff when someone opened a window in a nearby riverside apartment. The boat could only drift. The examiner was so impressed with my knowledge of sailing terms that he assumed, incorrectly, that I was an experienced sailor, so I passed. I then proceeded to learn to sail, sailing at every opportunity and in every kind of boat possible as the forthcoming Navy service moved me around the country and part of the world.

Sailing is a wonderful and absorbing pastime. The interaction of sun, wind, sails, hull, and water is pure poetry and music, sometimes quiet

and gentle, sometimes a blustering challenge to stay afloat, always changing. Racing against others in one-design sailboats of the same measurements is as competitive as a good game of chess, although to an observer it is like watching the grass grow!

The Ensign

In spite of my new, polished New English, deferment disappeared in early 1944. Instead of accepting employment at an unknown place in Tennessee called Oak Ridge, I gladly chose to accept a commission in the Navy, together with a clothing allowance for bright, new uniforms, which kept me in Boston for a couple of years.

I was among a group of graduate students selected by the Navy to learn the new science of radar, developed in part at M.I.T. We were supposed to wear uniforms at all times. The Navy assumed that its new ensigns would purchase their uniforms from the "ship's stores" at the nearby Charleston Navy Yard, where the great ship from the war of 1812, the Constitution, is anchored. However, there were several private tailors who had shops near the Navy Yard, and they were popular with young officers who wanted a more modern cut to their uniforms. I soon had an elegantly tailored ensign's outfit, together with a non-regulation jacket patterned after the new short jacket worn by General Eisenhower. A status symbol in the Navy was to wear clothing from other services, like the Air Force or Marines, even though we were supposed to do that only aboard ship.

I couldn't wait to shoulder my responsibilities as an officer, whatever they were. Not knowing the protocol, I saluted every person in sight wearing a uniform, but was surprised at how few people returned the salute. A week later our group entered a concentrated program, led by a rather tired Chief Petty Officer (CPO), to teach us the fundamentals of being officers in the U.S. Navy in wartime, including whom and when to salute, how to acknowledge a command with no intent of performing it, how to speak and understand Navy Language, how to appear to be a seasoned Navy officer, and how to procure cigarettes and nylons to barter with the natives.

The textbook response to a command from an officer is "Aye, aye, sir." According to the textbook, this means "I understand and will obey, sir." If between non-commissioned sailors, the "sir" is omitted. The "Aye, aye" is a convenient expression. It stops the person giving the order from saying more. In reality, the person ordered seldom does what is ordered. Thus, "Aye, aye" conveniently postpones arguments to a later time.

I now believe the Navy textbook to be wrong in the meaning of "Aye, aye". The textbook correctly states that the command, like so many Navy

terms, is of old English origin. But, the Navy is wrong when it says that "aye" means "I have understood and will carry it out." It seems to me that "aye" is likely a Cockney derivative of the Japanese "hai", pronounced by the phonetics "ha-ee". When said as one word it sounds almost like our English "hi". "Hai" is used by Japanese to mean "yes", usually expressed as "hai, hai".

However, the affirmative "hai, hai" does have a distinctive meaning. It does **not** mean "Yes, I agree" or "Yes, I will do so." Not knowing this, many American business men, seeking to tap the financial riches of post-war Japan, have plodded through long negotiations with their Japanese counterparts, reassured by "hai, hai" many times during the discussion. Thinking he has arrived finally at agreement, the American produces a contract for signing, only to hear in dismay a polite refusal from the Japanese. Actually, "hai" means only "Yes, I understand"; no agreement or promise of compliance is intended.

Thus, this new idea that "aye, aye" was derived from "hai, hai" can explain the prevailing practice of Navy personnel who do nothing after acknowledging "Aye, aye". The fact that there was no known interchange between Britain and Japan during the early years of the British Navy raises the question of how "hai hai" became known to Britain. However, that is a mere detail that can be solved with further research or imagination.

We ensigns were understandably embarrassed that our duty was entirely on the shore, in a safe, dry, classroom environment, with no connection to boats and water. We stood out like sore thumbs among the seasoned salts in the fine Officer's Club on Boston's Boylston Street, because of our new uniforms with the bright gold braid and stiff, starched, white hat covers.

One day we learned that our CPO instructor had added new gold braid to his now heavy uniform, in recognition of his extended service and hazardous duty in teaching young ensigns. But his new braid looked like the old. We needed no further instruction. A few twists of lemon from Officer's-Club martinis produced enough citric acid to dull the gold braid and buttons. The proper rakish look of the hat resulted from sitting on it until the white cover drooped a little towards the ears, like the bill hats worn by the British Air Force officers. It was too successful. People at the Officers Club asked why I was still only an ensign.

The rest of the Officer's Training Course was about Navy Language, and that was a mouthful! I already had a good start, having already learned sailboat language. But there were many new expressions for big boats and ships, fleets of ships, and for bureaucratic Navy organizations.

Navy Language is deceptive. It even uses a few words of English here and there. We were instructed to use this language in all activities dealing with the Navy, whether aboard ship, in the Officers Club, or in our special classrooms at M.I.T. We had to learn that the "head" is not the captain

but the bathroom. The "galley" is the kitchen unless it is a boat full of slaves and oars. "Sick bay" results when an oil tanker is improperly piloted, like the Exxon Valdez; it is also the local infirmary. "Belay" means to secure a line to something, such as a "belaying pin", unless it means to stop doing something. Thus, "belay belaying" means to stop securing a line, unless it means to resume action.

The Navy uses this complicated language so that it can have a four-year academy at Annapolis, where they teach officer cadets to talk, salute, and to sail. Annapolis is the most scholarly of U.S. military academies, the only U.S. educational institution in which all communication is officially in a language other than English.

The Longwood Cricket Club

The best of Boston was open to young Naval officers, through the countless invitations on file at the Officer's Club on Boylston Street. I accepted an invitation to play tennis with an old, established New England family, the Hardings, who lived in Chestnut Hill. They belonged to the Longwood Cricket Club, where there is little cricket but droves of beautiful grass courts manicured like putting greens, like the center court at Wimbledon. These were the first courts that were as beautiful to me as my two clay courts at Highland Park in Meridian. They surpassed anything that I could have imagined.

The Longwood club house is an extensive mansion, all white, with gold fixtures in the locker rooms. They required that all tennis clothing be completely white and clean, except for the warm-up sweater, which could have one thin black stripe and a thin red stripe. It was the first time that I had ever used white polish on tennis shoes. Even the balls were all white in those days. Of course, after a long set the balls developed a green tinge from the grass, but in a place like Longwood new balls are produced so readily that the all-white decor remained. Most of the men played in white flannels, but by then white tennis shorts had also become acceptable.

As so frequently happened, the Harding's invitation for young Navy officers to play tennis was not completely altruistic. There were two Harding daughters, Beth and Gerry. My role was to bring another male player and play mixed doubles. I found the perfect fourth in Rowland Bevans. He received military deferment because his graduate research was related to a military project, so he was happy to be the fourth for mixed doubles at Longwood. Bevans was the son of a Congregational minister in Quincy, Massachusetts. He was tall, fit, excelled in many sports, and had a classic New England accent. When the Hardings saw his prep-school

blazer, he would be readily accepted as a perfect fourth for play at Longwood. The Hardings might even think that he was Episcopalian!

The Longwood doubles were pleasant tennis, but not exhilarating. The Harding girls had received years of private lessons from the Longwood professional, but they were not really competitive. Bevans and I soon learned that we were expected to serve easy balls for them to return. Gerry was the closest to a real player. She was something of a tomboy; she ran hard after every ball. She was the first female player whom I had seen perspire. It didn't bother her at all. My heart warmed to her; here was a girl who had learned to really enjoy the game. Occasionally Beth and Gerry would sit out a set and allow Bevans and me to play singles, but not often.

The Harding girls were not nearly so pretty as the grass courts. At every opportunity I talked with the many court attendants about their work, and I finally met The Master of the Courts. He was not the teaching professional but the man in charge of all the maintenance. What a terrific job! I would readily have gone AWOL from the Navy and dumped my M.I.T. studies for a chance to perform the artistic manicuring of the Longwood grass courts.

I had already realized that I must not mention to the Hardings my dim background as a small town "tennis pro". If they had known, I would have been politely but firmly excluded. It was alright for their daughters to take lessons from the real tennis professional at Longwood, but it would be unacceptable to include such a person in a social game within this proper old New England family. I could expect even less tolerance if they knew that my real pro activities were as a grounds keeper, and that I identified more with The Master of the Courts than with the playing-teaching professional.

The Hardings thought that I was a little strange and were somewhat embarrassed about my fascination with court tending. However, they had learned to expect odd things from this unpolished Southern boy, and it was obvious that I was certainly not Episcopalian. When I first appeared at the Harding mansion in Chestnut Hill, to meet the family and to be introduced to the Longwood scene, not only were the daughters Beth and Gerry in tennis clothes, but their mother as well! I was unprepared for such unconventional habits. In Meridian male adults and older people, like Mr. Ben Cameron and Dr. H. M. Ivy, played tennis, but I had neither seen nor heard of a woman at that age playing tennis. Even Meridian's Nell Sanders, playing tennis while in her thirties, was an anomaly. I blurted to Mrs. Harding, "Are you still playing tennis?" She was a well bred, fine woman. She laughed politely and said, "Yes, at Longwood even we older women play at times." I liked her best of all.

On rainy days or in the evenings we played tennis with the Harding girls on the beautiful indoor courts at THE COUNTRY CLUB, in nearby Brookline. I had already learned about THE COUNTRY CLUB, which had an open invitation at the Officer's Club for young officers to play golf there. Playing golf with the daughter of a member was not required. I offended even the motherly director of the Officers Club when I asked what THE COUNTRY CLUB was. I learned that any well-bred citizen anywhere from Princeton, N.J. to the far north will know about the one and only COUNTRY CLUB. It is so exclusive that it needs no further identification. It is a private club, more elegant and with more gold fixtures than even the Longwood Cricket Club. The golf course was the most beautiful I had ever seen. Servants in uniforms or formal clothes opened doors, brought tall drinks, handed out and washed golf balls, and raked sand in the traps. There was a professional for every activity, including massages.

Adjacent to the building with the indoor tennis courts was an indoor arena for equestrians. In another building was a rink for ice skating and a curling ice, shuffleboard-like strips of ice where strange people with unmatching shoes bowl large oval-shaped stones and other people sweep the ice rapidly with brooms to help guide the stones to stop at the proper place at the other end. I have pointed out to my curling friends in Weston, Massachusetts, that a blow torch would be more effective, but they are traditionalists and prefer the brooms. There were several good squash courts, where I played years later, in the Boston-Cambridge squash league.

The indoor tennis courts at THE COUNTRY CLUB were clay, much like my courts at Highland Park, except they used wet-lime lines instead of my glowing dry-lime lines. I never managed to meet the Master of the Courts to suggest dry lines to him.

Squash

The adventures of the war years led me to San Diego, Hawaii, shipboard life in the Pacific, Japan, San Francisco, and back to M.I.T. Graduate studies continued to be the greatest fun, but skiing occupied most weekends and squash the late afternoons on week days. Squash is played in a box-like court, similar to a hand ball court. A hard rubber ball is used, between a hand ball and golf ball in hardness. The racket is as long as a tennis racket, but much lighter, with a long wooden shaft from the handle to the head. The strung head is almost round, only about eight inches across. The strings are gut, similar to tennis strings, but strung a few pounds tighter. The walls, floor, and ceiling of the court are all white, which makes it easy to see the black ball. The ball can be allowed to bounce on all surfaces except the ceiling. The ball must bounce no more than once on the floor before being returned. The return must reach the front wall about a foot above the floor or higher. That's all there is to it!

Because the ball is so hard and resilient, it can be hit quite hard. It goes the fastest of any of the games played with rackets. There is a lot of solid geometry involved, because the ball can carom off any of the walls, any number of times, as long as it only bounces once on the floor. The hard ball comes off the sweet spot of the racket so fast that it raises a bad welt and bruise if it hits a player. Mr. Cameron's sizzling shots from his backhand court were nothing compared to this. Of course, there are two players in a singles match. Each player is supposed to move aside after he hits a shot, enough to give the opponent room to make the return. However, if one moves too far out of the mid court, it will be easy for his opponent to put the return out of reach. Consequently, the players are close together. To keep one player from crowding the other, the rules provide that if a player is hit by the opponent's ball, that player loses the point. That rule invites some ungentlemanly strategy, such as hitting your opponent with the ball.

M.I.T. had a fine squash and tennis professional, Jack Summers. In his 50's, he could easily beat most of the best squash players around. That is the mark of a great game, when it can be played competitively so late in life.

I became far better at squash than I had ever been at tennis. Tennis is the better game, more demanding and difficult, but squash was the better game for me. It could fit into a tight daily schedule and was available when needed. Also, the squash ball has no fuzz; it lasts until it breaks apart, whereas in tennis having to buy new balls so frequently is a nuisance. However, I could never be happy as a caretaker of squash courts.

All that is required is repainting once or twice a year to cover the little black smudges where the ball has bounced off the floor and walls.

Squashmanship in the Boston-Cambridge League

I was finally able to win a few games from Bevans, who was still at M.I.T. on its research staff. We challenged each other daily on the indoor courts, but we faced many other players in the Cambridge-Boston city league. We graduate students and staff were not eligible for intercollegiate competition, but the city league was designed for people like us. Representing the M.I.T. Squash Club, we competed against Harvard University, THE COUNTRY CLUB, the Union Boat Club near the Old State House on the Boston Common, and the Harvard Club in Boston, and there were occasional matches in New York at the Harvard Club, the Yale Club, and the New York Athletic Club, and also at Princeton.

I had to learn new protocol. Most of the city clubs were exquisitely furnished, like THE COUNTRY CLUB that I described earlier. In dressing

out, we were offered massages to loosen the muscles and tall drinks to relieve the tension. At my first match at the Union Boat Club, I failed to notice that my opponent host did not drink, even though he ordered one for me. I wanted to try everything, but the double Scotch and soda quickly lost the match for me. The massages were worthwhile, though, after coming in from the frigid Boston winter.

There was an M.I.T. team for graduate students, research staff, and other employees and one for faculty. I played on both because, as so frequently happened, I did many things backwards and out of order. M.I.T. appointed me to the regular faculty before I finished as a graduate student. This was typical of the lucky accidents of my career. I was ill prepared and had no qualifications; college professors have no training in teaching. I was assigned to teach courses that I had never studied, frequently teaching graduate students older and smarter than I. My job was to teach some of the new science from World War II, most of which I had not learned. There were no textbooks, so I wrote text notes while struggling to learn the subject and stay ahead of the students. In spite of the pressure, that was the most fun I have had in teaching. As I had felt in earlier jobs, it seemed wrong that M.I.T. should pay me for doing such interesting things. The faculty job did interrupt my student work and deferred my graduation. I didn't mind, because by then I wanted to be a student forever. However, being on both squash teams became complicated at times and lasted longer than I intended.

One of the scheduled league matches was for our faculty squash team to play our graduate-student squash team. Each team maintained a ladder ranking of its players, and players of corresponding ladder ranking were paired for a match, the number one player on one team playing the number one player on the other. I was ranked number four on both B-league teams, so I was scheduled to play myself. I explained to Coach Summers, who was coach and match overseer for both teams, that I could easily do this in squash. I could hit one stroke for one team and the next for the other. It only required that I remember which team I was hitting for when I finally closed out a point. There being only one human body on the court, I would not get in the other player's way, so it would be a better game than usual. No one would dare to question my integrity by suggesting that I might not try hard for both sides.

Coach Summers could find no rule on this subject. It was important to find a precedent. The National Squash League is as tightly controlled as the U.S. Lawn Tennis Association, probably because so many Harvard and Yale lawyers are avid squash players. Coach Summers was swayed by my logic, but he demanded a demonstration. Unfortunately, I had to play so slowly, to remember which side I was hitting for at the time, that it was a dull, slow game. Coach Summers said that with that kind of play both of us would have to be demoted to the lower C League.

Ranking adjustment is ruled out for league squash. Each team ladder is established by a formal process of challenge by team players. Every week or so I would receive a formal notice of challenge from a team colleague lower on our team ladder. It was like a traffic citation, to be responded to within a specified time. The team ranking is posted and must be adhered to. Coach Summers was presented with a dilemma. He telephoned the National Squash League in New York, which advised that the match of Pigford versus Pigford should be defaulted because of incompatibility with existing legislation. Coach Summers did exactly that. In the Squash League, a default by one player is credited as a win to the other player. He listed a win by each of the Pigfords, resulting in one more win than there were matches. I heard that the lawyers added a few lines to the rules at their next meeting.

This was my first encounter with rigid legislation of a competitive sport. It was trivial compared with what I was to experience in competitive sailboat racing. Except in America's Cup races, the competitors themselves police the race. Each competitor must watch his opponents and inform the Race Committee of any infraction. That is why competitive sailing is the ultimate "gentleman's sport." In races each boat must carry a red protest flag, to fly from the shrouds to alert the Race Committee of an opponent's infraction. A Protest Committee hears the protest after the race. Some sailors take this very seriously. Once when I protested a former friend of mine for not rounding a buoy properly, he counter-protested me on the grounds that my protest flag was two inches under regulation size. I was disqualified. If I had realized what an intense sport sailing would later become for me and my family, I might have taken some courses in law at nearby Harvard. Harvard and M.I.T. allowed free exchange of students.

On other match days in the city league I played two matches, to represent both teams, traveling across the city from one club to another, until M.I.T. consolidated its graduate-student and faculty squash teams. Playing on two teams was good for my squash, but it was more tussling than I needed!

The Confederate Flag

It was a fellow Southerner, Frank Weems, from Birmingham, Alabama, who got me in trouble with the law about a Confederate flag. Frank and I were roommates in The Graduate House, a separate M.I.T. dormitory for graduate students. It was an impressive former hotel, six stories high, at the Cambridge end of the Harvard Bridge, overlooking the Charles River Basin, with the M.I.T. Sailing Pavilion nearby. Our suite of rooms was on the western corner of the top floor. An earlier roommate, John Carleton, had been awarded that prize suite because of his political power as

Commodore of the M.I.T. Sailing Club. He could survey all sailing activities from our window. John and I were both dedicated to our careers as perpetual students, sailing, squashing, skiing, studying new subjects, and doing new experiments, supported by fellowships or other stipends. Unfortunately, John made a disastrous mistake. Like many careless students, he fulfilled all degree requirements, so he had to graduate and go to work for a living. I kept the suite and added Frank as a new roommate.

One of the graduation presents from my Georgia Tech friends was a large Confederate flag, 4 by 8 feet, which they hoped would add security for my ventures in the North. Carleton didn't approve of it, but after he left the flag was proudly displayed on the wall of our Graduate House suite. It was the inspiration for many parties. Frank Weems was the best party person in our graduate school. Our suite was the most desirable for big parties, with girls from Radcliffe, Wellesley, and from the Boston finishing school called Katherine Gibbs. With Frank, it was almost a continuous party.

One weekend, when I was in New Hampshire skiing, Frank had climaxed one of his parties by taking the shower-curtain pole from our bathroom and erecting it as a flagpole from the large bay window of our tower suite, proudly displaying my large Confederate flag. It was reported that he did this with proper ceremony, playing "To the Colors" on his Boy Scout bugle. The flag did lend charm to the Graduate House, and our House Master, Professor Avery Ashdown, kindly ignored it.

About two weeks later, when Frank was in Connecticut for a job interview, I was contacted by a Boston newspaper, The Boston Globe. The reporter said that he had heard about the Confederate flag and wanted to do a human-interest story for the paper. I admitted that it was my flag, but pointed out that flying it for all of Boston to see was my roommate's doing, and that the story should focus on him. The reporter said that it was kind of urgent, that it would occupy only a small space in a back page of the paper, and that he would even give some publicity to a new Graduate School dance that I was helping organize. I had not yet learned to question what reporters say. The reporter brought his photographer, who took a picture of me hoisting the flag, obviously misleading because it was Frank who usually performed the hoisting ceremony.

The next morning the Boston Globe carried as its leading front-page story a large picture of me hoisting the flag, with a headline that announced "Flag of a Foreign Country at M.I.T." The reporter had been less than forthright with me. Evidently the Daughters of the American Revolution (the DAR), headquartered in Boston, had seen the flag from the Boston side. They complained to the District Attorney that the flag of a foreign country was being illegally displayed from a Cambridge building

that was neither an embassy nor a consulate. Apparently there is some ancient Massachusetts law that prohibits this, unless explicit permission has been obtained from authorities. It was enacted during the Revolution, to harass British sympathizers. The District Attorney had promised that he would investigate, and the Boston Globe heard about it. They mentioned nothing about Frank Weems in the article. I was identified as the culprit, a young instructor at M.I.T.

Dr. Ashdown, the House Master, was most sympathetic. He was all for free speech and freedom of expression, but it appeared that Frank's flag flying may actually have broken the law. I agreed to remove the flag. I was more concerned about my job as instructor at M.I.T. M.I.T. would not want its students to be taught by some foreigner who was under indictment.

The M.I.T. attorney assured me that he was determined to fight the District Attorney and the Daughters of American Revolution to the end. He was not interested in the fact that it was Frank who should be indicted, if anyone were to be indicted. The attorney had once visited New Orleans and had taken a river boat to Natchez. He was enchanted with the South and believed that everyone in the South lived on a plantation. Evidently he was ambitious and saw the chance of making a name for himself in a trial of principle, like Clarence Darrow in the Scopes trial.

Meanwhile, the District Attorney said that it appeared to be a federal issue, because the law that we had allegedly broken was enacted on behalf of the colonies, not just for Massachusetts. He was planning on turning the matter over to the U.S. Attorney. Fortunately, my department chairman, Professor Walt Whitman, convinced the M.I.T. attorney that it would be best for all concerned to ask the District Attorney and the DAR to drop the charge, with our promise that the flag would not leave the wall. Frank came back from Connecticut and was envious that he missed all the great publicity, but he agreed to behave. After that I worried about going skiing as long as he was my roommate.

Such a good story soon appeared on Associate Press's national newswire. The story was intended to say "Student Consecrates Confederate Flag in Massachusetts". That may be what the AP story said, but the Memphis Commercial Appeal was careless and printed it as "Student Desecrates Confederate Flag in Massachusetts". Everyone in the South believes The Commercial Appeal, so I got into more trouble.

It was only a few years later that I became acquainted with some of the Boston brahmins, many of whom were good Episcopalians and members of the DAR. I was glad that they had forgotten about the flag incident.

Another Diversion

There was little time for other diversions, but I did manage to meet Katy, real-named Catherine Cathey, a member of the research staff in M.I.T.'s metallurgical laboratory. Her boss, Professor Nick Grant, is one of the most innovative professors at M.I.T. in attracting graduate students to carry out his research on high-temperature alloys for gas turbines. He employed two beautiful young ladies on his research staff, even though they had no formal training in metallurgy. They added style and sophistication to the project. The other girl, Ava Maria Lewis, now Whittemore, lives in Weston, Massachusetts, and on Cape Cod. She is still Katy's best friend and mine. These girls learned fast. Ava became expert in precision lost-wax casting and Katy in X-ray crystallography.

Katy and Ava once lived in a third-floor apartment in a revolutionary war building in Boston, on an alley that had once been a cow path near the Old State House, so I could not cruise by there on my bicycle to attract Katy's attention. Their apartment at "The Alley" was just a stone's throw from Lewisburg Square, where Bostonians sing carols at Christmas in the snow-covered square and play Christmas music with hand-held silver bells. I frequently dropped in on Katy's M.I.T. laboratory and tuned her crystallographic X-ray unit. I managed to keep it out of order enough to justify frequent returning visits. Although a well-bred Southern girl from Florence, Alabama, Katy played tennis actively, she played hard, and she even perspired like a real person!

M.I.T.'s squash courts were a mere extension to the men's locker room and were out of bounds to females. I did manage to smuggle Katy into a squash court one evening. Besides having a devastating loft-type tennis forehand, however unorthodox, Katy was, and still is, lovely, intelligent, friendly, the light of my life!

M.I.T. had given me a fine fellowship that paid tuition and a stipend, none of which was taxable. Katy learned that if we were married I would become a tax deduction on her income as a member of the research staff. She is a shrewd person, so she selected the afternoon of the last day of the year, December 31st, to make it legal, receiving an entire year of tax deduction for a mere few moments of passion. Knowing that she might file for annulment the next day, the first day of 1949, I postponed separation by suggesting a honeymoon in Bermuda, consuming all of her savings and making her dependent on me as a continuing tax deduction. We played much tennis in Bermuda, as well as golf. Katy even liked to bicycle. The Bermuda games were close and challenging. Largely because of my sound ground strokes from Coach Sawyer, I was the winner. Rather than

quit as a loser, Katy continued as tennis opponent, tennis partner, confidant, wife, and friend.

Later chronicles deal with tennis in La Jolla, Charles Marion floating across a Pasadena black asphalt court in black basketball shoes, being proselytized by Berkeley's Dean O'Brien on the center court of the Claremont Tennis Club, traveling as a tennis-playing legal assistant to Judge J. D. Bond, struggling to hear in the reverberating bubble courts at Wellesley with Bob Howard, playing moon-ball tennis in Kyoto, and finally being defeated by Katy, after she took lessons from Frank Kovacs and used an illegal giant racket called the Prince.

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Coach Sawyer's lessons in tennis, basketball, and in life were what this little blond-headed kid needed to tussle, to find his place, and to compete. They contributed to enjoyment of life far beyond the pleasure of the sport itself. He was a dedicated and gifted teacher. If we were so fortunate as to have a Tom Sawyer at Berkeley, I would advise aspiring physics students to learn tennis and lessons of life from him.

Thomas Pigford

November, 1990

APPENDIX B**COMMENTS FROM COLLEAGUES**

"I express my admiration for the consistent standard of excellence in which Tom Pigford has served the University and the Department of Nuclear Engineering for those thirty years."

Dr. Glen Seaborg, former Chancellor of U.C. Berkeley, Nobel Laureate and former chairman, Atomic Energy Commission at Tom's 1989 retirement symposium

"Tom is a national treasure".

Dr. John P. Garrick, Chairman of the Board, PLG (Pickard, Lowe & Garrick) in a May 1996 conversation with Betty at a meeting in Los Vegas

"The story that best exemplifies the risks associated with ignoring Tom's prudent warnings goes back to the late 1960s when he served on the AEC's Atomic Safety and Licensing Board (ASLB) during the construction permit hearings on Diablo Canyon. The intervenors wished to put on an afternoon of testimony about the possibility of an offshore earthquake fault. The utility and the AEC staff opposed permitting such testimony, and the ASLB majority ruled in their favor. Tom dissented, asking what harm could be done by hearing the testimony and pointing out the harm that could occur were the issue not addressed. The nuclear plants were approved, and mid-way through construction the Hosgri Fault offshore was discovered, requiring the utility to retrofit the facility at a cost overrun of many billions of dollars.

"Throughout his career, Tom has been a voice of reason and thoughtfulness in an arena often dominated, on both sides, by a near-religious ideological commitment. Be it his courageous dissent on the National Research Council panel on Yucca Mountain standards or his concerns about renewal of reprocessing, had his warning been taken to heart, the prospects of a safer, more proliferation-resistant, and more publicly acceptable nuclear future would be substantially different than they are today. I must say

that my own view about nuclear power would likely have evolved in a different direction had the industry and its associated agencies been in the hands of more people of the caliber, foresight and integrity of Tom Pigford."

Daniel Hirsch, President, Committee to Bridge the Gap; former Director, Adlai C. Stevenson Program on Nuclear Policy, University of California, Santa Cruz

"Tom is an "internationally respected expert on reactor design, the fuel cycle and particularly, recently, the disposal of radioactive waste."

George Jasny, a former student to Tom's at MIT and vice president of Martin Marietta Corporation at Oak Ridge, in an introduction of Tom at a presentation to Friends of Oak Ridge National Laboratory, 2000

"Tom Pigford... has had a fantastic impact on nuclear technology, ... he is really the conscience for our nuclear technology. He embodies the best in the technology that we are all striving for. On the Three Mile Island Commission ... [he was] well known for his moderation, prudence, penetrating insight, and influence in this deliberation."

Dr. Walter Lowenstein, past president, American Nuclear Society, at Tom's 1989 retirement symposium

"As a journalist and writer on environmental and nuclear waste issues my advocacy of Yucca Mountain as a potential repository site had put me in direct conflict with officials of the host state of Nevada and good many of my friends in environmental groups. In these none too comfortable circumstances I joined with a great deal of satisfaction into a collaboration with Tom Pigford in which, as it turned out, we were to prepare several articles together on nuclear waste isolation and spent fuel reprocessing issues. For me, Tom was (and is) a veritable mountain of professional integrity and I wanted my opinions and conclusions measured against his exacting standards. I had been particularly impressed by his dissent as a minority of one on a critical conclusion reached by the National Research Council panel on technical standards of the Yucca Mountain repository project. In defining the 'critical group' the panel chose to look to statistical averaging methods which Tom saw as susceptible to manipulation. For him the only definition that would do was that of a subsistence farm family dependent on a deep well near the proposed repository site for its drinking water and the water used to grow food crops on which the family would rely for much of its diet. This was, as he saw 'the bounding case' and he insisted upon it. To ally myself with this

wise and courageous dissenter has been for me a comfort and matter of no little pride."

Luther Carter, journalist and co-author with Tom of four significant technical articles

"When appointed by President Carter to the Three Mile Island (TMI) Commission, Tom played a key role in developing challenging recommendations to the industry, to ensure that another accident of the magnitude of TMI could not happen, and also to ensure that events of less severity could not occur. In fact, it was Tom's wisdom, judgment, and practical experience that, in my opinion, led to the TMI Commission's recommendations being so well thought out. Those recommendations have stood the test of time and have served as a catalyst for significant change. The Commission has made a difference. The strides made by the industry in the past decade attest to the insight and wisdom of Tom and his fellow commissioners."

Admiral Dennis Wilkinson, captain of the first nuclear submarine, first president of the Institute of Nuclear Power Operations (INPO), which was created by the utility industry after the Three Mile Island accident

APPENDIX C

CITATIONS

Boston Junior Chamber of Commerce, 3/12/56:

"Boston Junior Chamber of Commerce honors Thomas H. Pigford, one of the Outstanding Young Men of Greater Boston for the year 1955. Through his outstanding achievement and unselfish efforts he has made a great contribution to his community and his chosen field of endeavor."

American Nuclear Society, 6/15/71:

"The American Nuclear Society Confers the Membership Grade of Fellow upon Thomas H. Pigford for his outstanding contributions to the advancement of nuclear Science and Engineering."

American Nuclear Society, 6/15/71, Arthur Holly Compton Award:

"In recognition of his many outstanding contributions to nuclear engineering education, including his key role in establishing nuclear engineering departments at the Massachusetts Institute of Technology and the University of California at Berkeley, and for a distinguished career as a nuclear engineer, author, and researcher."

President Jimmy Carter, 4/11/79:

"Jimmy Carter, President of the United States of America, to all who shall see these presents. Greeting:
Know ye, that reposing special trust and confidence in the Integrity and Ability of Thomas Harrington Pigford, of California, I do appoint him a Member of the Presidential Commission on the Accident at Three Mile Island, and do authorize and empower him to execute and fulfill the duties of that office, with all the powers, privileges, and emoluments thereunto of right appertaining, unto him the said Thomas Harrington Pigford during the pleasure of the President of the United States for the time being."

(Signed by President Jimmy Carter and Secretary of State Cyrus Vance)

U.S. Geological Survey, 3/8/81, John Wesley Powell Award for Citizen's Achievement:

"For contribution to the programs of the U.S. Geological Survey".

American Institute of Chemical Engineers, 11/19/80, Robert E. Wilson Award:

"To Thomas H. Pigford, whose clear thought and wise and persuasive counsel as an educator, researcher, and adviser to government and industry have made major and lasting contributions to the safe and effective development of nuclear energy."

American Nuclear Society, 11/1/83:

"This Silver Certificate in recognition of twenty-five years of continuous membership is awarded to Thomas H. Pigford. With sincere appreciation of your valuable contributions during twenty-five years of continuous membership in the American Nuclear Society. Your cooperation and assistance have helped materially in enabling the Society to experience sound growth and to make notable progress in accomplishing the goals and objective for which it was founded."

American Institute of Chemical Engineers, 11/12/85:

"The American Institute of Chemical Engineers presents its annual SERVICE TO SOCIETY AWARD to Thomas Harrington Pigford, reflecting outstanding initiative and unselfish contributions toward the solution of societal problems."

(Accompanied by a check for \$10,000)

Berkeley Citation, 6/87:

"The University of California, Berkeley, honors Thomas H. Pigford for distinguished achievement and for notable service to the University."

Institute of Nuclear Power Operations, 9/16/87:

RESOLUTION by the Board of Directors of the Institute of Nuclear Power Operations, September 16, 1987:

RESOLVED, that the Board of Directors of the Institute of Nuclear Power Operations recognizes the exceptional contributions of the Advisory Council. Since the Institute's very inception, the council has been a strong and persistent force in the quest for excellence in the performance of the nation's nuclear power plants.

Moreover, the Advisory Council itself has embodied this same

concept of excellence in its own performance.

FURTHER RESOLVED, that the Board of Directors recognizes and appreciates the outstanding contribution of Dr. Thomas H. Pigford as a charter member, and as chairman of the Advisory Council for its first eight years. Dr. Pigford's leadership and direction of the Council, his ability to probe and to articulate issues, and his untiring pursuit of excellence have been a source of inspiration to the Institute's staff, directors, and Council members alike. The Board further recognizes that Dr. Pigford's years of service to the Institute represent only a portion of a lifetime dedicated to the peaceful use of nuclear energy.

THEREFORE, the Board of Directors, through this resolution, extends its utmost respect and gratitude to the Advisory Council and to Dr. Pigford for his unwavering commitment and exceptional service to the Institute and the Industry.

National Research Council, 6/89:

"The Energy Engineering Board, Commission on Engineering and Technical Systems, Certificate of Appreciation in recognition of outstanding service, to Thomas H. Pigford."

Georgia Institute of Technology, 1995, Elected to Engineering Hall of Fame.

"The Georgia Institute of Technology honors Thomas H. Pigford for his outstanding contributions to engineering education and practice and for his dedication to achieving the highest standards of performance, integrity, and safety in the peaceful applications of nuclear energy."

APPENDIX D

Department of Nuclear Engineering
University of California
Berkeley, California 94720

July, 2001

CURRICULUM VITAE FOR
THOMAS H. PIGFORD

POSITION AND AFFILIATION

Professor of Nuclear Engineering (Emeritus), Graduate Professor
University of California, Berkeley
Berkeley, California 94720

Tel: 510-652-0393

Fax: 510-652-0747

E-mail: pigford@nuc.berkeley.edu

Home: 166 Alpine Terrace, Oakland, California 94618

DATE AND PLACE OF BIRTH

Meridian, Mississippi

April 21, 1922

EDUCATION

B.S. in Chemical Engineering, Georgia Institute of Technology (magna cum
laude), 1943

S.M. in Chemical Engineering, Massachusetts Institute of Technology, 1948

Sc.D. in Chemical Engineering, Massachusetts Institute of Technology, 1952

FAMILY

In 1948 Pigford married Catherine Kennedy Cathey, of Florence, Alabama, now
deceased.

There are two daughters: Cynthia Naylor, of Durham, California, and Julie

Brink, of Albany, California.

Pigford married Elizabeth Weekes, of Berkeley, California, in November, 1994.

EMPLOYMENT HISTORY

Dr. Pigford is Professor Emeritus and Graduate Professor of Nuclear Engineering at the University of California, Berkeley. He joined the University in 1959 to become the first Chairman of the new Department of Nuclear Engineering, for the period 1959-1964. He was appointed again as Department Chairman for the periods 1974-1979 and 1984-1988. He has also been Senior Scientist at the Lawrence Berkeley Laboratory of the University of California.

During 1959-1964 Dr. Pigford led the development of the new Neutronics Research Laboratory at the University of California, including the construction of the University's 1 Megawatt research nuclear reactor. He served as Reactor Administrator from 1959-1964, 1974-1979, and 1984-1988. He officially retired from the University of California in 1991, but since then he has continued his affiliation as Professor of the Graduate School.

During the period 1957-1959 Dr. Pigford participated in the founding of the General Atomic Laboratory in LaJolla, California. He was Director of Engineering, Director of Nuclear Reactor Projects, and Assistant Director of the Research Laboratory.

Dr. Pigford began his teaching career at the Massachusetts Institute of Technology. In 1946-1947 he was Instructor in Chemical Engineering and Assistant Director of the M.I.T. Graduate School of Chemical Engineering Practice at Parlin, N.J. From 1950 to 1952 he was Assistant Professor of Chemical Engineering and Director of the M.I.T. Graduate School of Engineering Practice at Oak Ridge, Tennessee. In 1952 he joined Professor Manson Benedict in inaugurating M.I.T.'s new graduate program in Nuclear Engineering at Cambridge, Mass. In 1955 he became Associate Professor of Nuclear and Chemical Engineering at M.I.T.

In 1952 he was Senior Development Engineer with the Aqueous Homogenous Nuclear Reactor Project, Oak Ridge National Laboratory.

PROFESSIONAL FIELD

Dr. Pigford's professional field has spanned a broad range of science and technology in chemical engineering, combustion, and nuclear engineering. Since 1950 he has been active in nuclear reactor design, nuclear reactor safety, operations and analysis of the nuclear fuel cycle, chemical engineering of

effluent control technologies, analysis of the release and transport of toxic contaminants in the environment, radioactive waste management, and the behavior and transport of toxic waste buried in geologic media. He is an active consultant to industry and government in these areas.

In 1959 Dr. Pigford initiated research at the University of California, Berkeley, on the diffusion of fission gases from uranium-carbide nuclear fuel, materials for direct conversion of fission energy to electrical energy, and thermionic fuel elements to convert fission heat to electrical energy.

His subsequent research in nuclear-chemical engineering led to the theoretical analysis of nuclear fuel cycles for uranium and thorium fueling of commercial light-water nuclear reactors, heavy-water reactors, gas-cooled reactors, and fast-breeder reactors. These analyses included reprocessing requirements, environmental controls, properties of radioactive waste, special fuel cycles to minimize proliferation of material for nuclear weapons, and special fuel cycles for partitioning and transmuting recycled actinides and fission products.

Since 1977 Dr. Pigford has led a research program at the University of California to develop theoretical means of predicting the long-term behavior of underground disposal of radioactive and chemical waste. Results of this research have been used in the design of geologic repositories for radioactive waste in the U.S. and abroad.

Beginning in 1993 he has been also engaged in studies related to nuclear weapons proliferation and arms control.

RESEARCH AND PUBLICATIONS

Dr. Pigford is coauthor, with Professor Manson Benedict, of the textbook "Nuclear Chemical Engineering", published by McGraw Hill in 1958. In 1983 Benedict and Pigford published a substantial revision of this text, together with an additional author, Dr. Hans Levi. The text has been translated into Russian and Japanese.

Dr. Pigford is author and coauthor of over 280 research papers and reports on nuclear reactor safety, nuclear reactor design, environmental control, and management of radioactive and toxic waste. His research group has included other faculty and professional research staff and graduate students working on research dissertations. The research has been sponsored by the U.S. Department of Energy and by government organizations in Sweden and Japan.

Since 1960 Dr. Pigford has been Editor of the McGraw Hill textbook Series in Nuclear Engineering.

Dr. Pigford holds several patents in the fields of chemical and nuclear

engineering.

PROFESSIONAL SOCIETIES, HONORS, AND AWARDS

Dr. Pigford was elected to the National Academy of Engineering in 1976. The Academy cited his contributions in nuclear reactor safety, nuclear fuel-cycle analysis, nuclear chemical engineering, and nuclear education. He was a member of the Academy's Honors and Awards Committee from 1985 through 1987 and was Chairman in 1986. He has been a member of the Academy's Peer Selection Committee for new membership.

Dr. Pigford is a cofounder and charter member of the American Nuclear Society. He is a former Director of the Society. In 1971 he was elected Fellow.

In 1971 Dr. Pigford received the Arthur H. Compton Award of the American Nuclear Society for his contributions in nuclear reactor safety and in nuclear engineering education. He received a Founder's Award in 1980.

From 1963 through 1974 Dr. Pigford was a member of the National Atomic Safety and Licensing Board Panel of the U.S. Atomic Energy Commission and of the U.S. Nuclear Regulatory Commission. He was a member of the first Atomic Safety and Licensing Board to evaluate nuclear reactor safety.

In 1979 Dr. Pigford was appointed by President Jimmy Carter to the Presidential Commission on the Accident at Three Mile Island.

In 1980 he was selected by representatives of the U.S. nuclear electric utility companies to be the first Chairman of the Advisory Council for the new Institute of Nuclear Power Operations, an organization founded by the U.S. utilities to promote safety and operations excellence in the aftermath of the accident at Three Mile Island. He served through 1988.

The American Institute of Chemical Engineers honored Dr. Pigford with the Robert E. Wilson award in 1980 and the Service to Society award in 1985.

In 1980 he received the John Wesley Powell Award given by the U.S. Geological Survey.

In 1986 he was appointed by the U.S. Secretary of Energy to be a member of the Expert Consultant Group to Evaluate the Chernobyl Accident and the N Reactor at Hanford.

In 1982 he was appointed by the U.S. Secretary of Energy to the Committee to Evaluate the New Production Reactor.

From 1980 through 1993 he was Chairman of the Advisory Committee on Nuclear Reactor Safety and Operations for the River Bend Nuclear Plant in Louisiana,

reporting to the Board of Directors of the Gulf State Utilities Co.

In 1987 he received the University of California, Berkeley award for "distinguished achievement, and for notable University service."

In 1988 he was appointed a member of the delegation from the U.S. National Academies of Science and Engineering to establish a program in Reactor Safety Cooperation with the USSR.

From 1987 through 1989 he was a consultant to the Board of Directors of the Pacific Gas & Electric Co.

In 1988 he was a member of the Oversight Committee for the Rancho Seco Plant, Sacramento Municipal Utility District.

From 1986 through 1990 he was a member of the General Electric Company's Advisory Committee on Nuclear Safety, for the government-sponsored SP-100 Project to develop a nuclear power plant for space applications.

From 1968 through 1976 he was a member of the Criticality Hazards Review Committee for the Union Carbide and Carbon Co., Y-12 and K-25 Plants, Oak Ridge, Tenn.

In 1995 he was elected to the Engineering Hall of Fame of the Georgia Institute of Technology.

Research in Radioactive Waste Management

From 1978 through 1985 Dr. Pigford was a member of the Board on Radioactive Waste Management of the National Research Council, the research arm of the National Academies of Science and Engineering.

From 1978 through 1981 he was a member of the National Research Council's Oversight Panel for the Waste Isolation Pilot Plant, an underground radioactive waste repository in New Mexico.

In 1980-83 he was Chairman of the National Research Council's Panel for the Waste Isolation System Study, a study to evaluate the U.S. program for development of a future geologic repository for the disposal and isolation of high-level radioactive waste.

In 1984 he was a member of the U.S. Department of Energy's Performance Assessment National Review Group for High-Level Radioactive Waste Management.

From 1986 through 1991 he was a member of the Expert Consultant Group for High-Level Waste Disposal: Project 90, for Sweden's Nuclear Power Inspectorate.

In 1975-1976 he was a member of the American Physical Society's Committee to Evaluate Nuclear Fuel Cycles and Waste Disposal.

In 1977 he was a member of the National Peer Review Panel for Geologic Criteria for Radioactive Waste Isolation, U.S. Nuclear Regulatory Commission.

In 1987 he was a member of the U.S. Department of Energy's Committee to Review Site Characterization for the Basalt Waste Isolation Plant Project at Hanford Washington.

In 1986-1995 he was a member of the Peer Review Committee for Performance Assessment, for the New Mexico Waste Isolation Pilot Plant, Sandia Laboratories.

In 1991-1995 he was a member of the National Research Council Panel for Separations Technology and Transmutation Systems.

In 1993-1995 he was a member of the National Research Council Panel on Technical Bases for the Yucca Mountain Standard.

In 1993-1994 he was a member of the National Academy of Science Panel on Reactor Options for the Disposition of Military Plutonium.

In 1996-1997 he was a member of the Committee for the International Symposium on Nuclear Fuel Cycle and Reactor Strategies: Adjusting to New Realities," International Atomic Energy Agency, Vienna, Austria.

Service to Educational Institutions

Dr. Pigford was a member of the Massachusetts Institute of Technology Corporation's Visiting Committee for the Department of Nuclear Engineering in 1977-1983.

He was a member of the Visiting Committee for the Department of Chemical and Nuclear Engineering, University of California, Santa Barbara, California in 1984-1990.

In 1993 he was a member of the Search Committee for Reactor Technology Chair for the Royal Institute of Technology, Stockholm, Sweden.

Other Public Service

In 1984-1988 he was a member of the Corporate Advisory Committee for the Oak Ridge National Laboratory.

In 1986-1987 Dr. Pigford was a member of the National Research Council's Panel

on Fission-Fusion Hybrids.

In 1980-1985 he was a member of the Visiting Committee for the Materials Science Division, Los Alamos National Laboratory.

In 1985-1991 he was a member of the University of California's Oversight Committee on Environmental Health and Safety for the Lawrence Livermore National Laboratory, the Los Alamos National Laboratory, and the Lawrence Berkeley Laboratory.

He has served on the Visiting Review Committee for the Magnetic Fusion Energy Program, Lawrence Livermore National Laboratory.

Additional Consulting to Industry and Government

E. I. DuPont de Nemours & Co., Savannah River Plant, reactor operations and chemical separations, 1954-1957.

Pratt and Whitney Aircraft Corporation, development and design of a nuclear reactor for aircraft propulsion, 1953-1957.

Foster Wheeler Corporation, design of nuclear power reactors, 1953-1957.

General Atomic, design of nuclear reactors for marine propulsion and for central-station nuclear power, 1959-1968.

Electric Power Research Institute, nuclear plant safety, radioactive waste management, programmatic review, 1976-1993.

Lawrence Livermore Laboratory, nuclear space power systems, nuclear and chemical engineering, 1959-1977.

Bechtel Corporation, nuclear reactor design, 1983-1984.

Science Applications Inc., nuclear fuel cycles and radioactive waste management, 1977, 1989.

Rockwell Hanford Operations/Westinghouse Hanford Operations, committee to evaluate the project to dispose of commercial high-level radioactive waste in basalt, 1986.

Battelle Pacific Northwest Laboratory, radioactive waste management, 1985-1988.

Atomics International, design and safety analysis of fast-breeder reactors, 1965-1969.

Thermo Electron Corporation, thermionic energy conversion, 1960-72.

R&D Associates, direct energy conversion in a nuclear reactor, 1965-1968.

Teknekron Corporation, nuclear and fossil fuel cycles, environmental control, nuclear plant safety, 1978.

S. Cohen & Associates, radioactive waste management, 1988, 1991, 1997.

Crystal Energy Corporation, enhanced combustion systems, 1989-90.

Oak Ridge Associated Universities, nuclear facility safety, service to DOE, 1989.

Chemnuclear Corporation, radioactive waste management, 1985.

U.S. Federal District Court, Fifth District, Yakima, Washington, Scientific Master of the Court concerning radioactive releases from the U.S. Government operations at Hanford, Washington, 1994.

APPENDIX E

July 2001

THOMAS H. PIGFORD**LIST OF PUBLICATIONS**Books

M. Benedict, T. H. Pigford, "Nuclear Chemical Engineering", New York: McGraw-Hill, 1958.

M. Benedict, T. H. Pigford and H.W. Levi, "Nuclear Chemical Engineering", Second Edition, New York: McGraw-Hill, 1981.

Reports and Journal Publications

1. T.H. Pigford, "Study of the Behavior of Clouds of Contaminants in the Lower Atmosphere in the K-25 Area," MIT Report K-904, April 1952.
2. T.H. Pigford, "Explosion and Detonation Properties of Mixtures of Hydrogen and Oxygen and Water Vapor," ORNL 1322 (1952).
3. M. Benedict, T.H. Pigford, R. Bakal, J.N. Addoms, and W. Bowman, "Engineering Analysis of Non-Aqueous Fluid Fuel Reactors," Reactor Science and Technology 4, 109-131 (1954).
4. T.H. Pigford, M. Troost, J.R. Powell, and M. Benedict, "Neutron Lifetimes and Void Coefficients for Research Reactors," A.I.Ch.E. 2, 219 (1956).
5. M. Benedict and T. H. Pigford, "Nuclear Chemical Engineering," McGraw-Hill Book Company (1957).
6. T.H. Pigford and M. Benedict, "Fuel Cycles in Single Region Thermal Reactors," Chem. Eng. Progress 53, 96-104 (Part I) and 145-151 (Part II) (1957).
7. T.H. Pigford, S. Bernsen, R. H. Howard, and R. Wallace, "The Marine Gas-Cooled Reactor Program," Proc. Oak Ridge Symp. on Gas-Cooled Reactors (1958).

8. T.H. Pigford, M. Benedict, R. Shanstrom, C.N. Loomis, and R. Van Ommeslaghe, "Fuel Cycles in Single-Region thermal Power Reactors," Proc. of Second Int. Conf. on the Peaceful Uses of Atomic Energy 13, (1958).
9. D.A. Jenkins, K.R. Van Howe, and T. H. Pigford, "Effect of Temperatures on Uranium-Thorium Fueling," Trans. Am. Nuc. Soc. 3 No.2, 400 (1960).
10. A.C. Jones, J.S. Martinez, T.H. Pigford, and A.J. Kirschbaum, "Angular Distribution of Neutrons from a Graphite Surface," Trans. Am. Nuc. Soc. 3, No. 2, 466 (1960).
11. J.S. Martinez, T.H. Pigford, and A.J. Kirschbaum, "Neutron Self-Shielding in Slab Absorbers," Trans. Am. Nuc. Soc. 4, No. 1, 156 (1961)
12. J.S. Martinez, T.H. Pigford, and A.J. Kirschbaum, "The Angular Distribution Shift Effect," Trans. Am. Nuc. Soc. 4, No. 2, 283 (1961).
13. R. Shaked, T.H. Pigford, D.R. Olander, "Calculation of the Fractional Release of Fission Gas from Solid Specimens During Post-Irradiation Anneal Experiments," Trans. Am. Nucl. Soc. 6, No.1, (1963).
14. H. Shaked, T.H. Pigford, D.R. Olander, "Diffusion of Xe-133 in Uranium Monocarbide," Trans. Am. Nuc. Soc. 6, No. 1, 131-132 (1963).
15. J.R.L. deLadonchamps and T.H. Pigford, "Effective Concentration in Lumped Fuel For Reactivity Lifetime Analysis," Trans. Am. Nuc. Soc., 6, No. 2, 269-270 (1973).
16. J.R.L. deLadonchamps and T.H. Pigford, "Effects of Flux Changes Upon Reactivity Lifetime and Burnup," Trans. Am. Nuc. Soc. 6, No. 2, 270-271 (1973).
17. F. Ruffeh, T.H. Pigford and D.R. Olander, "The Solubility of Helium in UO₂," Trans. Am. Nucl. Soc. 7, No. 1 90-91, (1964).
18. S.D. Lowe, T.H. Pigford, and P.L. Chambre', "Release of Fission Gas by Simultaneous Diffusion and Evaporation," Trans., Am. Nuc. Soc., 7, No.1, 95-96 (1964).
19. F.Ruffeh, T.H. Pigford, and D.R. Olander, "The Solubility of Helium in UO₂," Nucl. Sci. and Engr. 23 335-338 (1965).
20. H. Shaked, T.H. Pigford, and D.R. Olander, "Diffusion of Xenon in Uranium Monocarbide," Nucl. Sci. and Engr. 29 122-130 (1967).
21. D.R. Koenig and T.H. Pigford, "Work Function and Cesium Desorption Energies for Planar Tungsten Single Crystals," Proc. of M.I.T.

Conference on Physical Electronics, March, 1966.

22. D.R. Koenig and T.H. Pigford, "Work Function Depression and Surface Ionization for Planar (100) Tungsten Exposed to Cesium, "Proc. of M.I.T. Conference on Physical Electronics, March, 1966.
23. R. Wichner and T.H. Pigford, "Work Functions of Monocrystalline and Polycrystalline Rhenium," Proc. of the 1966 IEEE Thermionic Conversion Specialist Conference, Houston, Nov. 1966.
24. T.H. Pigford and J.B. Dunlay, "Thermoelectric Fuel Element, "U.S. Patent No. 3, 282, 741, Nov. 1, 1966.
25. P.L. Chambre', L.M. Grossman, T.H. Pigford, L. Ruby, V.E. Schrock, and H.P. Smith, "Berkeley Kinetic Studies, "BNL Conference on Nuclear Kinetics, 1967.
26. T.H. Pigford, "Nuclear Reactor Systems and Containment," Conference on Prestressed Concrete Nuclear Reactor Structures, Berkeley, 1968.
27. E.A. Guignard and T.H. Pigford, "Effect of Diode Geometry on Saturated Ion Current," Proc. 1968 IEEE Thermionic Conversion Specialist Conf., 247-253 (1968).
28. B.E. Thinger, T.H. Pigford, D. Lieb, and F. Rufeh, "Cesiated Work Functions of 0001 Rhenium," Proc. 1969 IEEE Thermionic Conversion Specialist Conf., (1969).
29. B.E. Thinger and T.H. Pigford, "Performance Characteristics of a 0001 Rhenium Thermionic Converter," Proc. 1969 IEEE Thermionic Conversion Specialist Conf. (1969).
30. C. Wang, D. Lieb, and T. H. Pigford, "Extended Space Charge Theory in the Emitter Sheath," Proc. 1969 IEEE Thermionic Conversion Specialist Conf. (1969).
31. J.C. Lee and T.H. Pigford, "An Analytical Extension of the Bethe-Tait Model for Zoned Fueling and Cylindrical Cores," Trans. ANS, 12, 917-8 (1969).
32. J.C. Lee and T.H. Pigford, "Analytical Estimate of Doppler-Controlled Energy Release in a Fast Reactor Disassembly Transient," Trans. NAS, 12, 2, 918-9 (1969).
33. J.C. Lee and T.H. Pigford, "Effect of Variable Fuel Density in a Disassembly Transient, "Trans. ANS, 12, 2, 919-20 (1969).

34. J.C. Lee and T.H. Pigford, "Effect of Core-Zoning on Energy Release in a Fast Reactor Disassembly Transient," *Trans. ANS*, 13, 1, 366 (1970).
35. T.H. Pigford, "Oxygen Effects in Cesium Thermionic Diodes," *Thermo Electron Report*, (NASA NAS 3-14413), 1969.
36. B. Gunther, D. Lieb, F. Rufe, and T.H. Pigford, "Cesium Solutions as a Source of Cesium and Oxygen," *Proc. 1970 Thermionic Specialists Conf.* (1970).
37. D. Lieb, F. Rufe, K. Stahlkopf, and T. H. Pigford, "The Formation of Metal-Oxygen Compounds for Additive Thermionic Converters," *Proc. 1971 Thermionic Specialists Conf.* (1971).
38. T.H. Pigford, "Protection of the Public from Radioactivity Produced in Nuclear Power Reactors," *IEEE Trans. on Nucl. Sci.* 19, 1, 15-26 (1972).
39. J.C. Lee and T.H. Pigford, "Explosive Disassembly of Fast Reactors," *Nucl. Sci. and Engr.* 48, (1), 28-44 (1972).
40. T.H. Pigford, M.J. Keaton, B.J. Mann, "Fuel Cycles for Electrical Power Generation," *Teknekron Report No. EED 101* (EPA Contract No. 68-01-0561), (1973).
41. R. Gavankar, P. Durbin, T.H. Pigford, "A Basis for Assessment of Radiation Risks for a Hypothetical Release of Plutonium Dioxide from a Fast Reactor Accident," *Proc. Topical Meeting on Fast Breeder Reactor Safety*, *Amer. Nuc. Soc.*, April 2-4, 1974.
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43. H. Sekimoto and T.H. Pigford, "A New Method for Calculating Space-Dependent Burnup," *Trans. Am. Nucl. Soc.* 18, 137-138 (1974).
44. T.H. Pigford, "Radioactivity in Plutonium, Americium, and Curium in Nuclear Reactor Fuel," *Conference on Environmental Aspects of Nuclear Power Generation*, University of California (Engineering Extension), Sept. 1973.
45. T.H. Pigford, "Radioactivity in Plutonium, Americium, and Curium in Nuclear Reactor Fuel," *Energy Policy Project*, Ford Foundation, June 1974.
46. J.T. Ward and T.H. Pigford, "A Dose Rate Kernel for Air Scattered Gamma Rays," *Trans. Am. Nucl. Soc.* 19, (1974).

47. Thomas H. Pigford, "Environmental Aspects of Nuclear Energy Production," Annual Reviews -Nuclear Sci. 24, 180-194 (1974).
48. T.H. Pigford, R.T. Cantrell, K.P. Ang, and B.J. Mann, Fuel Cycle for 1000 Mw High Temperature Gas Cooled Reactor, EPA Contract 68-01-0561, Teknekron EEED 102 (1975).
49. T.H. Pigford, R.T. Cantrell, and K.P. Ang, Fuel Cycle for 1000 Mw Uranium-Plutonium Water Reactor, EPA Contract 68-01-0561. Teknekron EEED-103 (1975).
50. T.H. Pigford and K.P. Ang, "The Plutonium Fuel Cycles," Second Annual Sciences Symposium "Plutonium-Health Implications for Man," Los Alamos (1974) Health Physics, Vol. 29, pp 451-468, October, 1975, Pergamon Press.
51. T.H. Pigford, "The Analysis of The Cost of Electrical Energy from Nuclear Power Plants," UCB-NE 3008 (Rev. 2), Sept. 1976.
52. T.H. Pigford and J. Choi, "Effect of Fuel Cycle Alternatives on Nuclear Waste Management," Proc. Symposium on Waste Management, ERDA CONF-761020 October, 1976.
53. T.H. Pigford and J. Choi, "Transmutation of Radionuclides in Power Reactors," UCB-NE 3241, Dec. 1976.
54. T.H. Pigford, "Fuel Cycle Alternatives for Nuclear Power Reactors," IEC Fundamentals, 16, No. 1, 75-81, February, 1977.
55. T.H. Pigford, "Nuclear Power Fuel Cycles," UCB-NE 3224, March 1977.
56. T.H. Pigford, "Start Up of First-Generation Fast Breeders with Plutonium or Enriched Uranium," UCB-NE 3240, March, 1977.
57. T.H. Pigford, "The Possibility of Nuclear Proliferation with Pa-231," UCB-NE-3238, March, 1977.
58. "Report to the American Physical Society by the Study Group on Nuclear Fuel Cycles and Waste Management," Rev. Mod. Phys, 50, No.1, Part KK, Jan., 1978 (with L.C. Hebel, E.O. Christensen, F. A. Donath, W.E. Falconer, L.J. Lidofsky, E.J. Moniz, T.H. Moss, R.L. Pigford, G.I. Rochlin, R.H. Silsbee, M.E. Wrenn).
59. T.H. Pigford, C.S. Yang, M. Maeda, "Denatured Fuel Cycles for Safeguards Control," Trans. ANS 27, 187, November, 1977.
60. T.H. Pigford and J.S. Choi, "Actinide Transmutation in Fission

- Reactors," Trans. ANS 27, 450 November, 1977.
61. T.H. Pigford and J.S. Choi, "Economics of Fuel Cycle Options in a PWR," Trans. ANS 27, 463, November 1977.
 62. T.H. Pigford and J.S. Choi "Economics of Fuel Cycle Options in a Pressurized Water Reactor," UCB-NE-3242, November, 1977.
 63. T.H. Pigford, "Properties of Radioactive Wastes to be Emplaced in a Geologic Repository," UCB-NE-3317, March, 1978.
 64. T.H. Pigford, "Radioactivity in Stored Coal Ash and In Nuclear Power Waste," UCB-NE-3323, April, 1978.
 65. T.H. Pigford, "Properties of Class A Radioactive Wastes: A Review of Adequacy of Available Information for A Generic Environmental Impact Statement for Waste Management in a Geologic Repository," UCB-NE-3297, January 1978.
 66. T.H. Pigford, "Thermoelasticity for Isotropic Homogenous Solids," UCB-NE-2992, March, 1974.
 67. T.H. Pigford, C.S. Yang, M. Maeda, "Denatured Fuel Cycles for Safeguards Control," Nuclear Technology, November, 1978.
 68. T.H. Pigford, "Radioactivity in Stored Coal Ash and in Nuclear Power Waste," Trans. ANS, November, 1978.
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APPENDIX F

August 2001

INTERVIEWS ON THE HISTORY OF THE UNIVERSITY OF CALIFORNIA

Documenting the history of the University of California has been a responsibility of the Regional Oral History Office since the Office was established in 1954. Oral history memoirs with University-related persons are listed below. They have been underwritten by the UC Berkeley Foundation, the Chancellor's Office, University departments, or by extramural funding for special projects. The oral histories, both tapes and transcripts, are open to scholarly use in The Bancroft Library. Bound, indexed copies of the transcripts are available at cost to manuscript libraries.

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B.A., 1984, University of Arizona, Asian Studies (minor in Forestry).

M.A., 1988, University of Hawaii, Geography. Thesis title: Forest Perception and Management in Kochi Prefecture, Japan, based on a year and a half of field work in Japan.

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Interviewer/Editor, Regional Oral History Office, in the field of natural resources and the environment, 1997-2000.

Program Coordinator, U.S. Community Forestry Research Fellowship Program, College of Natural Resources, University of California, Berkeley, 2000-present.

INDEX

- acceptable hazards, 110-111
 Alexandrite, 125
 Alvarez, Louis, 77
 alternatives for electric power generation, 55
 American Physical Society, 182
 American Institute of Chemical Engineers, 86
 American Nuclear Society, 85
 American Standard, 60
 Anderson, Clinton, 49
 Amster, Harvey, 87
 anti-Vietnam war, 102
 aqueous homogenous reactor, 23,26-30,56,62,68
 arms control, 188,141,170, 174,179,198,253
 Arabella, 8
 Arab oil embargo, 130,131
 Argonne National Laboratory, 88
 assistant professor, 23,84,96,98,243
 Atomic Energy Commission, 35-38,46, 56,62-67,79,101-107,112-119,139, 152,169
 Atomic Safety and Licensing Boards, 46,105,115-118,152,157
 Atoms for Peace Initiative, 43,51,113,242

 Babbit, Bruce, 142
 Babcock and Wilcox, 45,60,63,144,164
 Barnwell plant, 132
 Barrett, Cora, 142
 Battelle Laboratories, 162-185,191
 Bechtel Corporation, 47,50,61,157,164,178
 Benedict, Manson, 17,23,31,34,36, 37,42,72,75,120,129,176,251
 Berkeley research reactor, 38,74,76, 80,89,125,161,198
 volleyball court, 78
 radioactive emissions, 78
 chamber music, 237
 Chambre', Paul, 115, 141, 180-185, 188
 charcoal adsorber, 66
 chemical engineering
 at Berkeley, 5, 12, 81, 104-105, 117-118
 brother, 5, 173, 217
 at Georgia Tech, 6, 12, 217
 at MIT, 13, 15-17,242, 107, 120, 164, 171, 219
 at Savannah River, 174
 in Germany, 58
 chemical warfare, 6
 funding, 79-80
 staff, 79
 neutron diffraction, 80
 decommissioning, 83,198
 Berkeley Tennis Club, 252
 Bethe, Hans, 72,92,93,108,136
 bioengineering, 97
 biomedical, 5,127
 biological effects of radiation, 81,89,127
 biosphere, 183
 Blume, John, 156
 Board on Radioactive Waste Management, 203,212,238
 Bodega Bay, 85,100,101,111,223
 Boelter, Llewellyn, 74
 boiling water reactor, 27
 Bond, J. D., 113
 bootleggers, 4
 breeder reactor, 55,56,62,88,89,107, 112,178,231,237
 Brink, Carolyn, 252
 Brink, David, 252
 Brink, Eric, 252
 Brink, Julie, 252
 Brezinsky, Zbignew, 148
 Brookhaven National Laboratory, 106
 Brown, Jerry, 135,139
 Burkholder, Harry, 182,184-185,205

 calcium carbonate, 25
 California Public Utilities Commission, 138,139,148,156,159
 California restrictions
 Proposition 15, 135
 campus committees, 84
 capillary barriers in China and Japan, 217-219
 Carter, President, 126, 140
 Carter, Luther, 126, 216, 221,224
 castor oil, 4, 18
 caviar, 167
 Chernobyl reactor, 54,63,141,161-166
 graphite burning, 163
 operator error, 163
 sarcophagus, 166
 steam explosion, 16
 Choi, Jor-Shan, 125
 church, 2
 Citizens for Safe Energy, 154
 Civil Rights, 18
 clarinet, 8, 237
 Clear Lake, 61
 cloud propagation, 25
 closed-cycle gas turbine, 49
 coal heating, 7
 cold war effects, 90

College of Chemistry, Berkeley, 6
 College of Engineering, Berkeley, 6, 69-70
 Combustion Engineering, 43,56,60
 combustion project, 19
 committee on environmental health and safety, 234
 committee on privilege and tenure, 88,103,247,248
 Committee for the Technical Bases for Yucca Mountain Standard, 212
 commercial nuclear power, 23,40,44, 53,56,57
 Commonwealth Edison, 67
 conch shell, 4
 conflict of interest, 162,217,218
 construction cost, 39,45-48,53-69,120,130- 139,165-177,205,227,235
 construction permit, 46,114,117-118,162
 consulting, 32,36-44,75,92-95,135,139,150- 155,161,169,221
 corrosion, 33,81,88,126,207,208,230
 cotton, 3,14,15
 cow, 8

 Dakin, Phyllis, 251
 Davis, Kenneth, 37, 157
 declassification, 22,35,76
 Denton, Harold, 144-145
 department chairman, 43,82,247
 Depression, (The Great), 1,7,14
 Duma, 180
 Du Pont Company, 6, 41,141,195,219
 Durbin, Patricia, 90,91
 Dyson, Freeman, 93

 Earnest, Carolyn, 244
 Earnest, Catherine, 244
 Edgerton, Harold, 32
 Edgerton, Germeshausen, and Greer, 32,37
 Eisenhower, Dwight, 43,44,48, 51,113,242
 Elberg, Sanford, 78
 Electric Boat Company, 61
 Electric Power Research Institute, 211,213
 Energy Research and Development Administration [ERDA], 118
 environmental concerns, 33,64-84,117, 129,130,141,161,162,168,174, 180,206,220,225,239,247
 Environmental Protection Agency, 69,204,206, 225

 farm, 3,4
 Federal Republic of Germany, 112
 Fermi-1 reactor, 107,110,114,177
 designing nuclear bombs, 33,236
 Detroit Edison, 56,107
 Diablo Canyon, 1,46,70,110-111, 38,152-159,223
 mirror-image problem, 155
 public hearing, 152
 seismic design, 152
 management investigation, 156

 disposal of radioactive waste, 34,68,97,122- 126,136,184-199, 223-233,241-242
 at Yucca Mountain, Nevada, 223,241-242
 criteria for performance, 115,199,203-210,225,238
 in basalt, 199,217
 in deep sea beds, 135
 in granite, 201,226,230
 in natural salt, 128,189,199-201,206-208
 in tuff (volcanic ash), 199-208,222
 interim storage, 180,228,233,239,240
 into the sun, 135,168
 Russian programs, 180
 spent fuel from east Asia, 181
 doctoral dissertation, 20,22,27,251
 Donner Laboratory, 78,81,127,248

 Fermi-2 reactor, 110
 football, 5,11,19,251
 Ford, President, 132
 Foster Wheeler, 40
 Free Speech Movement, 102-104,243-245
 French horn, 9,13,249
 Florence, Alabama, 250-251
 fluid-fuel reactor, 27,30,56
 Fowler, Kenneth, 96
 Frankfurter, Felix, 107
 fuel cycle, 23,34-38,45-55,60,87-88,120,131-132,182
 fuel reprocessing, 129,132-134,178
 Purex process, 41,72
 gardening, 253
 Gainey, Emma Gene, 2, 12
 General Atomic, 47-59,63,72-75,80,178
 General Dynamics, 49, 60
 General Electric Co., 37,41,45,47,60-63,67,69,112,119,163,166
 General Electric Fast Reactor, 112,144
 General Public Utilities, 148
 Georgia Tech, 10,11,16,125,147,252
 geothermal power, 64,226

- NASA, 81,89,135,167-169
National Academies of Science
and Engineering, 162,174,
189,206,224,225
National Research Council, 194,
Nautilus, 143
navy students, 36
Naylor, Cindy, 12,251-252
Naylor, Daniel, 252
Naylor, Matthew, 252
Nelson, Gaylord, 119
Nevada, State of, 154
niobium, 31
North Carolina State College,
36
N-reactor at Hanford, 67,159-172
plutonium and power
production, 159
Wigner disease, 172
Zircaloy tubes, 172
nuclear-bomb propulsion, 50
Nuclear Chemical Engineering,
12, 176-168
nuclear engineering at Berkeley, 72-
84,70-100,124-129,189-190,245
Free Speech Movement, 102-
104,244
laboratory facilities, 75-81
restimulation, 116-117
women and minorities, 245-246
nuclear engineering at Michigan,
36,76
nuclear engineering at MIT, 23,34-
38,42-43
nuclear fuel cycle (see *fuel cycle*)
nuclear fusion, 89,96,125,127
nuclear instrumentation,
84,97,174,245
nuclear magnetic resonance, 36
nuclear materials, 22,88,94,121
nuclear power,
aircraft propulsion, 29-30
nuclear power (cont.)
Antartica, 57
aqueous homogenous, 223,56
carbon-dioxide cooled, 52,61
economics, 119-121,131,142
enriched uranium fuel, 24
fast breeder, 56,62,66,112
Hanford N-reactor, 57,169
heavy-water reactors, 40,54,59
helium cooled, 52-54
hindsight, 242
Overture to William Tell, 13
painter, 5
Pacific Gas and Electric Co.,
46,111,138,152
206,210,214,238-239,241
National Science Foundation,
38,79
National Testing Station, 190
insurance, 100,150
Kuwait, 44
legislation, 135
marine
propulsion, 49,51
organic, 59
outer space, 89,92,166
plutonium fuel, 179
proliferation, 232,236
Sweden, 197
nuclear reactor physics and
criticality,
54,86,108,114,127,172
nuclear reactor safety, 32,38,46,53-
69,83,101-115,126-
130,141-167,
172,175,183,194,295-228,234-237
Nuclear Science and Engineering, 82
nuclear waste research, 131-134,182-
192,232-241
applicability to chemical
waste, 183
borosilicate glass, 236
classical mathematics, 185,190
computer codes, 185,190
decay chains, 185,190
deep sea-bed disposal, 179
dissolution rate, 186,192-3
geologic disposal, 135,182,185-
189
postdoctoral fellows, 149,187
potential flow, 193
transmutation, 182
transport through geologic
media, 125-126
Nyer, Warren, 107
Oak Ridge, Tennessee, 5-6,17-
36,41,56-62, 72,251
Oak Ridge National Laboratory,
5,20,88,95,170
oboe, 249
O'Brien, Michael, 74
Olander, Donald, 94-97,128
organic-cooled reactor, 54-59
Overture to Oberon, 9
Pacific Northwest Laboratory,
186-187
Palladino, Nunzio, 152
part-time jobs, 14
paying college expenses, 9
penicillin, 18

Perry, Fred, 13
 Persephone, 251
 Petersen, Russell, 146
 physics, 10,16-23,34-50,71-103,127,137-138,153,235,243-245
 Pigford, Betty, 250-254
 Pigford, Cindy, 239
 Pigford, Julie, 237
 Pigford, Katy, 250-252
 Pigford, Paul, 4
 Pigford, Robert, 5-6
 Pitzer, Kenneth, 73
 plutonium, 33,40-57,77-90,129-133,166-180
 as nuclear fuel, 132,222,232-235
 civil, 132-14,232-237
 from surplus weapons, 33,40,52-57,72-77,87,133,169,180
 health effects, 90
 space power, 168
 pneumonia, 4
 polarizations, 137-138, 160
 Poole, Oscar, 9
 Pratt and Whitney Aircraft, 32,45
 pressurized-water reactors, 49,53-59,175-177,231
 Princeton Institute of Advanced Study, 93
 Princeton University, 19
 proliferation of nuclear weapons, 129-132,232-234
 public involvement, 85,106,110,114-118,130,134,138,142-148,152,162
 Purex process, 38-39,68
 Pyle, Robert, 84, 91

 racial prejudice, 19
 radar, 18,19
 radioiodine doses, 115,153-154,219
 Reed College, 38
 seismic design, 46,152-157
 Shaked, Hagai, 92
 Shell Development Co., 155-157
 shielding pilots, 31,32
 Shippingport, 47,53
 Sierra Club, 111-112,161
 skiing, 252-253
 smoke bombs, 26
 Smyser, Dick, 5
 Smyser, Katy, 5
 Smyser, Lucy, 5
 Smyser, Mary Pigford, 5-6
 socio-technological issues, 253
 sodium-cooled reactor, 54,58,62,93,108-110
 solar energy, 64,65,131,146,168
 solid-fuel reactors, 64-65,131,46,168
 Southern accent, 17
 religion, 15
 remedial training, 10,98
 reactor meltdown, 66,109
 reactor safety, 32,38,46,53,66-83,101-118,126-130,141-148,158,181
 Reagan, President, 133,152
 research reactors, 38,44,74-80,89,125, 161,198
 Reynolds Aluminum Co., 250-251
 Richmond Field Station, 75-77,248
 Rickover, Hyman, 35-6,47,53,61,47,150
 River Bend Nuclear Plant, 163
 advisory committee, 163-167
 reactor technical problems, 34,45,56-58,60
 reactors in Russia, Ukraine, and Lithuania, 53,173-178
 Roddis, Lou, 172
 resolving conflicts, 160
 Rockwell, 217-218
 roller skating, 5
 Roosevelt, Franklin D, 14
 Rossini, 13

 Sacramento Municipal Utilities District (SMUD),
 advisory committee, 163-165
 Rancho Seco nuclear plant, 163-165
 decommissioning, 165
 Savannah River, 40-41,54,95,195
 Sawyer, Diane, 142
 school band, 2, 8
 Schrock, Virgil, 96, 127
 Schlesinger, James, 119
 scientific master, 218-219
 Seaborg, Glenn, 41,56,72-78,83,87, 93,107,110,115,123,169,221
 Sears Roebuck, 12
 security, 2,35,41-42,148,180
 Soviet Union, 57,169,173-178
 Russian National Academy of Science, 175
 rubles, 175-176
 SP-100 reactor, 166-168
 sports, 11,251,253
 Sputnik, 50,88
 Standard Oil of California, 37
 Stanford University, 37-38,76,157
 starting a company, 37
 State of Nevada, 162,211,240
 State of Pennsylvania, 142-147
 State of Texas,
 40,137,199,201,205,245
 Strauss, Lewis, 36-37
 stringing tennis racquets, 14
 Starkville, Mississippi, 6
 Stockholm Conference, 197,215

- Suez Canal, 51
 Sweden, 174
 international committee, 196-197,226
 interim storage, 226
 project management, 236
- Taylor, Ted, 141,146-147
 teaching with reactors, 39
 Texas Instruments, 141
 Teller, Edward, 72-75,91-95,243
 Tennessee Valley Authority (TVA), 62
 tennis, 2,10-15,251-253
 Thigpen, Mr., 12-14
 Thornburg, Governor, 145
 Three Mile Island accident, 67,139,141,164
 effect on utilities, 149
 emergency cooling, 143-144
 hydrogen bubble, 145
 operator training, 151,173
 Presidential Commission, 141-142,151,161,220
 tomography, 97
 transuranic waste, 199
 tree house, 12
 tritium, 68-70,202-203
 USS Savannah, 51
- Vassar, 252
 Vines, Elsworth, 13
 von Weber, 9
 Vrana, 112,153-4
- Waste Isolation Pilot Plant (WIPP), 163,199, 206-226
 anhydrite layers, 208
 Carlsbad, New Mexico, 206
 oversight committee, 206
 rock bolts, 208
 safety criteria, 209-210,225
 salt creep. 149,205-6
 long-lasting markers, 226
- Waste Isolation system Study, 194,206
- Weekes, Betty Hood, 252-253
 Weekes, Janvrin, 253
 Weekes, Laura, 253
 Westinghouse, 41,45,47,60-61,63,87,217-218,221
 whiskey, 4-5
 Wilkinson, Eugene, 150
 Wilmington, Delaware, 6
 writing textbooks, 35,38,42
 woodworking, 2, 12-14
- xenon, 69-70
- Trunk, Anne, 142,147
- Union Carbide and Carbon, 21
 Union Oil, 64-650
 U.S.Congress,14-51,62,105-113,142-147,168-169,180,201-214,234-241
 U.S.Department of Energy, 121,136,142,162, 6-170,187-188,194-206,213,217-221,233,241
 U.S. Geological Survey, 155,158
 U.S. Navy, 18-19,28,36,47-9,58,87,147,150-151
 U.S. Nuclear Regulatory Commission, 38,107, 118-119, 138,142-148,150-152,156-158,160-165,199,203-214
 U.S. Postal Service, 1
 U.S. Supreme Court, 107-108
 University of California, 41
 University of California at Berkeley, 5,72
 University of Chicago, 108
 University of Delaware, 6,249-250
 University of Illinois, 6,9
 University of Michigan, 36,76
 University of Tennessee, 26
 UK gas-cooled reactors, 54
 uranium hexafluoride, 24
 Yucca Mountain project, 162-6,199-203,210,222-230,238-239
 capillary barrier, 228-230
 carbon-14 releases, 209-210
 chemical reducing agent, 227,230-231
 climate change, 224-225
 depleted uranium, 231-232
 industry oversight, 241
 interim storage, 239-240
 maximally exposed individual, 209-214
 modifications and cost, 227,230-232
 oxidizing environment, 230-231
 population dose, 209
 safety criteria, 204,216,238
 seismic design, 222-223
 uncertainty in dose calculations, 215
 upwelling of ground water, 224
 U.S. Congress, 211-216,234-240
 vicinity-average dose, 213,216,39-240
- zirconium alloy (Zircaloy), 54,58,109